Locked Out of College: When Admissions Bureaucrats Do and Do Not Discriminate*

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Abstract

How does a criminal record shape interactions with the State and society? We present evidence from a nationwide field experiment, showing that prospective applicants with criminal records are about five percentage points less likely to receive information from college admission offices. However, we demonstrate that bias does not extend to race. There is no difference in response rates to Black and White applicants. We further show that bias is all but absent in public bureaucracies, as discrimination against formerly incarcerated applicants is driven by private schools. Examining why bias is stronger for private colleges, we demonstrate that the private-public difference persists even after accounting for college selectivity, socio-economic composition and school finances. Moving beyond the measurement of bias, we evaluate an intervention aimed at reducing discrimination: whether an email from an advocate mitigates bias associated with a criminal record. However, we find no evidence that advocate endorsements decrease bureaucratic bias.

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Does a criminal record inhibit access to social goods and services? Politics has long been understood as "who gets what, when, and how" (Lasswell, 1936), but social scientists have only recently begun to explore how punitive policies influence this power structure (Manza and Uggen, 2006; Weaver and Lerman, 2010). The State's power to punish is augmented by its ability to attach stigmatic labels to legal transgressors (Foucault, 1977), labels that influence individuals' interactions with the State and society post-incarceration. Directly or indirectly, a criminal record can inhibit access to voting rights, employment, welfare and education (Pager, 2003; Manza and Uggen, 2006; Owens and Smith, 2012). Even where access is not explicitly prohibited, formerly incarcerated persons often have to disclose their criminal record. These requirements complicate the bureaucratic process, and open opportunities for discrimination.

Experiments are frequently used in the social sciences to measure discrimination in access to social goods, including voting registration, public housing, employment, and medical services (Bertrand and Mullainathan, 2004; Pager, Bonikowski and Western, 2009; White, Nathan and Faller, 2015). Most of these studies focus on racial discrimination, generally finding that minorities face discrimination in accessing goods and services (but see Einstein and Glick (2017)). Only a few experiments examine discrimination based on criminal record, primarily in hiring (Pager, 2003). Discrimination in higher education – an important determinant of political participation and recidivism – is understudied. Formerly incarcerated populations face deficits in educational attainment, and Blacks and other minority groups are under-represented in higher education (Hjalmarsson, Holmlund and Lindquist, 2015). Punitive labeling by one state institution – the penal system – could negatively affect access to this social good that is also partially provided by the state.

We test for discrimination against Black and formerly incarcerated college applicants through a randomized field experiment. We send emails to 2,917 college admissions offices, inquiring about the requirements for application and admission. We use a factorial design with three treatments, which we present in Table 1 and Figure B2. In each email, the applicant reveals that he[1] has

[1]Given that the incarcerated population in the US is overwhelmingly male, the applicants are always men.

a General Education Diploma (GED) and asks about eligibility for admission. The emails are randomly assigned with equal probability to disclose that the applicant got their GED either online or in a state penitentiary. To test for racial bias, the applicant is randomly assigned to have a putatively White or Black name. Our primary outcome of interest is the rate of response for different treatment conditions. The overall response rate is 74%. Recognizing that bias can be multidimensional, we also consider two additional outcomes: friendliness and thoroughness of the response.

Table 1: Overview of Treatment Conditions

Treatment	**Possible values**	**Implementation**
Criminal record	{Criminal record, no criminal record}	GED obtained in prison or online
Race	{Black, White}	Signaled via name of applicant
Advocate	{No advocate, Black advocate, White advocate}	Email is sent by a former teacher

Note: This table describes the implementation of the three treatment conditions. The first column is the name of the treatment. The second column lists all possible values for each treatment. The third column summarize how the treatment was implemented.

Moving beyond measurement of bias, we propose an intervention to help mitigate discrimination. We test whether the support of an advocate, in the form of a former teacher, can help marginalized populations extract information from potentially biased bureaucracies. Assuming that former teachers are unlikely to vouch for unqualified candidates, their endorsement can serve as a signal of applicant quality. If bias against formerly incarcerated individuals occurs when admission bureaucrats use a criminal record to proxy for unobserved characteristics of the applicant, an advocate can serve as an added credential (Gaddis, 2014). To implement the advocate intervention, we randomize whether the email is sent by the applicant himself, or whether it is sent by a former GED instructor of the applicant. This is is a low-cost intervention: Using a short and simple email, the teacher reaches out on behalf of the applicant to inquire about college eligibility. To increase comparability between the applicant and advocate emails, we also randomize the race of the advocate. A Black applicant can have either a Black or a White advocate, and vice versa. In Table 1 and Figure B2, we document the three treatment conditions.

Admissions officers have significant discretion over whether and how they reply to prospective applicants. While colleges have admissions policies governing how race and criminal record can or cannot be considered, these policies do not explicitly extend to information provision. Furthermore, admissions policies themselves are often ambiguous, creating the opportunity for admissions officials to use their own discretion when interacting with potential applicants. Few colleges specifically bar admission of applicants with felony criminal records, but almost all colleges retain the right to refuse admission based on past criminal activity, and often require additional forms or essays of formerly incarcerated applicants[2]. Thus, admissions eligibility is often unclear, and formerly incarcerated applicants must overcome this first hurdle before applying.

We stress that our design measures bias in bureaucratic responsiveness, rather than bias in admissions. Feasibly, admissions bureaucrats may have different incentives when responding to general inquiries versus when actually making the decisions to admit or reject an applicant. Still, bias in bureaucratic responsiveness at this entry point into the admission process is likely to depress enrollment for formerly incarcerated applicants. Non-responsiveness inhibits an applicants ability to get important information as to what is required to enroll. Given the additional bureaucratic procedures for applicants with criminal records, ascertaining eligibility is an important first step to enrollment. The relevance of information in college admissions is further underlined by a growing number of studies that examine interventions aimed at increasing access to admission information (Bettinger et al., 2012; Hoxby and Turner, 2015; Deming and Dynarski, 2010; Dynarski et al., 2018). This literature finds that a lack of information strongly decreases the probability of application, enrollment and eventual success in college, particularly for applicants from lower socioeconomic backgrounds. What is more, applications in which formerly incarcerated applicants struggle to access crucial information can cause such applicants to self-select out of the process (Rosenthal et al., 2015). Access to information is increasingly seen as a key determinant of success in higher education, highlighting the need to study bias in bureaucratic responsiveness to requests

[2]In August 2018, the Common Application removed the section asking about criminal record. This new policy took effect after the running of this experiment. Many colleges still require disclosure of criminal record in separate forms.

for admission information.

Drawing on the results from our field experiment, we offer several contributions: This is the first study, to the best of our knowledge, to establish the causal effect of a criminal record on bureaucratic responsiveness in the context of higher education. On average, formerly incarcerated individuals are about 5 percentage points less likely to receive a response from admissions offices. However, further results paint a more positive picture: First, even though we send queries from putatively low-SES applicants, response rates are relatively high, at about 75%. Second, contrary to a large body of empirical work, we demonstrate that bias does not extend to applicants' racial backgrounds: There is no difference in response rates for Black and White applicants.

We demonstrate that institutional context is a key mediator of bias: We observe that public institutions do not discriminate against prospective applicants with criminal records. Bias in response is driven by private colleges, which are about 10 percentage points less likely to respond than their public counterparts. We offer four different explanations for why bias is more prevalent in private institutions: (1) Differences in admissions selectivity, (2) the socio-economic makeup of the student bodies, (3) differences in school finances and (4) institutional missions. We test the first three of them using additional school characteristics from Chetty et al. (2017). We find that the the public / private different cannot be explained by observable school characteristics. We posit that differences in mission statements and explicit anti-discriminatory policies at public versus private institutions may drive these results, although we cannot directly test this proposition.

Our empirical results extend the literature in a number of ways: First, we demonstrate the mark of a criminal record for a public good that is provided both publicly and privately: Higher education. Second, we document that context matters, with admissions bureaucrats at private institutions exhibiting non-responsiveness towards applicants who have spent time in prison at a much greater rate than admissions bureaucrats at private universities. In addition, we move beyond measurement of bias to test a strategy for reducing bias, but do not find evidence that an advocate reaching out an incarcerated applicants behalf is effective in this context.

