

# UC Berkeley

## UC Berkeley Previously Published Works

### Title

Do Politicians Racially Discriminate Against Constituents? A Field Experiment on State Legislators

### Permalink

<https://escholarship.org/uc/item/457336kw>

### Journal

American Journal of Political Science, 55(3)

### ISSN

0092-5853

### Authors

Butler, Daniel M  
Broockman, David E

### Publication Date

2011-07-01

### DOI

10.1111/j.1540-5907.2011.00515.x

Peer reviewed

# Do Politicians Racially Discriminate against Constituents? A Field Experiment on State Legislators\*

Short Title: Do politicians racially discriminate?

**Daniel M. Butler**

(Corresponding Author)  
Assistant Professor  
Department of Political Science  
Yale University  
P.O. Box 208209  
New Haven, CT 06520-8209  
daniel.butler@yale.edu

**David E. Broockman**

Student  
Department of Political Science  
Yale University  
P.O. Box 203435  
New Haven, CT 06520-3435  
david.broockman@gmail.com

**FINAL REVIEW PENDING**

*American Journal of Political Science*

---

\* The authors names appear in reverse-alphabetical order and both contributed equally to this article. A previous version of this article was circulated under the title “Who Helps DeShawn Register to Vote? A Field Experiment on State Legislators”. Previous versions of this paper were presented at the Third Annual Conference on Experimental Political Science at NYU, as well as at seminars at Columbia University, Cornell University, Duke University, Harvard University, the University of Chicago, and Yale University. We thank the editor, the anonymous reviewers, seminar participants, Don Green, Greg Huber, Jim Vreeland, and Zoltan Hajnal for their comments. Replication materials for this study can be found online at Yale’s Institution for Social and Policy Studies data archive (<http://isps.research.yale.edu>). Finally, funding and institutional support for this study were provided by Yale’s Institution for Social and Policy Studies.

**Abstract**

We use a field experiment to investigate whether race affects how responsive state legislators are to requests for help with registering to vote. In an email sent to each legislator, we randomized whether a putatively black or white alias was used and whether the email signaled the sender's partisan preference. Overall, we find that putatively black requests receive fewer replies. We explore two potential explanations for this discrimination: strategic partisan behavior and the legislators' own race. We find that the putatively black alias continues to be differentially treated even when the emails signal partisanship, indicating that strategic considerations cannot completely explain the observed differential treatment. Further analysis reveals that white legislators of both parties exhibit similar levels of discrimination against the black alias. Minority legislators do the opposite, responding more frequently to the black alias. Implications for the study of race and politics in the United States are discussed.

Keywords: discrimination, descriptive representation, race and politics, responsiveness

## Introduction

Political equality is considered to be one of the defining characteristics of a democracy (Dahl 1956; Verba 2003). In the past, American democracy has consistently failed to live up to the standard of political equality, especially with regards to its treatment of racial minorities. Despite progress made in the latter half of the 20<sup>th</sup> century, many researchers argue that racial minorities continue to be politically disadvantaged and underrepresented relative to their white counterparts (e.g. Fraga 1992; Hajnal 2009). In contrast, other researchers have suggested that racial discrimination against blacks in the political sphere may no longer be a concern in the United States (for review see Hajnal 2009: 39), with some going as far as to argue that blacks and other minorities are in fact overprivileged in the political sphere (Thernstrom 1987; Chavez 1992). More broadly, especially in the wake of Barack Obama's election, many Americans have come to share the view that full equality for blacks has arrived or is due to arrive soon.

In recent years, political and judicial decision-makers have also sought to appraise America's progress towards racial equality. Although the United States Supreme Court upheld core provisions of the Voting Rights Act of 1965 in the case *Northwest Austin Municipal Utility District Number One v. Holder* in 2009, Chief Justice Roberts wrote in the 8-1 decision that "We are now a very different nation," going on to characterize whether "conditions" today have sufficiently improved to warrant striking down the Voting Rights Act as "a difficult constitutional question." As the *New York Times* reported, such language "suggest[s] that the court [is] steeling itself to make a major pronouncement about the role of race in American democracy."

