

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

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Abstract: Social decisions are often imbued with various types of biases. The consequence can be discrimination against certain groups of people. One of the more widely documented types of discrimination is race-based – racial minorities frequently find themselves at a disadvantage. 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 social and economic decisions. In this paper, we use a correspondence study to explore the independent and intersectional impact of racial and partisan discrimination in higher education. Specifically, we investigate responsiveness to e-mail requests for information sent to admissions departments at four-year colleges in the United States. While we find some evidence for partisan discrimination, our central finding is that African-Americans who reference politics of any sort 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.

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When it comes to higher education, few issues generate as much controversy as diversity. Racial diversity has long been a flashpoint for legal and social debate, including arguments about desegregation and the use of race as a factor in admissions decisions (e.g., Bowen and Bok 1998). While not garnering as much legal attention, the question of political diversity on college campuses has analogously led to heated discussion. Of particular note are concerns about liberal over-representation and discrimination against conservatives (e.g., Yancey 2018).

We examine the independent and intersectional impact of race and partisanship on decision-making at one point in the higher education process: informational requests from prospective applicants to four-year colleges. While it is unclear how responses to informational requests might affect subsequent enrollment, it is certainly plausible that a non-response could vitiate a potential applicant's interest. Regardless, the study provides a test of the effect of race and partisanship on social decision-making as a general matter. Are African-American prospective students who request information about a school less likely to receive a response than white students? Are prospective students who signal a Republican Party affiliation less likely to receive a response than those who do not?

Racial and Political Discrimination in Decision Making

We focus on individuals who work at colleges and receive requests for information about undergraduate admissions. Our interest is how race and/or political affiliation might affect the decision to respond to the request. There are three relevant literatures that lead to four hypotheses.¹

¹ Our descriptions of these literatures are admittedly simplified. Our goal is not to study mechanisms or conditions that moderate distinct predictions; rather, we explore possible types of bias in a novel setting. As far as we know, we are the first to explicitly do this.

First, an enormous literature reveals racial bias in a host of decisions: 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, Quillian et al. 2017), 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), *inter alia*. The racial discrimination hypothesis is that, relative to whites, African-Americans will be less likely to receive information upon request, due to their race, all else constant.

Second, a growing literature reveals political bias in nonpolitical settings. Partisans may discriminate against their political opponents – or report a willingness to do so – in situations ranging from job hiring (Inbar and Lammers 2012, Gift and Gift 2015) to everyday economic transactions (McConnell et al. 2018) to college admissions (Munro et al. 2010), among others (e.g., Huber and Malhotra 2013, Iyengar and Westwood 2015). It is often asserted that academic settings lean to the left or the Democratic Party (e.g., Rothman and Lichter 2009, Yancey 2011, 2018, Inbar and Lammers 2012, Honeycutt and Freberg 2017). The partisan discrimination hypothesis then is that, relative to non-partisans, Democrats will be more likely and Republicans will be less likely to receive information upon request, due to their partisan affiliations, all else constant. An assumption of this hypothesis is that an average liberal/Democratic bias among academics spills over into administrative hiring decisions – in many cases it is administrators who receive information requests (particularly at the undergraduate level). We will later address this assumption by measuring the likely political leaning of the respondents.

Recent work on partisan discrimination suggests that bias may instead reflect a general aversion to politics – some people prefer to avoid politics altogether due to its uncomfortable

conflictual nature (e.g., Klar, Krupnikov, and Ryan 2018, Chen and Rohla 2018). 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). The political engagement discrimination hypothesis, then, is that any mention of politics, regardless of the partisan connotation, will lower the likelihood of response since people tend to be averse to political discussion, all else constant.

Finally, the literature on minority group threat or racial threat theory suggests that prejudice occurs due to a “perception by the dominant group that an outside group threatens their group's prerogatives” (Quillian 1995: 586). These threats can come in various guises; they can be economic or political, and may be symbolic or value-based (Blalock 1967). For us, one possibility is that an African-American prospective applicant who mentions politics in any way may generate a feeling of threat; the idea of a politically engaged minority group member can cause (often unconscious) discomfort since it portends to displace the dominant position of the majority group.² The racial threat hypothesis is that, relative to an analogous white prospective applicant or an African-American applicant who does not mention politics, an African-American who mentions politics of any sort will be less likely to receive a response, all else constant.