1 Private and Public Provision of Higher Education

As a social good that is provided by both public and private institutions, higher education is a novel context in which to test for bias based on race and criminal record. Public universities and colleges are predominantly funded and operated by state governments. Private universities, though publicly subsidized, receive much of their funding from tuition and alumni contributions, and are run as private non-profit enterprises. Past experimental studies have tested for different forms of bias in public or private institutions separately, but no experiment has compared the behavior of bureaucrats across these institutional contexts. There are often tradeoffs in efficiency and equity between public and private provision of goods and services (Niskanen, 1968). Public and private bureaucracies face different incentives, audiences, leadership structures, and accountability mechanisms (Chubb and Moe, 1988). Also, public bureaucracies tend to place greater restrictions on bureaucratic discretion, reducing opportunity for bureaucrats to discriminate (Frant, 1993). Our experiment offers a unique opportunity to test whether public versus private bureaucrats behave differently towards marginalized groups, holding constant the good provided.

We propose four theoretical reasons for why the effect of criminal record or race on admissions responsiveness could vary across public and private colleges: Admission selectivity, socio-economic composition of the student body, financial considerations, and institutional missions.

Selectivity: Private schools are generally more selective[3] than public schools, so admissions bureaucrats may be less likely to respond to formerly incarcerated or Black applicants because they do not view them as suitable candidates for admission. Admission bureaucrats may not respond to inquiries if they believe that the probability of admission for the prospective applicant is extremely low. Prior incarceration is often associated with lower achievement and decreased school attendance (Blomberg et al., 2011). Bureaucrats may therefore perceive prior incarceration as a signal of low achievement or self-control. Being more selective, private schools bureaucrats may therefore be less likely to respond if prior incarceration is viewed as a signal of insufficient

[3] In our sample, private colleges are, on average, about 4.2 percentage points more likely to reject applicants than public colleges.

academic performance.

Socio-economic composition: Differences in the demographic and socio-economic makeup of students bodies between public and private colleges could shape admissions response to formerly incarcerated or Black applicants. Contact theory (Pettigrew, 1998) and familiarity bias (Tversky and Kahneman, 1974) suggest that bureaucrats may be more more willing to help applicants that they are familiar with, e.g. through prior interactions (Einstein and Glick, 2017). Our data shows that students at public colleges are more likely to be Black, and tend to come from more disadvantaged backgrounds compared to students at private colleges[4]. Public colleges may therefore be more responsive, as their student body consists of a greater number of students from racially and economically diverse backgrounds. Conversely, if private school students are wealthier and less racially diverse, a lack of prior contact may result in lower response rates by admission bureaucrats.

Financial considerations: Admissions bureaucrats may anticipate that formerly incarcerated or Black applicants are unable to pay for college without significant financial aid, so private schools, which are often more expensive and tuition-dependent, may exhibit lower levels of responsiveness.

Institutional missions: There may be differences in overarching mission statements or explicit policies of public versus private colleges to pursue diversity or support under-privileged groups. Public higher education policy in the US is founded on the principle of "[e]quality of opportunity for all students to attend public higher education in their state, without regard to their background or preparation" (Bastedo and Gumport, 2003, p. 341). In contrast, such a prerogative does not explicitly apply to private colleges, which might be less stringent in their efforts to prevent bias against, for example, formerly incarcerated applicants.

To test the first three of these explanations, we merge our data with college data from Chetty et al. (2017). We test whether treatment effects are driven by admissions rejection rate or average SAT score (selectivity), racial demographics, parental median income, and percentage of parents in the top 1% of the nationwide income distribution (socio-economic composition), and tuition

[4]For more information, see Table B8 in the SI.

sticker price and net costs after financial aid (financial considerations).

2 Empirical Strategy

We contact 2,917 public and private non-profit colleges across the United States. We constructed this sample from an exhaustive list of currently operating colleges, obtained from the National Center for Education Statistics (NCES). Each college admissions office receives one email, inquiring about information concerning the admissions process. The emails express interest in applying to the college, then report that the applicant has his GED, and ask if that affects his eligibility. Finally, the emails inquire what else is required to apply and whether the college is currently accepting applications. Each email is randomly assigned to treatment or control groups across three conditions: *Criminal record*, *race* and *presence of an advocate*. The first two treatment conditions are binary: The applicant either has a criminal record or not, and the applicant is either Black or White. The advocate treatment can take on three values: No advocate, Black advocate or White advocate. Our design therefore results in 2x2x3=12 different treatment arms. In Table 1, we give an overview of the treatment conditions, while Figure B2 in the supplementary information (SI) shows the email language across treatment combinations.

We use the location at which the GED was obtained to signal that the applicant has spent time in prison. The applicant either reports that he received his GED at a state penitentiary (treatment) or online (control)[5]. Signaling that the applicant has a GED allows us to keep socio-economic status constant across treatment conditions, thus lowering the probability that admissions officers conflate the treatments with social class. Applicant and advocate race is revealed by using either a putatively Black (Tyrone Booker, Darnell Banks) or White (Kevin Schmidt, Bob Krueger) name. We chose names that were not used in previous audit studies, and pre-tested those names for consistent racial connotations (we present the pre-test results in Table B1 in the SI).

[5]The state penitentiaries used in the emails were minimum security prisons in the same state as the college. We include state fixed effects in our empirical strategy to eliminate concerns that the effects are confounded by our choice of the penitentiary in each state.

The advocate treatment changes the email language slightly: Instead of the applicant, a named advocate identifies himself as a former teacher of the applicant and then proceeds to inquire about the exact same information as in the standard direct email. To rule out spurious effects that result from differences in email language, we have tried to make the applicant and advocate emails as similar as possible. Consequently, the advocate does not explicitly endorse the applicant beyond the fact that the advocate is inquiring on behalf of the applicant.

Using an automated script, we sent 2,934 emails over the course of eight weekdays between February 23, 2018 and March 6, 2018. Each day, we sent about 360 emails. We randomized the order in which emails were sent. 72 emails could not be delivered, since the email addresses that we obtained were not up to date, or, in a few cases, the college did not exist anymore. For this subset of emails, we collected new contact information, and tried resending all emails on March 12, 2018. For all except 17 colleges, this was successful, bringing our final sample size to 2,917.

Our main measure of bias is the responsiveness of the admissions offices, a binary variable coded as 1 if we received a response within 21 days and 0 otherwise. Recognizing that discrimination can be multidimensional (Hemker and Rink, 2017), we include two additional outcomes, thoroughness and friendliness. As in Einstein and Glick (2017), we conceptualize friendliness as a binary variable that we code as 1 if a response addresses the sender by name. Thoroughness is coded on a numeric scale from 0 to 3, based on whether the response answered the 3 questions posed in the email[6]. To avoid conditioning on a post-treatment variable (Coppock, 2018), non-responses for the friendliness and thoroughness outcomes are coded as 0.

To construct the sample, we collected contextual data from the Integrated Postsecondary Education Data System, administered by the NCES. The database contains information on whether each school is a public or private school, whether it is primarily a four-year or two-year institution, and the size of the student body. These definitions and classifications are established by the NCES. A breakdown of the covariate distributions is shown in Figure B1 in the SI. Four-year colleges con-

[6]Thoroughness was coded by Mechanical Turk workers. Each response was coded independently by two workers. We use the average of the resulting ratings as the thoroughness outcome for each response.

stitute about two thirds of the full sample, there are slightly more public than private institutions, and most schools have fewer than 5,000 students.

Each of these contextual variables could be associated with bureaucratic capacity, school policies or student recruitment strategies, which in turn may influence response rates. We use coarsened exact matching (CEM, see Iacus, King and Porro 2012 and Section A.2 in the SI) to ensure balanced treatment assignment across the three pre-treatment variables. Since the advocate race treatment depends on the presence of an advocate, we use simple randomization instead of pair matching to assign that treatment status. In Figure A3 in the SI, we show that balance across the covariates is almost perfect.

