Bureaucratic Inertia or Legal Responsiveness? College Admissions Officers' Behavior Before and After the Affirmative Action Ban*

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Abstract

How do street-level bureaucrats respond when courts reshape public policy? The 2023 Supreme Court decision striking down affirmative action altered the legal framework around college admissions, prompting speculation about its implications for administrative behavior and minority representation on campus. Yet existing work offers limited evidence on whether – and how – bureaucrats adapt to judicial interventions. We investigate whether college admissions officers shift behavior in response to the ruling. We report results from two original field experiments with over 3,000 U.S. admissions offices. In Study 1, we recontact institutions from a 2018 audit testing responsiveness to Black and White applicants, enabling a pre-post comparison. In Study 2, we randomly vary applicant race (Asian, Black, White) and references to the Court's decision. Across both studies, we find no evidence of racial bias, even when the ruling is salient. Together, these null results suggest limits to the judiciary's influence on bureaucratic behavior.

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1 Introduction

The United States Supreme Court's 2023 decision in *Students for Fair Admissions v. Harvard* and *Students for Fair Admissions v. University of North Carolina* (collectively, SFFA) struck down colleges' use of race-conscious admissions policies, overturning decades of precedent. The ruling reshaped the legal landscape for higher education, introducing new constraints while heightening the salience of race in public discourse. In its wake, observers anticipated major institutional change — especially among frontline admissions officers tasked with interpreting and implementing the law (e.g., Knox, 2023). Yet, a classic tension remains: Do legal rulings recalibrate bureaucratic behavior, or are such actors buffered by professional norms and institutional inertia?

In this paper, we use the case of SFFA to study how bureaucrats respond to legal change. While a large literature examines the Court's influence on political attitudes (e.g., Johnson and Martin, 1998; Clark et al., 2024), less is known about whether and how judicial decisions shift *behavior* – particularly among bureaucrats whose behavior continues to feature prominently in the political science literature (Einstein and Glick, 2017; Druckman and Shafranek, 2020; White, Nathan, and Faller, 2015). Some scholars argue that courts can serve as powerful signaling institutions, shaping public opinion and cueing organizational policy shifts even without direct enforcement (e.g., Horowitz, 1983). Others point to bureaucratic inertia and street-level discretion as buffers against rapid change (Glazer, 1978; Edelman et al., 1991). We speak to these competing expectations by examining whether a major legal decision altered how college admissions bureaucrats respond to inquiries from applicants of different racial backgrounds.

We focus on an early yet influential stage of the admissions pipeline: how colleges respond to information-seeking emails from prospective applicants. These interactions represent a key gate-keeping moment, shaped by the discretion of admissions staff, with the potential to signal institutional openness — or bias — well before formal application review. This setting allows us to test whether a highly salient Supreme Court ruling alters bureaucratic

responsiveness across racial groups.

We conducted two audit field experiments (see also, White, Nathan, and Faller, 2015; Gaddis, 2018; Druckman and Shafranek, 2020; Druckman, Levy, and Sands, 2021).[1] Study 1 replicates and follows up on a 2018 audit experiment from Brown and Hilbig (2022) (hereon, BH22), contacting 2,764 admissions offices with inquiries about a putatively White or Black applicant's enrollment eligibility. We present the same emails to the same schools after SFFA, using a "panel audit" design to measure within-school changes over time. This design offers a clear advantage over designs that only measure responsiveness differences after SFFA, where it would be unclear whether any observed bias (or lack thereof) reflects a change from the pre-SFFA context. We further refine this design by testing if changes are moderated by whether schools considered race in admissions prior to the SFFA ruling. This analysis allows us to account for the possibility that other over-time changes, which may be unrelated to the SFFA decision, affect differences in responsiveness to Black and White applicants.

Study 2 builds on the first via a second audit experiment that investigates admissions officers' responses to inquiries explicitly about race in applications under the new legal framework. This treatment is motivated by the fact that the first study may not sufficiently prime the SFFA decision to elicit differential responsiveness. Study 2 features a factorial design where emails to admissions offices vary across three treatments: (i) applicant race (Black, White, or Asian), (ii) whether the SFFA ruling is referenced, and (iii) whether the email includes a statement of the applicant's quality in the form of a leadership role (class president). To better reflect the SFFA context, we expand the randomly assigned racial categories to include Asian students and randomize explicit reference to the decision to test whether consideration of the ruling changes bureaucratic behavior.

Across both studies, we find consistent evidence that admissions bureaucrats respond at equal rates to applicants of different racial groups. Further, we find that the SFFA decision itself did not alter admissions officers' responsiveness to prospective students based

[1]The experiments were pre-registered at Open Science Framework and the pre-analysis plan can be founded at BLINDED.

on race. In Study 1, response rates declined for all applicants from 77.8% in 2018 to 55.2% in 2024.[2], but racial disparities remained absent. Schools that previously considered race in admissions did not exhibit differential changes in responsiveness compared to those that did not. In Study 2, we find no evidence of racial differences in responsiveness and no consistent effect of referencing SFFA, though we find suggestive evidence that referencing SFFA reduced response rates for Black applicants.

This paper makes both methodological and theoretical contributions. Methodologically, Study 1 implements a "panel audit," directly replicating previously published findings and advancing efforts to establish the external validity of studies of race-based bias in higher education communications. This approach responds to recent calls to prioritize evidence accumulation and generalizability through rigorous testing and retesting (Dunning, 2016; Slough and Tyson, 2024). It also allows rare inferential opportunities – namely, over-time analysis with repeated treatments — enabling stronger tests of institutional behavior change across time. Rather than relying on an unexpected-event-during-survey design, we employ a recontact strategy, illustrating a feasible approach for audit studies to assess the temporal consistency of bureaucratic behavior. Theoretically, we highlight an understudied dimension of racial bias in audit studies: whether shifts in institutional context alter the likelihood of agents exhibiting bias. SFFA presents a critical juncture, heightening the salience of race while reshaping colleges' compliance incentives. Our findings not only reinforce prior evidence of admissions officers' nondiscrimination, but also show that their behavior remains stable even after a major legal shift. This result distinguishes our study from one-off audit designs (Butler and Broockman, 2011; Broockman, 2013; White, Nathan, and Faller, 2015; Einstein and Glick, 2017; Druckman and Shafranek, 2020) and suggests broader limits to external interventions in shaping bureaucratic action.

[2]We discuss potential reasons for this general decline in responsiveness in detail in Section 5.1.

2 The potential for racial bias in admissions correspondence after SFFA

Since *Regents of the University of California v. Bakke* (1978), U.S. colleges have relied on race-conscious admissions to redress persistent inequities and foster diversity, but the Court's June 29, 2023 ruling in *Students for Fair Admissions v. Harvard & UNC* struck down such practices as unconstitutional, criticizing their imprecise goals and burdens on certain groups. Chief Justice Roberts' majority opinion preserved only a sliver of race awareness — students may describe how race shaped their experiences — pushing institutions to revamp essay prompts and emphasize socioeconomic or geographic criteria instead (Hartocollis and Saul, 2024). Admissions officers largely foresee diminished diversity and remain unsure which countermeasures will work (Knox, 2023), leaving higher education in a new, legally fraught experiment over how—or whether—to sustain racial inclusion.

Although the long-term effects of the SFFA ruling for minority representation remain uncertain, the ruling may restructure how colleges interact with prospective students at other stages of the admission process. The ruling re-contextualized the salience of race and revised the legal framework governing how race may be considered. As such, the ruling may create new openings for bureaucratic discretion and potential racial bias, or may effectively limit such discretion and constrain institutional priorities.

We examine one of the earliest points in the application process: correspondence with admissions offices to gather information on eligibility and how to best approach different parts of the application. The provision of such information can reduce application attrition (Bettinger et al., 2012), so racial bias in these initial exchanges could exacerbate enrollment inequities. After SFFA, this initial contact point may be even more important for prospective applicants. The ambiguity of the ruling, combined with the ambiguity as to how colleges are going to adapt their admissions policies to the ruling, makes it unclear as to how aspiring college students should approach discussing their racial and ethnic background in application

materials, and what kind of treatment they should expect based on their race throughout the college application process.

2.1 Why admissions officers may or may not discriminate by race

Admissions bureaucrats may treat applicants differently based on race, either consciously or unconsciously. Previous studies document race-based discrimination in a variety of contexts, such as voting registration offices (White, Nathan, and Faller, 2015), politicians' constituent outreach (Butler and Broockman, 2011), and hiring decisions (Bertrand and Mullainathan, 2004). Such bias may stem from overt prejudice (Becker, 1957), or more implicit biases leading bureaucrats to subconsciously down-weight the importance of responding to emails from applicants of certain races because of stereotypes (Greenwald and Banaji, 1995).

Differential treatment may also arise due to cultural, legal, or institutional pressures that influence downstream bureaucratic behavior (Scholz and Wei, 1986). Organizational and professional priorities that emphasize diversity and equity can reduce discriminatory behavior (Keiser et al., 2002). Moreover, legal standards of racial fairness set forth by the Civil Rights Act and other anti-discrimination laws have established general norms of egalitarian bureaucratic behavior across many institutional contexts (Skrentny, 1996)

Perhaps because of these factors, studies of college admissions show little evidence of bias (Gaddis et al., 2021). In their study of 4-year colleges, Druckman and Shafranek (2020) find no direct effect of applicant race on admissions correspondence, although they do find an interactive effect of applicant race and referencing politics in the email. Hanson (2017) studies admissions counselors varying applicant race (Black versus White), applicant quality, and writing quality and finds no effect of applicant race on response rates. Given this small number of studies, more evidence is required to make satisfactory conclusions about whether racial bias influences admissions correspondence, particularly in light of the new reality for college admissions after the SFFA ruling.

2.2 Why the SFFA ruling may or may not change admissions bureaucrats' responses to applicant race

The Supreme Court decision on race-conscious admissions upended the status quo for how college admissions offices consider race and for how applicants of different races navigate the college admissions process. However, it is difficult to anticipate the consequences of the new legal framework for *admissions correspondence*. The SFFA decision governs race in admissions evaluations, not general communication. Discretion remains, and personal bias may still influence responses. That said, fear of litigation may heighten caution and promote neutrality in response to applicant race (Edelman, 1990). Some institutions, aiming to sustain diversity, may even encourage increased support for underrepresented applicants. After state-level affirmative action bans, schools adjusted their admissions practices to maintain campus diversity (Antonovics and Backes, 2014).[3]

In the absence of clear guidance, SFFA may make race more salient while not altering the potential for bureaucratic discretion to influence less regulated points of the application process, such as informational correspondence. The lawsuits against Harvard and UNC alleged that Asian and White applicants were being penalized in admission decisions to preserve rates of Black and other underrepresented minority enrollment (*Students for Fair Admissions, Inc. v. President and Fellows of Harvard College*, 2023). Given Supreme Court decisions' capacity to change attitudes via landmark decisions (e.g., Clark et al., 2024), this context could be interpreted by admission bureaucrats as meaning that they must now compensate for past grievances against Asian or White applicants. Alternatively, per Hoekstra (2000)'s argument that Supreme Court decisions can entrench pre-existing attitudes that are at odds with the ultimate decision, they may interpret the case as representing further marginalization of already underrepresented Black applicants, and alter their behavior to compensate for that perceived loss. Thus, the SFFA ruling could cause racial bias (in several

[3]Nine states banned the policy in state-run universities at some point prior to SFFA. These include: California (1996), Washington (1998, reinstated in 2022), Florida (1999), Michigan (2006), Nebraska (2008), Arizona (2010), New Hampshire (2012), Oklahoma (2012), and Idaho (2020).

different directions) to emerge in admissions correspondence by altering the framing of racial group positioning in the college application process (Bleemer, 2022).