Correspondence Study of College Admissions Information Requests

To test the aforementioned hypotheses, we conducted a correspondence or audit study. A sizable literature in economics and sociology uses correspondence/audit studies to document

² Much of the racial threat literature focuses on how perceptions of the size of the minority population affects reactions among the dominant group (e.g., Craig et al. 2018). We differ from this insofar as our hypothesis suggests that an individual minority group member who mentions politics might trigger political threat and thus lead to avoidance (i.e., non-response). In our study pre-registration (at Aspredicted.org, study #11424), we neglected the racial threat hypothesis; we did not consider possible interactive race X politics effects. The relevance of this theory and the possibility of an interaction became clear to us only after seeing the preliminary results. Nonetheless, the theory is well developed by others, and it strikes us as reasonable to state its prediction as a hypothesis even though we only recognized its application in an exploratory fashion.

discrimination in settings such as housing or the labor market (e.g., Bertrand and Mullainathan 2004; Gaddis and Ghoshal 2015; Hanson and Hawley 2011; Zschirnt and Ruedin 2016; Quillian et al. 2017). Correspondence studies rely on written or electronic submission of fictitious application materials to assess the prevalence of discrimination in a given context. The idea is that the researcher sends out (e.g., job) applications that are identical except in that they randomly vary along a key dimension of interest, such as the applicant’s race, religion, age, gender, etc. The employers who receive the applications are unaware they are part of a study; due to random assignment, the researcher can conclude that if certain types of applicants (e.g., minorities) receive fewer interviews or job offers than other types (e.g., non-minority) then it is that applicant characteristic that causes the difference in offers. This is the case since the applicants are otherwise identical. One study in this vein, for example, shows that attaching “white-sounding” names to fictitious resumes yields 50 percent more callbacks than comparable resumes which are labeled with “African-American-sounding names” (Bertrand and Mullainathan 2004). The approach allows for clear causal inference of discrimination, at the aggregate (but not individual) level, in real world economic and social settings (for some critique, see Heckman 1998).

Like others – such as Butler and Broockman (2011) who study legislator responsiveness to information requests – we adapt this approach to a non-labor market setting. As explained, our interest lies in information requests from prospective college applicants. This setting is intriguing given the aforementioned debates about racial and political biases in academia. Moreover, studying responses to simple requests for “more information” allows us to extend prior work to a situation where responding has little cost: there is no job at stake and this is far removed from actual college admission decisions. Beyond the relatively minimal amount of time that it takes to

respond to an email, this situation is not zero-sum in the way that most job hiring decisions are (i.e., giving one person a job or even an interview necessarily means some others must be excluded, but sending an informational email does not come with the same constraints). Any biases we uncover likely would thus reflect (unconscious) processes that occur in analogous social situations (e.g., social interactions).³

Design and Procedure

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 (NCES) as of 2016. The NCES list offered a sampling frame of 2,590 schools. We then had a team of undergraduate students find each school's website and locate a contact e-mail for the undergraduate admissions department (we excluded schools that did not have a clear e-mail address and instead offered an on-line form). This led to a sample of 1,526 schools (see the appendix for discussion of how we arrived at this number). 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). Details on how we obtained the sample are in the appendix.

Our design involved sending an e-mail 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 (e.g.,

³ There are inherent ethical issues with audit studies since respondent consent is waived. For a general discussion, see Pager (2007: 126-128).

Pager 2007, Butler and Homola 2017). 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 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 described in Table 1. This allows us to test the aforementioned hypotheses by examining whether the e-mail response rates differ based on race, political affiliation, political engagement, and/or an interaction of race and political engagement. In the appendix, we provide further details about our design choices and implementation, as well as the precise e-mail texts.

