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

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Abstract

Although many countries recruit bureaucrats using affirmative action, the effect of affirmative action recruits on bureaucratic performance has rarely been examined. Many worry that affirmative action worsens bureaucratic effectiveness by diminishing the quality of recruits, while some posit that it improves effectiveness 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. Improved descriptive representation does not come at the cost of efficiency.

Keywords: Affirmative Action; Bureaucracy; India

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In many countries, members of some ethnic groups have lower levels of education, wealth, social connections, and political power than members of other groups, frequently due to discrimination or historical legacies of marginalization. To reverse these inequalities, a wide range of 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 (Arcidiacono 2005; Bertrand, Hanna and Mullainathan 2010), politics (Bhavnani 2017; Chauchard 2014; Dunning and Nilekani 2013; Jensenius 2017) and the private sector (Barrington and Troske 2001; Carter, Simkins and Simpson 2003; Griffin 1992; 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 (Bhavnani and Lee 2018; Bussell 2012; Dasgupta and Kapur 2017; Evans and Rauch 1999; Ferguson and Hasan 2013; Gulzar and Pasquale 2017; Iyer and Mani 2012; Pepinsky, Pierskalla and Sacks 2017; Vaishnav and Khosla 2016).

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 (Bolick 1996; Griffin 1992; Lewis 1997; Lott 2000). This concern is particularly relevant in bureaucracies with meritocratic recruitment procedures, 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

(Keiser et al. 2002; Krislov 2012; Meier and Nigro 1976). More representative bureaucracies might be more willing and able to serve citizens, especially the underprivileged.

This paper will examine the effects of affirmative action in India, which has an influential 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. 2017; Iyer and Mani 2012). While most 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 fairness is fiercely contested (Jaffrelot 2003).

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 marginalized groups and have worse formal qualifications. Often, these effects are observed together, or are very 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 type of statistical aggregation makes most existing empirical work on affirmative action invalid.

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 score. 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 systematically 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, types of administrative districts than others.

To solve this selection problem, we take three steps. First, all reported models contain two sets of fixed effects, one at the administrative 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, both for district-level time varying factors, and officer-level traits unrelated to affirmative action. Third, we employ an instrumental variables estimator, leveraging the fact that bureaucrats early in their careers are quasi-randomly assigned to districts. This 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 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 do also examine the effects of bureaucrats on the implementation of a second major (rural road building) government program as well. Under MGNREGA, all of India's rural households are guaranteed 100 days of unskilled employment on small public works projects. Our measure of bureaucratic effectiveness is therefore the number of households that received 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; Muralidharan, Niehaus and Sukhtankar 2016).

To estimate the effects of affirmative action on bureaucratic output, we examine whether the assignment of an affirmative action hire to a district changes MGNREGA outcomes in that district. Importantly, our analyses do not speak to the question of what would happen if affirmative action in the IAS were scrapped altogether. We instead estimate the marginal effect of replacing an early-career affirmative action hire with a non-affirmative action hire. We find that districts served by affirmative action recruits have similar levels of MGNREGA employment than other districts. The null effect of affirmative action is precisely estimated, and 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.

To explore the mechanisms by which the null estimated effect of affirmative action obtains, we note that all affirmative action recruits are disadvantaged group members and perform worse on the exam used to recruit them. We therefore disaggregate the affirmative action treatment bundle into these two components—disadvantaged group status and exam rank—to find countervailing effects. Disadvantaged group officers perform better than others, but this effect is somewhat attenuated among those with lower exam scores, though the negative effect of lower exam scores is not statistically significant. Consistent with Ferreira and Gyourko (2014) and Anzia and Berry (2011), the data also suggest that the superior performance of disadvantaged officers recruited without affirmative action might be due to skills that the overall recruitment exam score measures poorly.

Our results suggest that, at least within highly 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 a major influence on policy outcomes, particularly in developing countries (Evans and Rauch 1999). However, with a few exceptions focused on police departments (Lott 2000; Steel and Lovrich 1987) or parastatals (Deshpande and Weisskopf 2014), there has been little study of the effect of affirmative action in bureaucracies (Lewis 1997 is an exception). This section will briefly consider the existing literature on affirmative action before discussing the possible positive and negative effects of affirmative action on bureaucratic effectiveness.

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, especially universities. While such programs have multiple goals, most scholarly interest has focused on the effects on the applicants themselves. Many studies have found that affirmative action has positive effects on beneficiaries, measured by earnings and educational outcomes (e.g. Arcidiacono 2005). Others have argued that affirmative action reduces student success by "matching" students to schools they are unprepared to attend, or that welfare gains to successful applicants are negated by welfare 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 the applicants, but rather because they are supposed to benefit the underrepresented group as a whole. Since some election quotas have been implemented randomly or 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), 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). An alternative explanation of these non-effects is that quota candidates in the contexts examined are no less "skilled" than others (Besley et al. 2017).

Perhaps the closest analog to bureaucratic affirmative action is the practice of affirmative action in corporations. While many studies examine the causes of increases in descriptive representation at the firm level (e.g. Kalev, Dobbin and Kelly 2006), a smaller literature examines the effects of increases in diversity on firm performance (Carter, Simkins and Simpson 2003; Deshpande and Weisskopf 2014; Griffin 1992; 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 due to affirmative action, for example comparing firms that contract with the US government and therefore have affirmative action policies with those that do not (Griffin 1992). However, these studies face selection challenges, since contractors may differ systematically from other types of firms.

1.2 Negative Institutional Effects of Affirmative Action

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 (Welch 1976). 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." In the education sector, defenders of affirmative action often concede the existence of a social efficiency loss, though they contend that this is counterbalanced by equity gains or broader externalities (Fryer, Loury and Yuret 2007).

Evidence for the negative effects of affirmative action is mixed. Lott (2000) finds that more diverse police departments have poor performance, though this finding conflicts with Steel and Lovrich (1987), which reports null results. Marion (2009) finds that abolishing affirmative action among government contractors reduced overall costs, leading to efficiency gains. Lewis (1997) finds that minority US federal employees are 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), Indian railway workers (Deshpande and Weisskopf 2014) and American doctors (Davidson and Lewis 1997).

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 (Evans and Rauch 1999), 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.

1.3 Positive Institutional Effects of Affirmative Action

The debate on the institutional effects of affirmative action is far from one sided. Some scholars argue that affirmative action improves institutional performance. While this is not usually the main reason why affirmative action programs are implemented, it would be a powerful answer to the charge that reductions in social inequality are paid for with losses in government efficacy.