Because there remains significant uncertainty about whether the political system remains biased against minorities, we conducted a field experiment in October 2008 involving 4,859 U.S.

state legislators to test whether race affected how responsive legislators were. Each legislator in the experiment received an email asking for help with registering to vote. In the experiment, we randomized whether the email was sent from a putatively black alias or a putatively white alias. One reason for comparing whites and blacks, in addition to the important normative reasons raised above, is that blacks have a well-known history of disproportionately voting for Democrats, allowing us to test the possibility that discrimination may persist in the political system merely as an epiphenomenon of legislators' strategic partisan considerations. We attempted to estimate the importance of these considerations by also randomizing whether the email signaled the sender's partisan preference.

The results of our experiment show that the black alias receives significantly fewer responses than the white alias. Further analyses of the heterogeneous treatment effects by the legislator's party and the experimental groups that signal partisan affiliation show that legislators' strategic partisan considerations can at best explain only a portion of the observed differential treatment in favor of the white alias. We then explore an alternative explanation for the discrimination that we observed – the race of the legislators themselves. White legislators of both parties discriminate against the black alias at nearly identical, statistically significant rates, while minority legislators do the opposite, responding more frequently to the black alias. This suggests, as many have argued, that the race of elected officials significantly affects how well minorities are represented. Our results also suggest that race remains a significant barrier to equality in the American political system.

### **Should Race Affect Legislators' Responsiveness?**

Given that legislators are often assumed to be empty vessels that adapt to their constituency in order to maximize their vote share and that constituency service does not force

legislators to take unpopular positions that would alienate voters (Cain, Ferejohn, and Fiorina 1987), we would not expect legislators to discriminate against individual requests for constituency service on the basis of race. Indeed, such service helps legislators develop a reputation for getting things done for their constituents (Fenno 1978: 108). Similarly, there are reasons to expect that legislators' personal characteristics do not impact whether they exhibit discrimination. With regard to race in particular, prominent works have similarly argued that legislators of any race can be expected to serve minorities with similar effectiveness (e.g. Swain 1993).

However, other research suggests that racial discrimination remains present throughout American politics and society. Significant racial biases exist in the job market (e.g. Bertrand and Mullainathan 2004; Pager and Quillian 2005). Furthermore, evidence suggests that racial stereotypes are still widespread (Bobo 2001) and constitute a potent force in American politics (e.g., Kinder and Kam 2009). Combined with biases from existing institutions (Frymer 1999; Hajnal 2009), these factors may all lead to a situation where blacks are underrepresented. Furthermore, there are at least two additional reasons why we might expect legislators to engage in discrimination based on race.

#### *Descriptive Representation*

First, we might expect rates of reply to differ across the putatively racial aliases based on the race of the legislators themselves. Much previous research has suggested that legislators who share descriptive characteristics with their constituents may better represent and advocate for their interests and policy preferences (e.g., Whitby 1997; Canon 1999; Chattopadhyay and Duflo 2004; Griffin and Newman 2007; Grose, Magnum and Martin 2007). Indeed, one of the arguments for increasing the number of minorities and women who serve as elected officials is

based on the expectation that elected officials better represent those who share their personal characteristics (Canon 1999). Then again, as already noted, other prominent scholars downplay the relationship between race and representation entirely, arguing in line with more traditional assumptions about politicians that legislators of all races can adequately represent their constituents (e.g. Swain 1993; Thernstrom and Thernstrom 1997). Thus the question of whether descriptive representation affects responsiveness remains up for debate.

*Statistical versus Taste-based Discrimination.*

Second, we might observe legislators engaging in discrimination because of strategic partisan considerations. Fenno noted, “Every member has some idea of the people most likely to join his reelection constituency... During a campaign these people will often be ‘targeted’ and subjected to special recruiting or activating efforts” (1978: 9). Similarly, Bartels writes, “Rational candidates are impelled by the goal of vote maximization to discriminate among prospective voters, appealing primarily to those who either are likely to vote and susceptible to partisan conversion or reliable supporters susceptible to mobilization (or likely opponents susceptible to demobilization)” (1998: 68).