[Insert Table 1 About Here]

Results

Of the 1,526 e-mails we sent, five bounced back due to incorrect e-mail addresses and thus our final sample is 1,521. The schools 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; for those schools, we find the average acceptance rate to be 65.58% (N = 1,158), the average enrollment rate to be 33.12% (1,157), the average graduation rate to be 51.92% (1,361), the average student body growth from 2006 to

2016 to be 14.17% (1,365), and the average total students for the cohort enrolled in 2016 to be 857 (1,408).⁴

Our main analysis focuses on whether the school to which an e-mail was sent responded. Overall, an impressive 71.47% of schools replied to our inquiries.⁵ 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 sender is African-American (i.e., conditions 2, 4, 6, and 8). The partisan discrimination hypothesis entails variables indicating whether the sender is a Democrat (conditions 5 and 7) or a Republican (conditions 7 and 8), while the political engagement discrimination hypothesis has a variable for any mention of politics (conditions 3 through 8). Finally, we capture racial threat with a variable for the conditions where the sender is African-American and mentions politics in any way (conditions 4, 6, and 8).⁶

⁴ 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, etc.) are only collected from institutions that do not have open enrollment policies. Thus, the schools for which we do not have data presumably either do not participate in federal aid programs, have open enrollment policies, and/or for some other reason did not report. This amounts to 400 schools in our sample (about 25%). Thus, our summary of the data is only partial. (We do not have sufficient information on all the schools to otherwise impute for missing data.) Also, 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. Finally, the experimental conditions are balanced based on these school characteristics.

⁵ 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 9.47% of the schools; of those, 81.25% followed-up with a real response. Also, roughly 5% 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.

⁶ 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 and found that 94% included such follow-up opportunities. We also coded for whether the e-mail engaged the sender by discussing the particular

We present the results in Table 2 – model 1 offers a straightforward test of our hypotheses. (We use one-tailed tests given the directional nature of our predictions.) The results are stark. There is no evidence for either direct racial discrimination, partisan discrimination, or political engagement discrimination. None of the variables for those hypotheses near statistical significance. However, there clearly exists a racial threat dynamic such that when the e-mail sender is African-American *and* makes a political reference of any sort (interest in politics or any partisan affiliation), the response rate significantly drops. Ostensibly, the sender’s race and political content do not on their own affect decision-making, but a communication from a politically engaged African-American (consciously or unconsciously) reduces responsiveness. To get a sense of the substantive change, consider that the average response rate for the conditions that do not involve an African-American sending an e-mail referring to politics is 74.92% (949). This contrasts with an average of 65.73% (572) for e-mails that come from an African-American student who references politics ($t_{1519} = 3.86$; $p \leq .01$, for a one-tailed test).⁷

This 8.56% difference is similar in size to that found in the literature on political responsiveness (from elected officials): in a meta-analysis of that work, Costa (2017: 249) reports that African-Americans solicitations, relative to those from non-minorities, lead to 7.3% less responsiveness. It is somewhat smaller than that found in labor market studies: Quillian et al. (2017: 10871) state, based on their meta-analysis of 24 job market discrimination studies, that

extracurricular information provided in the e-mail. We find that about 20% engaged in this way. (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 of politics. (In these analyses, available from the authors, we assume a non-response is akin to no engagement.)

⁷ In the appendix, we present versions of model 1 that add control variables for: whether the contact e-mail address was a personal account or a general account, the region of the school, and the day of the week that our inquiry was sent. We also present a model with the aforementioned institutional characteristic variables. As mentioned, adding these institutionally variables trims our sample by about 25%. In all cases, our result regarding racial threat is robust. The appendix provides details on the impact of these control variables.

“on average, white applicants receive 36% more callbacks than equally qualified African Americans.” It seems sensible that our effect size echoes that found in legislative responsiveness studies (rather than the labor market literature) since those studies look, by definition, at political requests. As far as we know, though, we are the first to demonstrate this type of political racial threat bias – i.e., a request from a minority that references politics – in a setting akin to everyday social decision-making (e.g., Craig et al. 2018).

Interestingly, the racial threat result is not driven by one particular condition. In Table 3, we present the mean response rates by condition so as to break down the regression results. What is clear is that there is a notable drop-off in all three conditions where an African-American references politics, regardless of whether that involves general political engagement or affiliation with either of the parties. All three of those conditions have response rates of roughly 65% (and notably conditions 4 and 6 have identical response rates). As mentioned, this compares to a roughly 75% rate in other conditions.⁸

Let us now turn to a deeper exploration of the partisan discrimination hypothesis. We mentioned that an assumption underlying this prediction is that the respondents lean in a Democratic direction. While this seems plausible when it comes to certain faculty at universities, it may be a problematic assumption when it comes to those in the admissions offices who are receiving the e-mails. The individuals who staff administrative positions at a given school likely resemble the general community around it. To capture this, we acquired data on the partisan