Affirmative action might improve bureaucratic performance because recruits from marginalized groups might be more likely to serve members of their own groups effectively. This will either 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 also improves, albeit at a lower rate (leading to gains in equity and efficiency). 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 (Franck and Rainer 2012; Kramon and Posner 2014). Relatedly, affirmative action recruits might feel obliged to serve co-ethnics due to the way in which they were recruited. 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, provides strong support for this hypothesis. These works find that bureaucracies that are similar to the population they serve perform better than other bureaucracies (Keiser et al. 2002; Krislov 2012; Meier and Nigro 1976). Similarly, at least in some circumstances, bureaucrats from an area provide better service than outsiders. This echoes individual level findings on the effects of disadvantaged group teachers (Dee 2005).

Alternatively, affirmative action programs could also improve institutional performance by disrupting preexisting patterns of discrimination (Besley et al. 2017). 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 (Anzia and Berry 2011; Ferreira and Gyourko 2014). 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 with a common origin story and often a 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.

Caste-based affirmative action has been a contentious topic since before independence. The post-independence constitution guaranteed SCs and STs seats 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 seats 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 and its traditions. The IAS is an elite organization, supervising the work of less prestigious "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.[1] 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 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. In the third stage, there is a personal interview and "qualifying" questions on language proficiency. 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 their total main exam scores (that is, the sum of the second and third stage evaluations) 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 and the Indian Police Service.

While the IAS is recruited and paid by the central government, its officers spend much of their careers serving in state government, either as general purpose district administrators or in the state secretariat. At the beginning of their careers, IAS officers are assigned to

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

the "cadre" of a particular state though a complicated process designed to ensure a mix of "local" and outside officers and an even distribution of talent (Iyer and Mani 2012). Bertrand et al. (2017) 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. The district administration has a very broad set of responsibilities (Potter 1996), 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. 2017), 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, perceived competence (Bertrand et al. 2017) 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 is 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,[2] 15% for SCs and 7.5% for STs.[3]

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, 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.[4] 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.[5] In the years since 1995, 22% of disadvantaged group recruits (13% of all recruits) scored above the general cutoff, and thus met the qualifications expected of non-affirmative action candidates. Most (81%) of these "merit" disadvantaged group members were OBC, the most socio-economically advantaged

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

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

[4]These cutoffs mean, for example, that the OBC quota was filled with OBC officers that were ranked between 96 and 466 on the main exam.

[5]A few of these candidates benefited from forms of positive discrimination other than benefiting from a lower exam rank cutoff. They were below the cutoff on the preliminary exam, attempted the exam more times than the maximum for non-disadvantaged group members, or were older than the age limit for non-disadvantaged group members. We return to this issue below.

of the target categories.

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 therefore to outcomes in those districts. 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.[6] To code IAS officers' caste, exam rank, and whether they were recruited via affirmative action, we supplemented this with data from a second government website, 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. To avoid cherry picking from among these, 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 implemented in 2006, MGNREGA guarantees one member of each household at least 100 days of employment on small-scale local 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,[7] who are responsible for

[6] https://supremo.nic.in/knowyourofficerIAs.aspx, accessed February 21, 2017. Previously, this site only included data for current IAS officers, thereby excluding those who had retired, resigned or died in office. The version of the database scraped for this paper includes all IAS officers.

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

the implementation of the program, and the effects of bureaucrats as well.

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 twenty 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 as per their entitlements under this Act," "accord timely sanction to shelf of projects," and "ensure timely release and utilization of funds."[8] For this reason, MGNREGA implementation represents a good test of bureaucratic output, and recent studies on the Indian bureaucracy have used it for this purpose (Gulzar and Pasquale 2017; Muralidharan, Niehaus and Sukhtankar 2016). Naturally, political and social factors also play a role in determining MGNREGA supply and demand (e.g. Marcesse 2018). 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. 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.[9] The raw data are from MGNREGA Public Data Portal.[10] Online Appendix Table A.1 summarizes the data.

To see if our findings generalize beyond MGNREGA, we also examine the effects of

[8] http://nrega.nic.in/Circular_Archive/archive/Roles_responsibilites.pdf, accessed April 24th, 2018.

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

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

bureaucrats on the number of villages newly connected by road under India's premier road building program, the Pradhan Mantri Gram Sadak Yojana (PMGSY). 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 2018). Road building is a complex process, and more able bureaucrats are able to push road construction through the planning, contracting and construction phases. That said, we believe that 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. While we considered supplementing our outcome measures with subjective measures of bureaucratic honesty and performance (similar to Bertrand et al. 2017), such an approach would be inappropriate for our question, given that views of bureaucratic performance 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:

$$Y_{it} = \alpha + \beta AA_{it} + \gamma \mathbf{X_{it}} + \delta_i + \theta_{st} + \epsilon_{it} \quad (1)$$

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 the demand for MGNREGA (dummies for whether districts experienced positive or negative rainfall shocks)[11] and bureaucrat characteristics (their mean age, the proportion of female

[11] These dummies are set to 1 if the rainfall in a district-year deviated from the long-run average rainfall for that district by at least 20%. Underlying data are from Indian Meteorological Department.

bureaucrats, a measure of mean bureaucrat education,[12] the proportion of "local" bureaucrats, who are serving in the state from which they are from, bureaucrats' mean years of experience).[13] We also include 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 Scheduled Castes and Tribes. These variables are intended to capture variation in the ability of politicians to transfer resources from the state government to districts. To control for district-level unobservables, we include district fixed effects (δ). 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 a non-affirmative action IAS officer for an affirmative action recruit, controlling for bureaucrat, district and state-year confounds.

A potential problem with this specification is that the treatment (AA) is likely endogenous to outcome variable. This is the case for two reasons. First, omitted and potentially 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 substantially influenced by them and is controlled for using state-year fixed effects). Although the precise

[12] This is their bachelor's degree class—the best performing students have first class degrees; those with third class degrees performed the worst.