The nature of admissions work may also contribute to officers' lack of response to the ruling. Because BH22 and other studies of higher education administrators' behaviors (e.g., Gaddis et al., 2021) find little to no racial bias in communications prior to the SFFA decision, this suggests that these pre-existing behavioral standards likely serve as anchors for administrators' actions in the post-SFFA period. The pressures of daily work life also encourage employees to develop routines that guide their behavior, insulating their actions from external influences (Glazer, 1978; Zacka, 2017). Hence, individuals' tendency to default to established behaviors and navigate workplace identity conflicts can mediate the effect of regulatory changes on their actions. Additionally, when employees' identities conflict with the realities of their work or their organization's goals, they often rely on discretionary behaviors to mitigate this disruption (Miller, 1967; Edelman et al., 1991). Hence, both the nature of the ruling *and* the demands of admissions work could contribute to the absence of any clear effect of SFFA on behavior.

2.3 Competing hypotheses

Based on the above discussion, we offer a set of competing hypotheses which we test through our experimental designs. Our experiments, whose designs we describe in detail in the following section, collectively test for racial bias in college admission correspondence, how that bias has potentially changed since the Supreme Court ruling, and whether the ruling directly alters the effect of applicant race. In our analysis of racial bias, we test for differential response rates to Black and White applicants in the first study, and to Asian, Black, and White applicants in the second study. The competing hypotheses for these tests of racial bias are as follows:

H1: Equal responsiveness: *No differences in responsiveness between groups.*

Admissions communication has shown minimal bias in previous studies (Gaddis et al., 2021). Hence, organizations' and workers' tendency towards maintaining status quo behaviors combined with the vague nature of the Supreme Court decision could suggest that the SFFA ruling would have no effect on admissions bureaucrats' behaviors, continuing a norm of *equal responsiveness.*

H2: Majority group responsiveness: *White applicants will be most likely to receive a response compared to Asian or Black applicants.*

The SFFA decision may have raised the personal and professional stakes for admissions bureaucrats when addressing race in admissions. Due to their majority group status, White applicants may be seen as less risky to engage with compared to Asian and Black applicants. This can lead to *majority group responsiveness.*

H3: Aggrieved group responsiveness: *Asian applicants will be more likely to receive a response than Black applicants.*

Asian applicants were the chief complainants associated with the SFFA cases, and the Students for Fair Admissions organization continues to threaten legal action against schools that do not increase Asian enrollment. A fear of litigation, or the belief that Asian students are more deserving of attention — for ethical reasons or reasons related to statistical discrimination — could lead to this *aggrieved group responsiveness* on the part of admissions bureaucrats.

H4: Historically marginalized group responsiveness: *Black applicants will be more likely to receive a response.*

The SFFA decision prompted discussions within higher education about its potential effects on underrepresented minority admissions, and previous, state-level affirmative action bans have been met with intra-institutional policies meant to aid Black and Hispanic prospective

applicants. Hence, admissions bureaucrats may respond to the ban by particularly focusing on Black students in an attempt to encourage their applications, leading to *historically marginalized group responsiveness.*

In addition to our race treatments, in Study 2 we also prime admissions bureaucrats' specific considerations of the SFFA decision by referencing the Supreme Court. These treatments test the following competing hypotheses:

H5a: Judicial compliance: *Referencing the Supreme Court decision will promote equal responsiveness by race.*

Nominally, the SFFA decision bars race-based discrimination — positive or negative — from occurring in college admissions. Making the decision salient will lead admissions bureaucrats to exhibit compliant behavior both because of the potential personal costs of litigation, and the diffuse, normative benefits associated with following the law.

H5b: Judicial noncompliance: *Referencing the Supreme Court decision will intensify racial bias.*

Referencing the SFFA decision may alternatively cause admissions bureaucrats to discriminate between applicants based on race. Priming fear of litigation may lead to preferential treatment for Asian or White prospective students. Alternatively, given the case's unpopularity with admissions bureaucrats and institutions' diversity goals, referencing the decision could lead to preferential treatment for Black prospective students in an attempt to encourage applications.

3 Experimental designs

The goal of our study is to test whether racial differences in bureaucratic responsiveness to applicants changed after the SFFA decision. To do so, we conduct two pre-registered audit experiments (Gaddis, 2018).

The first study is a panel audit experiment assessing differences in admissions bureaucrats' response to Black and White applicants before and after SFFA. To conduct this panel audit experiment, we re-contact admissions offices at schools that were included in BH22, a 2018 audit experiment study testing for discrimination against formerly incarcerated applicants and by applicant race. In the 2018 contact, admissions offices were asked about whether a GED was sufficient for admittance to the school, randomizing the race of the applicant (Black versus White) and whether the applicant had received their GED online or in a state penitentiary. The 2018 contact also randomized whether the email was sent by an advocate (a former teacher) on behalf of the applicant, and the race of the advocate.

In 2024, we recontacted still active schools from BH22. We then measure 1) whether schools respond at different rates by the race of the applicant in the re-contact, 2) whether the effect of applicant race is different than it was in the first contact, and 3) whether any changes from 2018 to 2024 are a function of whether schools considered race in their admissions prior to the Supreme Court case (i.e. at the time of the first contact but not at the time of the second contact).

The second experiment tests for differential responsiveness to Asian, Black, and White applicants asking about schools' race considerations in admissions. In this study, we contact each school asking about how race should be discussed in the application and whether applicants can be penalized if they talk about their racial background. In each email, we randomize the race of the applicant, whether the email specifically mentions the Supreme Court case, and randomize a signal of applicant quality through the inclusion of "'24-'25 Class President" in their email signature.

This experiment builds on and complements the first experiment in several ways. First, we ask college admissions offices specifically about how to discuss race in the application. While the first study tests for bias by race in correspondence not directly about race, this second study asks the most direct, and relevant, question about race in admissions after the Supreme Court case, whether applicants should avoid discussing race in their applica-

tions. Second, we include a third racial category, Asian, along with Black and White racial categories. The context of the Supreme Court case was that Asian applicants were being discriminated against by Harvard in admissions consideration. Some have argued that this penalization of Asian applicants was done in order to preserve admittance rates of other groups, such as Black or White applicants (Hartocollis and Saul, 2024). As such, how admissions offices respond to Asian applicants relative to other groups is a first-order question after SFFA. Third, while Study 1 cues race by the name of the applicant, in Study 2 we use both the name of the applicant and specifically state the applicant's race in the text of the email (i.e., "As an Asian applicant, what is an appropriate way to talk about my race in the application?"). This design choice removes uncertainty as to whether the admissions bureaucrat reading the email accurately perceives the intended race of the applicant, strengthening the treatment and removing a factor that might cause us to underestimate racial bias (Kaufman, Celaya, and Grumbach, 2025). Fourth, by randomizing mention of SFFA, we measure how directly referencing the legal case alters admissions bureaucrats' behavior, and whether bias by applicant race is mitigated or augmented if the applicant specifically references the Supreme Court case when asking how race should be discussed in the college application.

All together, the empirical tests derived from these two studies can be summarized as follows: First, do racial differences in bureaucratic responsiveness to applicants change after the SFFA decision? Second, do Black-White differences in responsiveness change differentially depending on whether colleges considered race in admissions prior to the SFFA decision? Third, after the SFFA decision, is there differential bureaucratic responsiveness to Asian, Black, or White applicants? Lastly, does making the SFFA decision salient to admissions officers affect racial gaps in responsiveness?

Both studies were fielded between September 23 and October 3, 2024. The experiments were administered concurrently, and we sent 5,771 emails over 8 weekdays (Monday-Thursday). For the first study, we recontacted 2,764 schools from the 2018 contact that were still active as of 2024. For the second study, we contacted all of the 3,007 schools in our

data. We sent the emails for Study 1 from 8 different Gmail accounts containing the name of the applicant, and the emails from Study 2 were sent from 12 different email accounts (2 emails per name and 2 names per racial treatment group). We spread sending across days and emails to reduce the chance of being classified as spam. We further randomized the day of the week each school was contacted. For schools contacted for both studies (i.e. the 2,764 schools in Study 1, as all schools were in Study 2), we randomized which study's email a school was sent first and sent the other study's email a week after the first email.[4]

To code schools as having responded versus not, we discard automatic responses, and count a school as having responded if we receive a response email within three weeks of the initial contact. Figure B2 in the Supporting Information presents histograms on the time between first contact and a school's response. Most schools responded within one day of the contact email.

In the following subsections, we describe the specifics of each study, including email language and the corresponding estimation strategies. In Supporting Information Section A.2, we further outline the specifics of our contact strategy for each study. We also discuss ethical considerations related to audit studies in Section A.4 in the Supporting Information.

3.1 Study 1: Revisiting Brown and Hilbig (2022) after *SFFA*

In February 2018, Brown and Hilbig (2022) contacted 2,917 college admissions offices across the United States. Each email asked about whether a GED was sufficient to enroll in the college. The emails included four randomized treatment: 1) whether the applicant disclosed they had received their GED online or in a state penitentiary, 2) whether the applicant had a putatively White or Black name, 3) whether the email was sent by the applicant themself or by a teacher on their behalf, and 4) the race of the teacher. These treatments were block randomized using coarsened exact matching on school size, whether the school was public versus private, and whether the school offered 2-year or 4-year programs.

[4]In Tables D10 and D11 we report response rates for both studies by whether the school received the email from Study 1 or Study 2 first. We find no differences in response rates by email order.

Figure 1: Study 1 Email Language

From: [**Email Address from Black or White Applicant**]
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**]

The primary goals of that study were to test for bias against formerly incarcerated college applicants and for bias based on applicant race. From that study, college admissions offices were 5 percentage points less likely to respond to emails from formerly incarcerated applicants compared to those who got their GED's online. Between Black and White applicants, however, there was no difference in response rate.

In September/October of 2024, we recontacted the 2,764 still active schools from the initial 2018 study. Each school was sent an email from an applicant using the same name as in 2018.[5] Thus, each school was assigned the same race treatment in both the first and second time periods of contact. We did not include the other treatments in this second contact, as our primary focus is on the effect of the SFFA decision on Black-White differences in responsiveness.[6] Figure 1 shows the email language used to contact schools.

We use four different names (White: Kevin Schmidt, Bob Krueger; Black: Darnell Banks, Tyrone Booker) to cue Black and White students in Study 1. These are the same names used in BH22, and were pre-tested at the time of that study and were classified by survey

[5]For schools originally assigned in the 2018 contact to the no advocate treatment, we sent the 2024 email from the same email address. For those assigned to the advocate treatment in 2018, which we do not incorporate in 2024, we sent from one of the two email addresses associated with the name of the applicant (which was the same as in 2018), randomly assigning which of the two email addresses to send from.

[6]In the Supporting Information Section D, we present results demonstrating our main results are not biased by the exclusion of the other treatment conditions from the 2018 study.

respondents as the correct race more than 90% of the time (Brown and Hilbig, 2022). For each of the four names, we used two different email accounts to reduce the risk that our emails were classified as spam by email service providers. For each name, the two emails only differ in the sequence of three digits following the applicant's name. Table A1 in the Supporting Information lists the emails used to run the experiment in Study 1. To assess race effects, we pool results across email and name.[7]

Study Design & Estimation

With this setup, we assess whether there is a change in the difference in response rates to Black and White applicants before and after the Supreme Court decision. We estimate two quantities of interest. The first is a before-after comparison of the race treatment effects in 2024 and 2018. The second leverages variation in which schools considered race prior to SFFA ending the consideration of race in college admissions for all schools to estimate a triple-difference estimator of how changes across time vary by whether a school saw a change in whether it considered race between the two experimental contacts.[8]

Before-after: We contact college i at two points in time ($t = 0$ and $t = 1$). We denote the pre-SFFA period as $t = 0$ and the post-SFFA period as $t = 1$. D_i is a binary variable indicating whether the applicant is Black or White, which is constant across both periods. This means each college is exposed to the same race treatment both before and after the

[7]Tables B2-B5 report response rates by name and email address and regression analysis of effects of name and email address on response rate conditional on race. We find no evidence of differential response rates by name conditional on race, and no evidence of differential response rate by email conditional on name.