⁸ 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 has the luxury of allowing for clear causal inference (Coppock n.d.). (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.) Further, such analysis is complicated since we sent the e-mails on different days of the week that may have affected response time (although adding day sent controls do not affect our main results). See the appendix.

leaning of the county where each school is located (see Gift and Gift 2015: 671 for discussion); we follow common practice by using presidential vote in the county as a proxy for such leaning (e.g., Druckman et al. 2009: 354). Thus, the higher percentage of the county that voted for Clinton in 2016, the more Democratic the county, and the more likely the respondent would be a Democrat.⁹ We add the Clinton vote share measure to the model along with interactions between vote share and each of the partisan dummy variables. The interactions provide a stronger test of the partisan discrimination hypothesis since we would predict bias for (against) Democrats (Republicans) as the area becomes more Democratic.¹⁰

We report the results in the second column (model 2) of Table 3. It shows that controlling for county partisanship matters. Specifically, in Republican counties (e.g., at the extreme where the interaction term = 0), there is a significant bias *against* Democratic inquiries ($p = .10$). Yet, this flips as a county becomes more Democratic, as indicated by the significant interaction term ($p = .059$, one-tailed test). That these terms are significant at all is telling insofar as these are fairly rough proxies for the likely partisanship of the respondent. That said, the effect is not large. For example, consider a Democratic white male who solicits information from a school in an extremely Democratic county (among the top 10% of Clinton support in the sample – 73%

⁹ We thank Ryne Rohla for generating the presidential vote data. We could instead use Trump vote share and focus on Republicans (the correlation between Trump county vote share and Clinton county vote share, in our sample, is -.99). The results are virtually the same; we present a model using Republican vote share in the appendix. We also obtained data for presidential vote within one mile and one-half mile of each school. We opt for the county data as those seem more reflective of the general leanings of the area and the pool of potential employees (areas very close the school are likely skewed by the students themselves and some faculty) (see Gift and Gift 2015). The correlations between the county level data and the half-mile and mile Clinton vote share data, respectively, are .77 and .81. The results we report with the county level data are not robust to using these closer proximity data. Finally, 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.

¹⁰ We do not add additional interactions as we have no theoretical rationale for expecting area political leanings to condition responsiveness based on other features of the experimental treatments. It also would lead to extreme collinearity. When we do run models with individual interaction terms between Clinton vote share and the other condition variables, none are significant. These results are in the appendix.

Clinton vote). In this case, the probability of response is 78.16% (standard error: 3.86%).¹¹ In contrast, if he wrote to a school in a very conservative county (bottom 10% – 24% Clinton vote), the probability of response is 73.38% (standard error: 4.17%). Put another way, moving from a very Democratic to a very Republican county leads to partisan discrimination against Democrats of about 4.78% (i.e., 78.16 minus 73.38).¹² Interestingly, this is quite similar in size to the partisan discrimination reported by Gift and Gift (2015) in their study of job market bias in conservative and liberal counties. Notably, there is *no* apparent discrimination against Republicans; any bias seems to be pointed at Democrats, perhaps because so much public debate concerning political diversity in academia leads those working in such contexts to consciously avoid bias against conservatives (see, e.g., Burgess et al. 2007 on such conscious avoidance).¹³

There are two final items of note in model 2 of Table 2. First, schools in more Democratic counties are less likely to respond in general, as is made clear by the significant and negative coefficient on the Clinton vote share variable. For instance, for a non-political white male (i.e., condition 1), soliciting information in the aforementioned extreme Democratic county yields a predicted response rate of 69.03% (4.30%) whereas this jumps to 76.10% (3.62%) in the extreme Republican county.¹⁴ Second, putting aside any partisan dynamics, the racial threat result remains strong and unchanged (and it is not contingent on county partisanship; see the

¹¹ We compute these probabilities using *Clarify* (Tomz et al. 2003). If this sender wrote a county that has the average Democratic leaning in the sample (48.59% Clinton vote), the chance of receiving a response is 76.00% (standard error: 2.46%).

¹² The 95% confidence intervals on these predictions overlap, but note that can occur in cases with statistically significant differences as suggested by the regression in Table 2 (Gelman and Stern 2006).