[13] As we report below, our results are robust to excluding controls for bureaucrats' characteristics, which are arguably post-treatment.

mechanism by which district assignments are made vary by state and are opaque, Bhavnani and Lee (2018) explicitly 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).[14]

That the district assignments of early-career bureaucrats are quasi-random is consistent with our fieldwork as well and the logic of the assignment process. District assignments are made by the state 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, 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 (for example, by posting "loyal" officers to areas that are important to politicians—see Iyer and Mani 2012), 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

[14]Note that 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.

early in officers' careers than later.[15] 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, Government of India 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 Online Appendix Tables A.2, A.3 and A.4. These show that the proportion of early-career affirmative action recruits that serve in district-years 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 educational institutions), 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) and the characteristics of the bureaucrats that served in those districts (mean age, the proportion of female bureaucrats, their education and experience). Of the 20 balance tests we report, there is only a small degree of imbalance with respect to one variable—the proportion of "local" bureaucrats that are serving in the state from which they are from. This imbalance could occur by chance, especially since the balance tests do not control for multiple comparisons. Nonetheless, due to an abundance of caution, and to improve the precision of our estimates, we control for all these variables.[16]

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 (Online Appendix Table A.5). Although affirmative action hires generally have longer postings (regression 1),

[15] Note that one of the most important factors in assessing the desirability of postings—the availability of opportunities for secondary education of children—is less likely to be relevant to officers in their late twenties and early thirties.

[16] Time-invariant district characteristics are controlled for using district fixed effects. Our results are robust to not controlling for bureaucrat characteristics, which are arguably post-treatment.

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:

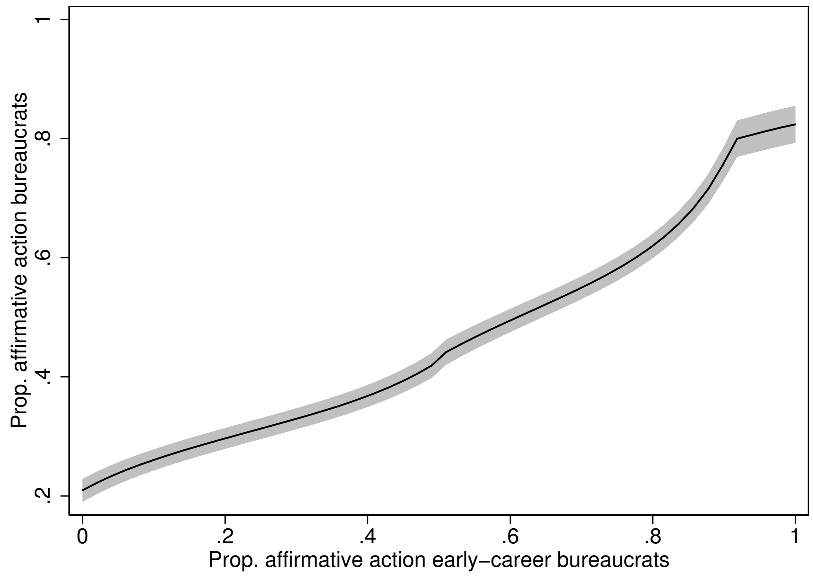
$$AA_{it} = \kappa + \lambda Z_{it} + \mu \mathbf{X_{it}} + \nu_i + \xi_{st} + \epsilon_{it} \tag{2}$$

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. Clots-Figueras (2012) similarly instruments for a variable (the proportion of female legislators) with the variable calculated for a subset of the data (the proportion of female legislators elected in close elections). In Figure 1, we graphically confirm 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 fairly similar in terms of whatever skill the exam is capturing.[17] This approach allows us to recover a second 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.

[17] 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 1: 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.

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 Online Appendix Table A.6). In this and the following regressions, standard errors are clustered at the level of the administrative district since bureaucrats are assigned to districts. 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 a number of possible confounds, specifically dummies for whether districts experienced positive or negative rainfall shocks, bureaucrat age, gender, education, a dummy for whether officers are from the state in which they serve, their years of experience, and the proportions of local representatives from the Congress, the BJP, the state's governing party, and elected from constituencies reserved for SCs and STs.[18] 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 by state-years (such as political support for MGNREGA). The controlled correlation between affirmative action and MGNREGA performance is positive, small and statistically significant at the 10% level.

To better rule out endogeneity concerns, including the specific concern hat affirmative action recruits are posted to areas where MGNREGA performance would be good, we switch to using the 2SLS estimator described previously. The first column of regression 4 displays the first stage of the 2SLS estimator, and confirms that the instrument (the proportion of affirmative action recruits in the first five years of their careers) is indeed statistically and substantively positively related to the proportion of affirmative action recruits, even after controlling for the variables described previously. Moreover, 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 an affirmative action recruit increases the log households that receive at least 100 days of employment under MGNREGA by 0.02 standard deviations.[19] Moreover, the 95% confidence interval for this estimate is narrow $(-.10, .14)$. This allows us to rule out costs to MGNREGA implementation larger than one-tenth of a standard deviation. In short, and contrary to the concerns of critics, affirmative action recruits perform no worse than regular

[18]The rainfall shock dummies were set to 1 if monsoon (June–September) rainfall in a district-year deviated from the long run (1950–2000) average monsoon rainfall in that district by more than 20%. In a robustness test reported later, we show that our results are robust to dropping controls for bureaucrat characteristics, which are plausibly post-treatment.

[19]At the mean, this is the equivalent of a 0.6% increase in the number of households that received at least 100 days of employment under MGNREGA.

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	0.11	0.14*	0.02		0.02
bureaucrats	[0.08]	[0.08]	[0.05]		[0.06]
Prop. affirmative action				0.64***	
early-career bureaucrats				[0.04]	
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	0.10	0.88		0.88
First stage F-statistic for AA bureaucrats					323

Notes: The dependent variable is the standardized logarithm of households that received 100 days or more of employment under MGNREGA. Controls are 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, bureaucrats' mean years of experience, 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

recruits.

4.1 Robustness tests

We next investigate the robustness of the null estimated effects of affirmative action to addressing concerns about the dependent variable, regression specification, key independent variable and identification strategy.

To check if our null results are driven by our choice of dependent variable, we check for robustness using another MGNREGA-related outcome, and using the outcome of a second major government (road building) program. In regression 1 of Table 2 (full results are in

Online Appendix Table A.7), we switch our MGNREGA-related dependent variable to the logarithm of person-days of employment. 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. In regression 2, we use 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. The point estimate of the effects of affirmative action recruits is positive and statistically insignificant. There remains no evidence that affirmative action recruits worsen bureaucratic performance.

To check the robustness of our results to specification changes, we start by no longer controlling for potentially bureaucrat characteristics, which are plausibly post-treatment. The null result is somewhat strengthened by this change, insofar as the confidence interval is narrower (regression 3). 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 4). As an alternative, we add the lag dependent variable (regression 5). Our results are robust to these modifications.

We note that our key independent variable—the proportion of affirmative action recruits—is calculated using data on all IAS officers. We check the robustness of the results to three changes in the calculation of this variable. First, 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 6). Recall further that IAS officers can serve in junior and senior (district collector, commissioner or magistrate) positions in the bureaucracy. Might we find a negative effect of affirmative action recruits if we focus on more powerful, senior officers? Regression 7 suggests that this is not that case.

