

**The Intersection of Racial and Partisan Discrimination:
Evidence from a Correspondence Study of Four-Year Colleges**

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Short Title: The Intersection of Racial and Partisan Discrimination

Abstract: Social decisions are often imbued with biases that can lead to discrimination against certain groups of people. Racial minorities frequently find themselves on the receiving end of such discrimination. Recent work also reveals partisan bias such that members of one political party unfairly favor their co-partisans or discriminate against members of the other party. In this paper, we use an e-mail correspondence study to explore the impact of racial and partisan discrimination in higher education. We find no direct evidence of a racial or political bias; however, we do find that African Americans who reference politics in any way receive substantially fewer responses. This coheres with the theory of racial threat: members of a majority group are averse to minorities who might threaten their political, economic, or social status.

Keywords: partisan discrimination, racial discrimination, admissions decisions, correspondence study, higher education

Supplementary material for this article is available in the appendix in the online edition.

Replication files are available in the *JOP* Data Archive on Dataverse (<https://dataverse.harvard.edu/dataverse/jop>). For confidentiality reasons, some variables in the data are redacted but the full data are available from the authors upon request and promise of maintaining confidentiality.

The study was conducted in compliance with relevant laws and was approved by the Northwestern University Institutional Review Board.

When it comes to higher education, few issues generate as much controversy as diversity. Racial diversity in particular has long been a flashpoint for debate, including arguments about desegregation and the use of race as a factor in admissions decisions. While not garnering as much legal attention, the question of political diversity on college campuses has similarly led to heated discussion. Of particular note are concerns about liberal over-representation and discrimination against conservatives. Recent meta-analyses document the enduring impact of racial discrimination in American life (Quillian et al. 2017). At the same time, a growing body of work suggests that partisan political considerations may sometimes influence social and economic decisions (McConnell et al. 2018). We offer an empirical test of racial and partisan biases at one point in the higher education process: informational requests from prospective applicants to four-year colleges. Responses to such requests have the potential to impact applicants' decisions (e.g., do they end up applying?), and offer a testing ground for social decision-making more generally.

Racial and Political Discrimination in Decision-Making

Our interest is in the real-world prevalence of racial and partisan discrimination in college admissions. We thus focus on the behavior of individuals who work at colleges and receive requests for information from prospective students about undergraduate admissions. Our question is whether the race and/or partisanship of these prospective students affects their likelihood of receiving a response. We pre-registered three (all else constant) hypotheses (<https://aspredicted.org/7wx5x.pdf>) – each of which draws on extant literatures too vast to review here (see the online appendix for detailed theoretical discussion).

- The racial discrimination hypothesis builds on an enormous literature showing racial discrimination in many settings (Quillian et al. 2017): relative to whites, African Americans will be less likely to receive information upon request.

- The partisan discrimination hypothesis extends work showing partisans discriminate against their political opponents (McConnell et al. 2018): partisans will be less (more) likely to receive information upon request from members of the other (same) party.
- The political engagement discrimination hypothesis follows recent scholarship on how people prefer to avoid political discussion altogether (Klar and Krupnikov 2016): any mention of politics, regardless of the partisan connotation, will lower the likelihood of response.

Correspondence Study of College Admissions Information Requests

We test these hypotheses with a correspondence study. This approach involves sending fictitious requests that vary key attributes so as to document the presence of discrimination based on these attributes (Quillian et al. 2017). Our population includes all accredited degree-granting colleges and universities in the United States that offered at least one bachelor's degree program per the National Center for Education Statistics as of 2016. As explained in the online appendix, we arrived at a sample of 1,519 schools.

Our design involved sending an e-mail, purportedly from a prospective applicant, requesting more information about the school to each contact. The content of the e-mail varied two factors – race and political reference – but was otherwise constant. The e-mail address and signature included a name connoting either an African American male or a White male (respectively, Jabari Washington or Dalton Wood). The content of the e-mail provided some personal background information in the form of extracurricular involvement. It then varied political mentions in one of four ways: (1) *no politics*, where the individual states he has been active with a civics club, (2) *politically engaged*, where the individual states he has been active with a club that organizes political discussions, (3) *Democratic*, where the individual states he has been active with the Young Democrats, or (4) *Republican*, where the individuals states he

has been active with the Young Republicans. Thus, each school was randomly assigned to one of eight conditions, as shown in Table 1. This allows us to test the aforementioned hypotheses by examining whether the e-mail response rates differ based on race, partisan affiliation, or political engagement. Given the presumed rarity of explicit references to political affiliation in this context, the partisan message serves as a strong signal. As such – and as we discuss further in the online appendix – it may put an upper bound on any partisan effect. In the online appendix we also provide details about our design choices and implementation, as well as the precise e-mail texts.

[Insert Table 1 About Here]

Results

Our main analysis focuses on whether the school to which an e-mail was sent responded. Overall, an impressive 71.56% of schools replied to our inquiries (see the online appendix on how we coded responses). To test our predictions, we regress a binary variable indicating response on a set of variables identifying key experimental conditions for each hypothesis. For the racial discrimination hypothesis, we add a variable indicating whether the prospective student is African American (conditions 2, 4, 6, and 8). For the partisan discrimination hypothesis, we add variables indicating whether the prospective student is a Democrat (conditions 5 and 7) or a Republican (conditions 7 and 8). We initially assume the message recipients (admissions staff) are Democrats, reflecting the tendency of employees at colleges and universities to be more liberal than the general public (Honeycutt and Freberg 2017). We then relax this assumption by measuring the likely partisanship of respondents using presidential vote (i.e., Clinton vote share in 2016) in the county in which each college resides as a proxy (Gift and Gift 2015). The idea is that, all else constant, respondents are more likely to be Democrats (Republicans) in areas with

higher Democratic (Republican) vote share. Finally, for the political engagement discrimination hypothesis, we add a variable to reflect any mention of politics (conditions 3 through 8).