[8]Data on schools race considerations is sourced from the Common Data Set (CDS), an annual survey of colleges and universities administered by the College Board. Schools are coded as 'considering race' if they select any option other than 'not considered' when asked the relative importance of a prospective student's racial or ethnic status in first-year admission decisions (options include 'very important,' 'important,' 'considered,' and 'not considered.') Data are collected from the nearest-dated CDS to the SFFA decision in 2023. In addition to the 27% of our sample had a locatable CDS, we also code schools in states with affirmative action bans as not considering race for a sample of 1,025 schools (289 considering race and 737 not considering). In the Supporting Information (Table B1), we expand the set of schools we code as 'not considering' by including schools that have open admissions policies (see, for example, Bowen and Bok, 1998).

SFFA decision. The race treatment assigned to college i was determined in the original audit study by BH22. The response of college i at time t is Y_{it}, a binary variable indicating whether a response was received. The primary quantity of interest is:

$$\tau_{PrePost} = \underbrace{E[Y_{i1}(D_i = 1) - Y_{i1}(D_i = 0)] - E[Y_{i0}(D_i = 1) - Y_{i0}(D_i = 0)]}_{\text{Change in bias between 2024 and 2018}}$$

The original study by BH22 found no significant differences in response rates between Black and White applicants. In other words, $E[Y_{i0}(D_i = 1) - Y_{i0}(D_i = 0)]$ was estimated to be close to zero. In this before-and-after design, we estimate the following linear model[9]:

$$Y_{i1} - Y_{i0} = \alpha + \tau_{PrePost} D_i + \beta \mathbf{X}_i + \varepsilon_i \tag{1}$$

where $\tau_{PrePost}$ measures the change in the difference between Black and White applicant response rates before and after the SC decision. $\mathbf{X}_i$ is the set of school characteristic control variables.[10] We estimate models with and without controls for school characteristics used in block randomization (public versus private, school size, and 2-year versus 4-year programs). Models with controls also include state fixed effects, to emulate BH22. All models use heteteroskedastic-robust standard errors.

Triple differences: One potential issue with the simple before-and-after approach is that other factors may have changed between the pre- and post-SFFA periods that influence the responsiveness of admissions offices to prospective students of different races. For example, shifts in the applicant pool, changes in admissions office personnel, or political changes like a new sitting president could all potentially impact response rates.

To address potential confounding factors that might influence responsiveness over time, we exploit the fact that some colleges had already stated they did not consider race in

[9]Our pre-analysis plan reports this estimation strategy in the style of a two-way fixed effect design. We present the first-differences model here for ease of interpretation but note that these two models are equivalent.

[10]We use school characteristics measured at the time of the 2018 contact and treat them as time-invariant in the estimation.

admissions prior to the SFFA decision. For these colleges, we would not expect the Supreme Court ruling to affect their responsiveness to applicants of different races. Therefore, any changes in Black-White differences in responsiveness at these colleges can serve as an estimate of trends unrelated to the court decision. This logic is similar to a typical difference-in-differences design, where the parallel trends assumption allows the researcher to use the control group to impute counterfactual outcome trends in the treatment group. The logic in our case is similar – our design requires the following assumption: the change in Black-White responsiveness differentials in the control group, i.e., colleges that did not consider race, before and after the SFFA decision, can be used to estimate the change in Black-White response rate differentials in the treated group. In this case, the treated group is colleges that considered race prior to SFFA, and were therefore affected by the Supreme Court decision. The triple difference-in-differences quantity of interest is as follows:

$$\tau_{DiDiD} = \underbrace{\big(E[Y_{i1}(1,1) - Y_{i1}(0,1)] - E[Y_{i0}(1,1) - Y_{i0}(0,1)]\big)}_{\text{Change in bias among AA-colleges}} - \underbrace{\big(E[Y_{i1}(1,0) - Y_{i1}(0,0)] - E[Y_{i0}(1,0) - Y_{i0}(0,0)]\big)}_{\text{Change in bias among colleges that never had AA}}$$

Here, the first bracketed term is the change in bias among colleges that considered race prior to the SFFA decision, and were therefore affected by the SFFA decision. The second bracketed term is the change in bias among colleges that never considered race, and were therefore not affected by the SFFA decision. As a result, if the second term is not zero, there were some factors unrelated to the SC decision that affected Black-White differences in responsiveness. Conversely, if the second term is zero, there is no evidence that bias in responsiveness changed due to factors unrelated to the SFFA decision.[11]

By comparing changes in Black-White responsiveness between colleges that considered race prior to the SFFA decision and those that did not, we can therefore isolate the effect of

[11]No bias in responsiveness implies that Black-White differences in responsiveness are zero or close to zero.

the Supreme Court ruling. We again use linear regression to estimate the triple difference. Let A_i be an indicator for whether school i considered race in admissions prior to SFFA. We use the following specification[12] with heteroskedastic-robust standard errors:

$$Y_{i1} - Y_{i0} = \alpha + \gamma D_i + \delta A_i + \tau_{DiDiD}(D_i \times A_i) + \beta \mathbf{X}_i + \varepsilon_i. \tag{2}$$

The coefficient τ_{DiDiD} captures the triple-difference estimate of the effect of the Supreme Court decision on the difference in responsiveness to Black and White applicants.

3.2 Study 2: Asking Admissions Bureaucrats about Race

Study 2 consists of a 3x2x2 factorial design where each admissions officer is sent a single email. It was fielded to 3,007 public and private non-profit colleges. This sample was constructed from an exhaustive list of colleges operating in 2024, obtained from the National Center for Education Statistics (NCES). Each email contains three treatments: 1) the race of the applicant, 2) whether the email mentions SFFA, and 3) whether the email signature signals that applicant is their high school class president. All possible treatment combinations are permitted and equally likely. Figure 2 shows the email language.

Applicant race treatment. Each email is randomly assigned to appear as though it is from an Asian, Black, or White applicant. The applicant's race is explicitly stated in the body of the email and further signaled by the applicant's name. Emails in the Asian treatment group use one of two putatively Asian names (Andy Wang or Peter Li), emails in the Black treatment group use one of two putatively Black names (Jermaine Wood or Jermaine Williams), and emails in the White treatment group use one of two putatively White names (David Hoffman or William Snyder). These names have been pre-tested to verify that they are interpreted as the "correct" race, based on prior work by Crabtree et al. (2023).

[12] We again present the first-differences version of the equation for ease of interpretation but note that the pre-analysis plan reports an equivalent stacked data model.

Figure 2: Study 2 Email Language

From: [**Email Address from Black or White or Asian Applicant**]
To: [Admissions Email Address]
Subject: Admissions Info

Hello,

I am interested in applying to [School], but I have a couple questions about [**Randomize Insert: how the recent Supreme Court decision on affirmative action changes/BLANK**] how I should approach the application. As a/n [**Randomize Insert: Asian / Black / White**] applicant, what is an appropriate way to talk about my race in the application? Will I be penalized if I talk about my racial background?

Thank you,

[**Applicant Name**]

[**Randomize Insert: '24-'25 Class President/BLANK**]

For each of the six names cuing the three different racial groups, we again created two email accounts to reduce the risk of being classified as spam when sending the emails. For a given name, the email addresses only differ in the sequence of four digits after the applicant's name. Table A2 in the Supporting Information lists the emails used to run the experiment in Study 2. As in Study 1, in our estimation of race effects, we pool results across email and name. [13]

Supreme Court treatment. Each email is also independently randomly assigned to include or omit language referencing the recent Supreme Court decision on affirmative action. The inclusion of this language highlights the decision's implications for discussing race in the application process.

Class president treatment. Finally, we include a treatment indicating whether the applicant was the class president of their graduating class. Emails assigned to this treatment

[13] In Study 2, we find one case of differential response rate by name (David Hoffman versus William Snyder) conditional on race and no cases of differential response rate by email conditional on name. Tables B2-B5, report these results.

include a statement about class presidency at the end of the email, while emails not assigned to the treatment omit this statement. The purpose of this treatment is to ensure that admissions offices do not conflate applicant race with academic or extracurricular performance, as the class president treatment signals that the applicant is an accomplished student.

3.2.1 Loss of data: jermaine.williams7106@gmail.com

After sending all of the 251 emails randomly assigned to be sent from the Gmail address jermaine.williams7106@gmail.com, we were locked out of this account by Gmail's servers. This was due to Gmail classifying the Gmail account as "potentially hacked or hijacked". Due to this classification, we were locked out of the account and could not access or observe at all the responses to the emails sent from this account. We appealed this decision but were not able to recover the account's data. The possibility of this setback occurring was part of our motivation to spread the sending of emails out over many days and email accounts, to mitigate the damage from any problems with an individual account.

As a result, we dropped schools assigned to this email address from all Study 2 analyses. Since schools were randomly assigned to names and emails, dropping these observations should not bias our results. However, it does reduce the statistical power of our estimation of the effect of an applicant being Black. In the Supporting information Section C, we conduct a simulation analysis that imputes response outcomes for these unobserved data, under the assumption that response rates for this account would look similar to the other Jermaine Williams (jermaine.williams7016@gmail.com) account. From these simulations, we find that having full access to the data would likely produce similar estimates and statistical significance as in our main results. Therefore, we conclude that our overall conclusions from Study 2 are not threatened by losing access to this email's responses.

3.2.2 Estimation

To estimate the treatment effects, we use the following linear regression model:

$$Y_i = \alpha + \beta X_i + \sum_{r \in \{Asian, Black\}} \delta_r D_{ri} + \gamma Z_i + \tau P_i + \varepsilon_i \quad (3)$$

Where Y_i is a binary variable indicating whether the email received a response, D_{ri} are indicator variables for Asian and Black applicants (with White as the reference category), Z_i is the binary Supreme Court treatment, and P_i is the binary class president treatment. X_i represents the vector of pre-treatment covariates: an indicator denoting whether the institution is public or private, an indicator denoting whether it is two-year or four-year, a categorical measure of an institution's student population size, and state fixed effects. All models in Study 2 estimate heteroskedastic-robust standard errors.

We further test whether racial differences in responsiveness are moderated by (i) the Supreme Court treatment and (ii) the class president treatment. To investigate whether racial differences in responsiveness are moderated by the Supreme Court treatment, we estimate the following model:

$$Y_i = \alpha + \beta X_i + \sum_{r \in \{Asian, Black\}} \delta_r D_{ri} + \gamma Z_i + \tau P_i + \sum_{r \in \{Asian, Black\}} \phi_{rZ}(D_{ri} \cdot Z_i) + \varepsilon_i \quad (4)$$

Similarly, to examine whether racial differences in responsiveness are moderated by the class president treatment, we estimate the following model:

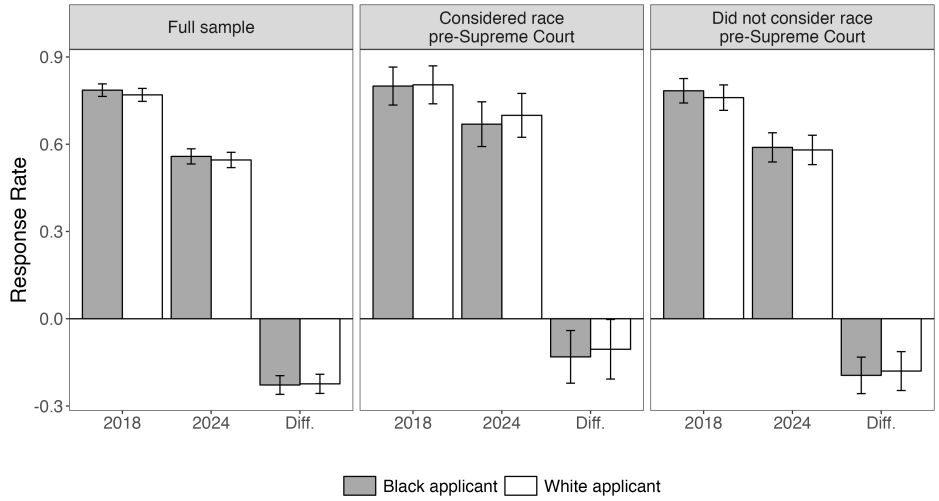
$$Y_i = \alpha + \beta X_i + \sum_{r \in \{Asian, Black\}} \delta_r D_{ri} + \gamma Z_i + \tau P_i + \sum_{r \in \{Asian, Black\}} \phi_{rP}(D_{ri} \cdot P_i) + \varepsilon_i \quad (5)$$

4 Results

4.1 Study 1

First, we present the results from Study 1: the panel audit study comparing response rates to Black and White applicants in the 2018 and 2024 experimental contacts. Figure 3 presents response rates across treatment conditions in Study 1. Response rates declined from 2018 to 2024 across all subgroups. The average response rate to the 2018 contact was 77.8%, while

Figure 3: Response rates across treatment conditions in Study 1



Notes: Figure plots response rates across treatment conditions including 95% confidence intervals.

the average response rate in 2024 was 55.2%.