Third, recall that some disadvantaged group members who scored above the general cutoff

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

Estimator:	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	OLS	2SLS
Model:	Alt. DV 1	Alt. DV 2	Fewer controls	No FE	Lg. DV	Rounded	DOs	Strict AA	Random assign.	Reduced form	Four year
	1	2	3	4	5	6	7	8	9	10	11
Prop. affirmative action bureaucrats	0.03	0.05	0.03	0.12*	-0.00		0.01		-0.05		0.03
	[0.05]	[0.08]	[0.06]	[0.07]	[0.03]		[0.08]		[0.12]		[0.06]
Prop. affirmative action bureaucrats, rounded						0.02					
						[0.06]					
Prop. affirmative action senior bureaucrats							0.06				
							[0.09]				
Prop. affirmative action bureaucrats, strict defn.								0.05			
								[0.06]			
Prop. affirmative action early-career bureaucrats										0.01	
										[0.05]	
Lagged dependent variable					0.80***						
					[0.02]						
Controls for bureaucrat characteristics?	Y	Y	N	Y	Y	Y	Y	Y	Y	Y	Y
District controls?	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
State-year fixed effects?	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
District fixed effects?	Y	Y	Y	N	N	Y	Y	Y	Y	Y	Y
Observations	1,292	1,641	2,047	2,047	1,525	2,047	2,047	1,084	484	2,047	2,047
Adjusted R-squared	0.96	0.78	0.88	0.63	0.92	0.88	0.88	0.91	0.94	0.88	0.88
First stage F-stat. for AA bureaucrats	135	219	368	719	370	149	186	462	47		249
First stage F-stat. for senior AA bureaucrats							115				

Notes: The dependent variable for regression 1 is the logarithm of the person-days of employment under MGNREGA; in regression 2 it is the logarithm of the number of villages connected by road under PMGSY. The dependent variable for all other regressions is the logarithm of households that received 100 days or more of employment under MGNREGA. All dependent variables are standardized. Controls for bureaucrat characteristics are 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, bureaucrats' mean years of experience. District controls are the proportion of state legislators from the Congress, the BJP, the state's governing party, and from constituencies reserved for Scheduled Castes and Tribes. In addition, regressions 1 and 3–11 control for dummies for whether districts experienced positive or negative rainfall shocks. Regression 2 controls for the logarithm of the number of unconnected roads. Standard errors are clustered by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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 8 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 9).[20] The estimated effect of affirmative action hires is still statistically indistinguishable from 0, although the confidence interval is somewhat larger. We also estimate the reduced-form effect of the instrument on the dependent variable (regression 10). 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 11).

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

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

rather the effect of being assigned an affirmative action officer relative to being assigned a general category officer with a similar exam score.[21]

Figure 2 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 optimization procedure described in Calonico, Cattaneo and Titiunik (2014). The plot suggests that affirmative action recruits are associated with marginally higher levels of MGNREGA employment at the discontinuity. Detailed results presented in Online Appendix Table A.8 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.

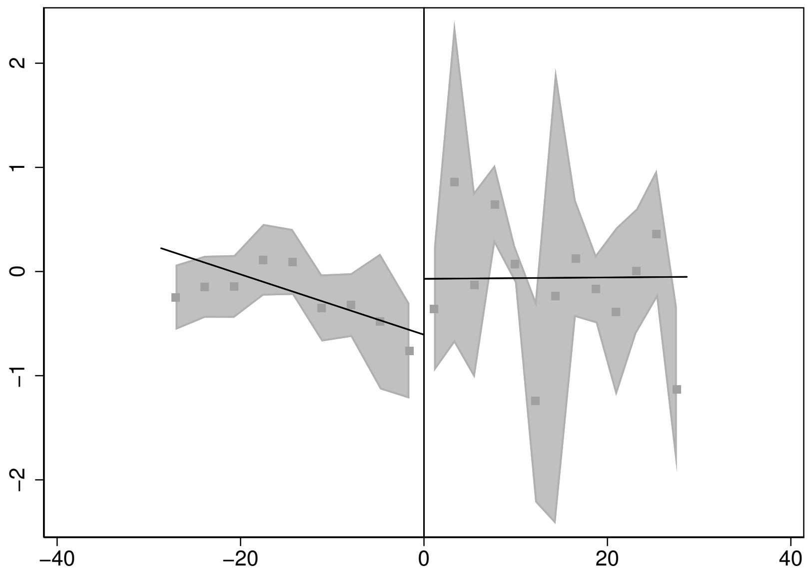
The main 2SLS estimate of the effects of affirmative action recruits, and a series of robustness tests, all suggest that affirmative action recruits do not worsen MGNREGA implementation. This null is the focus of the paper and is precisely estimated. We next turn to understanding reasons for this non-effect. This discussion is necessarily more speculative.

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

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 re-

[21] 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 2: 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 Online Appendix Table A.8. 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.

cruits (Table 3, regression 1; full results are in Online Appendix Table A.9) to our main 2SLS specification. 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. Recall that disadvantaged group and affirmative action recruits are not perfectly collinear since highly ranked disadvantaged group members are recruited via the general category. The regression results suggest that disadvantaged group recruits that are not recruited via affirmative action slightly improve MGNREGA performance. The effect is 0.12 standard deviations in size, and is positive and statistically significant. However, and as in the previous models, recruitment via affirmative action is associated with a small and statistically insignificant improvement 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 ($p = 0.06$), increasing the exam rank of recruits has the opposite effect, though this latter effect is not statistically significant. That said, the point estimates suggest that the positive effects of disadvantaged group bureaucrats are neutralized by recruits with exam ranks 63 and higher. In our data, all but five affirmative action recruits had exam ranks greater than or equal to 63.