We present the results in Table 2. The first column operates from the assumption that respondents are Democrats. We see no evidence for the partisan discrimination hypothesis or the political engagement hypothesis – neither partisans (whether Democrat or Republican) nor politically engaged individuals are treated differently. We do see, however, that African American solicitations receive significantly fewer responses. The second column introduces interactions to proxy for admissions officials’ partisanship: in this case, we should see discrimination against (for) Republicans (Democrats) as Clinton vote share increases.¹ We do observe interactions that are correctly signed insofar as Republicans face decreasing response rates, and Democrats increasing response rates, as Clinton vote share increases. Both interactions, though, fall short of significance (the Democrat interaction is $p = .12$). Thus, we have no clear evidence of partisan discrimination despite the ostensible strength of the partisan signal; there appear to be limits to partisan discrimination, as other researchers have suggested (Lelkes and Westwood 2017). We also continue to see no political engagement effect, but observe a very strong racial discrimination effect.

[Insert Table 2 About Here]

Do these results suggest racial bias in the college admissions process? To probe further, we turn to the individual condition percentages in Figure 1. These reveal that there is no direct effect of racial bias. The white non-political condition (1) compared to the African American non-political condition (2) shows no significant difference and, if anything, the results move in

¹ In the online appendix, we provide a test with a proxy for the respondent’s race, finding no moderating impact on racial bias.

the direction of a preference *for* the African American student, perhaps reflecting diversity missions (72.78% to 77.54%) ($z = 1.07; p < .30$). The main drop-off – which drives the regression result – comes from the conditions featuring an African American student *who mentions politics in any way*. All three conditions that involve an African American mentioning anything political (conditions 4, 6, 8) have response rates of approximately 66%. This compares to a roughly 75% rate in other conditions (comparing the former three conditions to the latter five gives $t_{1517} = 3.76; p < .01$). The average difference between those conditions is about 9.0%, which is similar to the racial bias effect found in studies of legislative responsiveness (Costa 2017: 249). This is sensible since both situations involve a minority signaling a political interest.

[Insert Figure 1 About Here]

We did not anticipate this precise dynamic, but the result coheres with the theory of racial threat. This theory suggests that prejudice can occur due to a “perception by the dominant group that an outside group threatens their group’s prerogatives” (Quillian 1995: 586; also see Craig et al. 2018). These threats may be political, economic, or cultural (Blalock 1967). In our case, the idea of a politically engaged minority group member may have caused (possibly unconscious) discomfort since it portends to displace the dominant position of the majority group. This explains why the response rates drop for African Americans only when politics is mentioned (see Thornhill 2018 for a similar result). An alternative possibility is that an African American male prospective student primes negative stereotypes about future behavior (Harper 2015), and this becomes accentuated when the student suggests political engagement. The mechanism in play is a matter for future work, as is the issue of whether we would observe a similar pattern of results with female prospective students. These are questions that likely could best be tested by directly surveying admissions staff concerning their reactions to distinct student profiles. Our contribution to such an effort is identifying an understudied and often ignored type of

intersectional discrimination – while dimensions of race or politics on their own may not invite disparate treatment, particular mixtures of the two can be a deleterious source of bias.

These findings also offer some lessons for ongoing debates about pre-registration and replication. Our null results on straightforward racial and partisan discrimination highlight the importance of testing extant theories in new situations to probe their reach. Also, that our main result is not one that was pre-registered speaks to the importance of conducting exploratory analyses and potentially connecting new findings to existing theories when appropriate.

Conclusion

Whether schools' non-responses to informational requests affect admissions decisions is an open question (Thornhill 2018: 11). Even so, it is easy to imagine that a non-response could vitiate the interest of a potential applicant. The consequence would be a subgroup of potential students (politically engaged African Americans) screening themselves out of certain colleges' applicant pools. Regardless, the results speak to social decision-making processes in general. Along these lines, we want to accentuate three points. First, despite the initial impression from the regressions, our results provide no evidence of ordinary racial bias and, if anything, reflect the widespread call for diverse campuses. Second, we find no solid evidence of partisan or political bias in any direction – a finding that is somewhat surprising, given both concerns about the lack of political diversity in higher education and the growing literature documenting such effects. Third and most important is the demonstration of intersectional bias. Political scientists have focused attention on racial and partisan bias individually – sometimes even comparing them to one another (Iyengar and Westwood 2015). This is an important research agenda, but one that would benefit by incorporating the possibility of intersectional biases such as the race X political bias we find. As minorities continue to garner more political representation, it becomes

increasingly important to assess majority group reactions and consider ways to ensure equal access to social and political institutions.