¹³ In the appendix, we run versions of model 2, Table 2, with the aforementioned control variables. The interaction between Clinton vote share and Democratic sender remains significant when we add type of e-mail, region, and day the e-mail was sent. However, when we run the model on the subsample for which we have the institutional control variables, it is no longer significant. Interestingly, it is significant for a regression on the subset of data for which the institutional variables are not available. The result thus seems driven by a subset of schools that either do not participate in federal aid programs and/or have open enrollment practices (see prior note).

¹⁴ The underlying dynamic is not entirely clear to us. It is not due to region as the result remains when we control for region. See the appendix.

appendix). Overall, the bottom line is clear evidence of a racial threat bias – there is no discrimination against a non-political African-American prospective student, but as soon as he makes any reference to politics, discrimination becomes apparent.

Conclusion

Few settings, if any, generate as much debate about racial and political diversity as higher education. We have sought to document sources of bias / discrimination at one gatekeeping point in the college admissions process – basic requests for information. As mentioned, we do not claim that these responses ultimately influence the nature of the student body. But they do constitute one point of contact, and likely reflect social decision-making more broadly.

That we find some evidence of partisan discrimination – using the rough proxy of county vote – is telling and makes clear that more work is needed in distinct decision-making domains. Perhaps more importantly, our central finding concerning racial threat adds to a large literature on the topic (e.g., Craig et al. 2018) but with a new twist. In our case, the discrimination does not occur as a result of aggregate demographic changes, as are often the focus in the racial threat literature. Instead, it results from a minority making an explicit mention of the political space – and it is exactly that kind of intrusion that can generate concern about a majority group’s status. Clearly more work is necessary to explore the robustness of this type of racial threat.

At a more general level, political science scholarship has paid much attention to racial prejudice and, more recently, partisan discrimination – in some cases even comparing the two against one another (Iyengar et al. 2012; Iyengar and Westwood 2015). Our results suggest that, in some cases, it may be the *interaction* of race and politics that generates discriminatory behavior. As minorities continue to garner more political representation, it becomes increasingly

vital 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	Dalton Wood	"...been active with a civics club..."
(2) African-American Non-Political	Jabari Washington	"...been active with a civics club..."
(3) White Politically Engaged	Dalton Wood	"... been active with a club that organizes political discussions..."
(4) African-American Politically Engaged	Jabari Washington	"... been active with a club that organizes political discussions..."
(5) White Democrat	Dalton Wood	"...been active with the Young Democrats..."
(6) African-American Democrat	Jabari Washington	"...been active with the Young Democrats..."
(7) White Republican	Dalton Wood	"... been active with the Young Republicans..."
(8) African-American Republican	Jabari Washington	"... been active with the Young Republicans..."

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

Table 2: Response Rate Regressions

	(1)	(2)
	E-Mail Response	E-Mail Response
African-American	0.256 (0.239)	0.245 (0.240)
Democrat	0.086 (0.160)	-0.545* (0.432)
Republican	-0.033 (0.158)	0.070 (0.420)
Political Mention	0.086 (0.210)	0.083 (0.210)
Af. Am. X Political	-0.692*** (0.272)	-0.684*** (0.273)
Clinton Vote Share		-0.746* (0.470)
Clinton Vote X Republican		-0.199 (0.782)
Clinton Vote X Democrat		1.296* (0.828)
Constant	0.983*** (0.163)	1.351*** (0.285)
Log-Likelihood	-901.27	-897.57
Observations	1,521	1,518

Entries are logit coefficient with standard errors in parenthesis.

*** \leq .01; ** \leq .05; * \leq .10 for one-tailed tests.

Table 3: Experimental Condition Response Rates

Experimental Condition	Percentage (N)
(1) White Non-Political	72.78% (191)
(2) African-American Non-Political	77.54% (187)
(3) White Politically Engaged	73.82% (191)
(4) African-American Politically Engaged	65.97% (191)
(5) White Democrat	77.37% (190)
(6) African-American Democrat	65.97% (191)
(7) White Republican	73.16% (190)
(8) African-American Republican	65.26% (190)

Appendix

1. Sample

As explained in the text, 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 an "online" 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 National Center for Education Statistics there were some apparent duplicates. Overall, of the 2,590 schools in our initial list, we successfully obtained 1,526 e-mail contacts (59%). 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), or our team failed to find contact information.