Table 3: Mechanisms for the effects of affirmative action bureaucrats on MGNREGA implementation

Dependent variables:	HHs that recd. 100+ days 1	HHs that recd. 100+ days 2	Ln person-days recd. by SCs/STs 3	Prop. spent on materials 4	HHs that recd. 100+ days 5
Prop. affirmative action bureaucrats	-0.07		0.01	0.09	
	[0.07]		[0.08]	[0.13]	
Prop. minority bureaucrats	0.12**	0.12*		-0.07	
	[0.06]	[0.06]		[0.12]	
Bureaucrats' ln exam rank		-0.03			
		[0.03]			
Prop. SC/ST bureaucrats			0.09		
			[0.08]		
Prop. other minority bureaucrats			0.08		
			[0.07]		
All affirmative action bureaucrats?					0.03
					[0.07]
Some affirmative action bureaucrats?					-0.04
					[0.13]
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
First stage F-statistic for AA bureaucrats	162		120	113	
First stage F-statistic for minority bureaucrats	228	218		123	
First stage F-statistic for exam rank		101			
First stage F-statistic for SC/ST bureaucrats			139		
First stage F-statistic for other minority bureaucrats			113		
First stage F-statistic for all AA bureaucrats?					83
First stage F-statistic for some AA bureaucrats?					13

Notes: Controls are 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, bureaucrats' mean years of experience, 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

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

Table 3 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 (Keiser et al. 2002; Krislov 2012; Meier and Nigro 1976). To test this "representative bureaucracy" hypothesis, we specify the log person-days of MGNREGA employment received by SCs/STs as the dependent variable,[22] and examine whether SC/ST bureaucrats in particular positively influence this outcome. Regression 3 does not suggest that this is the case.[23]

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. Consistent with this, in a study of MGNREGA audit data, Afridi, Iversen et al. (2014) find that materials-related complaints were more than twice the value of labor complaints.[24] Regression 4 does not support this account: the proportion of expenses on materials is unaffected by disadvantaged

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

[23] The coefficients for the proportion of SC/ST and other bureaucrats are similar.

[24] Wage expenditures also provide opportunities for corruption, including through the creation of fictitious beneficiaries.

group officers.

A third possibility that we test for is that merit disadvantaged group members improve MGNREGA performance by diversifying the upper leadership of administrative districts. To test for this, we replace the proportion of affirmative action recruits in a district with two dummies. The first of these is set to 1 if all the IAS officer(s) in a district are affirmative action hires, and the second is set to 1 if some of the IAS officers in a district are affirmative action hires. District IAS officers are more diverse when the second dummy is set to 1. 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.

A final explanation for the positive effect of merit disadvantaged group members is that disadvantaged group members arguably overcome greater hurdles than others, and might therefore be of higher quality (Anzia and Berry 2011; Ferreira and Gyourko 2014). In the context under study, 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.[25] 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.

In Table A.10, we specify the the subjective interview score as the dependent variable and examine its correlates. These regressions suggest that merit and affirmative action disadvantaged group members perform worse than others on the subjective portion of the

[25]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.

exam, while controlling for their performance on the written portion of the test.[26] Put differently, merit disadvantaged group members perform better than others on the more objective, written portions of the exam. This is a possible explanation for the small, positive and statistically significant effect of merit disadvantaged group members.

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.

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, despite the null overall effect, though it might stem from differences in quality rather than ethnic favoritism.

Our findings underline the fact that affirmative action is a composite intervention, one that changes several aspects of personnel recruitment. Partly as a result, the effects of

[26]Regression 6 suggests that merit disadvantaged group members that received the same written score as others received 5.6 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.

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. Similarly, the relevance of the assessment procedure will influence the net effects of AA. If the qualification demanded is not meaningful, or simply measures cheating or test-taking skill, hiring unqualified candidates will not necessarily be costly.

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 core institutional responsibilities.

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Online Appendix

List of Tables

Table A.1: Summary statistics

	Obs.	Mean	Std. Dev.	Min.	Max.
Ln households received 100+ days of NREGA employment	2,047	7.94	1.66	0.69	11.75
Ln person days of NREGA employment	1,292	14.63	1.13	10.77	17.36
Ln person days of NREGA employment for SCs/STs	2,024	13.70	1.34	8.98	16.58
Prop. of NREGA expenditures on materials	1,532	28.83	10.84	0.15	68.28
Ln households received 100+ days of NREGA employment, standardized	2,047	0.00	1.00	-4.37	2.29
Ln person days of NREGA employment	1,292	0.03	0.97	-3.30	2.39
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, 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. minority bureaucrats	2,047	0.57	0.43	0.00	1.00
Prop. affirmative action early-career bureaucrats	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 minority officers	2,047	0.57	0.35	0.00	1.00
Bureaucrats' age	2,047	34.21	4.13	24.50	56.00
Prop. female bureaucrats	2,047	0.18	0.33	0.00	1.00
Bureaucrats' degree class	2,047	1.36	0.59	1.00	3.00
Prop. local bureaucrats	2,047	0.25	0.38	0.00	1.00
Bureaucrats' years experience	2,047	5.73	3.27	0.06	21.10
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 A.2: Balance tests for the instrument for the proportion of affirmative action bureaucrats, 1/3

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 educ. inst.
	1	2	3	4	5	6	7	8
Prop. affirmative action	23472.21	-5971.70	-10324.33	8243.24	98.09	79.95	93.06	82.97
early-career bureaucrats	[62714.48]	[39114.01]	[15192.60]	[10215.33]	[84.38]	[75.46]	[67.32]	[75.88]
Bureaucrats' age	-10539.74	-5420.82	-2196.14	-2064.07**	1.10	-0.30	-0.59	-0.10
	[6694.22]	[4466.00]	[1676.18]	[898.15]	[5.83]	[5.39]	[4.84]	[5.43]
Prop. female bureaucrats	217096.74***	175880.99***	30875.57*	30996.01***	46.74	51.70	109.54*	50.01
	[81575.70]	[54358.94]	[17769.62]	[10430.38]	[72.83]	[67.26]	[61.77]	[67.33]
Bureaucrats' degree class	-79526.35**	-23518.41	-205.60	-1541.31	5.75	3.93	2.52	5.25
	[31472.87]	[19758.15]	[6622.22]	[7503.62]	[26.03]	[23.44]	[20.42]	[23.58]
Prop. local bureaucrats	-92798.09*	-65248.75*	-3411.14	7735.52	-3.84	2.90	-5.07	6.56
	[51505.66]	[33640.70]	[9790.12]	[8808.65]	[57.58]	[50.45]	[40.44]	[51.70]
Bureaucrats' years experience	18100.32	15863.07**	747.92	3836.90***	-0.68	-0.51	1.16	-0.82
	[11564.94]	[7963.04]	[2108.05]	[1471.12]	[7.16]	[6.56]	[5.41]	[6.63]
Positive rainfall shock dummy	-135598.78*	-94881.52*	-29251.28*	-20625.01*	-153.92**	-145.85**	-133.05**	-146.39**
	[78570.01]	[50679.44]	[17544.65]	[11883.01]	[73.20]	[67.42]	[59.56]	[67.48]
Negative rainfall shock dummy	47037.22	14906.14	-2403.52	-856.95	-71.72	-64.22	-44.00	-64.04
	[62258.79]	[41072.48]	[13077.90]	[9807.26]	[56.54]	[51.23]	[46.93]	[51.46]
Prop. Congress MLAs	-171775.67*	-110076.23	-86349.02***	-30170.38*	-602.62***	-547.99***	-447.88***	-552.81***
	[102330.50]	[69344.75]	[25650.29]	[18204.47]	[104.75]	[92.81]	[78.88]	[94.51]
Prop. BJP MLAs	320049.46**	276310.16***	13843.82	-68637.00***	-470.18***	-437.12***	-335.06***	-439.95***
	[146495.02]	[104522.95]	[26832.57]	[18048.95]	[98.28]	[90.20]	[74.90]	[90.83]
Prop. MLAs in state gov.	-61899.77	-72965.52	-6837.80	700.41	124.66*	100.99	112.11*	98.48
	[104544.54]	[72091.94]	[21219.58]	[17544.09]	[72.57]	[66.02]	[57.02]	[66.76]
Prop. MLAs reserved for	-100618.07	57962.83	175018.13***	-36715.39***	-395.55**	-352.85**	-415.58***	-350.11**
Scheduled Castes	[130887.19]	[86333.86]	[46352.58]	[13024.17]	[167.31]	[144.89]	[135.32]	[145.88]
Prop. MLAs reserved for	-668181.82***	-491991.69***	-158048.33***	428353.57***	-51.90	-50.81	-153.37**	-51.20
Scheduled Tribes	[102086.72]	[65833.78]	[20055.82]	[33642.40]	[84.99]	[80.52]	[70.04]	[80.60]
State-year fixed effects?	Y	Y	Y	Y	Y	Y	Y	Y
Observations	1,985	1,985	1,985	1,985	1,971	1,971	1,971	1,971
Adjusted R-squared	0.46	0.44	0.58	0.51	0.22	0.22	0.31	0.22