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Table 1: Experimental Conditions

| Condition | Name | Treatment Text* |
|------------------------------------------------|-------------------|-----------------------------------------------------------------------|
| (1) White Non-Political (N = 191) | Dalton Wood | "...been active with a civics club..." |
| (2) African American Non-Political (187) | Jabari Washington | "...been active with a civics club..." |
| (3) White Politically Engaged (191) | Dalton Wood | "... been active with a club that organizes political discussions..." |
| (4) African American Politically Engaged (191) | Jabari Washington | "... been active with a club that organizes political discussions..." |
| (5) White Democrat (190) | Dalton Wood | "...been active with the Young Democrats..." |
| (6) African American Democrat (191) | Jabari Washington | "...been active with the Young Democrats..." |
| (7) White Republican (190) | Dalton Wood | "... been active with the Young Republicans..." |
| (8) African American Republican (190) | Jabari Washington | "... been active with the Young Republicans..." |

*See the online appendix for the full e-mail text.

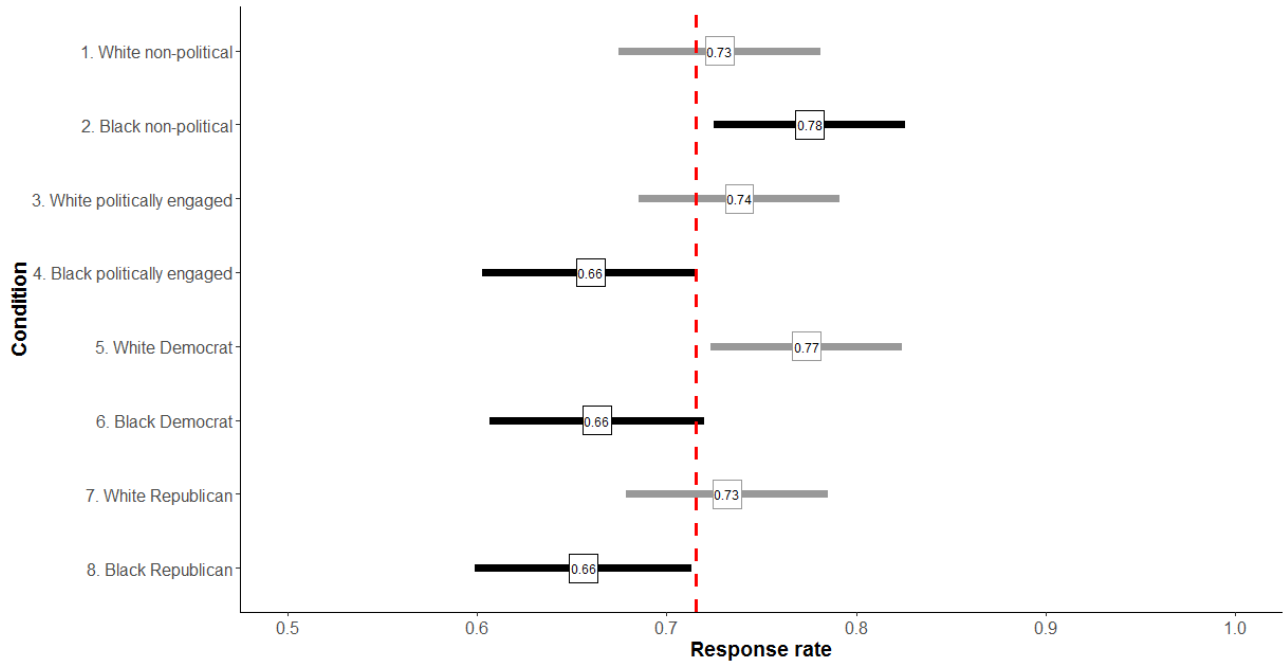
Table 2: Response Rate Regressions

| | (1) E-Mail Response | (2) E-Mail Response |
|---------------------------|---------------------|---------------------|
| African American | -0.268** (0.114) | -0.275** (0.115) |
| Democrat | 0.095 (0.160) | -0.532 (0.437) |
| Republican | -0.024 (0.158) | 0.096 (0.424) |
| Political Mention | -0.263 (0.163) | -0.265 (0.164) |
| Clinton Vote Share | | -0.899* (0.471) |
| Clinton Vote X Democrat | | -0.226 (0.786) |
| Clinton Vote X Republican | | 1.279 (0.832) |
| Constant | 1.243*** (0.134) | 1.686*** (0.272) |
| Log-Likelihood | -902.25 | -897.64 |
| Observations | 1,519 | 1,516 |

Entries are logit coefficient with standard errors in parenthesis.

*** p<0.01, ** p<0.05, * p<0.1 for two-tailed tests.

Figure 1: Experimental Condition Response Rates



Response rates for each condition are displayed within the corresponding boxes, with error bars representing 90 percent confidence intervals. The dotted red line represents the mean response rate across all conditions.

Appendix for “The Intersection of Racial and Partisan Discrimination: Evidence from a Correspondence Study of Four-Year Colleges”

1. Relevant Literature

We presented three pre-registered hypotheses in the text and mentioned that each derives from sizable literatures. Here we briefly touch on selections of that work.