This general decline in response rates could be due to several compounding factors. First, the 2018 contact occurred in February, while the 2024 contact occurred in September, so admission offices were likely receiving a higher number of applicant emails at the time as college applications deadlines are generally in the Fall semester. Second, based on data from the Common Application, the number of college applications has risen by 39% since the 2019-2020 cycle (Hughes et al., 2024), so college admissions offices are likely receiving many more inquiries about the application process. Third, advancements in spam filtering technologies have made it increasingly challenging for legitimate emails to bypass these defenses. In 2024, for example, both Google and Yahoo implemented stricter spam filter algorithms, which may inadvertently classify genuine inquiries as spam, potentially reducing the likelihood of responses (Kim, 2024). We cannot comprehensively observe the rate at which the emails we sent were classified as spam, although a few of the responses we received apologized for a delayed response and referenced that the email was sent to the spam folder. To the extent that this contributed to the declined responses rates, however, these changes in the email information environment represent real obstacles to securing information that applicants

must navigate. We further acknowledge that our 2024 response rates are still in line with response rates found in previous audit studies (Gaddis et al., 2021).[14]

Each of these factors may have contributed to the declined responses rates from 2018 to 2024, but do not bias our estimation of the treatment effects because these factors are constant across treatment groups.[15] In Figure 4, we present estimates of the Black-White difference in responsiveness in the 2018 and 2024 experiments. We find no evidence for differences in responsiveness in either experiment.[16] The point estimate for the effect of a Black applicant on response in the 2018 experiment is 1.62 percentage points (from the model with control variables), but this estimate is not statistically distinguishable from zero, with a 95% confidence interval ranging from -1.47 to 4.71 percentage points.[17] The treatment effect estimate for the 2024 response is 1.38 percentage points (95% CI: -2.31 to 5.06 percentage points), and is similarly statistically indistinguishable from zero.

Beyond the main results, it is relevant to examine whether we have sufficient statistical power to rule out substantially meaningful effects. To quantify the study's sensitivity, we calculated the Minimum Detectable Effect Size (MDES), defined as the smallest true effect that the study design could detect with 80% power at the 5% significance level (Bloom, 1995). For the 2024 experiment, the standard error of the Black coefficient from the specification with covariates is 0.019 (see section A.5). The MDES is then approximately ± 0.053, or ± 5.3 percentage points.[18] We are therefore reasonably confident (with 80% power) that the true effect is not larger than 0.0532 and not smaller (more negative) than -0.0532. We consider

[14] While speculative, the lower response rate in the 2024 study suggests admissions offices had not become aware of being audited. If awareness had increased, we would likely expect higher response rates, as prompt responses could reflect positively on the office. Thus, the observed drop in responsiveness is inconsistent with the hypothesis of heightened audit awareness.

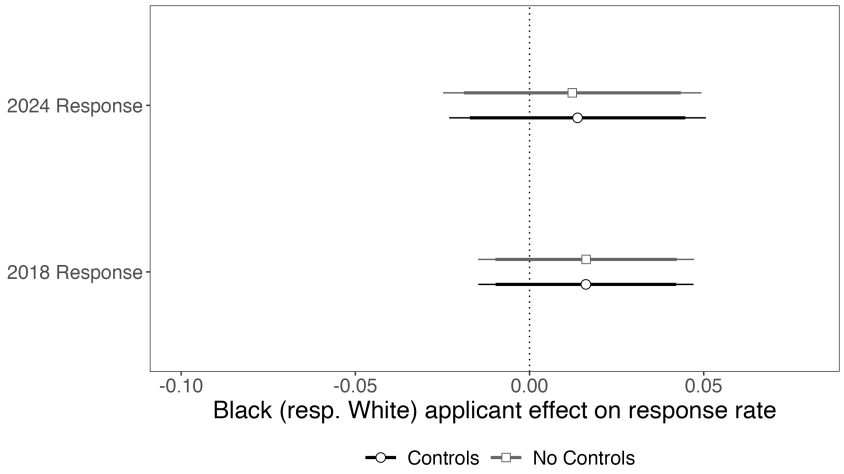
[15] The treatment effect in this context can most appropriately be thought of as an intent-to-treat effect, since compliance (opening an email) is not measured.

[16] Results with coefficients for all variables in the models are reported in Table A4.

[17] The results reported here for the 2018 experiment are reported for the sample of 2,764 schools contacted in both 2018 and 2024, rather than the full sample from BH22.

[18] The MDES is calculated using the formula: $\text{MDES} = \text{SE} \times (z_{\alpha/2} + z_{1-\beta})$, where SE is the standard error of the coefficient (0.019), $z_{\alpha/2}$ is the critical z-value for a two-tailed test at the significance level α (1.96 for $\alpha = 0.05$), and $z_{1-\beta}$ is the critical z-value corresponding to the desired statistical power $1 - \beta$ (0.84 for 80% power). Thus, $\text{MDES} \approx 0.019 \times (1.96 + 0.84) \approx 0.053$.

Figure 4: Main results from Study 1



Notes: The coefficients shown here is the effect of the race treatment, with Black applicants defined to be $D_i = 1$, and White applicants defined to be $D_i = 0$. We estimate the effect of the race treatment separately for the 2018 study (pre-SFFA) and the 2024 study (post-SFFA). For response rates by treatment status, see Figure 3.

Black-White differences of 5 percentage points or less reasonably small. While we cannot rule out small differences in responsiveness (that is, below ≈ 5 percentage points), our design is sufficiently powered to rule out meaningfully large differences.

Finally, we assess whether Black-White response rate differentials have changed over time. The effect sizes and confidence intervals suggest that the treatment effect estimates in 2018 and 2024, which each themselves cannot be statistically distinguished from zero at conventional significance threshold, are also not statistically distinct from each other. This indicates that there is no measurable change in differential response rate between Black and White applicants between the two experiments. We formally estimate this in columns 1 and 2 of Table 1, which report estimates from Equation 1. From specifications with and without controls, the estimate difference in 2024 and 2018 response rate due to the race of the applicant is -0.4 and -0.2 percentage points, respectively, with both estimates not statistically significant.[19]

We further test for differential changes in responsiveness when comparing schools that

[19]Results with coefficients for all variables in the models are reported in Table 1.

considered race versus schools that did not. We present the results from Equation 2 in columns 3 and 4 of Table 1, and find no evidence that responsiveness pre- and post-SFFA changed differentially when comparing schools that considered race vs. schools that did not (not statistically significant point estimates of -1.1 and -1.4 percentage points, respectively). We do note, however, that the statistical precision of the triple difference-in-difference estimates is lower than the quantities of interest in the previous estimation. This is a result of the limited sample due to data availability and smaller cells due to multiple interactions in the estimation.

Table 1: Pre-post response rate differences by race and pre-Supreme Court race consideration

	2024 Response - 2018 Response			
	(1)	(2)	(3)	(4)
Black (resp. White)	-0.004	-0.002	-0.015	-0.003
	(0.023)	(0.024)	(0.047)	(0.047)
Considered race			0.075	-0.006
			(0.062)	(0.071)
Black (resp. White) × Considered race			-0.011	-0.014
			(0.084)	(0.086)
Controls	No	Yes	No	Yes
State FEs	No	Yes	No	Yes
R^2	1.04×10^{-5}	0.021	0.003	0.060
Observations	2,764	2,764	1,025	1,025

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

In the Supporting Information, we report pre-registered analyses for treatment effect heterogeneity by school characteristics. In Tables D1-D3 we examine effect heterogeneity by whether a school is public versus private, by school size, and by whether a school offers 2-year or 4-year programs. We report this for the 2018 experiment (Table D1, the 2024 experiment (Table D2) and the change in this response across the two contacts (Table D3). We find no evidence of effect heterogeneity by any of these pre-registered school characteristics. In the above-referenced tables, we also report exploratory (not pre-registered) analyses of effect

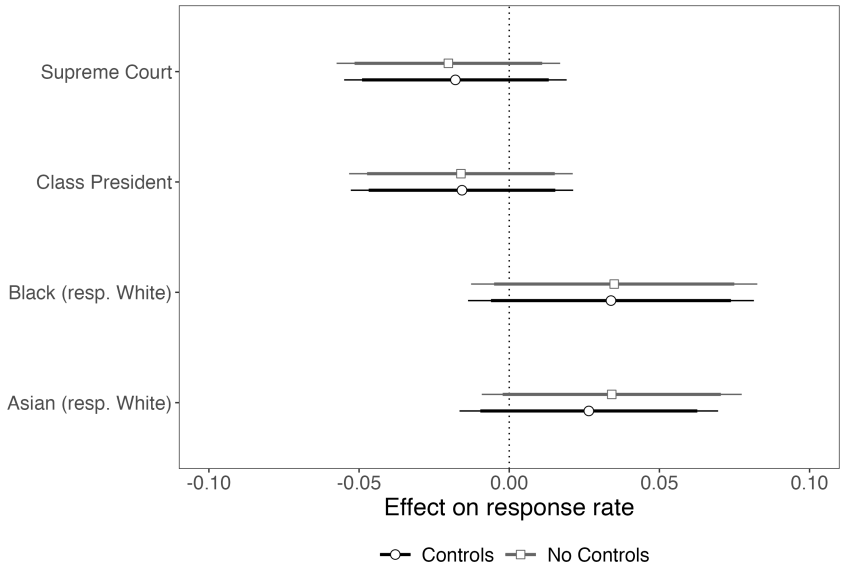
heterogeneity by student body racial Herfindahl index, whether a school is in a majority Republican county, and whether the school has an admissions rate below 50%. We find no evidence of effect heterogeneity by school racial diversity nor by the partisan composition of a school's county. For the 2018 experiment, we find evidence that schools with lower admissions rates were more likely to respond to a Black applicant. We do not find such effect heterogeneity in the 2024 study.

The findings from Study 1 present clear and consistent evidence on racial bias in admissions correspondence. First, we find no evidence in either experiment that college admissions officers responded at different rates to Black or White applicants inquiring about GED eligibility. Second, there is no evidence that bureaucrats changed their behavior in this context from 2018 to 2024. Lastly, whether a school considered race in admissions prior to SFFA does not moderate any changes across time in the treatment effects. These findings provide consistent evidence against hypotheses that Black or White applicants may be favored by admissions officers in terms of responsiveness, as well as evidence that the SFFA ruling did not influence how Black or White applicants are treated when corresponding with admissions offices.

4.2 Study 2

Next, we summarize our results from Study 2, measuring the effect of applicant race (Asian, Black, and White), referencing SFFA Supreme Court case, and applicant quality (class president) treatments on whether admissions officers respond to inquiries about how to discuss race in college applications. The overall response rate for Study 2 was 45.8%, lower than in Study 1. This is likely due to a more detailed inquiry in Study 2. Study 1 asked a short question about GED eligibility but Study 2 asks multiple questions about how race should be discussed in the college application. As such, Study 2 likely requires more work for the admissions bureaucrat to respond compared to Study 1, potentially leading to the

Figure 5: Main results from Study 2



Notes: The outcome is binary and takes the value one if the email received a response. Thick bars represent 95% confidence intervals and thin bars represent 90% confidence intervals. Raw response rates by treatment condition are shown in figure B1 in the appendix.

lower observed response rate.[20]

Figure 5 summarizes the main treatment effects from Study 2.[21] We find no statistically significant evidence to support racial bias in admissions correspondence. The effect estimate for Black applicants compared to White applicants is 3.67 percentage points (with controls) but is not statistically distinguishable from zero. The effect estimate for Asian applicants compared to White applicants is 2.79 percentage points, and is similarly not significant. Accordingly, there also is no statistically significant difference in response rates between Asian and Black applicants. We also find negative (-1.82 percentage points), albeit insignificant, effects of mentioning SFFA directly. Finally, we find no evidence that the class president treatment affects results (-1.70 percentage points).[22]

[20]See table D9 for a more detailed exploration of response differences between the two studies. We find some evidence that smaller institutions and public institutions are less likely to reply to the email from study 2 compared to the email from study 1. We do not find evidence that any of the treatments in either study 1 or study 2 leads to changes in response rates for the same institution, comparing across the two studies.