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

Table A.3: Balance tests for the instrument for the proportion of affirmative action bureaucrats, 2/3

Dependent variables:	Mean age	Prop. female	Mean bachelor's division	Mean local	Mean experience (years)	Pos. rainfall shock	Neg. rainfall shock
	1	2	3	4	5	6	7
Prop. affirmative action	0.52	-0.02	0.08	-0.08**	-0.09	-0.02	0.01
early-career bureaucrats	[0.38]	[0.04]	[0.06]	[0.04]	[0.21]	[0.03]	[0.04]
Prop. female bureaucrats	-1.00*		-0.14**	-0.08	-0.52*	-0.00	-0.02
	[0.53]		[0.07]	[0.06]	[0.31]	[0.04]	[0.05]
Bureaucrats' degree class	0.11	-0.05**		-0.05	-0.28	-0.03	-0.02
	[0.45]	[0.03]		[0.04]	[0.20]	[0.02]	[0.03]
Prop. local bureaucrats	-0.14	-0.05	-0.09		-0.29	-0.06*	0.03
	[0.41]	[0.04]	[0.07]		[0.28]	[0.03]	[0.04]
Bureaucrats' years experience	0.84***	-0.01*	-0.02	-0.01		-0.00	-0.01
	[0.07]	[0.01]	[0.01]	[0.01]		[0.01]	[0.01]
Positive rainfall shock dummy	-0.08	-0.00	-0.05	-0.05*	-0.10		-0.23***
	[0.22]	[0.02]	[0.04]	[0.02]	[0.12]		[0.03]
Negative rainfall shock dummy	-0.13	-0.01	-0.02	0.01	-0.11	-0.15***	
	[0.18]	[0.02]	[0.03]	[0.02]	[0.12]	[0.02]	
Prop. Congress MLAs	0.33	0.04	-0.11	-0.10*	-0.40	0.04	-0.03
	[0.71]	[0.06]	[0.11]	[0.06]	[0.36]	[0.07]	[0.09]
Prop. BJP MLAs	0.92	0.05	-0.17	-0.09*	-0.53	0.01	-0.02
	[0.71]	[0.05]	[0.11]	[0.05]	[0.35]	[0.06]	[0.09]
Prop. MLAs in state gov.	-0.49	-0.06	0.02	0.00	0.26	-0.00	0.02
	[0.51]	[0.04]	[0.07]	[0.05]	[0.31]	[0.06]	[0.06]
Prop. MLAs reserved for	0.18	-0.04	0.23*	0.05	0.91*	0.07	0.03
Scheduled Castes	[0.91]	[0.09]	[0.14]	[0.08]	[0.52]	[0.07]	[0.12]
Prop. MLAs reserved for	-1.46	-0.02	0.08	-0.02	0.69	-0.01	0.14
Scheduled Tribes	[0.95]	[0.09]	[0.13]	[0.16]	[0.85]	[0.15]	[0.15]
Bureaucrats' age		-0.01*	0.00	-0.00	0.29***	-0.00	-0.00
		[0.00]	[0.01]	[0.01]	[0.03]	[0.00]	[0.00]
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	0.67	0.57	0.63	0.50	0.81	0.44	0.45

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

Table A.4: Balance tests for the instrument for the proportion of affirmative action bureaucrats, 3/3

Dependent variables:	Prop. Congress MLAs	Prop. BJP MLAs	Prop. MLAs in state gov.	Prop. MLAs reserved SCs	Prop. MLAs reserved STs
	1	2	3	4	5
Prop. affirmative action	0.00	0.01	-0.00	0.00	-0.01
early-career bureaucrats	[0.02]	[0.01]	[0.02]	[0.01]	[0.01]
Bureaucrats' age	0.00	0.00	-0.00	0.00	-0.00
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Prop. female bureaucrats	0.02	0.02	-0.03	-0.01	-0.00
	[0.02]	[0.02]	[0.02]	[0.02]	[0.01]
Bureaucrats' degree class	-0.02	-0.02	0.00	0.02*	0.00
	[0.02]	[0.02]	[0.01]	[0.01]	[0.00]
Prop. local bureaucrats	-0.03	-0.02*	0.00	0.01	-0.00
	[0.02]	[0.01]	[0.02]	[0.01]	[0.01]
Bureaucrats' years experience	-0.00	-0.00	0.00	0.00	0.00
	[0.00]	[0.00]	[0.00]	[0.00]	[0.00]
Positive rainfall shock dummy	0.01	0.00	-0.00	0.01	-0.00
	[0.01]	[0.01]	[0.02]	[0.01]	[0.01]
Negative rainfall shock dummy	-0.00	-0.00	0.00	0.00	0.00
	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]
Prop. BJP MLAs	-0.53***		0.56***	0.05	0.13***
	[0.07]		[0.09]	[0.08]	[0.04]
Prop. MLAs in state gov.	0.01	0.37***		0.05	-0.00
	[0.07]	[0.06]		[0.06]	[0.03]
Prop. MLAs reserved for	0.27**	0.08	0.12		-0.19**
Scheduled Castes	[0.13]	[0.13]	[0.15]		[0.08]
Prop. MLAs reserved for	0.44***	0.47***	-0.02	-0.41***	
Scheduled Tribes	[0.10]	[0.11]	[0.16]	[0.12]	
Prop. Congress MLAs		-0.48***	0.02	0.15*	0.11***
		[0.06]	[0.09]	[0.08]	[0.04]
State-year fixed effects?	Y	Y	Y	Y	Y
District fixed effects?	Y	Y	Y	Y	Y
Observations	2,047	2,047	2,047	2,047	2,047
Adjusted R-squared	0.78	0.85	0.77	0.70	0.94