2. Sender Names

We follow much prior work in using names to signal race (e.g., Bertrand and Mullainathan 2004, Butler and Broockman 2011, Gaddis and Ghoshal 2015, Butler and Homola 2017). 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 in the e-mail. We opted to use only male names to ensure enough statistical power (i.e., varying gender would have doubled our conditions); that related work has used only male names as well (e.g., Munro et al. 2010, Iyengar and Westwood 2015) makes our work comparable to that literature.

The precise names we used come from Druckman et al. (n.d.a., n.d.b.). In their appendix, 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 with 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.

3. E-mail Text

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.

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]

4. Sending Messages

We set up e-mail accounts with the relevant names so that each condition had a unique e-mail. Specifically, our e-mail accounts were g-mail accounts that stated 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-mails 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). As shown in our appendix supplemental regressions, we find some effects of day sent on response, but all of our results are robust to controlling for the day sent. (Further, our central results regarding racial threat and Democratic bias by county hold even if we exclude conditions 7 and 8 from our analyses.)

5. Supplemental Regressions

Table A-1 includes various models that add control variables to our model 1 in Table 2. The most important point is that, in all these models, our main result regarding racial threat remains robust. Model 1 in Table A-1 adds regions, with the South being the excluded category. We find that schools in the Northeast and Midwest are more responsive, all else constant, than schools in the South and the West (which does not significantly differ from the South).¹⁵ Model 2 adds e-mail type to our main model where 0 = an institutional e-mail address (e.g., admissions@college.edu) and 1 = a personal e-mail address. It also adds the day of the week in which our inquiry was sent, with the excluded day being Monday (no e-mails were sent on Friday). The results show a significant positive effect from personal addresses which is not surprising since any personalization tends to generate responsiveness (e.g., Druckman and Green 2013). We also find that sending an e-mail on a Sunday increases responsiveness which may reflect that e-mail being near the top of the respondent's in-box on Monday morning. (That said, only condition 8 e-mails were sent on Sunday – see section 4 of the appendix – and thus generalization should be made with great caution.) Sending an e-mail on a Wednesday also increases responsiveness for unclear reasons. Model 3 adds all the controls that are present in Models 1 and 2. All the results are the same with the exception that there is evidence that e-mails sent on Saturday increases the response rate. Finally, in Model 4, we add the institutional level controls. This causes the Northeast and the e-mail type variables to become insignificant; however, it is not clear whether this is due to the added control variables or, as noted in the text, the substantial loss in cases since we lacked institutional data for nearly 25% of our schools. This last model also shows increased responsiveness as the number of students in a school increases and as the graduation rate of the school increases. The former may reflect increased institutionalization (and staff) at the admissions offices of larger schools while the latter may also reflect increases in services (that facilitate college completion).

¹⁵ If we break the regions down to narrower areas – by Census Divisions – we find greater responsiveness in New England, Middle Atlantic, East North Central, West North Central, and the Pacific – compared to South Atlantic, West South Central, East South Central, and Mountain. The main insight here is that the Pacific Division of the West is more responsive than the Mountain Division.

Table A-1: Response Rate Regressions With Control Variables

	(1) E-mail Response	(2) E-mail Response	(3) E-mail Response	(4) E-mail Response
African-American	0.238 (0.240)	0.267 (0.240)	0.250 (0.240)	0.353 (0.322)
Democrat	0.098 (0.161)	0.092 (0.161)	0.103 (0.161)	0.072 (0.205)
Republican	-0.019 (0.159)	0.037 (0.178)	0.051 (0.179)	-0.216 (0.220)
Political Mention	0.070 (0.211)	0.165 (0.213)	0.148 (0.213)	0.229 (0.276)
Af. Am. X Political	-0.696*** (0.273)	-0.837*** (0.277)	-0.838*** (0.278)	-0.995*** (0.366)
Northeast	0.385*** (0.155)		0.362*** (0.157)	0.219 (0.200)
Midwest	0.366*** (0.146)		0.299** (0.149)	0.405** (0.196)
West	0.168 (0.180)		0.130 (0.182)	0.277 (0.272)
Personal E-mail		0.321** (0.178)	0.295** (0.179)	0.117 (0.238)
Tuesday		-0.306 (0.516)	-0.188 (0.519)	0.090 (0.640)
Wednesday		0.625* (0.447)	0.692* (0.449)	0.435 (0.518)
Thursday		0.312 (0.444)	0.407 (0.447)	0.531 (0.519)
Saturday		0.510 (0.450)	0.577* (0.452)	0.387 (0.523)
Sunday		1.391** (0.656)	1.425** (0.658)	1.447** (0.747)
Percent Admitted				0.303 (0.399)
Percent Enrolled				0.268 (0.443)
Percent Size Change (2006 to 2016)				0.009 (0.143)
Total Students (Cohort Enroll 2016)				0.0002*** (0.00008)
Graduation Rate				0.018*** (0.005)
Constant	0.776*** (0.180)	0.208 (0.494)	-0.020 (0.504)	-0.912* (0.708)
Log-Likelihood	-896.88	-892.71	-889.35	-577.91
Observations	1,521	1,521	1,521	1,121