Because blacks in recent decades have consistently voted for Democratic candidates about 90 percent of the time, while whites have typically split their votes more evenly (ANES 2005), Republican legislators receiving an email from someone with a putatively black name would probably infer that he or she is more likely to vote for the Democratic candidate. Republicans therefore might be less responsive to a request from someone named DeShawn due to strategic considerations. This is one form of what economists refer to as “statistical discrimination,” since it is based on rational expectations given overall statistical trends (see Altonji and Blank 1999). Statistical discrimination stands in contrast to what economists term

“taste-based discrimination,” which is based on factors like racial prejudice that are not readily explicable by rational choice (e.g. Becker 1957).

Fryer and Levitt (2004) highlight that it is difficult to convincingly differentiate between taste-based and statistical discrimination; what appears to be taste-based discrimination may often be another form of statistical discrimination. Indeed, researchers can almost never definitively classify discrimination as taste-based. However, this does not mean that we cannot effectively study discrimination. For example, we can test for given types of statistical discrimination by including the relevant information that legislators might infer from an individual’s group identity directly into the message and then observing whether any residual discrimination remains. In this paper we test for statistical discrimination driven by the average partisan preferences of different racial groups. Our goal is not to definitively identify taste-based discrimination, but to see whether there is evidence of discrimination even after experimentally controlling for a potentially important source of statistical discrimination: voters’ expressed partisan preferences.

Is the distinction between statistical and taste-based bias important for democratic practice? From the perspective of someone on the receiving end of discrimination, the answer is no. Such discrimination is unfair whatever its source and violates the democratic principle of equality embodied in such ideals as one person/one vote. In the *NAMUDNO* case cited at the beginning of this article, the Supreme Court was concerned with systemic discrimination regardless of its source. Therefore, even if we are unable to convincingly identify and control for *all* potential sources of statistical discrimination, our findings are important because they demonstrate the existence of systemic discrimination.

While it is thus sufficiently significant to simply document whether political inequality



exists, we think that it is important for democratic practice to try to differentiate between these two types of discrimination because knowing the reasons for discrimination helps identify the potential range of solutions possible for overcoming it. For instance, it might be the case that individuals of low socio-economic status face discrimination for taste-based reasons *or* because they vote at lower rates, which makes officials less responsive to them than to individuals of a higher socio-economic status who are more likely to vote (i.e. a form of statistical discrimination). The solution to making officials more responsive to those with low socio-economic status will depend on which of these reasons best explains politicians' discriminatory behavior. In the latter case, efforts might focus on helping increase voter turnout among those with a lower socio-economic status. If, however, officials discriminate because of their taste-based preferences then different tactics would be necessary. To the extent that research should not simply identify discriminatory behavior but also inform attempts to correct such behavior, understanding the source of discrimination will be crucial. This article represents a step in that process.

### **The Experimental Design**

Our experiment allows us to evaluate the competing claims about the nature of racial political inequality and representation described in the previous section. Our research design is similar to an approach taken in Putnam's seminal book *Making Democracy Work* (1993: 73). Like Putnam we contact public officials to measure their level of responsiveness; however, we build on Putnam's approach by randomizing the personal characteristics of the individuals who are ostensibly making contact. More generally, academics in other fields and federal agencies employ this type of approach to measure whether there is discrimination in such arenas as housing markets, job markets, and even government agencies (Fix and Turner 1998; Bertrand

and Mullianathan 2004; Pager and Quillian 2005).

### *Why Responsiveness?*

As Pitkin (1967: 209) defines the term in her classic work, political representation is “acting in the interest of the represented, in a manner responsive to them.” Determining how responsive legislators are to their constituents and not just how they vote is important for at least four reasons. First, as Hall (1996) notes, roll-call votes tell us nothing about the intensities of legislators’ preferences or their priorities. Looking at the level of effort paints a fuller picture of how well legislators represent their constituents. Second, government officials provide individuals with important avenues for accessing government services. As Young (1990) argues, researchers should focus on inequities in the processes by which resources and political power are distributed, not simply the end results of these processes. Third, evidence suggests that when minorities and women view their representatives as more responsive, they participate in politics at higher rates (Chattopadhyay and Duflo 2004; Griffin and Keane 2006). Thus if descriptive representation affects the responsiveness of officials, it may in turn affect the political activity of traditionally underrepresented groups. Finally, it is advantageous from a methodological perspective that the email senders have straightforward interests: they want a response. It is therefore clear when the legislator is acting in the interest of the minority group; such clarity is not always possible with roll call votes where there are often complex policy interests at stake and confounding variables present.