[21]Results with coefficients for all variables in the models are reported in Table A6.

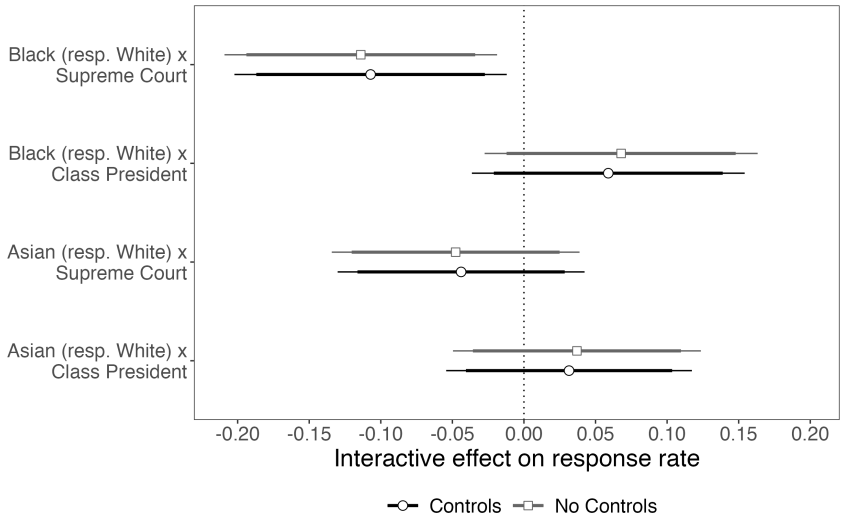
[22]We present raw response rates by treatment condition in figure B1 in the appendix.

As with the first study, we again calculated the Minimum Detectable Effect Size (MDES), defined as the smallest true effect that the study design could detect with 80% power at the 5% significance level (Bloom, 1995). Using the standard errors from Model 2 in Table A6, the MDES for the Supreme Court treatment (SE=0.019) is approximately ±0.053 (5.3 percentage points). For the Asian (resp. White) treatment (SE=0.022), the MDES is ±0.062 (6.2 percentage points), and for the Black (resp. White) coefficient (SE=0.024), the MDES is ±0.067 (6.7 percentage points). Given these MDES values and the non-significant coefficients observed in Model 2 for these variables, we cannot rule out small effects below these thresholds. However, the study is sufficiently powered to conclude that effects larger than roughly 5-7 percentage points for these treatments are unlikely.

We then further investigate whether the SFFA and class president treatment effects vary with applicant race. In Figure 6, we report the results from the treatment interaction models (Equations 4 and 5). We find no significant evidence that differential response rates between Asian and White applicants are moderated by referencing the SSFA decision. We do, however, find evidence that mentioning the SFFA decision reduces response rate by 10.7 percentage points for Black applicants compared to White applicants. As we show in Figure 7, which plots the response rates by race and other treatment subsets, these differences appear to stem from the fact that admissions officers are somewhat more responsive to Black applicants when there is no mention of the Supreme Court decision. When the SFFA decision is mentioned, responsiveness to Black applicants decreases, while responsiveness to White applicants increases, and responsiveness to Asian applicants is mostly unchanged. This results in response rates across racial groups all being closer when the SFFA decision is referenced.

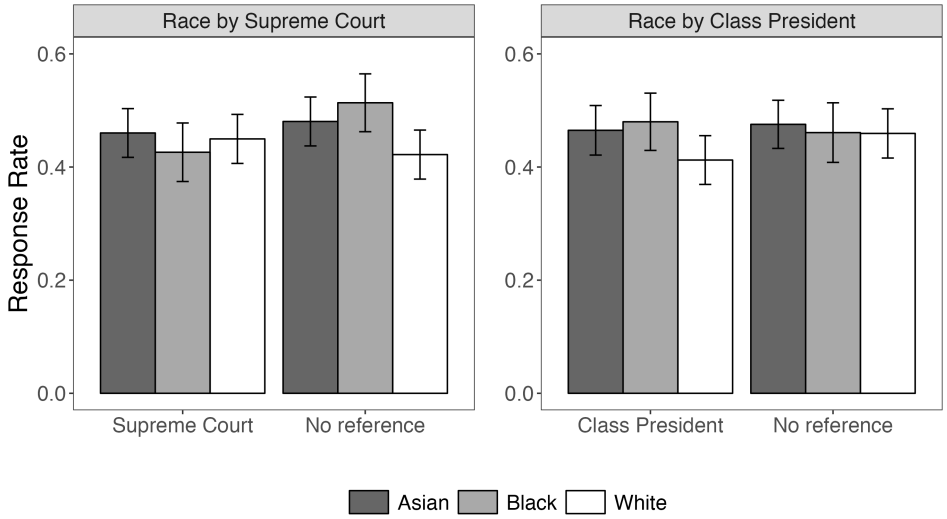
For both Black and Asian applicants, we find insignificant effects for the interaction of applicant race (with respect to White applicants) and class president treatment interaction. This lack of a moderation effect by class president treatment suggests that conflating race and academic or extracurricular achievement is not a major concern in this setting. It does not

Figure 6: Treatment interactions in Study 2



Notes: The outcome is binary and takes the value of one if the email received a response. Thick bars represent 95% confidence intervals and thin bars represent 90% confidence intervals.

Figure 7: Response rates by race and Supreme Court treatment, class president treatment in Study 2



Notes: The figure shows response rates, conditional on the interaction between the race treatment and (i) the Supreme Court treatment and (ii) the class president treatment in study 2.

seem to be the case that responsiveness to different racial groups changes when admissions officers receive information about the applicant's academic or extracurricular achievements in the form of the class president treatment.

In the Supporting Information Tables D4-D5, we report pre-registered tests for heterogeneity in treatment effects across institutional characteristics (public versus private, school size, 2-year versus 4-year). We also present exploratory analyses in those tables (as in Study 1) for heterogeneity by school diversity, the partisan composition of each school's county, and school admissions rate. For both race and Supreme Court treatments, we find generally no evidence of effect heterogeneity by any of these institutional characteristics. The one exception is that we find that applicants who reference the SFFA decision in their email are more likely to get a response from a less diverse school than from a more diverse one.

In summary, Study 2 builds on the findings in Study 1 by providing evidence that differential response by applicant race is minimal even when applicants ask directly about race in college admissions. This experiment provides a much more likely test wherein racial biases may emerge, yet still we find that Asian, Black, and White applicants are equally likely to receive a response when sending this kind of inquiry. The smallest differences emerge between Black and Asian applicants, which is perhaps surprising given adversarial framing of SFFA between those two groups (*Students for Fair Admissions, Inc. v. President and Fellows of Harvard College*, 2023; *Students for Fair Admissions, Inc. v. University of North Carolina*, 2023). Whether admissions officers view either of these groups as aggrieved prior to or in the aftermath of SFFA, these sentiments do not influence response rates. For the general question of differential responsiveness by race, Study 2 supports the findings from BH22 and Study 1 of no racial bias in college admissions correspondence.

Unlike BH22 and Study 1, however, we do find contexts in which differential treatment by race may emerge. Specifically, Black applicants who reference the Supreme Court see reduced response rates compared to Black applicants who do not reference SFFA. We do not see statistically significant effects of Supreme Court treatment for other racial groups, although the response rates for White applicants who reference the Supreme Court case do seem to increase. Thus, it may be the case that Black applicants are particularly penalized for raising the saliency of the legal implications and contemporary political context of how

race is handled in college admissions. Conversely, it may also be the case that the SFFA reference causes admissions bureaucrats to be even more careful not to let any differential response rates by race emerge, as the response rates in the reference SFFA subset across racial groups are all closer to each other than in the subset where SFFA is not referenced. As such, we find some evidence that raising the saliency of the legal implications of how race is considered in admissions might promote equal responsiveness by race.

5 Conclusion

This study investigates whether admissions bureaucrats exhibit racial bias in responsiveness to prospective students following the Supreme Court's 2023 ruling against race-conscious admissions in *Students for Fair Admissions v. Harvard*. We conducted two large-scale field experiments involving over 3,000 U.S. colleges to assess potential changes in admissions practices. In the first, we re-contacted admissions offices from a 2018 audit to evaluate changes in responsiveness to Black and White applicants before and after the decision. The second study tested admissions officers' responses to inquiries about how race should be discussed in applications, randomly varying applicant race, references to the Court ruling, and indicators of applicant achievement.

Across both studies, we find no evidence of racial bias in responsiveness before or after the ruling, nor differential changes among schools that previously considered race in their admissions decisions. While schools that did consider race prior to SFFA were plausibly more affected by the court decision, we find no evidence that this changed how admissions officers respond to applicant race. We further show that the absence of Black-White differences in responsiveness in the second study holds across many institutional subgroups, reducing concerns that subgroup variation masks treatment effects.

Study 2 finds no overall effect of referencing the Supreme Court decision. However, we do find suggestive evidence that referencing the decision reduces response rates for Black

applicants compared to White applicants. This echoes findings by Druckman and Shafranek (2020), who show that Black prospective students receive fewer responses when referencing their political participation. Together, these results raise broader concerns that Black students — perhaps more likely to reference the ruling due its racialized impact — could face a systemic disadvantage in outreach to admissions officers.

Our findings enhance understanding of when and where racial bias emerges in the college application process. Specifically, we corroborate recent work finding limited racial bias in admissions correspondence (Gaddis et al., 2021). While bias has been observed in outreach to other institutions — such as employers and government officials — it does not appear in college admissions. Bias may still emerge at other stages of the process, but our evidence demonstrates equitable treatment at the outreach stage.

Our findings also inform institutional responses to the SFFA decision. First, we find no evidence that universities are adapting admissions communications to offset the ban on explicit affirmative action. Second, if adaptations are occurring to pursue diversity, they likely occur at later stages of the admissions process. We acknowledge that since our studies fielded in Fall 2024, the SFFA decision has been invoked by federal agencies under President Trump to restrict the use of race in higher education, including in "administrative support" like the contact we study.[23] The evolving interpretation of the ruling underscores the need for future research on institutional responses.

Our methodological approach also points to promising directions for bias research. To our knowledge, this is the first audit study to recontact the same institutions with the same treatments using a panel audit format. Repeat interventions such as this are likely feasible for many audit studies, and can help establish the temporal consistency (or lack thereof) of the findings from these studies. Furthermore, they allow for flexible testing of how institutional developments may alter bureaucratic behavior.

Finally, our results contribute to broader theories of bureaucratic behavior under insti-

[23]See `https://www.ed.gov/media/document/dear-colleague-letter-sffa-v-harvard-109506.pdf`

tutional change. Even major legal decisions like SFFA may leave day-to-day bureaucratic practices largely intact. Our findings suggest that bureaucratic routines can be resilient to formal policy shifts, highlighting the need to distinguish between legal change and behavioral change when assessing the impact of external shocks.

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Supporting Information

Intended for online publication only.