Racial Discrimination Hypothesis

An enormous literature reveals racial bias in a host of decisions and across a wide array of settings: African Americans receive lower quality medical treatment (e.g., Burgess et al. 2010; Zestcott et al. 2016), fewer employment opportunities (e.g., Pager et al. 2009), fewer rental options in the shared economy (Edelman et al. 2017), less political response from elected officials (Butler and Broockman 2011; Costa 2017), harsher sentences in the criminal justice system (Mitchell 2005), disproportionately more suspensions and expulsions from schools (Fenning and Rose 2007), and fewer opportunities to take advanced high school courses (Loveless 2016), *inter alia*. Furthermore, while such disparities have narrowed, African American students continue to enroll in and graduate from college at lower rates than whites (e.g., Hudson et al. 2005; McDaniel et al. 2011) and when they do, they bear higher levels of student loan debt (Jackson and Reynolds 2013). Finally, despite changes to the legal landscape and widespread acceptance regarding norms of racial equality, recent meta-analyses (Quillian et al. 2017) suggest that the prevalence of racial discrimination in hiring has not decreased since 1989. All of this work leads straightforwardly to our prediction that, relative to whites, African

Americans will be less likely to receive information from colleges and universities upon request, all else constant.²

Partisan Discrimination Hypothesis

A growing literature reveals political bias in nonpolitical settings. Partisans discriminate against their political opponents – or report a willingness to do so – in situations ranging from job hiring (Gift and Gift 2015) to everyday economic transactions (McConnell et al. 2018), among others (e.g., Huber and Malhotra 2017; Iyengar and Westwood 2015). This literature also suggests that higher education may not be immune to such biases. Inbar and Lammers (2012) find that social and personality psychologists openly admit their willingness to discriminate against conservative colleagues; similarly, Munro et al. (2010) use a laboratory experiment to show that partisans will discriminate against members of the other party in a (hypothetical) college admissions decision. Accordingly, we expect that partisans will be less (more) likely to receive information upon request from members of the other (same) party.³

We lack direct political and demographic information about individual respondents. However, it is often asserted that academic settings lean to the left or toward the Democratic Party (e.g., Honeycutt and Freberg 2017; Inbar and Lammers 2012; Rothman and Lichter 2009;

² Brown and Hilbig (2018) conduct a study similar to ours and find no direct effect of race, however (also see Einstein and Glick 2017). (This, as it turns out, is consistent with what we find.)

³ One finding counter to this hypothesis is that of Fosse et al. (2014), who conducted a study similar to ours but focused on graduate education. They find “traces” but no significant evidence of political bias. (This, as it turns out, is also consistent with what we find.)

Yancey 2011, 2018). Given this, we expect to observe that Republicans (Democrats) will be less (more) likely to receive responses to informational requests than Democrats (Republicans), all else constant. As explained in the text, though, we relax this assumption with a proxy for the likely partisanship of the respondent using county-level presidential vote (see Gift and Gift 2015).

Political Engagement Discrimination Hypothesis

Recent work suggests that apparent aversion to one's political opponents in social settings actually reflects a general aversion to politics itself, rather than specifically discriminatory tendencies. Some people simply prefer to avoid politics altogether due to its uncomfortable conflictual nature (e.g., Chen and Rohla 2018; Klar, Krupnikov, and Ryan 2018) – and indeed, many Americans have such distaste for partisan conflict that they may even disguise their own partisan identities and pose instead as independents (Klar and Krupnikov 2016). For example, Klar and Krupnikov (2016: 63) report that forty percent of individuals express “discontent at the thought of working with [a] politically inclined colleague – even though the hypothetical colleague *agrees* with them!” (italics in original). This leads to our prediction that any mention of politics, regardless of the specific partisan connotation, will lower the likelihood of response, all else constant.

2. Sample

We obtained a list of undergraduate institutions from the National Center for Education Statistics (2016; <https://nces.ed.gov/ipeds/use-the-data>). The undergraduate students who assisted us with locating contact information for this project were instructed to access the undergraduate admissions section of each school's website. They were directed to first look for a general contact e-mail such as admissions@college.edu, rather than the e-mail address of any specific person. If a general contact was unavailable, they were asked to find an address for a specific

person in the admissions office, typically with a title such as “admissions counselor” (looking for the ostensibly highest “ranking” person). If the school had multiple distinct admissions offices for different undergraduate schools, then we focused on Liberal Arts and Sciences. If the school had no e-mail contact information and only a website solicitation form, it was excluded from our sample since we wanted to keep the mode of contact and information, including the subject line, constant (and logistically this would have created difficulty on the timing of sending inquiries since each form would have had to be manually completed). This was the main reason that schools were excluded from the sample, and the excluded set contained some well-known institutions. We also excluded women-only schools since our inquiries all came from male prospective student names. Finally, on the initial list obtained from the National Center for Education Statistics there were some apparent duplicates. Overall, of the 2,590 schools in our initial list, we successfully obtained 1,524 e-mail contacts (59%).⁴ However, of the 1,524 e-mails we sent, five bounced back due to incorrect e-mail addresses and thus our final sample is 1,519.

To summarize, a school did not make it onto this final list for one of the following reasons: it is a women’s only school (roughly 20 or so schools), it only has an online contact form, it has duplicate contact information with another school (e.g., some branch campuses have the same contact information as the main campus but are listed as distinct in our initial list), it no longer exists (i.e., the list had not updated a school closure), our team failed to find contact information, or the e-mail bounced back.

⁴ We actually located 1,525 contacts, but one school was inadvertently listed twice in our dataset and was sent two distinct e-mails from different conditions. Consequently, we excluded that school from our analyses (the school did not respond to either e-mail).