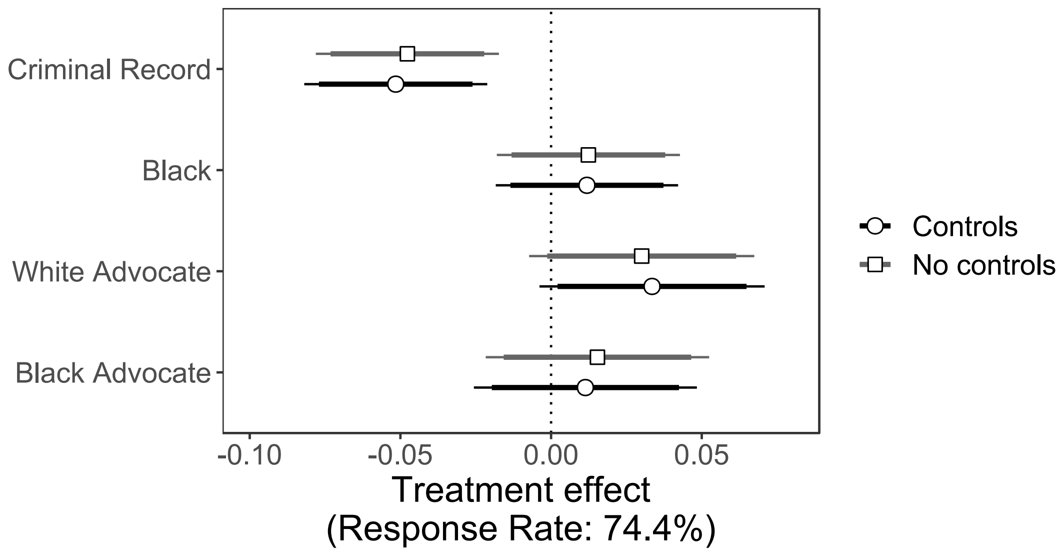
For each of the three outcomes, we run two baseline models: with and without covariates. We include controls for the three pre-treatment covariates used to define the CEM strata. Subsequently, we interact the treatments to test whether bias varies by race or by presence of an advocate. We also include non-parametric estimations of our main treatment effects, to demonstrate that the results are not model-dependent (Table B3 in the SI). Finally, we test whether the treatment effects vary between public and private colleges. To do so, we re-estimate the main specifications separately for the subset of private and public schools. To examine the mechanisms that underlie institutional differences in bias, we interact the criminal record treatment with seven college characteristics that were originally compiled by Chetty et al. (2017).

3 Results

In Figure 1, we present the main results. All else equal, admissions bureaucrats are 5.2 percentage points less likely to respond to a formerly incarcerated applicant. For applicants with putatively Black names, we do not find any evidence of bias in response rates. In fact, average response rates for Black applicants are somewhat higher than for White applicants, although these estimates are statistically indistinguishable from zero. In Table B7 in the SI, we present specifications that include interactions between the treatments. We do not have sufficient power to reject the null hypothesis for any of the interactions. However, we note that bias in responsiveness for formerly incarcerated applicants is stronger when the applicants are Black.

Regarding the advocate treatment, there is little evidence that the intervention increases response rates. While we observe increased response rates for White (but not Black) advocates in one specification, the interaction models (see Table B7 in the SI) show that the advocate effect decreases for applicants with criminal records.

Figure 1: Main Results



Note: The figure show coefficient estimates from the main specifications. Each pair of coefficients refers to a treatment, which is shown on the y-axis. The outcome is a binary response indicator. Positive effect sizes indicate that the treatment condition increases response rates. The average response rate is 74.4%. The covariates are public/private, two-year/four-year, institution size and state fixed effects. The solid horizontal lines indicate 95% confidence intervals.

Having established that formerly incarcerated applicants are subject to bias, we examine whether the treatment effect varies between private and public schools in Figure 2. We observe striking heterogeneity when comparing bias at private versus public colleges. Private college admissions bureaucrats are about 10 percentage points less likely to reply to formerly incarcerated applicants. For public schools, there is no detectable difference in response rates. These estimates are statistically distinct from one another, as we show in Table B6 in the SI. The aggregate effects reported in Figure 1 are therefore driven by bias in private college admissions offices. We also find that private colleges do not appear to discriminate based on race, while public colleges tend to be more responsive to Black applicants. These effects are, however, not precisely estimated (significant only at $\alpha = 0.1$).

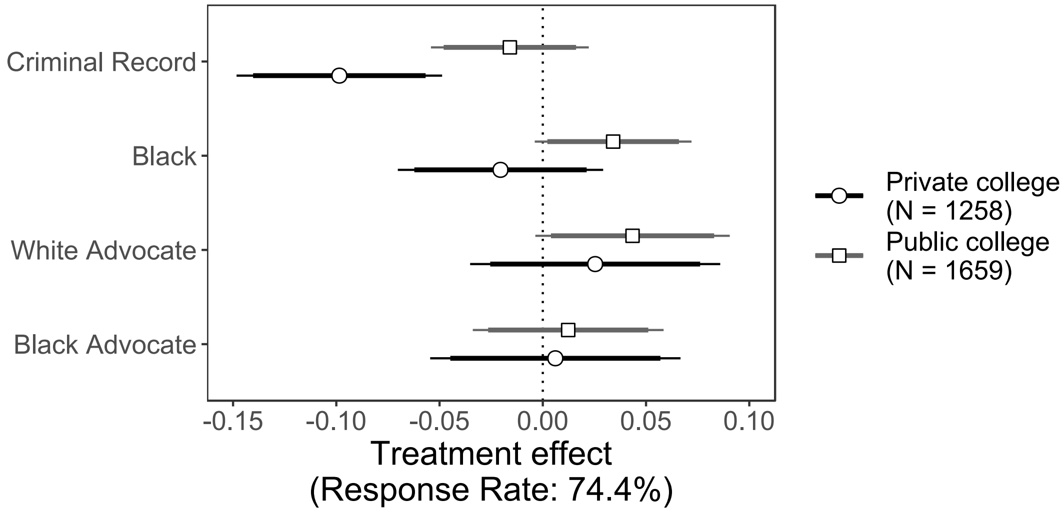
The public-private heterogeneity does not appear to be driven by differences in school size, degree offerings, socio-economic makeup of student bodies, admissions selectivity or tuition dependency. Tables B10 and B11 in the SI present the effect of time in prison on responsiveness subset by public and private, with interactions between the treatment and different school characteristics including rejection rate, average standardized test scores of incoming classes, tuition price, financial aid, percent of students' coming from families in top quartile of income distribution, proportion of student body that is Black, and parent median income. These characteristics vary between public and private colleges, and could potentially explain the differences between bureaucratic behavior at public versus private institutions. The magnitude of the treatment effect coefficients are relatively unchanged despite the inclusion of interactions, and the coefficients on the interactions are all effectively zero. Thus, none of these characteristics appear responsible for the different responses to formerly incarcerated applicants by admission bureaucrats at public and private colleges.

Since bias can be multidimensional, we also examine two alternative measures of bias: Thoroughness and friendliness. In Sections B.1 and B.2, we re-estimate all specifications discussed previously using these two outcomes. By and large, we observe the same patterns for the two alternative outcomes.

4 Discussion

Using a nationwide randomized field experiment, we establish the causal effect of a criminal record on bureaucratic discrimination in college responsiveness. Punitive labeling by the state has demonstrable effects on access to higher education, a vital social good with downstream effects on political participation, labor market outcomes, and recidivism. While not a direct test for bias in college admission, our results speak to a growing literature that highlights lack of information as a barrier to successful college admissions (see e.g. Hoxby, 2009), especially for low-SES applicants (Dynarski et al., 2018).

Figure 2: Results for Public and Private Schools



Note: The figures show coefficient estimates for public and private schools separately. The treatments are shown on the y-axis. The outcome is a binary response indicator. The average response rate is 74.4%. All specifications include covariates and state fixed effects. The covariates are two-year/four-year and institution size. The solid horizontal lines indicate 95% confidence intervals.