### *Treatment Conditions*

Box 1 provides the full text of the email sent to state legislators, with each legislator receiving just one email. We signaled the race of the email sender by randomizing whether the email was signed by and sent from an email account with the name Jake Mueller or the name

DeShawn Jackson. We also manipulated the text in order to signal the partisan preference of the email sender.

We chose the first names Jake and DeShawn because Fryer and Levitt (2004) show that these names are among the most racially distinct. Among individuals named DeShawn, almost all are black; among individuals named Jake, almost all are white. Similarly, we chose the surnames Mueller and Jackson because data from the 2000 Census indicate that among common surnames, these were, respectively, among those most strongly correlated with self-identification as white or black (Word, Coleman, Nunziata, and Kominski n.d.).

(Insert Box 1 About Here)

We signaled the partisan preference of the email sender by including text in the letter asking whether there was anything the sender needed to do in order to register in future primary elections; here we randomized whether they asked about Democratic primary elections, Republican primary elections, or did not specify a party (see Box 1). Crossing the race treatment with the partisanship treatment gives a total of 6 treatments. We designed these treatment manipulations to first measure whether legislators discriminated against blacks, and then to test whether there was evidence that this discrimination could be explained by legislators simply inferring the partisan preference of the sender from their race. By holding constant the partisan preference of the letter's sender, we can see if the discrimination we observed was due to strategic partisan considerations and also determine if any residual discrimination remains that is not attributable to these considerations.<sup>1</sup>

---

<sup>1</sup> To verify that the larger patterns of partisan support among whites and blacks in the United States were also reflected in the individuals with the names used for the aliases in our study, we examined the distribution of party registration among the individuals with these names in an available voter file (Kentucky's). The data, available in the Supporting Information, indicate that the last name Jackson and the first name DeShawn are indeed both strong signals of a Democratic partisan preference. The ratio of people registered as Democrats compared to the number registered as Republicans is 2:1 among people with the last name Jackson and 8:1 among people with the first name DeShawn. In contrast, people with the first name Jake or Jacob and the last name Mueller are split evenly across the

Also note that the text of the email dealt specifically with a request for constituency service. Because this experiment only examines constituency service, we cannot determine whether legislators respond differently to their constituents in other domains. While we do not expect legislator behavior to significantly differ across domains of responsiveness, only further research can definitively determine whether this is true.

### *The Sample*

Our sample includes state legislators in 44 US states with valid email addresses that were available online through state legislative websites in September 2008.<sup>2</sup> Note that we treat state legislators' email addresses and not necessarily the state legislators themselves. The response (or lack thereof) to any of the emails that we sent may have come from someone other than the legislator, such as a staff member. However, because we use the legislators' official email addresses from their respective state's legislative website, the persons responding to the request did so in an official capacity on the behalf of the legislators. Additionally, as elaborated in Appendix D of the Supporting Information, there is no evidence for a heterogeneous treatment effect in more highly professionalized legislatures, indicating that this concern is unlikely to threaten the external validity of our results.

### *Experimental Execution and Responsiveness Measurement*

Once we collected the data, we assigned legislators to treatment groups using block randomization by state, legislative chamber, political party, and whether the legislator was up for reelection. This method balances the number of legislators sharing these characteristics across treatment groups, while allowing each observation to remain equally likely to be assigned to

---

two parties. Again, this is strong evidence that legislators are likely to infer that DeShawn Jackson is much more likely to have a preference for Democratic candidates than someone named Jake Mueller.