A Additional information on the experiments and the research design

A.1 Study email addresses

Table A1: List of Names for Study 1

Name	Email	Putative race
Kevin Schmidt	kevin.schmidt143@gmail.com	White
Kevin Schmidt	kevin.schmidt134@gmail.com	White
Bob Krueger	bob.krueger143@gmail.com	White
Bob Krueger	bob.krueger134@gmail.com	White
Darnell Banks	darnell.banks143@gmail.com	Black
Darnell Banks	darnell.banks134@gmail.com	Black
Tyrone Booker	tyrone.booker143@gmail.com	Black
Tyrone Booker	tyrone.booker134@gmail.com	Black

A.2 Comparing response rates 2018 and 2024

Table A2: List of Names for Study 2

Name	Email	Putative race
William Snyder	william.snyder7016@gmail.com	White
William Snyder	william.snyder7106@gmail.com	White
David Hoffman	david.hoffman7016@gmail.com	White
David Hoffman	david.hoffman7106@gmail.com	White
Jermaine Wood	jermaine.wood7016@gmail.com	Black
Jermaine Wood	jermaine.wood7106@gmail.com	Black
Jermaine Williams	jermaine.williams7016@gmail.com	Black
Jermaine Williams	jermaine.williams7106@gmail.com	Black
Andy Wang	andy.wang7016@gmail.com	Asian
Andy Wang	andy.wang7106@gmail.com	Asian
Peter Li	peter.li7016@gmail.com	Asian
Peter Li	peter.li7106@gmail.com	Asian

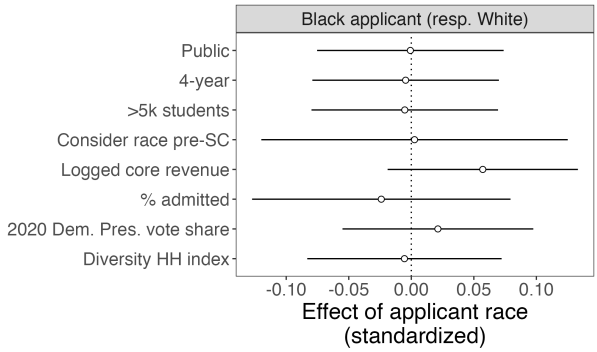
Table A3: Pre-post response rate differences by school characteristics

	2024 Response - 2018 Response	
	(1)	(2)
Constant	-0.084***	
	(0.025)	
Public (resp. Private)	-0.051*	-0.058*
	(0.024)	(0.027)
4-year (resp. 2-year)	-0.041·	-0.039·
	(0.022)	(0.020)
Above 5k students (resp. below 5k)	0.044*	0.049**
	(0.021)	(0.017)
State FEs	No	Yes
R^2	0.003	0.025
Observations	2,764	2,764

*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

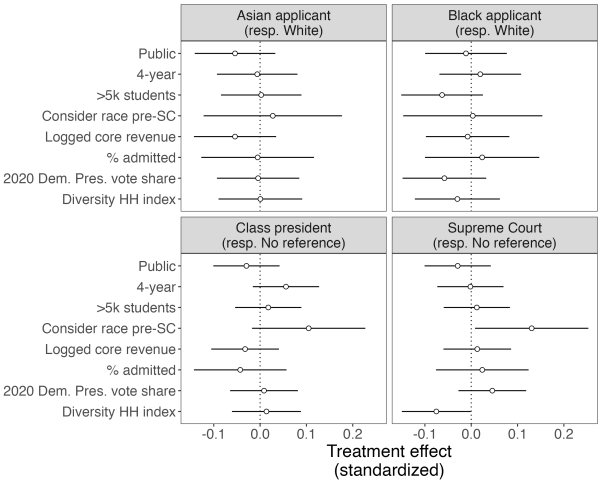
A.3 Covariate balance

Figure A1: Covariate balance in Study 1



Notes: the figure presents covariate balance results. Each estimate is the coefficient from regression a given covariate on the binary treatment indicator. Each covariate is standardized prior to estimating balance. For some covariates, we have nonneglible amounts of missing values, which accounts for the larger standard errors for some the estimates.

Figure A2: Covariate balance in Study 2



Notes: the figure presents covariate balance results. Each etimate is the coefficient from regression a given covariate on the binary treatment indicator. Each covariate is standardized prior to estimating balance. For some covariates, we have nonneglible amounts of missing values, which accounts for the larger standard errors for some the estimates.

A.4 Ethical Considerations

When designing the two experiments, we took several measures to address ethical concerns related to the burdens that audit studies can place on bureaucratic institutions, as well as the potential effects on the communities that depend on these institutions. To reduce administrative burden, we crafted our email messages to be concise, asking questions that did

not require lengthy responses. Additionally, we limited our communication with admissions offices to one email per study.

Our analysis relies on two studies that each reach out to the same relatively large sample of schools. Reaching out to fewer colleges, or conducting only one of the two studies would have lowered the overall administrative burden of our research. The two studies are designed to address related but distinct questions that each parse the predictors of administrative compliance with legal decisions. Moreover, while a smaller sample size may have been adequate to detect substantively large main effects for both studies, our pre-registered design and heterogeneous effects analyses require a larger number of observations. Because our design decisions are both analytically and substantively important — such as accounting for institutions' prior race considerations, or status as a public or private school — and the burden on individual schools to answer emails is minimal, we opted to use a relatively large sample.

Our two studies employ deception, which carries the risk of potentially influencing admissions bureaucrats' future behaviors. We use deception because it is the only feasible method to test for real-world bias in responsiveness, and we believe the social significance of our research justifies our approach (see Einstein and Glick, 2017, for a similar discussion). To protect anonymity, our analysis presents results in aggregate form only, without reporting or sharing any identifiable information for schools or individuals.

We further note that we do not analyze the content of the email responses we received. This aligns with the previously discussed goal of protecting anonymity of the individuals who wrote the responses. One of the IRBs that approved this study indicated that analyzing responses would require the consent of the individuals who wrote the responses – as stated above, our study necessarily involves deception and does not have a consent component. Therefore, we currently view it as infeasible to analyze responses while complying will all IRBs that approved this study.

A.5 Main results regression tables

Table A4: Response rate differences by race in 2018 and 2024 contacts, Study 1

	DV: response (0/1)			
	(1)	(2)	(3)	(4)
Constant	0.770***		0.546***	
	(0.011)		(0.013)	
Black (resp. White)	0.016	0.016	0.012	0.014
	(0.016)	(0.016)	(0.019)	(0.019)
Two-year institution		-0.003		-0.048*
		(0.020)		(0.024)
Small institution		-0.041*		-0.072**
		(0.018)		(0.023)
Public institution		0.076***		0.053*
		(0.022)		(0.026)
Controls	No	Yes	No	Yes
State FEs	No	Yes	No	Yes
R^2	0.0004	0.035	0.0002	0.049
Observations	2,764	2,764	2,764	2,764

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

Table A5: Pre-post response differences by race and pre-Supreme Court race consideration

	2024 Response - 2018 Response			
	(1)	(2)	(3)	(4)
Constant	-0.224***		-0.180***	
	(0.017)		(0.034)	
Black (resp. White)	-0.004	-0.002	-0.015	-0.003
	(0.023)	(0.024)	(0.047)	(0.047)
Two-year institution		-0.044		0.009
		(0.030)		(0.074)
Small institution		-0.031		-0.015
		(0.028)		(0.047)
Public institution		-0.024		-0.067
		(0.032)		(0.051)
Considered race			0.075	-0.006
			(0.062)	(0.071)
Black (resp. White) × Considered race			-0.011	-0.014
			(0.084)	(0.086)
Controls	No	Yes	No	Yes
State FEs	No	Yes	No	Yes
R^2	1.04×10^{-5}	0.021	0.003	0.060
Observations	2,764	2,764	1,025	1,025

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

B Additional results

B.1 Response rates by treatment condition in study 2

Table A6: Response rate differences by race, Supreme Court, and class president treatments, Study 2

	DV: response (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.454***		0.430***		0.470***	
	(0.021)		(0.024)		(0.024)	
Asian (resp. White)	0.034	0.026	0.058·	0.049	0.016	0.011
	(0.022)	(0.022)	(0.031)	(0.031)	(0.031)	(0.031)
Black (resp. White)	0.035	0.034	0.091**	0.087*	0.0003	0.004
	(0.024)	(0.024)	(0.034)	(0.034)	(0.035)	(0.035)
Supreme Court	-0.020	-0.018	0.027	0.027	-0.021	-0.018
	(0.019)	(0.019)	(0.031)	(0.031)	(0.019)	(0.019)
Class President	-0.016	-0.016	-0.015	-0.015	-0.048	-0.043
	(0.019)	(0.019)	(0.019)	(0.019)	(0.031)	(0.031)
Two-year institution		-0.020		-0.022		-0.019
		(0.024)		(0.024)		(0.024)
Small institution		-0.121***		-0.122***		-0.121***
		(0.023)		(0.023)		(0.023)
Public institution		0.006		0.006		0.006
		(0.025)		(0.025)		(0.025)
Asian (resp. White) × Supreme Court			-0.048	-0.044		
			(0.044)	(0.044)		
Black (resp. White) × Supreme Court			-0.114*	-0.107*		
			(0.049)	(0.049)		
Asian (resp. White) × Class President					0.037	0.032
					(0.044)	(0.044)
Black (resp. White) × Class President					0.068	0.059
					(0.049)	(0.049)
Controls	No	Yes	No	Yes	No	Yes
State FEs	No	Yes	No	Yes	No	Yes
R^2	0.002	0.049	0.004	0.051	0.003	0.050
Observations	2,756	2,756	2,756	2,756	2,756	2,756

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

B.2 Triple difference-in-differences results with alternative coding of considered race pre-SFFA ruling

Here we present estimates of the triple difference-in-difference estimation but with schools where data for whether they considered race is missing coded as not considering race.

Figure B1: Response rates by treatment condition in Study 2

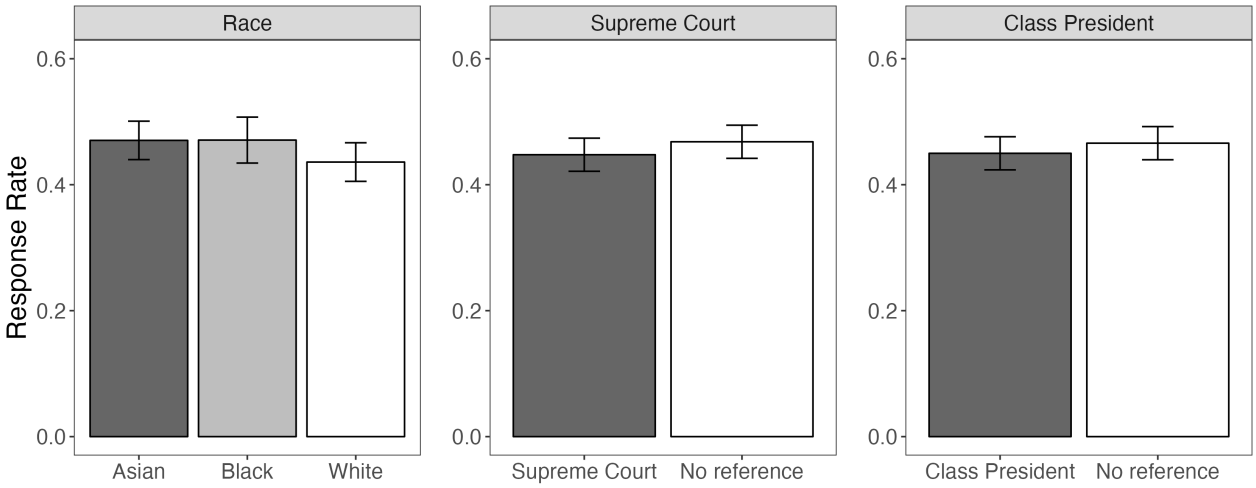


Table B1: Pre-post response rate differences by race and pre-Supreme Court race consideration, alternative considered race coding

	2024 Response - 2018 Response	
	(1)	(2)
Black (resp. White)	-0.001	0.002
	(0.025)	(0.025)
Considered race	0.133*	0.119*
	(0.055)	(0.058)
Black (resp. White) × Considered race	-0.025	-0.038
	(0.074)	(0.075)
Controls	No	Yes
State FEs	No	Yes
R^2	0.004	0.023
Observations	2,764	2,764