While we document bias against formerly incarcerated applicants, we also highlight two positive results: A high overall response rate and no evidence for racial bias in responsiveness. Even though we send queries from applicants with putatively low-SES, about three quarters of all inquires receive replies within three weeks. Unlike audit studies in other contexts (Costa, 2017), we do not find evidence of racial discrimination. We offer two possible explanations for this: First, all applicants have GEDs, which holds social class constant. If bias against putative Black applicants occurs when admissions bureaucrats conflate race with social class or education, holding education constant should eliminate some of this bias. This finding is consistent with Einstein and Glick (2017), who find no racial bias when applying to public housing – a setting where applicants are of similar socioeconomic strata and where minorities are disproportionately represented. Second, colleges may have successfully implemented policies to curb racial bias in admissions offices, e.g. because they strive for a diverse student body. Yet, while colleges may have been successful at curbing one dimension of bias in responsiveness, i.e. racial bias, more effort needs to be directed at decreasing bias against formerly incarcerated applicants.

Our work highlights stark differences in the behavior of public versus private institutions. We find that discrimination by public colleges is close to zero, while private colleges discriminate at significantly higher rates. We propose four explanations for the observed public-private divide: (1) admissions selectivity, (2) the economic and racial makeup of the student body, (3) financial aid and dependence on tuition, and (4) differences in institutional missions. Using college characteristics compiled by Chetty et al. (2017), we find little evidence for the first three mechanisms. In the absence of these seemingly likely explanations, we posit that admissions bureaucrats operate under more general differences in missions statements, potentially to serve more diverse and more under-privileged student bodies. Public universities in part receive their operating goals from state governments (Chubb and Moe, 1988), and may give greater focus to marginalized applicants. These accommodations may be particularly relevant for formerly incarcerated applicants, as their exclusion from various aspects of public life post-incarceration is state-constructed.

Moving beyond measurement, we propose a strategy for marginalized populations to navigate biased bureaucracies (see Butler and Crabtree, 2017). Instead of a direct inquiry by applicant, some emails are sent by advocates. However, we find little evidence that emails from former teachers of students can ameliorate the effects of a criminal record. While we find some evidence for increased response rates for White (but not Black) advocates, the interaction models (see Table B7 in the SI) show that the advocate effect is diminished greatly for applicants with criminal records. We offer two explanations for why the advocates were unable to reduce bias. First, the advocate email does not contain a direct endorsement of the applicant. Since our aim was to make emails comparable across advocates and applicants, we did not include an explicit endorsement. The advocate email might therefore be a weak signal of applicant quality – too weak to convince admissions bureaucrats. Second, applicants will likely have to submit teacher recommendations when they apply to college. Bureaucrats may expect that any potential applicant will be able to obtain endorsements from their teachers anyway, so the advocate email might not reveal additional information to bureaucrats. Although we have little evidence in favor of advocate endorsements as a strategy to reduce bias, we stress that future research should examine more nuanced implementations of the advocate intervention.

Our findings show that the absence of racial differences in responsiveness does not imply unbiasedness along other dimensions, such as applicants' criminal histories. Echoing the results in Einstein and Glick (2017), we find that bias may vary substantially, even when we hold constant the social good that is provided. Future researchers could examine why the same bureaucracy is biased along one dimension, but not the other. In addition, we argue that future work on bureaucratic bias needs to emphasize institutional differences in service provision, as many goods and services are routinely provided both by private and public actors, such as housing, health care, transportation and education.

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A Supplementary Information (Online Only)

A.1 Emails

For each of the four names, we created two different emails accounts to reduce the risk of being classified as spam when sending the emails. The email addresses only differ in the order of the three digits that make up the middle part of the address. The inclusion of the three-digit number was necessary to make sure that the email address was not already in use. In Table A2, we list the eight emails that we use to run the experiment. In our estimation of race effects, we pool results across email and name. Individual names and emails did not exhibit measurable effects on our outcomes, conditional on race. This is shown in Table B2.

Table A2: List of Emails

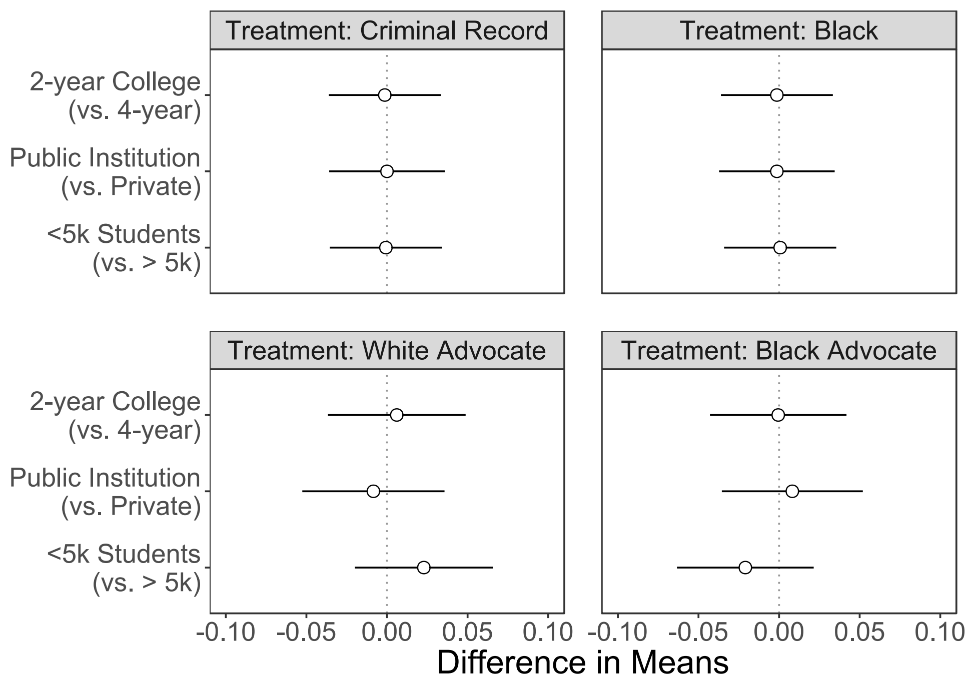
darnell.banks143@gmail.com
darnell.banks134@gmail.com
tyrone.booker143@gmail.com
tyrone.booker134@gmail.com
kevin.schmidt143@gmail.com
kevin.schmidt134@gmail.com
bob.krueger143gmail.com
bob.krueger134@gmail.com

Note: This table shows the list of emails we have created for the experiment. For each applicant name, we create two emails that vary only in the order of the last three digits. We sent an equal proportion of emails from each account.

A.2 CEM & Treatment Assignment

In Section 2, we give a brief overview of the randomization procedure that we employ to guarantee balance across contextual variables. CEM uses pre-treatment variables, coarsens them, and then creates strata based on the coarsened covariate. Since two of the three variables are already binary, CEM can only coarsen the student body size variable. We then use these strata to form pairs of observations that are members of the same stratum. Within pairs, the criminal record, race, and advocate treatment conditions are randomized so that one unit in the pair receives treatment and one unit receives control for each treatment category. Since the advocate race treatment depends on the presence of an advocate, we do not use pair matching to assign treatment status. Rather, we use simple randomization to determine whether an advocate has a putatively Black or White. We also randomize the name of the email sender or the advocate and the email account from which the message is sent. Conditional on the vector of treatments for each observations, the choice of name and email account is always only between two options. Therefore, we use a simple random draw to determine which email account and which name will be used.