<sup>2</sup>After sending the emails, about 5 percent of them immediately bounced back as undeliverable because the email addresses were no longer valid. For our analysis we limit the sample to the emails that were successfully sent out.

each of the treatment groups.<sup>3</sup> We sent the emails on the first weekend of October 2008 because several of the states' voter registration deadlines were the following week. We wanted to send the emails before these deadlines passed, but also during the time when the legislators were busy with the campaign season, so that they could potentially use that extra level of activity as an excuse for ignoring the email.<sup>4</sup> In addition, by sending out emails just weeks before the 2008 General election, we ensured that the strategic partisan considerations we tested for were highly salient for legislators.<sup>5</sup> Finally, our dependent variable for the analysis is whether the state legislator responded at all by November 4<sup>th</sup>, Election Day in 2008. The advantage of this measure is that it is objective: did the legislator reply or not?

### *Ethical Considerations*

While field experiments are becoming increasingly common in political science, field experiments on public officials by academics are relatively rare (for prominent exceptions see Putnam 1993; Bergan 2009). In fact, most field experiments on public officials have been funded and conducted by federal agencies as a way of auditing whether government programs discriminate on the basis of race (Fix and Turner 1998; see especially chapter 6). Because the use of field experiments on public officials by academics is relatively rare, we discuss here the ethical considerations we took into account before conducting our experiment and explain the

---

<sup>3</sup> To test the robustness of our randomization scheme, we tested for any differences among the other observables on which we did not block: the legislative district's total population, the racial composition of the district, the race of the legislator, and the Squire (2007) index of state legislative professionalism. The results of our randomization check indicate that our randomization scheme was highly successful,  $\chi^2(52) = 30.03$ ,  $p = .9966$ .

<sup>4</sup> We believe this was successful because even among legislators who ultimately did reply, several noted the business of the campaign season as a reason for the lateness of their response. The following example comes from a legislator in Alaska in response to the Jake alias: "I apologize that your message arrived in the midst of my email account being bombarded with messages from around the world about Sarah Palin. In our efforts to clear these messages, I fear we overlooked your message..."

<sup>5</sup> Because we sent all of the emails at the same time, the time between when legislators received the email and the voter registration deadline differed across states. Since the partisan composition of legislatures also varies across states, one potential concern is that any differences we observed between the parties might simply be the result of differences in how long each group had to respond before the voter registration deadline came. We tested this possibility, the results of which appear in Appendix A of the Supporting Information, and found no significant differences between the two parties.

steps we took to ensure that our experiment would involve minimal risk to our subjects. Our hope is that others will engage in similar introspection before experimenting on public officials. Indeed, we hope that researchers using more traditional observational, survey, and interview methods will make similar considerations in their work since the issues we discuss are not necessarily unique to experimental work (Dexter 1964). We should also note that we received IRB exemption before conducting this experiment.

In conducting the experiment we considered three ethical issues. The first was the use of deception: we used fictitious aliases when contacting legislators and experimentally manipulated what information was conveyed. This was particularly important because we wanted to test whether public officials engage in discrimination based on the race and partisanship of the individual contacting them. The ability to randomly assign these characteristics to individuals is only available in a field experiment with fictitious individuals; similar considerations explain why fictitious names and resumes are used in similar studies of labor market discrimination (e.g. Bertrand and Mullianathan 2004). While some deception was thus necessary for the completion of this particular experiment, we believe that researchers should employ deception with great care and attempt to minimize its use.

Second, we considered how to minimize any harm that our experiment might cause. Consequently we have taken steps to maintain the anonymity of legislators' responses in order to ensure that our experiment is not used to tarnish the reputation of any given legislator. This is also important from a scientific perspective because it would be misleading to report the behavior of a given legislative office. Because we do not observe all potential outcomes (the response to a request from all combinations of race and party signal) for any given legislator, we do not know how they would have responded to the other treatments. We can only make average

comparisons across groups of legislators.

Finally, we considered how to minimize the burden placed on legislators' time. In so doing we tried to achieve the standard set by Putnam when he describes his own experiment as "slightly deceptive, but innocuous and highly informative" (1993: 73). We felt that some burden was necessary because, as Hall (1996) suggests, seeing how legislators choose to expend time and effort is the best way to learn about their priorities. That said we tried to choose a request that would be fairly easy to respond to so that we did not prevent legislators from doing work for their constituents. Based on the responses we received, we believe that we were successful. Of the replies that we received (nearly half of the legislative offices did not reply), the median reply was 291 characters long. Assuming an average word length of 5 characters plus a space after each word, the median message we received was only 49 words long, roughly the length of the remainder of this paragraph. Accordingly, we believe that the way we conducted our experiment caused no significant harm to the state legislators who were our subjects or their constituents who may have been seeking their help at the time. Likewise, any future experiments should similarly try to keep requests short and simple.