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

B.3 Response rates and treatment effect by sender names and email addresses in study 1

Table B2: Responses rates by name for study 1

		2024		2018			
	Schools	Resp. Rate	SE	Resp. Rate	SE	Diff.	SE
Black							
Darnell Banks	680	0.553	0.019	0.781	0.016	−0.228	0.024
Tyrone Booker	712	0.563	0.019	0.791	0.015	−0.228	0.023
White							
Bob Krueger	709	0.544	0.019	0.762	0.016	−0.217	0.024
Kevin Schmidt	663	0.548	0.019	0.778	0.016	−0.231	0.024

Table B3: Responses rates by email for study 1

		2024		2018			
	Schools	Resp. Rate	SE	Resp. Rate	SE	Diff.	SE
Bob Krueger							
bob.krueger134@gmail.com	339	0.522	0.027	0.743	0.024	−0.221	0.035
bob.krueger143@gmail.com	370	0.565	0.026	0.778	0.022	−0.214	0.033
Darnell Banks							
darnell.banks134@gmail.com	338	0.577	0.027	0.763	0.023	−0.186	0.034
darnell.banks143@gmail.com	342	0.529	0.027	0.798	0.022	−0.269	0.032
Kevin Schmidt							
kevin.schmidt134@gmail.com	324	0.571	0.028	0.790	0.023	−0.219	0.034
kevin.schmidt143@gmail.com	339	0.525	0.027	0.767	0.023	−0.242	0.033
Tyrone Booker							
tyrone.booker134@gmail.com	355	0.544	0.026	0.775	0.022	−0.231	0.032
tyrone.booker143@gmail.com	357	0.583	0.026	0.807	0.021	−0.224	0.032

B.4 Response rates and treatment effect by sender names and email addresses in study 2

Table B4: Study 1 response rate differences by sender name

Race	2024	2018	Diff.	2024	2018	Diff.
		Black			White	
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.563***	0.791***	-0.228***	0.544***	0.762***	-0.217***
	(0.019)	(0.015)	(0.023)	(0.019)	(0.016)	(0.024)
Darnell Banks	-0.010	-0.010	-0.0004			
	(0.027)	(0.022)	(0.033)			
Kevin Schmidt				0.003	0.017	-0.014
				(0.027)	(0.023)	(0.034)
R^2	0.0001	0.0001	1.15×10^{-7}	9.57×10^{-6}	0.0004	0.0001
Observations	1,392	1,392	1,392	1,372	1,372	1,372

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*
Note: Omitted categories are Tyrone Booker and Bob Krueger for the Black and White treatment subsets, respectively.

Table B5: Study 1 response rate differences by sender email

	2024	Diff.	2024	Diff.	2024	Diff.	2024	Diff.
	Bob Krueger		Darnell Banks		Kevin Schmidt		Tyrone Booker	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	0.565***	-0.214***	0.529***	-0.269***	0.525***	-0.242***	0.583***	-0.224***
	(0.026)	(0.033)	(0.027)	(0.032)	(0.027)	(0.033)	(0.026)	(0.032)
bob.krueger134	-0.043	-0.008						
	(0.037)	(0.048)						
darnell.banks134			0.048	0.083·				
			(0.038)	(0.047)				
kevin.schmidt134					0.046	0.023		
					(0.039)	(0.048)		
tyrone.booker134							-0.039	-0.007
							(0.037)	(0.045)
R^2	0.002	3.71×10^{-5}	0.002	0.005	0.002	0.0003	0.002	3.24×10^{-5}
Observations	709	709	680	680	663	663	712	712

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*
Note: Omitted category for each name subset is the 143@gmail.com version of the email.

B.5 Timing of responses

Table B6: Responses rates by name for study 2

	Schools	Resp. Rate	SE
White			
David Hoffman	517	0.393	0.021
William Snyder	490	0.482	0.023
Asian			
Andy Wang	531	0.469	0.022
Peter Li	498	0.472	0.022
Black			
Jermaine Williams	230	0.487	0.033
Jermaine Wood	490	0.463	0.023

Table B7: Responses rates by email for study 2

	Schools	Resp. Rate	SE
White			
david.hoffman7016@gmail.com	257	0.381	0.030
david.hoffman7106@gmail.com	260	0.404	0.030
william.snyder7016@gmail.com	258	0.465	0.031
william.snyder7106@gmail.com	232	0.500	0.033
Asian			
andy.wang7016@gmail.com	271	0.465	0.030
andy.wang7106@gmail.com	260	0.473	0.031
peter.li7016@gmail.com	238	0.492	0.032
peter.li7106@gmail.com	260	0.454	0.031
Black			
jermaine.williams7016@gmail.com	230	0.487	0.033
jermaine.wood7016@gmail.com	229	0.424	0.033
jermaine.wood7106@gmail.com	261	0.498	0.031

C Simulation analysis of missing data

Table B8: Study 2 response rate differences by sender name

	DV: Response (0/1)		
Race	White	Asian	Black
	(1)	(2)	(3)
Constant	0.482***	0.469***	0.463***
	(0.023)	(0.022)	(0.023)
David Hoffman	-0.089**		
	(0.031)		
Peter Li		0.003	
		(0.031)	
Jermaine Williams			0.024
			(0.040)
R^2	0.008	8.79×10^{-6}	0.0005
Observations	1,007	1,029	720

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

Note: Omitted categories are Jermaine Wood, Andy Wang, and William Snyder for the Black, Asian, and White treatment subsets, respectively.

Table B9: Study 2 response rate differences by sender email

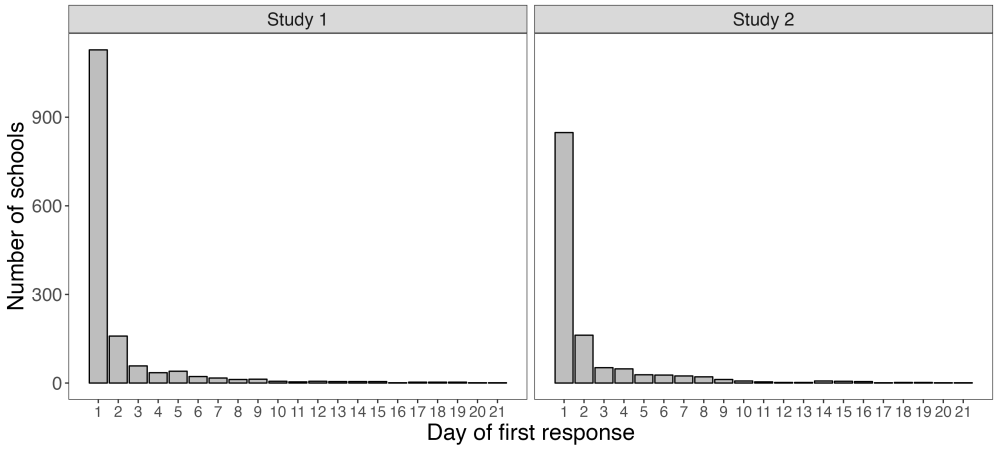
	DV: Response (0/1)				
Name	Andy Wang	David Hoffman	Jermaine Wood	Peter Li	William Snyder
	(1)	(2)	(3)	(4)	(5)
Constant	0.473***	0.404***	0.498***	0.454***	0.500***
	(0.031)	(0.030)	(0.031)	(0.031)	(0.033)
andy.wang7016	-0.008				
	(0.043)				
david.hoffman7016		-0.022			
		(0.043)			
jermaine.wood7016			-0.074·		
			(0.045)		
peter.li7016				0.038	
				(0.045)	
william.snyder7016					-0.035
					(0.045)
R^2	6.64×10^{-5}	0.0005	0.006	0.001	0.001
Observations	531	517	490	498	490

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

Note: Omitted category for each name subset is the 7106@gmail.com version of the email.

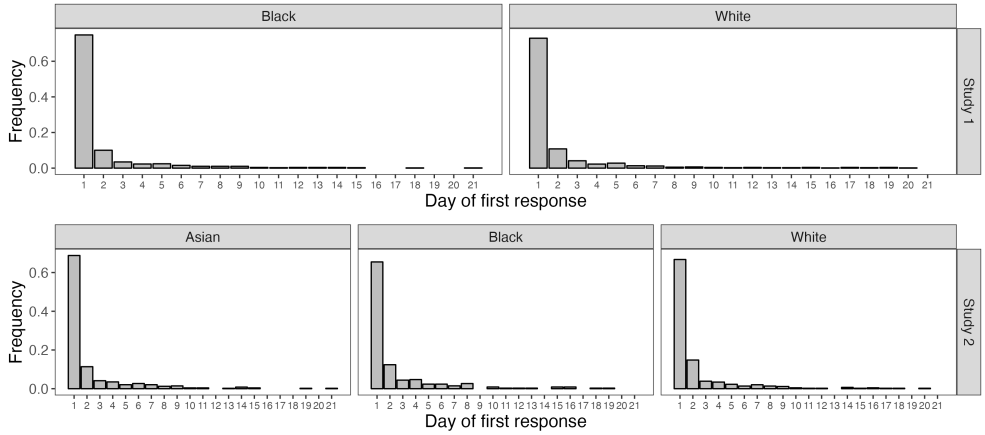
To test whether we might find statistically significant estimates if we had access to the lost data from the jermaine.williams7106@gmail.com account, we conduct simulation analysis where we impute response outcomes for the missing data. We conduct a simulation of 500 iterations, where for each iteration we impute the response variable outcome by randomly sampling (with replacement) from responded versus not with

Figure B2: Distribution of response time by study



Note: Figure shows distribution of first response received from each school that responded within 21 days. The distribution for study 1 is shown on the left (2024 contact) and the distribution for study 2 is shown on the right.

Figure B3: Distribution of response time by study by race treatment



Note: Figure shows distribution of first response received from each school that responded within 21 days by race treatment. The distribution for study 1 is shown on the top (2024 contact) and the distribution for study 2 is shown on the bottom.

probability of responding equal to the response rate for schools sent emails from the other Jermaine Williams (jermaine.williams7016@gmail.com) account with the same randomized treatment categories for Supreme Court and Class President. We then estimate the main effect and interaction specifications (equations 3, 4, and 4 in the manuscript) and store the results. We then take the average estimate for each models' coefficients across iterations as well as the average upper and lower bound of the 90% and 95% confidence intervals. We plot these simulated results alongside the actual results from the manuscript in Figures C1 and C2.