The schools that ended up in our sample represent all 50 states and Washington DC. In terms of regional breakdown, 25% are located in the Northeast, 30% in the Midwest, 31% in the South, and 14% in the West (using the Census Bureau's categorization). We obtained institutional data about the characteristics of a subset of our sample.⁵ For those schools, we find the average acceptance rate is 65.56% (Std. Dev. = 20.16%; N = 1,157), the average enrollment rate is 33.13% (17.71%; 1,156), the average graduation rate is 51.92% (19.95%; 1,360), the average student body growth from 2006 to 2016 is 14.35% (62.67; 1,362), the average total students for the cohort enrolled in 2016 is 857 (1,207.45; 1,407), and the average percentage of African Americans in the student body is 14.20 (19.10%; 1,363). Thirty-four percent are public

⁵ These institutional variables come from data collected as part of the Integrated Postsecondary Education Data System (IPEDS) and made available through the National Center for Education Statistics (<https://nces.ed.gov/ipeds/use-the-data>). These data are gathered through a series of interrelated institutional surveys that take place annually. Required by the Higher Education Act of 1965, these surveys collect information on general higher education topics from every college, university, and technical/vocational institution in the United States that participates in federal student financial aid programs (i.e., Title IV-eligible institutions) (see https://nces.ed.gov/training/datauser/IPEDS_01.html). The admissions data (e.g., percent admitted) are only collected from institutions that do not have open enrollment policies. Overall, there also was missing data for some of the schools in our sample and thus our summary of the data is partial. Finally, we report total enrollment for the class entering in 2016 – this correlates with the total number of students at the school (in 2016) at .92.

schools, 58% are private non-profits, and 8% are private for-profit schools (1,366).⁶ Finally, most of the sources of contact – 89% – are general school addresses such as admissions@college.edu. For the other 11% of cases, we failed to find a general address and thus used a specific individual’s address (as listed on the given school’s admissions office website). It also is worth noting that there are three schools for which we do not have presidential vote data – two Alaska schools where county-level data are not available and one online school whose main address is unclear.

We unfortunately have no way to know the demographic characteristics of each respondent (although see discussion in appendix 7) but we presume that random assignment means that the experimental groups are, on average, equivalent in terms of relevant demographics (e.g., race of the respondent). Similarly, we presume that all groups had a mix of respondents in terms of their jobs – ranging from high ranking admissions officers (particularly at smaller schools) to work-study students. An examination of some of the respondents who stated their job titles (in the responses) suggested such a mix. Even for cases where the respondent would not be involved in ultimate admissions decisions, responsiveness can affect prospective student interest and, more generally, we believe the study offers an important test of biases in social decision-making. Also, it is worth noting that whoever received the request was in a position where the job is to respond: this is clear since the e-mail addresses exist, in part, for prospective applicants’ inquires, is evident from the high overall response rate and, as noted below, is consistent with the extent to which the responses offered routes for follow-up information.

3. Sender Names

⁶ The experimental conditions are balanced based on these covariates.

We follow much prior work in using names to signal race (e.g., Bertrand and Mullainathan 2004; Butler and Broockman 2011; Butler and Homola 2017; Gaddis and Ghoshal 2015). As mentioned in the text, the name was made clear in the e-mail account from which the message was sent and in the signature of the message. We opted to use only male names to ensure enough statistical power (varying gender would have doubled our conditions); that related research has used only male names as well (e.g., Iyengar and Westwood 2015; Munro et al. 2010) makes our work comparable to that literature.

The precise names we used come from Druckman, Trawalter, and Montes (2018) and Druckman et al. (2018). In their appendices, they describe birth record evidence that the name Dalton Wood is a white person's name and Jabari Washington is an African American person's name roughly 90% of the time. Both also are typically middle-class individuals (measured in terms of parental education). The authors further offer survey evidence that more than 90% of respondents from Amazon's Mechanical Turk classified the race of the given names as we intended.

4. E-mail Text

Below are the texts of the e-mails. An obvious initial question concerns the mundane realism of these e-mails: just how often do prospective students mention political party affiliations? While we do not have a direct answer to this, we believe the partisan treatments are both justifiable and theoretically interesting. First, an initial concern would be that the partisan mentions are so irregular that respondents did not take them seriously (i.e., a lack of experimental realism). We have no evidence of this given: a) an overall response rate of close to 72% with no significant drop-off for white prospective students who mention a partisan affiliation (versus those who do not), and b) no difference between African Americans who mention a party versus African Americans who mention being involved in a group that organizes

political debates (political engagement). We imagine mention of a political interest of some sort is not atypical.⁷ Second, one could read our treatments as proxies for partisan information that could be obtained via other means such as social media searches. A 2018 survey of college admissions officers reports that 25 percent of them visit applicants' social media pages (down from 40 percent in 2015) (Kaplan Test Prep 2018). Third, given evidence of experimental realism, our partisan test offers an upper bound on any type of partisan discrimination, as there is little other individuating information offered and the partisan reference thus also signals engagement (e.g. Klar et al. 2018). That we end up finding no clear evidence for partisan discrimination, then, is suggestive that it does not exist in this context.⁸

Subject Line: Prospective Student Inquiry

Dear **[full name of school]** Admissions,

I'm writing to obtain more information about **[full name of school]**. I'm a high school junior and I think your school would be a great fit for me. I have done well academically and have been involved in many extracurricular activities.

I have been a member of an intramural sports club and a theater club. I also am passionate about **[the community and have been active with a civics club / politics and have been active with a club that organizes political discussions and debates / politics and have been active with the Young Democrats / politics and have been active with the Young Republicans]**. I hope to continue with these types of activities on your campus.

⁷ Our treatments also resemble, to some extent, those used in similar studies (e.g. Brown and Hilbig 2018; Fosse et al. 2014; Thornhill 2018). Moreover, there are organizations of Young Democrats (<https://www.yda.org/>), Young Republicans (<http://yrnf.gop/>), and even High School Democrats (<https://hsdems.org/>). Finally, unlike some other social referents, there may be no norms against mentioning partisanship per se (see Iyengar and Westwood 2015).