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

Table A.5: Differences between assignment length in years of all and early-career assignments

Assignments:	All	Early-career
	1	2
Dummy for affirmative action	0.20**	0.03
recruit	(0.09)	(0.04)
Exam year fixed effects?	Y	Y
Observations	1,305	1,131
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 A.6: 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	0.11	0.14*	0.02		0.02
bureaucrats	[0.08]	[0.08]	[0.05]		[0.06]
Bureaucrats' age		-0.00	-0.01	0.00	-0.01*
		[0.01]	[0.01]	[0.01]	[0.00]
Prop. female bureaucrats		0.08	-0.02	-0.01	-0.02
		[0.11]	[0.06]	[0.05]	[0.05]
Bureaucrats' degree class		-0.04	0.05	0.02	0.04
		[0.06]	[0.04]	[0.03]	[0.03]
Prop. local bureaucrats		0.08	-0.07	-0.11**	-0.07
		[0.09]	[0.06]	[0.05]	[0.05]
Bureaucrats' years experience		-0.03**	0.01	0.00	0.01
		[0.01]	[0.01]	[0.01]	[0.01]
Positive rainfall shock dummy		0.09	0.03	0.02	0.03
		[0.07]	[0.03]	[0.02]	[0.03]
Negative rainfall shock dummy		-0.26***	0.04*	-0.00	0.04**
		[0.06]	[0.03]	[0.02]	[0.02]
Prop. Congress MLAs		-0.65***	-0.10	0.02	-0.10
		[0.13]	[0.08]	[0.06]	[0.06]
Prop. BJP MLAs		-0.39***	-0.04	0.05	-0.04
		[0.14]	[0.08]	[0.06]	[0.06]
Prop. MLAs in state gov.		-0.15	0.07	0.04	0.07
		[0.12]	[0.07]	[0.05]	[0.06]
Prop. MLAs reserved for		0.34*	0.07	0.08	0.07
Scheduled Castes		[0.20]	[0.09]	[0.08]	[0.08]
Prop. MLAs reserved for		0.73***	-0.06	0.10	-0.06
Scheduled Tribes		[0.14]	[0.19]	[0.08]	[0.16]
Prop. affirmative action				0.64***	
early-career bureaucrats				[0.04]	
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	0.10	0.88		0.88
First stage F-statistic for AA bureaucrats					323

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

Table A.7: Robustness tests for the effects of affirmative action bureaucrats on MGNREGA implementation

Estimator:	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS	OLS	2SLS
Model:	Alt. DV 1	Alt. DV 2	Fewer controls	No FE	Lg. DV	Rounded	DOs	Strict AA	Random assign.	Reduced form	Four year
	1	2	3	4	5	6	7	8	9	10	11
Prop. affirmative action bureaucrats	0.03	0.05	0.03	0.12*	-0.00		0.01		-0.05		0.03
	[0.05]	[0.08]	[0.06]	[0.07]	[0.03]		[0.08]		[0.12]		[0.06]
Prop. affirmative action bureaucrats, rounded						0.02					
						[0.06]					
Prop. affirmative action senior bureaucrats							0.06				
							[0.09]				
Prop. affirmative action bureaucrats, strict defn.								0.05			
								[0.06]			
Prop. affirmative action early-career bureaucrats										0.01	
										[0.05]	
Lagged dependent variable					0.80***						
					[0.02]						
Bureaucrats' age	-0.00	-0.01		-0.01	0.00	-0.01*	-0.01*	-0.01	-0.02**	-0.01	-0.01*
	[0.00]	[0.01]		[0.01]	[0.00]	[0.00]	[0.00]	[0.01]	[0.01]	[0.01]	[0.00]
Prop. female bureaucrats	0.07	-0.04		0.11*	0.06**	-0.02	-0.01	0.02	-0.08	-0.02	-0.02
	[0.04]	[0.08]		[0.06]	[0.02]	[0.05]	[0.05]	[0.06]	[0.06]	[0.06]	[0.05]
Bureaucrats' degree class	-0.02	-0.08**		0.08**	0.03**	0.04	0.05	0.08**	-0.01	0.05	0.04
	[0.02]	[0.04]		[0.04]	[0.01]	[0.03]	[0.03]	[0.04]	[0.06]	[0.04]	[0.03]
Prop. local bureaucrats	0.01	-0.05		0.11*	0.01	-0.07	-0.07	-0.07	0.03	-0.07	-0.07
	[0.04]	[0.06]		[0.05]	[0.02]	[0.05]	[0.05]	[0.06]	[0.06]	[0.05]	[0.05]
Bureaucrats' years experience	-0.00	-0.00		0.01	0.00	0.01	0.01	0.01	0.02	0.01	0.01
	[0.01]	[0.01]		[0.01]	[0.00]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]
Positive rainfall shock dummy	-0.04**		0.04	0.01	-0.01	0.03	0.03	0.09***	-0.01	0.04	0.03
	[0.02]		[0.03]	[0.05]	[0.02]	[0.03]	[0.03]	[0.03]	[0.04]	[0.03]	[0.03]
Negative rainfall shock dummy	0.02		0.04*	-0.02	0.02	0.04**	0.04**	0.07***	0.06*	0.04*	0.04**
	[0.02]		[0.02]	[0.04]	[0.02]	[0.02]	[0.02]	[0.02]	[0.03]	[0.03]	[0.02]
Prop. Congress MLAs	-0.05		-0.10	-0.13	-0.02	-0.10	-0.10	-0.16*	0.02	-0.10	-0.10
	[0.05]		[0.06]	[0.11]	[0.03]	[0.06]	[0.06]	[0.08]	[0.12]	[0.08]	[0.06]
Prop. BJP MLAs	-0.11**		-0.05	-0.20*	0.00	-0.04	-0.04	0.01	0.11	-0.04	-0.04
	[0.04]		[0.06]	[0.11]	[0.04]	[0.06]	[0.06]	[0.10]	[0.08]	[0.08]	[0.06]
Prop. MLAs in state gov.	0.15***		0.08	0.04	0.01	0.07	0.07	-0.00	0.22**	0.07	0.07
	[0.05]		[0.06]	[0.09]	[0.03]	[0.06]	[0.05]	[0.10]	[0.09]	[0.07]	[0.05]
Prop. MLAs reserved for Scheduled Castes	-0.06		0.08	0.33**	0.05	0.07	0.07	0.14	-0.24	0.07	0.07
	[0.06]		[0.07]	[0.15]	[0.04]	[0.08]	[0.07]	[0.13]	[0.16]	[0.09]	[0.08]
Prop. MLAs reserved for Scheduled Tribes	0.23**		-0.04	0.39***	0.03	-0.05	-0.06	-0.42	-0.58***	-0.05	-0.06
	[0.11]		[0.16]	[0.10]	[0.04]	[0.16]	[0.16]	[0.30]	[0.18]	[0.19]	[0.16]
Ln unconnected roads		0.00***									
		[0.00]									
State-year fixed effects?	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
District fixed effects?	Y	Y	Y	N	N	Y	Y	Y	Y	Y	Y
Observations	1,292	1,641	2,047	2,047	1,525	2,047	2,047	1,084	484	2,047	2,047
Adjusted R-squared	0.96	0.78	0.88	0.63	0.92	0.88	0.88	0.91	0.94	0.88	0.88
First stage F-statistic for AA bureaucrats	135	219	368	719	370	149	186	462	47		249
First stage F-statistic for senior AA bureaucrats							115				