Figure C1: Comparison of main results from Study 2 and results with imputed response outcomes for missing data

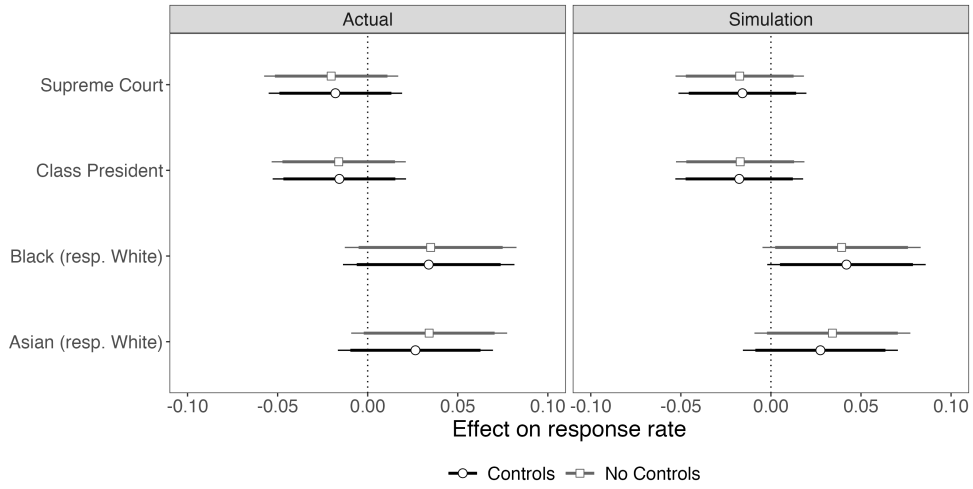
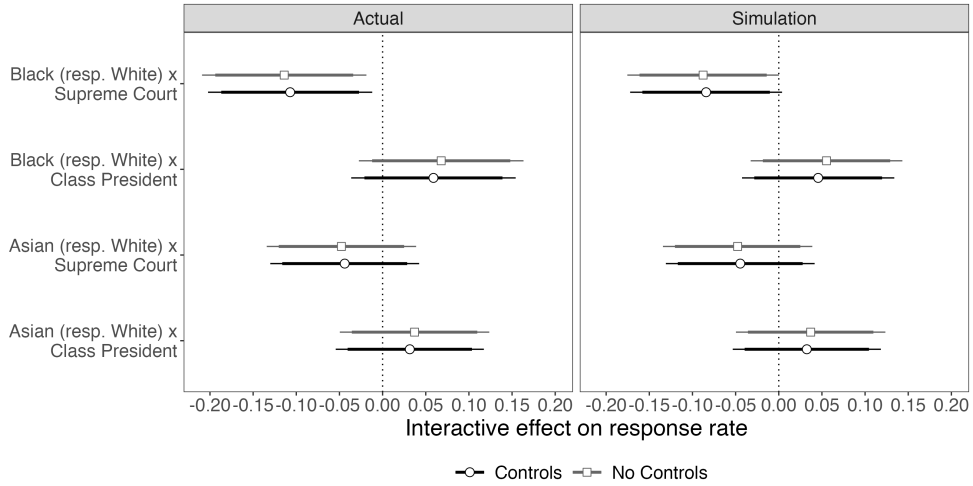


Figure C2: Comparison of treatment interactions from Study 2 and results with imputed response outcomes for missing data



D Treatment effect heterogeneity

D.1 By institutional characteristics

Table D1: Study 1: Heterogeneous treatment effects by pre-treatment covariates - 2018 response

	DV: 2018 response (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Black (resp. White)	0.017	0.023	-0.008	0.027	0.005	0.069**
	(0.020)	(0.025)	(0.026)	(0.023)	(0.022)	(0.024)
Black (resp. White) × Two-year institution	-0.004					
	(0.033)					
Black (resp. White) × Small institution		-0.011				
		(0.032)				
Black (resp. White) × Public institution			0.042			
			(0.033)			
Black (resp. White) × Below median HH index				-0.026		
				(0.033)		
Black (resp. White) × Rep. majority in county					0.013	
					(0.032)	
Black (resp. White) × Below 50% admission rate						-0.181**
						(0.061)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.035	0.035	0.036	0.039	0.037	0.066
Observations	2,764	2,764	2,764	2,547	2,642	1,441

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

Table D2: Study 1: Heterogeneous effects by pre-treatment covariates - 2024

	DV: 2024 response (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Black (resp. White)	-0.002	0.026	-0.011	0.0005	0.035	-0.026
	(0.023)	(0.030)	(0.029)	(0.028)	(0.026)	(0.029)
Black (resp. White) × Two-year institution	0.046					
	(0.039)					
Black (resp. White) × Small institution		-0.020				
		(0.039)				
Black (resp. White) × Public institution			0.043			
			(0.038)			
Black (resp. White) × Below median HH index				0.015		
				(0.039)		
Black (resp. White) × Rep. majority in county					-0.047	
					(0.038)	
Black (resp. White) × Below 50% admission rate						0.102
						(0.071)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.049	0.049	0.049	0.052	0.051	0.075
Observations	2,764	2,764	2,764	2,547	2,642	1,441

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

Table D3: Study 1: Heterogeneous effects by pre-treatment covariates - 2018-2024

	2024 - 2018 response					
	(1)	(2)	(3)	(4)	(5)	(6)
Black (resp. White)	-0.020	0.004	-0.003	-0.026	0.030	-0.095**
	(0.029)	(0.037)	(0.037)	(0.034)	(0.032)	(0.036)
Black (resp. White) × Two-year institution	0.050					
	(0.049)					
Black (resp. White) × Small institution		-0.010				
		(0.049)				
Black (resp. White) × Public institution			0.001			
			(0.048)			
Black (resp. White) × Below median HH index				0.042		
				(0.049)		
Black (resp. White) × Rep. majority in county					-0.060	
					(0.049)	
Black (resp. White) × Below 50% admission rate						0.284**
						(0.089)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.021	0.021	0.021	0.024	0.022	0.048
Observations	2,764	2,764	2,764	2,547	2,642	1,441

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

D.2 By BH22 criminal record and reference treatment

Table D4: Study 2: Heterogeneous effects by pre-treatment covariates - Race treatment

	DV: response (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Asian (resp. White)	0.025	0.032	0.042	0.055	-0.013	0.042
	(0.027)	(0.037)	(0.033)	(0.032)	(0.030)	(0.034)
Black (resp. White)	0.039	-0.008	0.051	0.088*	0.001	0.064
	(0.030)	(0.041)	(0.037)	(0.036)	(0.034)	(0.036)
Asian (resp. White) × Two-year institution	0.006					
	(0.046)					
Black (resp. White) × Two-year institution	-0.016					
	(0.051)					
Asian (resp. White) × Small institution		-0.008				
		(0.046)				
Black (resp. White) × Small institution		0.065				
		(0.051)				
Asian (resp. White) × Public institution			-0.027			
			(0.044)			
Black (resp. White) × Public institution			-0.030			
			(0.049)			
Asian (resp. White) × Below median HH index				-0.071		
				(0.046)		
Black (resp. White) × Below median HH index				-0.098		
				(0.051)		
Asian (resp. White) × Rep. majority in county					0.083	
					(0.045)	
Black (resp. White) × Rep. majority in county					0.088	
					(0.050)	
Asian (resp. White) × Below 50% admission rate						-0.072
						(0.084)
Black (resp. White) × Below 50% admission rate						-0.016
						(0.097)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Remaining treatments	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.049	0.050	0.049	0.054	0.053	0.078
Observations	2,756	2,756	2,756	2,547	2,634	1,419

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

D.3 Within-school across experiments differences in response rate

Table D5: Heterogeneous effects by pre-treatment covariates – Supreme Court treatment

	DV: response (0/1)					
	(1)	(2)	(3)	(4)	(5)	(6)
Supreme Court	-0.022	0.002	-0.034	-0.061*	0.005	-0.027
	(0.024)	(0.032)	(0.029)	(0.028)	(0.026)	(0.029)
Supreme Court × Two-year institution	0.012					
	(0.040)					
Supreme Court × Small institution		-0.032				
		(0.040)				
Supreme Court × Public institution			0.028			
			(0.038)			
Supreme Court × Below median HH index				0.106**		
				(0.039)		
Supreme Court × Rep. majority in county					-0.049	
					(0.039)	
Supreme Court × Below 50% admission rate						0.089
						(0.074)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Remaining treatments	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.049	0.049	0.049	0.055	0.043	0.079
Observations	2,756	2,756	2,756	2,547	2,634	1,419

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

D.4 Response rates by which study's email was sent first to schools in both studies

Table D6: Heterogeneous effects by consideration of race pre-SFFA - Study 2

	DV: response (0/1)		
	(1)	(2)	(3)
Asian (resp. White)	0.020	0.013	0.012
	(0.046)	(0.039)	(0.039)
Black (resp. White)	0.041	0.065	0.061
	(0.049)	(0.041)	(0.041)
Considered Race	0.093	0.223***	0.076
	(0.065)	(0.057)	(0.059)
Class President	-0.017	0.047	-0.017
	(0.033)	(0.039)	(0.033)
Supreme Court	0.042	0.043	0.028
	(0.033)	(0.033)	(0.039)
Asian (resp. White) × Considered Race	-0.023		
	(0.086)		
Black (resp. White) × Considered Race	0.081		
	(0.091)		
Class President × Considered Race		-0.226**	
		(0.072)	
Supreme Court × Considered Race			0.052
			(0.072)
Controls	Yes	Yes	Yes
R^2	0.105	0.114	0.105
Observations	936	936	936

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05*

Table D7: Pre-post response rate differences by race, and felon treatment in BH22

	2024 Response - 2018 Response	
	(1)	(2)
Black (resp. White)	-0.023	-0.025
	(0.033)	(0.033)
Felon (resp. Non-Felon)	0.018	0.016
	(0.034)	(0.034)
Black (resp. White) × Felon (resp. Non-Felon)	0.039	0.045
	(0.047)	(0.047)
Controls	No	Yes
State FEs	No	Yes
R^2	0.00120	0.02226
Observations	2,764	2,764

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

Table D8: Pre-post response rate differences by race, and reference treatment in BH22

	2024 Response - 2018 Response	
	(1)	(2)
Black (resp. White)	-0.016	-0.011
	(0.033)	(0.033)
Reference (resp. No Reference)	-0.046	-0.044
	(0.034)	(0.034)
Black (resp. White) × Reference (resp. No Reference)	0.023	0.016
	(0.047)	(0.047)
Controls	No	Yes
State FEs	No	Yes
R^2	0.00090	0.02185
Observations	2,764	2,764

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

Table D9: Within-school difference in response rate across experiments by treatment categories and school characteristics

	Study 2 Response - Study 1 Response			
	(1)	(2)	(3)	(4)
Study 1: Black (resp. White)	0.005	0.005	0.004	0.021
	(0.018)	(0.018)	(0.018)	(0.030)
Study 2: Asian (resp. White)	0.021	0.019	-0.004	0.0003
	(0.021)	(0.029)	(0.029)	(0.035)
Study 2: Black (resp. White)	0.041·	0.046	0.067*	0.002
	(0.023)	(0.031)	(0.031)	(0.038)
Study 2: Supreme Court	-0.026	-0.025	-0.025	-0.020
	(0.018)	(0.030)	(0.018)	(0.030)
Study 2: Class President	-0.005	-0.005	-0.012	-0.023
	(0.018)	(0.018)	(0.030)	(0.029)
Two-year institution	0.041·	0.041·	0.041·	-0.013
	(0.023)	(0.023)	(0.023)	(0.055)
Small institution	-0.050*	-0.050*	-0.050*	-0.088*
	(0.022)	(0.022)	(0.022)	(0.036)
Public institution	-0.058*	-0.058*	-0.058*	-0.034
	(0.025)	(0.025)	(0.025)	(0.039)
Study 2: Asian (resp. White) × Study 2: Supreme Court		0.002		
		(0.042)		
Study 2: Black (resp. White) × Study 2: Supreme Court		-0.010		
		(0.045)		
Study 2: Asian (resp. White) × Study 2: Class President			0.051	
			(0.042)	
Study 2: Black (resp. White) × Study 2: Class President			-0.048	
			(0.045)	
Considered race				0.021
				(0.039)
State FEs	Yes	Yes	Yes	Yes
R^2	0.026	0.026	0.028	0.062
Observations	2,535	2,535	2,535	936

Heteroskedasticity-robust standard-errors in parentheses
*Signif. Codes: ***: 0.001, **: 0.01, *: 0.05, .: 0.1*

Table D10: Response rates in Study 1 by whether schools received email first for Study 1 or Study 2

	Resp. Rate	SE	Race treatment
Study 1 email sent first	0.54	0.01	Full sample
Study 2 email sent first	0.56	0.01	Full sample
Study 1 email sent first	0.55	0.02	White
Study 1 email sent first	0.54	0.02	Black
Study 2 email sent first	0.55	0.02	White
Study 2 email sent first	0.58	0.02	Black

Table D11: Response rates in Study 2 by whether schools received email first for Study 1 or Study 2

	Resp. Rate	SE	Race treatment
Study 1 email sent first	0.45	0.01	Full sample
Study 2 email sent first	0.49	0.01	Full sample
Study 1 email sent first	0.41	0.02	White
Study 1 email sent first	0.47	0.02	Asian
Study 1 email sent first	0.48	0.03	Black
Study 2 email sent first	0.47	0.02	White
Study 2 email sent first	0.50	0.02	Asian
Study 2 email sent first	0.48	0.03	Black