⁸ We thank the editor, Neil Malhotra, for guidance on this discussion.

I would like to speak with an admissions counselor or a currently enrolled student who can tell me more about [full name of school]. Can you please let me know who I should get in touch with?

Sincerely,
[Dalton Wood /Jabari Washington]

5. Sending Messages

We set up e-mail accounts with the relevant names so that each condition had a unique e-mail address. Specifically, our e-mail accounts were Gmail accounts consisting of the sender's name (i.e. "dalton.wood..." or jabari.washington...") followed by four numbers indicating a potential birth month and year (i.e., "0101," "0201," "0301," "0401"). Thus, for example, two of the eight e-mail addresses are dalton.wood0101@gmail.com and jabari.washington0101@gmail.com.

We used a mail merge procedure that allowed for personalization of the e-mails (see e-mail text). The e-mails were sent from Saturday, June 9, 2018, through Thursday, June 14, 2018. They were sent across more days than we had originally anticipated because we realized, after launching, that mailing quotas on Gmail constrained the number of e-mails that could be sent on a given day. This also meant that our e-mails across conditions were not completely randomized with regard to the day sent. Specifically, for conditions 1-6, half the e-mails were sent on Saturday with the others being sent on Wednesday and Thursday. Conditions 7 and 8 (Republican sender) were spread over more days (other than Friday). All of our results are robust to controlling for the day sent. Further, our central result regarding lower response rates to African American requestors who mention politics holds even if we exclude conditions 7 and 8 from our analyses.

6. Coding E-Mail Responses

We excluded auto-responses since our interest is in a human decision to respond to the e-mail. It was relatively straightforward to identify an auto-response since in most cases it said something along the lines of “auto-response” or “computer generated response.” For cases where there was any ambiguity, we used the time of response to judge (auto-responses are typically sent immediately upon receipt, and thus would follow very closely in time to our e-mail sending). We received auto-responses from 9.48% of the schools; of those, 81.25% followed up with a real response. Also, roughly 3% of schools that sent an initial response sent a second follow-up response, and a handful sent a third, fourth, or more. Additionally, 2% of schools also added our e-mail address to a general mailing list. We exclude follow-ups and mailing lists from our analyses. Finally, we stopped checking for responses after 52 days.

In terms of content, the typical response provided information about the given school and offered ways for the sender to obtain additional information (e.g., offering to set up a phone call, providing contact information for another person, inviting the sender to visit the campus). We coded the content of nearly all of the e-mails and found that 94% included such follow-up opportunities. We also coded for whether the e-mail engaged the sender by discussing the particular extracurricular information provided in the e-mail. We find that about 20% engaged in this way.⁹ (We checked the reliability of the coding with a subsample of 272 e-mails and find a kappa score of .79.) We do not find robust substantive effects on engagement from the experimental conditions, other than a marginal effect of increased engagement with any mention

⁹ These percentages are based only on cases where there is a response. If we assume non-responders did not include follow-up information or engagement (i.e., code those variables to 0 for non-responders), the relevant percentages are 68% and 14%.

of politics. (In these analyses, available from the authors, we assume a non-response is akin to no engagement.)

In terms of time to respond, the average is 2.90 days (4.75; 1087). We cannot draw further inferences about time to respond, however, since response is contingent on experimental condition assignment and thus analyzing response times no longer allows for clear causal inference (Coppock 2019). (The possible solutions for this – such as assuming “as if” a non-responder would have responded in a certain amount of time had he/she been in a distinct condition – seem fairly untenable in this case.)

7. Supplemental Regressions

In this section, we provide a variety of supplemental analyses. The bottom line is that the racial threat dynamic is robust and we find no evidence that it differs by the individual political conditions, or is conditioned by the percentage of African American students at the school, the private-public status of the institution, or elite status as captured by the admissions rate.

Table A-1 presents the same regression as model 2 of Table 1 but adds an interaction term for racial threat (i.e., African American X Political). This captures each experimental condition with an African American who mentions politics in any way (conditions 4, 6, 8). The results show a clear significant effect for racial threat, as is also made clear by Figure 1 in the paper. The second model in Table A-2 adds individual interaction terms for the African American Democrat and African American Republican conditions. That these are not significant reveals that it is all three of the African American political conditions that matter and in the same ways.

Table A-2 adds a variable for the percentage of the student body that is African American (for schools for which we could obtain the data). One possible logic here is that increased racial contact at these schools (due to the relatively larger percentage of African Americans) conditions

responsiveness to an African American requestor (in general or when it comes to racial threat). This variable also could correlate with the likelihood of a respondent (i.e., admissions staff) being African American. In either case, we expect that a greater percentage of African American students would, if anything, reduce discrimination. The first model in the table adds an interaction between that percentage and our African American dummy variable; the second model adds an interaction between that percentage and the racial threat dummy variable; and the third model adds both interactions. In all models, the percentage of African Americans is highly significant in a negative direction (and Clinton vote share becomes insignificant). We are not sure of the dynamic here. (There is a substantially higher percentage of African American at southern schools – an average of 25% as opposed to 9% in other regions – but controlling for region does not eliminate the significant negative effect.) Otherwise, none of the interactions are near significant. This may be surprising but we suspect that it reflects the complexity of racial contact effects (e.g., Paluck et al. 2018) and the contingency of such effects on general context/norms (Christ et al. 2019).