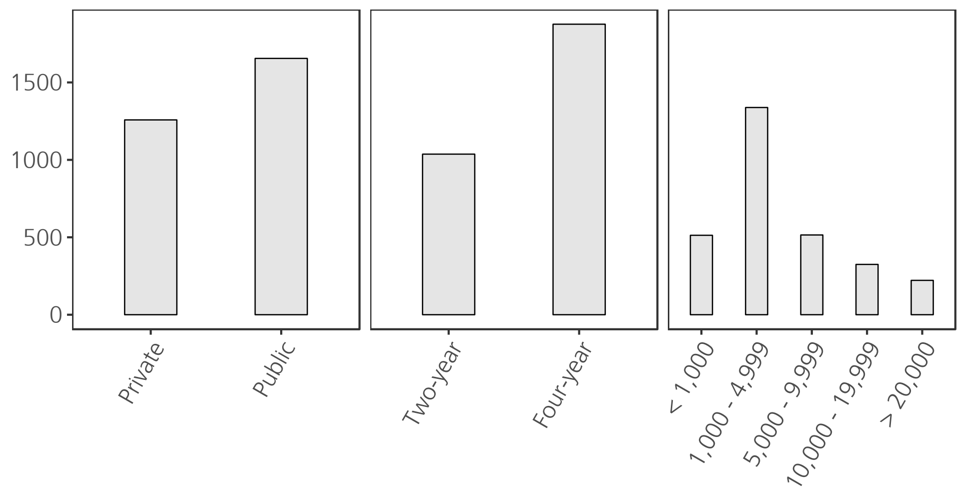
Figure A3: Covariate Balance after CEM



The panels represents the differences in means between treated and untreated units, separately for each covariate. All variables shown in the plot are binary.

B Tables and Figures (Online Only)

Figure B1: School Characteristics



Note: This figure shows the absolute frequencies of different categories of institutions in our sample. The categories in the last panel correspond to the number of students at each institutions.

Table B1: Racial Connotation Pre-Test Results

Applicant name	**Correctly classified**	**CI**
Tyrone Booker	0.95	[0.924, 0.981]
Darnell Banks	0.90	[0.862, 0.941]
Jamal Gaines	0.91	[0.866, 0.944]
Kevin Schmidt	0.95	[0.918, 0.978]
Bob Krueger	0.96	[0.936, 0.987]
Todd Novak	0.95	[0.923, 0.980]

Note: The table shows how many times each name was correctly classified, i.e. how frequently the names was assigned to the race that we intended it to be perceived as. In total, the names were classified by 200 respondents on MTurk.

Figure B2: Email Instruments

Criminal Record/Advocate Treatment

From: **[EMAIL ADDRESS]**
To: **[ADMISSIONS EMAIL ADDRESS]**
Subject: Admissions Info

Hello,

A past student of mine, **[APPLICANT NAME]**, is interested in applying to **[SCHOOL]**, but is worried he is not eligible. He has his GED, which he got at **[PENITENTIARY]**. Does this affect his eligibility? What else does he need to apply? Are you currently accepting applications?

Thank You,

[INSTRUCTOR NAME]

Criminal Record/ No Advocate

From: **[EMAIL ADDRESS]**
To: **[ADMISSIONS EMAIL ADDRESS]**
Subject: Admissions Info

Hello,

I am interested in applying to **[SCHOOL]**, but I am worried I am not eligible. I have my GED, which I got at **[PENITENTIARY]**. Does this affect my eligibility? What else do I need to apply? Are you currently accepting applications?

Thank You,

[APPLICANT NAME]

No Criminal Record/Advocate Treatment

From: **[EMAIL ADDRESS]**
To: **[ADMISSIONS EMAIL ADDRESS]**
Subject: Admissions Info

Hello,

A past student of mine, **[APPLICANT NAME]**, is interested in applying to **[SCHOOL]**, but is worried he is not eligible. He has his GED, which he got online. Does this affect his eligibility? What else does he need to apply? Are you currently accepting applications?

Thank You,

[INSTRUCTOR NAME]

No Criminal Record/ No Advocate

From: **[EMAIL ADDRESS]**
To: **[ADMISSIONS EMAIL ADDRESS]**
Subject: Admissions Info

Hello,

I am interested in applying to **[SCHOOL]**, but I am worried I am not eligible. I have my GED, which I got online. Does this affect my eligibility? What else do I need to apply? Are you currently accepting applications?

Thank You,

[APPLICANT NAME]

Note: The figure shows that exact wording of the emails that we sent to the colleges in the sample. In total, there are 2*2*3=16 different treatment conditions. In this figure, we only show that differences in email wording for the *Criminal Record* and *Advocate* treatments. Applicant and advocate race are signaled using putatively Black or White names, while the email text stays the same.

Table B2: Response Rates and Treatment Distribution by Name

Name	Response rate	Total emails	% Criminal Record	% Black	% White Advocate	% Black Advocate
Bob Krueger	0.758	756	50.1	0	26	24.9
Darnell Banks	0.771	721	50.2	100	25.9	22.5
Kevin Schmidt	0.775	711	51.2	0	25.3	27.6
Tyrone Booker	0.786	746	48.5	100	21.5	26.4

Note: This tables contains response rates, conditional on the name of the applicant. The last four columns are the relative frequencies of each treatment. For the two advocate treatments, the shares shown in the table signify the probability that an applicant with each respective name was assigned a Black or White advocate. To give an example, 26% of all applicants named 'Bob Krueger' had a White advocate, and 24.9% had a Black advocate.

Table B3: Mean Response Rates for the Treatment and Control Groups

	Response Rate						
	Treatment Mean	Control Mean	Diff. in Means	SE	P-value	$N_{\text{Treatment}}$	N_{Control}
Criminal Record	0.749	0.797	-0.047	0.015	0.002	1467	1467
Black	0.779	0.767	0.012	0.015	0.437	1467	1467
White Advocate	0.791	0.763	0.030	0.019	0.105	724	1467
Black Adovcate	0.776	0.763	0.012	0.019	0.522	743	1467
Advocate (Pooled)	0.783	0.763	0.021	0.015	0.176	1467	1467

Note: This table contains mean response rates conditional on treatment status. The first column indicates the treatment variable. For the advocate treatment, the control mean always refers to the mean response rate that were not sent by an advocate. We also show the standard error of the difference in means between response rates, and the corresponding p-values for the null hypothesis that the true difference is zero.

Table B4: Response Rates for All Possible Treatment Combinations

Criminal Record Treatment	Race Treatment	Advocate Treatment	Response Rate	N
No Criminal Record	White	No Advocate	0.751	360
No Criminal Record	White	White Advocate	0.856	174
No Criminal Record	White	Black Advocate	0.783	190
No Criminal Record	Black	No Advocate	0.810	397
No Criminal Record	Black	White Advocate	0.808	172
No Criminal Record	Black	Black Advocate	0.803	174
Criminal Record	White	No Advocate	0.748	346
Criminal Record	White	White Advocate	0.756	203
Criminal Record	White	Black Advocate	0.746	194
Criminal Record	Black	No Advocate	0.737	364
Criminal Record	Black	White Advocate	0.747	175
Criminal Record	Black	Black Advocate	0.776	185

Note: This table reports mean response rates and number of observations for all 12 possible treatment combinations.

Table B5: Main Results – Response Outcome

	Dependent variable: Response (0/1)					
	Full Sample				Advocate Only	
Criminal Record	-0.048^{***}	-0.052^{***}	-0.048^{***}	-0.051^{***}	-0.056^{***}	-0.064^{***}
	(0.015)	(0.015)	(0.015)	(0.015)	(0.022)	(0.022)
Black	0.012	0.012	0.012	0.012	-0.001	-0.003
	(0.016)	(0.015)	(0.016)	(0.015)	(0.022)	(0.022)
White Advocate	0.030	0.034^{*}				
	(0.019)	(0.019)				
Black Advocate	0.015	0.011			-0.015	-0.021
	(0.019)	(0.019)			(0.022)	(0.022)
Advocate (Pooled)			0.023	0.022		
			(0.016)	(0.015)		
Intercept	0.779^{***}	0.769^{***}	0.779^{***}	0.768^{***}	0.820^{***}	0.856^{***}
	(0.016)	(0.111)	(0.016)	(0.111)	(0.022)	(0.143)
Covariates	No	Yes	No	Yes	No	Yes
State FE	No	Yes	No	Yes	No	Yes
N	2917	2917	2917	2917	1459	1459
R-squared	0.004	0.041	0.004	0.040	0.005	0.062

Note: The outcome is a binary response indicator. The treatments are all binary. The covariates are public/private, two-year/four-year and a five-category scale of institution size. Standard errors are shown in parentheses. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table B6: Treatment Effects for Public and Private Schools