Entries are logit coefficient with standard errors in parenthesis. *** \leq .01; ** \leq .05; * \leq .10 for one-tailed tests.

Table A-2 provides models with distinct interactions with county vote. Model 1 simply uses Trump (Republican) county vote share instead of Clinton (Democratic) county vote share. Not surprisingly, the model closely resembles model 2 in Table 2. The next five models use Clinton (Democratic) county vote share and include each possible treatment interaction, one at a time. We present these models for exploratory reasons – indeed, we have no theoretical prediction that county partisanship would condition treatments other than the partisan treatments. And indeed, other than the Democratic treatment interaction presented in the text, none of the interactions are near significant. In all cases, the main racial threat result holds. We do not include, in the table, a model with all possible interactions given the lack of theoretical predictions and because adding a full set of interaction leads to extreme collinearity. That said, our main results remain close to significant with one-tailed tests, with the racial threat having $p = .09$ and the Democratic interaction having $p = .106$.

Table A-2: Response Rate Regressions With Interactions

	(1)	(2)	(3)	(4)	(5)	(6)
	E-mail Response	E-mail Response	E-mail Response	E-mail Response	E-mail Response	E-mail Response
African-American	0.245 (0.239)	0.057 (0.402)	0.245 (0.240)	0.244 (0.240)	0.244 (0.240)	0.247 (0.239)
Democrat	0.697** (0.410)	0.086 (0.160)	0.084 (0.160)	0.087 (0.160)	-0.581* (0.410)	0.086 (0.160)
Republican	-0.232 (0.383)	-0.033 (0.158)	-0.035 (0.158)	-0.033 (0.158)	-0.029 (0.159)	0.273 (0.398)
Political Mention	0.086 (0.210)	0.083 (0.210)	0.084 (0.210)	-0.145 (0.439)	0.082 (0.210)	0.084 (0.210)
Af. Am. X Political	-0.685*** (0.273)	-0.682*** (0.273)	-0.992*** (0.427)	-0.680*** (0.273)	-0.684*** (0.273)	-0.683*** (0.273)
Trump Vote Share	0.621* (0.475)					
Trump Vote X Republican	0.459 (0.793)					
Trump Vote X Democrat	-1.359** (0.835)					
Clinton Vote Share		-0.699* (0.475)	-0.756** (0.427)	-0.852 (0.681)	-0.818** (0.376)	-0.327 (0.387)
Clinton Vote X Af. Am.		0.385 (0.658)				
Clinton Vote X Af. Am. X Political			0.634 (0.669)			
Clinton Vote X Political				0.461 (0.778)		
Clinton Vote X Democrat					1.367** (0.778)	
Clinton Vote X Republican						-0.618 (0.735)
Constant	0.705*** (0.266)	1.327*** (0.287)	1.356*** (0.268)	1.404*** (0.377)	1.387*** (0.248)	1.143*** (0.250)
Log-Likelihood	-897.41	-898.98	-898.71	-898.98	-897.61	-898.80
Observations	1,518	1,518	1,518	1,518	1,518	1,518

Entries are logit coefficient with standard errors in parenthesis. *** $\leq .01$; ** $\leq .05$; * $\leq .10$ for one-tailed tests.