Table A-3 presents regressions in the same sequential order of interactions in Table A-2, but this time the focus is on whether the institutions are public (the excluded category), private non-profit, or private for-profit (for schools for which we could obtain the data). Brown and Hilbig (2018) find a striking effect of institution type when it comes to responses to applicants who mention being incarcerated – private schools are about 10% less likely to respond to formerly incarcerated applicants as compared to public schools. While this is certainly not the same as our focus (public schools are part of a state infrastructure as are prisons), it does suggest the possibility of differential response. They also find that public institutions are marginally more likely to respond to African American prospective applicants. Our results suggest that public schools are overall more responsive than either type of private school (as indicated by the

negative and significant coefficients on both types of private schools). While the findings point in the direction of public schools being more responsive to African American requestors, those interactions (like Brown and Hilbig 2018) are not significant. The racial threat interactions are also not significant (and Clinton vote share becomes insignificant).

Table A-4 presents regressions looking at “elite schools” which we defined as those with admissions rates in roughly the top decile of our sample (127 schools; the results are robust to using other cut-offs) for schools for which we could obtain the data. The idea here is that such schools may be under national pressure to diversify and thus will be more responsive to African American prospective applicants. We find no significant effects or interactions (and Clinton vote share becomes insignificant).

Table A-5 presents regressions that add the percentage of African Americans in the county in which the school sits interacted with the African American condition dummy and the racial threat (i.e., African American X Political) dummy.¹⁰ We do this to proxy for the likely race of the respondent (i.e., the admissions staff; see discussion in Appendix section 2). The idea is that any evidence of racial discrimination will be smaller in counties with larger African American populations since those counties are less likely to have a white respondent. We find

¹⁰ We use data from the 2010 Census; the average county in our sample is 13% African American (standard deviation of 14%; there were a few counties for which we were unable to obtain data). We also collected data from the 2017 American Community Survey, although that data only covers counties with populations above 65,000. It is thus incomplete. The two data sources correlate at .997 and thus it seems reasonable to use the 2010 data to ensure full coverage. The results are the same if we instead use the 2017 data on the subset of the sample for which they are available.

that schools in counties with larger African American populations are less likely to respond at all (and the main effect from the Clinton vote share becomes insignificant). However, even though the interactions move in the expected positive directions, they are nowhere near significance. This suggests that either this is an insufficient proxy for the race of the respondent or that race does not condition the results.

Table A-1: Response Rate Regressions with Racial Threat Interaction

| | (1) E-Mail Response | (2) E-Mail Response |
|-------------------------------------|---------------------|---------------------|
| African American | 0.245 (0.240) | 0.245 (0.240) |
| Democrat | -0.538 (0.438) | -0.452 (0.471) |
| Republican | 0.099 (0.426) | 0.090 (0.461) |
| Political Mention | 0.076 (0.210) | 0.050 (0.232) |
| Af. Am. X Political | -0.676** (0.273) | -0.628* (0.329) |
| Democrat X Af. Am. X Political | | -0.167 (0.323) |
| Republican X Af. Am. X Political | | 0.017 (0.319) |
| Clinton Vote Share | -0.897* (0.472) | -0.895* (0.472) |
| Clinton Vote X Democrat | 1.294 (0.835) | 1.304 (0.836) |
| Clinton Vote X Republican | -0.234 (0.789) | -0.234 (0.788) |
| Constant | 1.428*** (0.288) | 1.427*** (0.287) |
| Log-Likelihood | -894.56 | -894.36 |
| Observations | 1,516 | 1,516 |

Entries are logit coefficients with standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1 for two-tailed tests.

Table A-2: Response Rate Regressions with Percentage African Americans at School

| | (1) E-Mail Response | (2) E-Mail Response | (3) E-Mail Response |
|------------------------------------|----------------------|----------------------|----------------------|
| African American | 0.158 (0.282) | 0.199 (0.263) | 0.227 (0.310) |
| Democrat | -0.308 (0.474) | -0.298 (0.474) | -0.296 (0.475) |
| Republican | 0.184 (0.460) | 0.195 (0.461) | 0.198 (0.461) |
| Political Mention | 0.100 (0.233) | 0.100 (0.233) | 0.101 (0.233) |
| Af. Am. X Political | -0.779*** (0.298) | -0.848*** (0.314) | -0.871** (0.342) |
| Clinton Vote Share | -0.312 (0.517) | -0.307 (0.518) | -0.305 (0.518) |
| Clinton Vote X Democrat | 0.929 (0.899) | 0.907 (0.899) | 0.900 (0.900) |
| Clinton Vote X Republican | -0.395 (0.856) | -0.415 (0.856) | -0.421 (0.857) |
| % Af. Am. | -2.400*** (0.417) | -2.429*** (0.378) | -2.399*** (0.417) |
| % Af. Am. X Af. Am. | 0.252 (0.620) | | -0.167 (0.984) |
| % Af. Am. X Af. Am. X Political | | 0.443 (0.658) | 0.581 (1.046) |
| Constant | 1.568*** (0.319) | 1.570*** (0.318) | 1.564*** (0.320) |
| Log-Likelihood | -765.40 | -765.26 | -765.25 |
| Observations | 1,360 | 1,360 | 1,360 |

Entries are logit coefficients with standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1 for two-tailed tests.