	Dependent variable: Response (0/1)		
Criminal Record	-0.052^{***}	-0.099^{***}	-0.053^{***}
	(0.015)	(0.024)	(0.015)
Black	0.012	0.009	-0.021
	(0.015)	(0.015)	(0.024)
Advocate	0.034^{*}	0.032^{*}	0.033^{*}
	(0.019)	(0.019)	(0.019)
Black Advocate	-0.022	-0.019	-0.022
	(0.022)	(0.022)	(0.022)
Public Institution (vs. Private)	0.081^{***}	0.024	0.038
	(0.021)	(0.027)	(0.027)
Criminal Record × Public Institution		0.083^{***}	
		(0.031)	
Black × Public Institution			0.057^{*}
			(0.031)
Intercept	0.769^{***}	0.723^{***}	0.705^{***}
	(0.111)	(0.110)	(0.110)
Covariates	Yes	Yes	Yes
State FE	Yes	Yes	Yes
N	2917	2917	2917
R-squared	0.041	0.045	0.044

Note: The outcome is a binary response indicator. The treatments are all binary. The covariates are two-year/four-year and a five-category scale of institution size. Standard errors are shown in parentheses. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table B7: Main results with interactions

	Dependent variable: Response (0/1)			
Criminal Record	−0.052***	−0.034	−0.043*	−0.052***
	(0.015)	(0.022)	(0.022)	(0.015)
Black	0.012	0.029	0.012	0.027
	(0.015)	(0.022)	(0.015)	(0.022)
White Advocate	0.034*	0.033*	0.055**	0.061**
	(0.019)	(0.019)	(0.027)	(0.027)
Black Advocate	0.011	0.012	0.009	0.013
	(0.019)	(0.019)	(0.027)	(0.027)
Criminal Record × Black		−0.034		
		(0.031)		
Criminal Record × White Advocate			−0.042	
			(0.038)	
Criminal Record × Black Advocate			0.005	
			(0.038)	
Black × White Advocate				−0.057
				(0.038)
Black × Black Advocate				−0.003
				(0.038)
Intercept	0.769***	0.761***	0.760***	0.757***
	(0.111)	(0.112)	(0.112)	(0.112)
Covariates	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
N	2917	2917	2917	2917
R-squared	0.041	0.041	0.041	0.041

Note: The outcome is a binary response indicator. The average response rate is 74.4%. The covariates are public/private, two-year/four-year, institution size and state fixed effects. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

Table B8: Selected School Characteristics for Private and Public Colleges

	Mean	
	Private	Public
Rejection Rate (2013)	0.378	0.336
Avg. SAT (2013)	1077.414	1049.878
Sticker Price in $ (2013)	30312.230	5780.843
Net Price in $ (2013)	17590.030	7955.190
Pct. of Students with Parents in Q1	0.075	0.139
Pct. Black (2000)	0.109	0.132
Parent Median Income in $	96279.550	70383.510

Note: This tables shows averages for seven school characteristics, separately for public and private colleges. The data is based on Chetty et al. (2017).

Table B9: Effect of a criminal record, conditional on school characteristics

	Dependent variable: Response (0/1)						
Criminal Record	−0.079***	−0.085***	−0.065***	−0.063***	−0.061***	−0.067***	−0.060***
	(0.024)	(0.027)	(0.018)	(0.018)	(0.019)	(0.018)	(0.019)
Rejection Rate	−0.007						
	(0.016)						
Rejection Rate * Criminal Record	−0.020						
	(0.024)						
Avg. SAT		0.032*					
		(0.019)					
Avg. SAT * Criminal Record		0.022					
		(0.027)					
Sticker Price			0.006				
			(0.015)				
Sticker Price * Criminal Record			−0.019				
			(0.018)				
Net Price				0.005			
				(0.015)			
Net Price * Criminal Record				−0.021			
				(0.018)			
Pct. with Parents in Q1					−0.054***		
					(0.014)		
Pct. with Parents in Q1 * Criminal Record					0.035*		
					(0.019)		
Pct. Black						−0.040***	
						(0.012)	
Pct. Black * Criminal Record						−0.016	
						(0.018)	
Parent Median Income							0.023*
							(0.013)
Parent Median Income * Criminal Record							−0.011
							(0.019)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Remaining Treatments	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1,262	1,012	2,003	2,006	1,900	1,992	1,900
R-squared	0.020	0.031	0.015	0.015	0.019	0.028	0.012

Note: The outcome is a binary response indicator. All college characteristics are standardized, such that the coefficients measure one-standard deviation increases. The covariates are two-year/four-year and a five-category scale of institution size. In all models, we also include the remaining two treatments, i.e. the advocate treatment and the applicant race treatment. Standard errors are shown in parentheses. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table B10: Effect of a criminal record, conditional on school characteristics (**Public Schools**)

	Dependent variable: Response (0/1)						
Criminal Record	−0.011	−0.022	−0.032	−0.031	−0.025	−0.035	−0.024
	(0.037)	(0.041)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)
Rejection Rate	−0.021						
	(0.026)						
Rejection Rate * Criminal Record	−0.024						
	(0.037)						
Avg. SAT		0.044					
		(0.029)					
Avg. SAT * Criminal Record		0.007					
		(0.041)					
Sticker Price			0.022				
			(0.018)				
Sticker Price * Criminal Record			−0.0002				
			(0.023)				
Net Price				0.005			
				(0.018)			
Net Price * Criminal Record				0.008			
				(0.023)			
Pct. with Parents in Q1					−0.059***		
					(0.017)		
Pct. with Parents in Q1 * Criminal Record					0.038		
					(0.023)		
Pct. Black						−0.036**	
						(0.015)	
Pct. Black * Criminal Record						−0.010	
						(0.023)	
Parent Median Income							0.045**
							(0.018)
Parent Median Income * Criminal Record							−0.015
							(0.023)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Remaining Treatments	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	452	393	1,149	1,151	1,096	1,138	1,096
R-squared	0.020	0.026	0.006	0.004	0.015	0.014	0.010

Note: The outcome is a binary response indicator. All college characteristics are standardized, such that the coefficients measure one-standard deviation increases. We limit the sample to public colleges. The covariates are two-year/four-year and a five-category scale of institution size. In all models, we also include the remaining two treatments, i.e. the advocate treatment and the applicant race treatment. Standard errors are shown in parentheses. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

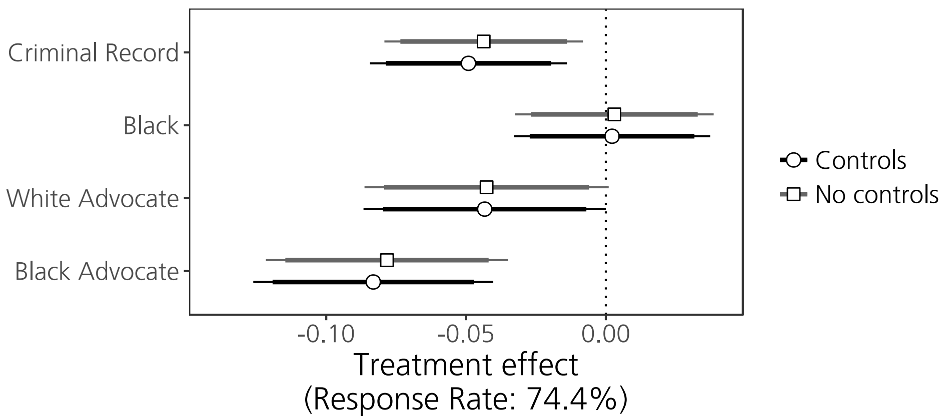
Table B11: Effect of criminal record conditional on school characteristics (**Private Schools**)