## **Results**

Just over half of the state legislators responded to our emails: we received 2,747 responses to the 4,859 emails that were successfully sent (a 56.5% response rate). However, the putatively white and black aliases did not enjoy similar rates of reply. Table 1 shows these differences and the overall rates of reply for each of our experimental groups. Among the emails that did not signal partisanship, legislators responded to 60.5% of the emails sent from the Jake alias but only 55.3% of those from the DeShawn alias, a statistically significant difference of 5.1 percentage points ( $p=0.04$ ). The OLS regression results in Table 4 in the appendix shows that

this result is robust to controlling for a number of legislator, district, and state characteristics (see Column 1 of Table 4). (Researchers interested in the other determinants of legislative responsiveness may also be interested in the coefficient values in Table 4, though with the usual caveats applied to these non-randomized characteristics.)

(Insert Table 1 about here.)

However, note that there appears to be no discrimination along racial or partisan lines in the experimental groups that signaled the partisanship of the sender (shown in the right half of Table 1). Of course, as we argued above, there are theoretical reasons to expect heterogeneous treatment effects by the party of the legislator; Republicans and Democrats are likely to react quite differently to these partisan signals. Indeed, in order to test whether legislators are using the voter's race to infer the voter's partisan preference and engage in statistical discrimination, we must examine the response rates by party of the legislator. Similarly, there are theoretical reasons to expect heterogeneous treatment effects by the race of the legislator. The estimated -5.1 percentage point difference (see column 1 of Table 1) represents the average treatment effect across all legislators and may miss important heterogeneity in the treatment effect by race of the legislator.

The next two subsections test for heterogeneous treatment effects by the legislator's party and race. When interpreting the heterogeneous treatment effects by the legislators' party and race it is important to remember that we did not randomize legislators' characteristics, and that some confounding variable may be driving the observed results. As a robustness check, we try to minimize this potential concern by controlling for the numerous legislator, district, and state factors, including whether the legislator was up for reelection, their legislative chamber, the population and median household income of the district, census data on the percent white and



percent black of the population in the district, the Squire index of legislative professionalism for the state, and whether the state is located in the South. These OLS regression results are given in Table 4 in the Appendix and show that the results continue to hold when controlling for these factors.

The Supporting Information also shows that our results are robust to the inclusion of interaction terms for the Squire index of state legislative professionalism, the percent of a district that is black, whether a legislator was up for re-election, and whether the legislators were from the South. In all cases these interaction terms are insignificant while our main findings continue to strongly hold.

Still, the results regarding the race and party of the legislator should be interpreted with caution because they were not randomized; while we can say with confidence that legislators with certain characteristics discriminated in certain ways, we cannot robustly attach causality to these characteristics.

#### *Partisanship and Strategic Considerations*

Recall that one motivation of our experiment was to test whether there is evidence that legislators engage in statistical discrimination by responding favorably to those who, based on their race, were expected to be of the same political party. We are able to answer this question because we randomized both the putative race and partisan preference of the email sender. In particular, we randomized whether the email asked about registering for Democratic primary elections, Republican primary elections, or did not specify a party (refer to Box 1 for the exact wording).

If the discrimination we observed against the DeShawn alias is due to legislators using race to infer partisan preference, then we should observe two things: (1) when no partisanship is

signaled, Republicans should favor the Jake alias and Democrats should favor the DeShawn alias, and (2) when the sender's partisan preference is signaled, the observed discrimination between DeShawn and Jake should disappear. That is, when partisanship is signaled, the sender's race conveys no additional information about their partisan preference and any statistical discrimination based on strategic partisan considerations should disappear.