Table A-3: Response Rate Regressions with Private/Public Schools

| | (1) E-Mail Response | (2) E-Mail Response | (3) E-Mail Response |
|-------------------------------------|----------------------|----------------------|----------------------|
| African American | 0.296 (0.329) | 0.255 (0.262) | 0.489 (0.466) |
| Democrat | -0.153 (0.478) | -0.164 (0.477) | -0.145 (0.477) |
| Republican | 0.371 (0.460) | 0.371 (0.461) | 0.382 (0.461) |
| Political Mention | 0.118 (0.232) | 0.116 (0.232) | 0.118 (0.232) |
| Af. Am. X Political | -0.845*** (0.299) | -0.878** (0.358) | -1.076** (0.488) |
| Clinton Vote Share | -0.249 (0.516) | -0.255 (0.516) | -0.234 (0.518) |
| Clinton Vote X Democrat | 0.597 (0.902) | 0.622 (0.900) | 0.580 (0.902) |
| Clinton Vote X Republican | -0.820 (0.852) | -0.816 (0.853) | -0.842 (0.854) |
| Private Non-Profit | -0.352* (0.206) | -0.393** (0.188) | -0.353* (0.206) |
| Private For-Profit | -1.552*** (0.311) | -1.655*** (0.281) | -1.553*** (0.311) |
| Priv. N.P. X Af. Am. | -0.016 (0.281) | | -0.256 (0.515) |
| Priv. F.P. X Af. Am. | -0.236 (0.460) | | -0.549 (0.718) |
| Priv. N.P. X Af. Am. X Political | | 0.074 (0.283) | 0.289 (0.517) |
| Priv. F.P. X Af. Am. X Political | | -0.032 (0.493) | 0.415 (0.766) |
| Constant | 1.507*** (0.336) | 1.546*** (0.331) | 1.499*** (0.336) |
| Log-Likelihood | -768.40 | -768.49 | -768.19 |
| Observations | 1,363 | 1,363 | 1,363 |

Entries are logit coefficients with standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1 for two-tailed tests.

Table A-4: Response Rate Regressions with Elite Schools (Based on Admit Rates)

| | (1) E-Mail Response | (2) E-Mail Response | (3) E-Mail Response |
|---------------------------------------|----------------------|----------------------|----------------------|
| African American | 0.373 (0.318) | 0.462 (0.314) | 0.371 (0.326) |
| Democrat | 0.158 (0.559) | 0.156 (0.559) | 0.158 (0.559) |
| Republican | -0.036 (0.502) | -0.038 (0.501) | -0.036 (0.502) |
| Political Mention | 0.124 (0.261) | 0.125 (0.261) | 0.124 (0.261) |
| Af. Am. X Political | -0.941*** (0.352) | -1.016*** (0.355) | -0.939*** (0.362) |
| Clinton Vote Share | -0.426 (0.608) | -0.446 (0.606) | -0.426 (0.609) |
| Clinton Vote X Democrat | -0.035 (1.057) | -0.029 (1.057) | -0.035 (1.057) |
| Clinton Vote X | -0.089 (0.956) | -0.089 (0.955) | -0.089 (0.956) |
| Elite School | 0.034 (0.336) | 0.169 (0.307) | 0.034 (0.336) |
| Elite School X Af. Am. | 0.711 (0.491) | | 0.730 (0.850) |
| Elite School X Af. Am. X Political | | 0.574 (0.509) | -0.023 (0.883) |
| Constant | 1.431*** (0.357) | 1.426*** (0.356) | 1.431*** (0.357) |
| Log-Likelihood | -620.00 | -620.40 | -620.00 |
| Observations | 1,156 | 1,156 | 1,156 |

Entries are logit coefficients with standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1 for two-tailed tests.

Table A-5: Response Rate Regressions with Percentage African Americans in County

| | (1) E-Mail Response | (2) E-Mail Response | (3) E-Mail Response |
|---------------------------------------|----------------------|----------------------|---------------------|
| African American | 0.166 (0.264) | 0.271 (0.241) | 0.174 (0.309) |
| Democrat | -0.517 (0.438) | -0.486 (0.440) | -0.513 (0.444) |
| Republican | 0.100 (0.426) | 0.132 (0.427) | 0.103 (0.431) |
| Political Mention | 0.085 (0.211) | 0.085 (0.211) | 0.085 (0.211) |
| Af. Am. X Political | -0.711*** (0.275) | -0.802*** (0.296) | -0.721** (0.336) |
| Clinton Vote Share | -0.420 (0.511) | -0.385 (0.513) | -0.416 (0.517) |
| Clinton Vote X Democrat | 1.245 (0.835) | 1.183 (0.840) | 1.238 (0.847) |
| Clinton Vote X | -0.231 (0.789) | -0.297 (0.792) | -0.238 (0.801) |
| Af. Am. Population | -1.378** (0.603) | -1.266** (0.560) | -1.378** (0.603) |
| Af. Am. Pop. X Af. Am. | 0.740 (0.770) | | 0.680 (1.377) |
| Af. Am. Pop. X Af. Am. X Political | | 0.646 (0.785) | 0.074 (1.403) |
| Constant | 1.381*** (0.295) | 1.347*** (0.291) | 1.379*** (0.298) |
| Log-Likelihood | -890.46 | -890.58 | -890.45 |
| Observations | 1,515 | 1,515 | 1,515 |

Entries are logit coefficients with standard errors in parenthesis. *** p<0.01, ** p<0.05, * p<0.1 for two-tailed tests.

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