	Dependent variable: Response (0/1)						
Criminal Record	−0.119***	−0.127***	−0.114***	−0.111***	−0.112***	−0.112***	−0.113***
	(0.030)	(0.035)	(0.030)	(0.030)	(0.030)	(0.029)	(0.030)
Rejection Rate	−0.005						
	(0.021)						
Rejection Rate * Criminal Record	−0.011						
	(0.031)						
Avg. SAT		0.031					
		(0.026)					
Avg. SAT * Criminal Record		0.036					
		(0.035)					
Sticker Price			−0.005				
			(0.021)				
Sticker Price * Criminal Record			0.054*				
			(0.030)				
Net Price				0.002			
				(0.020)			
Net Price * Criminal Record				0.012			
				(0.030)			
Pct. with Parents in Q1					−0.040*		
					(0.023)		
Pct. with Parents in Q1 * Criminal Record					−0.008		
					(0.030)		
Pct. Black						−0.051**	
						(0.021)	
Pct. Black * Criminal Record						−0.022	
						(0.029)	
Parent Median Income							0.002
							(0.021)
Parent Median Income * Criminal Record							0.033
							(0.031)
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Remaining Treatments	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	810	619	854	855	804	854	804
R^2	0.025	0.043	0.032	0.025	0.033	0.047	0.026

Note: The outcome is a binary response indicator. All college characteristics are standardized, such that the coefficients measure one-standard deviation increases. We limit the sample to **private** colleges. The covariates are two-year/four-year and a five-category scale of institution size. In all models, we also include the remaining two treatments, i.e. the advocate treatment and the applicant race treatment. Standard errors are shown in parentheses. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

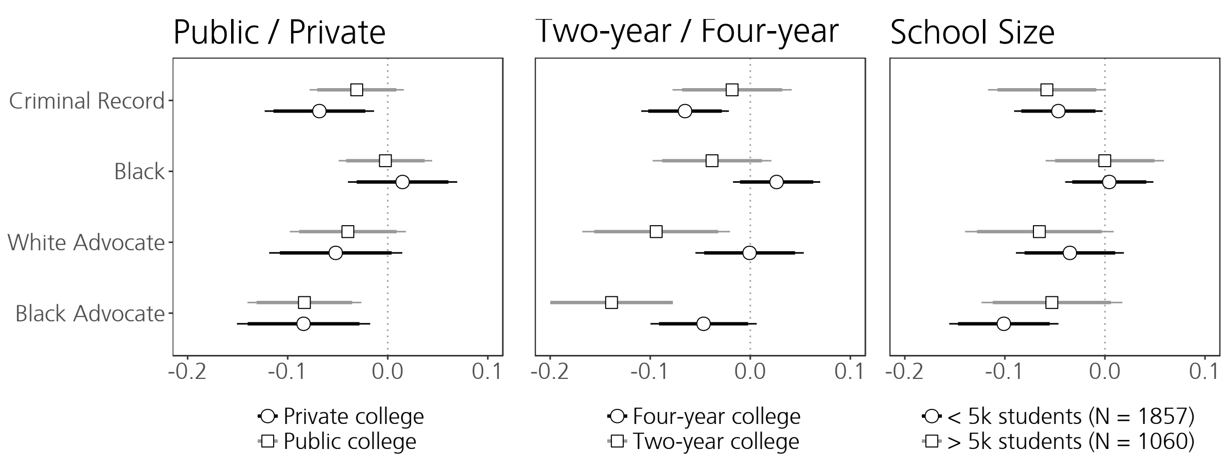
B.1 Friendliness outcome

Figure B3: Main Results – Friendliness



Note: The figure show coefficient estimates from the main specifications. Each pair of coefficients refers to a treatment, which is shown on the y-axis. The outcome is a binary friendliness indicator. Positive effect sizes indicate that the treatment condition increases friendliness. The covariates are public/private, two-year/four-year, institution size and state fixed effects. The solid horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals.

Figure B4: Results Conditional on School Characteristics – Friendliness



Note: The figures show coefficient estimates by school characteristics. Each pair of coefficients refers to a treatment, which is shown on the y-axis. The outcome is a binary friendliness indicator. Positive effect sizes indicate that the treatment condition increases friendliness. Each panel splits the sample into two groups defined by a school characteristic. We then estimate coefficients separately for the resulting sub-samples. All specifications include covariates and state fixed effects. The covariates are public/private, two-year/four-year and institution size. The solid horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals.

Table B12: Main Results – Friendliness

	Dependent variable: Friendliness (0/1)					
	Full sample				Advocate emails only	
Criminal Record	-0.044^{**}	-0.049^{***}	-0.043^{**}	-0.049^{***}	-0.067^{***}	-0.077^{***}
	(0.018)	(0.018)	(0.018)	(0.018)	(0.026)	(0.026)
Black	0.003	0.002	0.003	0.002	-0.005	-0.002
	(0.018)	(0.018)	(0.018)	(0.018)	(0.026)	(0.026)
White Advocate	-0.043^{*}	-0.043^{*}				
	(0.022)	(0.022)				
Black Advocate	-0.078^{***}	-0.083^{***}			-0.036	-0.037
	(0.022)	(0.022)			(0.026)	(0.026)
Advocate (Pooled)			-0.061^{***}	-0.064^{***}		
			(0.018)	(0.018)		
Intercept	0.649^{***}	0.695^{***}	0.649^{***}	0.694^{***}	0.622^{***}	0.743^{***}
	(0.018)	(0.129)	(0.018)	(0.129)	(0.026)	(0.170)
Covariates	No	Yes	No	Yes	No	Yes
State FE	No	Yes	No	Yes	No	Yes
N	2917	2917	2917	2917	1459	1459
R-squared	0.007	0.057	0.006	0.057	0.006	0.084

Note: The outcome is a binary friendliness indicator. The covariates are public/private, two-year/four-year and a five-category scale of institution size. Standard errors are shown in parentheses. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table B13: Main Results with Interactions – Friendliness

	Dependent variable: Friendliness (0/1)			
Criminal Record	-0.049^{***}	-0.058^{**}	-0.025	-0.049^{***}
	(0.018)	(0.025)	(0.025)	(0.018)
Black	0.002	-0.007	0.002	0.006
	(0.018)	(0.025)	(0.018)	(0.025)
White Advocate	-0.043^{*}	-0.043^{*}	-0.019	-0.032
	(0.022)	(0.022)	(0.032)	(0.031)
Black Advocate	-0.083^{***}	-0.083^{***}	-0.059^{*}	-0.087^{***}
	(0.022)	(0.022)	(0.031)	(0.031)
Criminal Record * Black		0.019		
		(0.036)		
Criminal Record * White Advocate			-0.048	
			(0.044)	
Criminal Record * Black Advocate			-0.048	
			(0.044)	
Black * White Advocate				-0.024
				(0.044)
Black * Black Advocate				0.009
				(0.044)
Intercept	0.695^{***}	0.699^{***}	0.678^{***}	0.690^{***}
	(0.129)	(0.129)	(0.130)	(0.130)
Covariates	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
N	2917	2917	2917	2917
R-squared	0.057	0.058	0.058	0.058

Note: The outcome is a binary friendliness indicator. All regressions include covariates and state fixed effects. The covariates are public/private, two-year/four-year and a five-category scale of institution size. The last model only considers cases where an advocate sent the email. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

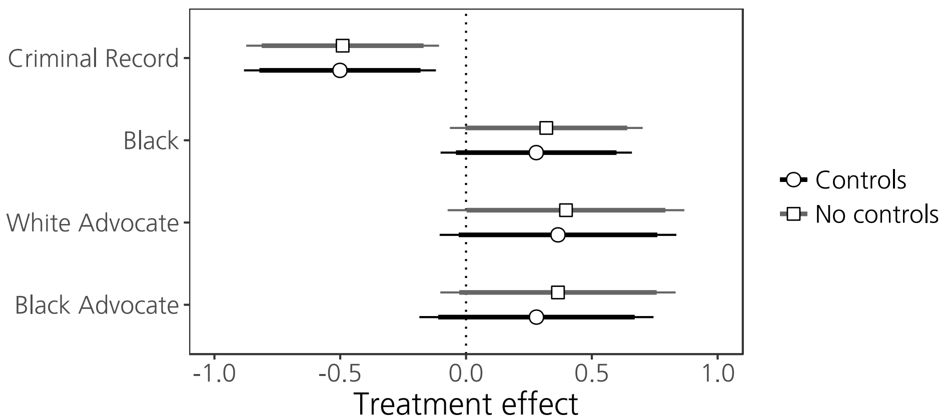
Table B14: Results for Public and Private Schools – Friendliness