We begin by testing whether there is evidence that the legislators noticed and acted on the partisan preference signaled in the email. Because the partisan signal was part of the second paragraph of the email, it is possible that those reading the email may have missed this signal. However, the results in Table 2 suggest that the legislative offices *did* notice and react to the partisanship signal. On average, Republican legislators were 4.3 percentage points more likely to respond to those who expressed interest in a Republican primary than to those who indicated interest in a Democratic primary ( $p=0.10$ ). Democrats on the other hand were just over 5 percentage points more responsive to those who expressed interest in a Democratic primary than those who expressed interest in a Republican primary ( $p=0.03$ ). These results are robust to inclusion of the control variables (see column 3 of Table 4). Legislators are more responsive to requests from individuals of their own party.

(Table 2 About Here)

Given that there is evidence that legislators are more responsive to co-partisans, is there evidence that they infer partisanship from race and use this information to respond strategically? As we explained above, we are able to test this question by comparing the difference in response rates when no partisanship is signaled (the last line of Column 1) to the estimated response rates when partisanship is signaled (the last lines of Columns 2 and 3). The results show that there is no evidence to suggest that Democratic legislators engage in statistical discrimination based on

inferred partisanship; in both experimental conditions the difference in how responsive Democratic legislators are to the putatively white and black aliases is statistically insignificant.

The evidence is more mixed for Republican legislators. When partisanship is not signaled (see Column 1), Republicans are estimated to be 8.1 percentage points less responsive to the DeShawn alias than the Jake alias. This pattern is consistent with the possibility that Republican legislators may use the race of the individual to infer something about the voter's partisanship. Since blacks in recent decades have consistently voted for Democratic candidates about 90 percent of the time, while whites have typically split their votes, Republican legislators receiving an email from someone named DeShawn would probably infer that he is likely to vote for Democratic candidates, and therefore respond less frequently.

If this discrimination were explained entirely by strategic partisan considerations, we would expect the rates of response to each racial alias to be indistinguishable when they shared the same partisan signal. However, columns 2 and 3 of Table 2 show that Republicans continue to reply less to the black alias by 4.8 percentage points ( $p=0.06$ ) even when the sender has indicated a partisan preference. The difference in differences indicates that about 3.3 percentage points, or about 40 percent of the original effect, may be due to strategic partisan considerations, though this difference in the differences is not statistically significant. Thus, while there is some evidence that strategic considerations regarding voters' perceived partisanship may partially motivate the patterns of discrimination that we observed, there remain significant levels of discrimination that cannot be explained by these considerations.<sup>6</sup>

---

<sup>6</sup> In the Supporting Information we also show that there is no statistically significant relationship between whether a legislator was running for re-election in 2008 and the level of discrimination they practiced against the DeShawn alias. This finding indicates that statistical discrimination motivated by factors beyond partisanship (such as beliefs about blacks' average ideology or propensity to vote) may also not be able to readily explain the discrimination we observed. As we did not randomize this characteristic, however, we cannot say so definitively. Furthermore, most of the legislators in our sample that were not up for re-election in 2008 might run for re-election in a future election cycle. However, as we discuss in a subsequent section, legislators running for re-election were much more likely to

*Descriptive Representation and Responsiveness*

The results in Tables 1 demonstrated that the DeShawn alias was less likely to receive a response than the Jake alias when partisanship was not signaled. While the results in the previous section tested whether statistical discrimination could explain this finding, they ostensibly suggest that the partisanship of the legislator best explains the discrimination we found, as Republicans, but not Democrats, are more responsive to the Jake alias. However, those results do not take into account the possible influence of the legislators' own race.

As discussed above, previous research on descriptive representation suggests that legislators may be more responsive to individuals from their same racial group. In other words, there may be heterogeneous treatment effects by the legislator's race, with white Democrats and Republicans exhibiting differential treatment in favor of the Jake alias, and minority Democrats and Republicans exhibiting differential treatment in favor of the DeShawn alias. Because minorities constitute 20.4 percent of the Democratic legislators in our sample but only 2.5 percent of their Republican counterparts,<sup>7</sup> part of the reason that we do not observe Democrats exhibiting significant discrimination on average may be that we miss the heterogeneous treatment effects by race within party. That is, if whites and minorities discriminate in opposing directions (with white legislators favoring Jake and minority legislators favoring DeShawn), then Democrats would appear not to discriminate on average even if these two groups within the Democratic party did in fact discriminate.