Notes: The dependent variable for regression 1 is the logarithm of the person-days of employment under MGNREGA; in regression 2 it is the logarithm of the number of villages connected by road under PMGSY. The dependent variable for all other regressions is the logarithm of households that received 100 days or more of employment under MGNREGA. All dependent variables are standardized. Standard errors are clustered by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: 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	0.25	0.23	0.28	24.3	1,422
Early-career officers with controls	0.10	0.27	0.71	35.8	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 standardized logarithm of households that received 100 days or more of employment under MGNREGA. The estimate is the average treatment effect with locally linear regression with triangular kernel. Controls are 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, bureaucrats' mean years of experience, 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.

Table A.9: Mechanisms for the effects of affirmative action bureaucrats on MGNREGA implementation

Dependent variables:	HHs that recd. 100+ days 1	HHs that recd. 100+ days 2	Ln person-days recd. by SCs/STs 3	Prop. spent on materials 4	HHs that recd. 100+ days 5
Prop. affirmative action bureaucrats	-0.07		0.01	0.09	
	[0.07]		[0.08]	[0.13]	
Prop. minority bureaucrats	0.12**	0.12*		-0.07	
	[0.06]	[0.06]		[0.12]	
Bureaucrats' ln exam rank		-0.03			
		[0.03]			
Prop. SC/ST bureaucrats			0.09		
			[0.08]		
Prop. other minority bureaucrats			0.08		
			[0.07]		
Bureaucrats' age	-0.01*	-0.01*	-0.00	0.01	-0.01*
	[0.00]	[0.00]	[0.00]	[0.01]	[0.00]
Prop. female bureaucrats	-0.01	-0.02	0.01	0.12	-0.02
	[0.05]	[0.05]	[0.04]	[0.07]	[0.05]
Bureaucrats' degree class	0.03	0.03	0.04	0.05	0.05
	[0.03]	[0.03]	[0.02]	[0.05]	[0.03]
Prop. local bureaucrats	-0.08*	-0.08*	0.00	-0.02	-0.07
	[0.05]	[0.05]	[0.04]	[0.07]	[0.05]
Bureaucrats' years experience	0.01	0.01	0.01	-0.00	0.01
	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]
Positive rainfall shock dummy	0.03	0.03	0.03	-0.02	0.03
	[0.03]	[0.03]	[0.02]	[0.05]	[0.03]
Negative rainfall shock dummy	0.04*	0.05**	0.02	-0.07**	0.05**
	[0.02]	[0.02]	[0.02]	[0.04]	[0.02]
Prop. Congress MLAs	-0.10	-0.10	-0.06	-0.03	-0.10
	[0.06]	[0.06]	[0.05]	[0.11]	[0.06]
Prop. BJP MLAs	-0.04	-0.05	-0.07	0.22*	-0.04
	[0.06]	[0.06]	[0.05]	[0.12]	[0.06]
Prop. MLAs in state gov.	0.07	0.07	0.05	-0.06	0.07
	[0.06]	[0.06]	[0.04]	[0.10]	[0.06]
Prop. MLAs reserved for	0.07	0.07	0.00	0.17	0.07
Scheduled Castes	[0.07]	[0.07]	[0.05]	[0.14]	[0.08]
Prop. MLAs reserved for	-0.05	-0.05	-0.02	0.19	-0.06
Scheduled Tribes	[0.15]	[0.15]	[0.12]	[0.33]	[0.16]
All affirmative action					0.03
bureaucrats?					[0.07]
Some affirmative action					-0.04
bureaucrats?					[0.13]
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
First stage F-statistic for AA bureaucrats	162		120	113	
First stage F-statistic for minority bureaucrats	228	218		123	
First stage F-statistic for exam rank		101			
First stage F-statistic for SC/ST bureaucrats			139		
First stage F-statistic for other minority bureaucrats			113		
First stage F-statistic for all AA bureaucrats?					83
First stage F-statistic for some AA bureaucrats?					13

Notes: All dependent variables are standardized. Standard errors are clustered by district. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.10: Scores on the subjective oral interview or personality test component of the UPSC exam

	1	2	3	4	5	6
Dummy for minority bureaucrat	-12.60***		-3.46*	-24.98***		-5.65***
	(1.08)		(1.57)	(1.45)		(1.69)
Dummy for affirmative action recruit		-14.79***	-12.22***		-36.92***	-32.88***
		(1.44)	(1.97)		(2.27)	(2.56)
Score for written components of UPSC exam				-0.29***	-0.41***	-0.41***
				(0.03)	(0.03)	(0.03)
Exam year fixed effects?	Y	Y	Y	Y	Y	Y
Observations	1,336	1,336	1,336	1,336	1,336	1,336
Adjusted R-squared	0.11	0.13	0.13	0.28	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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.