	Dependent variable: Friendliness (0/1)		
Criminal Record	-0.049^{***}	-0.071^{***}	-0.049^{***}
	(0.018)	(0.027)	(0.018)
Black	-0.043^{*}	-0.045^{**}	-0.044^{**}
	(0.022)	(0.022)	(0.022)
White Advocate	-0.083^{***}	-0.082^{***}	-0.082^{***}
	(0.022)	(0.022)	(0.022)
Black Advocate	0.002	0.001	0.009
	(0.018)	(0.018)	(0.027)
Public Institution (vs. Private)	0.035	0.005	0.030
	(0.025)	(0.032)	(0.031)
Criminal Record × Public Istitution		0.039	
		(0.036)	
Black × Public Institution			-0.012
			(0.036)
Intercept	0.695^{***}	0.666^{***}	0.646^{***}
	(0.129)	(0.128)	(0.128)
Covariates	Yes	Yes	Yes
State FE	Yes	Yes	Yes
N	2917	2917	2917
R-squared	0.057	0.060	0.059

Note: The outcome is a binary friendliness indicator. The treatments are all binary. The covariates are public/private, two-year/four-year and a five-category scale of institution size. Standard errors are shown in parentheses. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

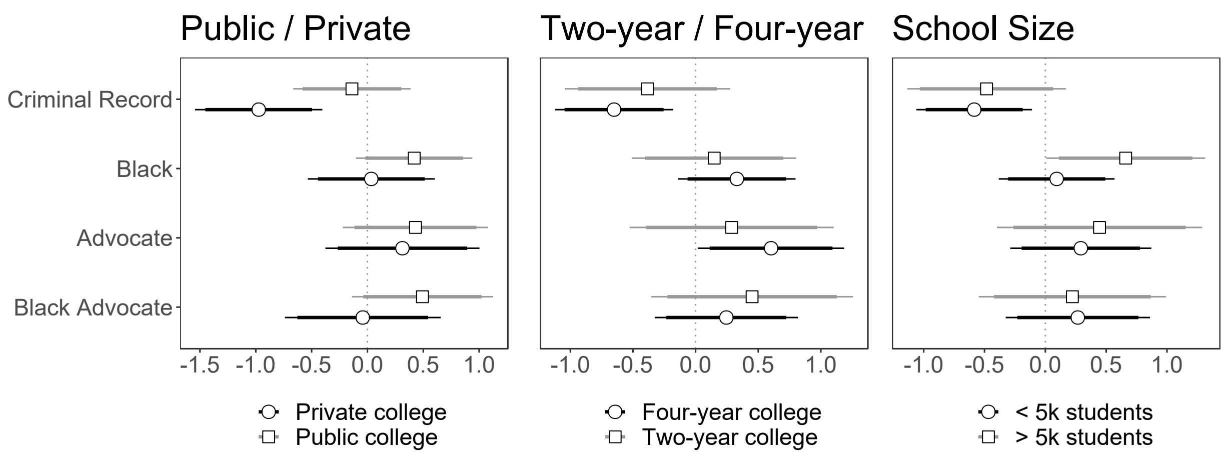
B.2 Thoroughness outcome

Figure B5: Main Results – Thoroughness



Note: The figures show coefficient estimates from the main regressions for the thoroughness outcome. The outcome ranges from 0–3. The covariates are public/private, two-year/four-year and a five-category scale of institution size. The solid horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals.

Figure B6: Results Conditional on School Characteristics – Thoroughness



Note: The figures show coefficient estimates by school characteristics. Each pair of coefficients refers to a treatment, which is shown on the y-axis. The outcome is thoroughness, which ranges from 0–3. Each panel splits the sample into two groups defined by a school characteristic. We then estimate coefficients separately for the resulting sub-samples. All specifications include covariates and state fixed effects. The covariates are public/private, two-year/four-year and institution size. The solid horizontal lines indicate 90% (thick lines) and 95% (thin lines) confidence intervals.

Table B15: Main Results – Thoroughness

	Dependent variable: Thoroughness (0–3)					
	Full sample				Applicant emails only	
Criminal Record	−0.490**	−0.501**	−0.490**	−0.500**	−0.578**	−0.674**
	(0.196)	(0.195)	(0.195)	(0.195)	(0.277)	(0.277)
Black	0.319	0.279	0.319	0.278	0.081	−0.008
	(0.196)	(0.194)	(0.195)	(0.194)	(0.278)	(0.275)
White Advocate	0.397*	0.366				
	(0.240)	(0.240)				
Black Advocate	0.365	0.280			−0.031	−0.102
	(0.239)	(0.237)			(0.277)	(0.279)
Advocate (Pooled)			0.381*	0.322*		
			(0.196)	(0.195)		
Intercept	6.141***	6.803***	6.141***	6.803***	6.697***	7.662***
	(0.196)	(1.360)	(0.196)	(1.360)	(0.279)	(1.760)
Covariates	No	Yes	No	Yes	No	Yes
State FE	No	Yes	No	Yes	No	Yes
N	2362	2362	2362	2362	1187	1187
R-squared	0.005	0.055	0.005	0.055	0.004	0.088

Note: The outcome is a 0–3 thoroughness scale. The covariates are public/private, two-year/four-year and a five-category scale of institution size. Standard errors are shown in parentheses. ***p < .01; **p < .05; *p < .1

Table B16: Main Results with Interactions – Thoroughness

	Dependent variable: Thoroughness (0–3)			
Criminal Record	−0.501**	−0.369	−0.395	−0.508***
	(0.195)	(0.276)	(0.276)	(0.195)
Black	0.279	0.409	0.276	0.523*
	(0.194)	(0.273)	(0.194)	(0.276)
White Advocate	0.366	0.363	0.544	0.756**
	(0.240)	(0.240)	(0.341)	(0.335)
Black Advocate	0.280	0.283	0.312	0.369
	(0.237)	(0.237)	(0.332)	(0.335)
Criminal Record × Black		−0.264		
		(0.391)		
Criminal Record × White Advocate			−0.354	
			(0.479)	
Criminal Record × Black Advocate			−0.070	
			(0.475)	
Black × White Advocate				−0.805*
				(0.478)
Black × Black Advocate				−0.168
				(0.475)
Intercept	6.803***	6.733***	6.714***	6.618***
	(1.360)	(1.365)	(1.368)	(1.365)
Covariates	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
N	2362	2362	2362	2362
R-squared	0.055	0.055	0.055	0.056

Note: The outcome is a 0–3 thoroughness scale. All regressions include covariates and state fixed effects. The covariates are public/private, two-year/four-year and a five-category scale of institution size. The last model only considers cases where an advocate sent the email. ***$p < .01$; **$p < .05$; *$p < .1$

Table B17: Results for Public and Private Schools – Thoroughness

	Dependent variable: Thoroughness (0–3)		
Criminal Record	-0.501^{**}	-0.936^{***}	-0.505^{***}
	(0.195)	(0.290)	(0.195)
Black	0.279	0.253	0.087
	(0.194)	(0.194)	(0.290)
White Advocate	0.366	0.338	0.357
	(0.240)	(0.240)	(0.240)
Black Advocate	0.280	0.283	0.278
	(0.237)	(0.237)	(0.237)
Public institution (vs. Private)	1.175^{***}	0.649^{*}	0.873^{**}
	(0.270)	(0.339)	(0.340)
Criminal Record * Public Institution		0.807^{**}	
		(0.393)	
Black * Public Institution			0.348
			(0.392)
Intercept	6.803^{***}	6.396^{***}	6.162^{***}
	(1.360)	(1.346)	(1.341)
Covariates	Yes	Yes	
State FE	Yes	Yes	
N	2362	2362	2362
R-squared	0.055	0.060	0.058

Note: The outcome is a 0–3 thoroughness scale. The covariates are public/private, two-year/four-year and a five-category scale of institution size. Standard errors are shown in parentheses. $^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$