Table A-3 provides models with our main interactions and control variables. In models 1-3, we add the aforementioned controls (see Table A-1). In all cases, our racial threat (i.e. the interaction) result remains robust as do our results regarding Democratic responsiveness. We also see that the controls echo what we present in Table A-1 with Northeast and Midwest schools exhibiting consistent increased responsiveness, more response from personal e-mail addresses, and a Sunday increased responsiveness rate. (We also again find some evidence for a Wednesday impact and, in model 3, a Saturday impact.) In model 4, we add the institutional controls. Here the racial threat result remains but the Democratic responsiveness finding is no longer significant. Interestingly, in model 5, we run the model for the subset of schools for which the institutional data are not available. Here we see the Democratic responsiveness finding appears. Thus, it seems that the subset of schools that did not report institutional data drive the result. As mentioned, these schools presumably either do not participate in federal aid programs and/or have open enrollment practices (or for some other reason did not report the data). The Democratic bias then is not evident for colleges writ large but rather institutions that operate more like private entities. (There are some state funded schools in this list which are typically branch campuses that may not have reported distinctive institutional data, relying instead on the main campuses' reporting.) We also see some alterations in the significance of some of the controls in the smaller samples presented in models 4 and 5.

Table A-3: Response Rate Regressions With Interactions and Controls

	(1) E-Mail Response	(2) E-Mail Response	(3) E-Mail Response	(4) E-Mail Response	(5) E-Mail Response
African-American	0.226 (0.240)	0.256 (0.240)	0.238 (0.241)	0.335 (0.323)	0.125 (0.401)
Democrat	-0.562* (0.433)	-0.535 (0.434)	-0.557* (0.435)	-0.095 (0.560)	-1.221** (0.758)
Republican	0.007 (0.421)	0.132 (0.432)	0.069 (0.433)	-0.210 (0.521)	0.687 (0.880)
Political Mention	0.066 (0.211)	0.161 (0.213)	0.143 (0.214)	0.224 (0.276)	-0.008 (0.373)
Af. Am. X Political	-0.689*** (0.274)	-0.831*** (0.278)	-0.833*** (0.279)	-0.979*** (0.367)	-0.632* (0.479)
Clinton Vote Share	-0.958** (0.483)	-0.842** (0.474)	-1.058** (0.488)	-0.970* (0.649)	-1.259* (0.802)
Clinton Vote X Republican	-0.036 (0.785)	-0.178 (0.789)	-0.023 (0.791)	-0.021 (0.988)	-0.516 (1.494)
Clinton Vote X Democrat	1.361* (0.830)	1.287* (0.832)	1.361** (0.834)	0.388 (1.060)	3.041** (1.459)
Northeast	0.457*** (0.159)		0.442*** (0.161)	0.313* (0.207)	-0.129 (0.346)
Midwest	0.362*** (0.147)		0.293** (0.149)	0.410** (0.197)	-0.084 (0.272)
West	0.227 (0.183)		0.200 (0.186)	0.371* (0.279)	0.238 (0.288)
Personal E-mail		0.333** (0.179)	0.304** (0.180)	0.146 (0.239)	0.122 (0.303)
Tuesday		-0.325 (0.518)	-0.193 (0.522)	0.073 (0.645)	-0.806 (1.070)
Wednesday		0.624* (0.449)	0.704* (0.451)	0.424 (0.524)	0.773 (0.993)
Thursday		0.305 (0.446)	0.400 (0.449)	0.477 (0.525)	-0.051 (0.990)
Saturday		0.510 (0.451)	0.575* (0.454)	0.344 (0.528)	0.599 (1.004)
Sunday		1.411** (0.658)	1.476*** (0.662)	1.482** (0.754)	1.013 (1.573)
Percent Admitted				0.182 (0.402)	
Percent Enrolled				0.196 (0.442)	
Percent Size Change (2006 to 2016)				-0.011 (0.143)	
Total Students (Cohort Enroll 2016)				0.0002*** (0.00008)	
Graduation Rate				0.018*** (0.005)	
Constant	1.222*** (0.291)	0.613 (0.546)	0.463 (0.554)	-0.393 (0.777)	0.438 (1.165)
Log-Likelihood	-892.50	-888.70	-884.53	-575.68	-259.26
Observations	1,518	1,518	1,518	1,120	398

Entries are logit coefficient with standard errors in parenthesis. *** \leq .01; ** \leq .05; * \leq .10 for one-tailed tests.

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