(Table 3 About Here)

---

respond overall, indicating that legislators' re-election status did effect their responsiveness, thus making this non-finding more substantively significant.

<sup>7</sup> We identified which legislators were black or members of other minority groups (Latino, Arab-American, Native American, and Asian-American) by using, respectively, the directories created by the National Conference of Black State Legislators, the National Association of Latino Elected Officials, the Arab American Institute, the National Caucus of Native American State Legislators and the UCLA Asian American Studies Center.

Table 3 reports the reply rates broken down by the race and party<sup>8</sup> of the legislator when the email does not signal partisan preference. Among Democrats there was significant heterogeneity in their observed discriminatory behavior that is related to the race of the legislator. White Democrats were 6.8 percentage points less likely to respond to the DeShawn alias than to the Jake alias ( $p=0.07$ ), while minority Democrats were 16.5 percentage points more likely to respond to the DeShawn alias than the Jake alias ( $p=0.02$ ). Holding constant the overall differences in responsiveness between white legislators and those of any minority group, this represents a 23.3 percentage point difference in rates of differential treatment between minority Democrats and their white counterparts ( $p<0.01$ ).

Like white Democrats, white Republicans were also less responsive to the DeShawn alias by 7.6 percentage points ( $p=0.04$ ), essentially the same behavior that we observed among white Democrats. (The small number of Republican legislators who are minorities makes it difficult to form conclusions about their behavior.)

The results in Table 3 were also robust to the inclusion of control variables (see columns 5 and 6 of Table 4 in the Appendix). It is particularly noteworthy that when we allow the treatment effect to vary by the race of the legislator, Republican legislators are no longer estimated to be significantly more responsive to the Jake alias than the DeShawn alias (see the results in column 6 of Table 4). In other words, a legislator's race, and not her party, is more important in predicting discrimination

Our initial finding that Democrats did not discriminate missed significant heterogeneity in the Democratic Party. Recall that when we estimated the level of differential treatment Democrats exhibited as a whole (Table 2), we found no statistically significant differences in

---

<sup>8</sup> Those interested in viewing the full results of the experiment, including the treatment groups that examined partisan affiliation broken down by both race and party, are directed to the Supporting Information.

how likely they were to respond to the Jake alias than the DeShawn alias. However this masks the fact that once race is taken into account, white Democrats discriminate at rate similar to white Republicans. Much of the reason that we observe Democrats exhibiting on average less differential treatment than their Republican counterparts towards the Jake alias is thus related to the racial composition of Democratic legislators. The minority legislators in the Democratic party exhibit differential treatment in favor of the DeShawn alias that, when averaged with the differential treatment of Jake exhibited by white Democratic legislators, makes it appear that Democrats on average do not engage in discrimination. Yet, when comparing white Republicans and white Democrats, the differences in their levels of discrimination are far smaller—less than one percentage point.

One potential criticism of this finding is that because districts with more minorities are more likely to elect minorities (a pattern that indeed holds true in our data), the number of minorities in a district might be the actual explanatory cause, and the race of the legislator might merely be associated with this variable (see Grose 2005). However, using Census data on the racial composition of state legislative districts, we find no evidence for a heterogeneous treatment effect among white legislators based on the racial composition of their districts. These results appear in Appendix D of the Supporting Information.

A graphical summary of our results appear in Figure 1. Bars extending to the left indicate that the legislators were less likely to respond to the DeShawn alias than the Jake alias, while bars extending to the right indicate the opposite. The bars' lengths correspond to the size of the effect we observed in percentage points.

(Insert Figure 1 About Here).

## **Discussion**

In October 2008 we conducted a field experiment to test whether legislators' responsiveness to a request for help with registering to vote depended on the race of the email sender. Our analysis showed the following:

- United States state legislators were less responsive to requests from blacks than from whites for help with registering to vote when no signal about partisanship was given (by 5.1 percentage points).
- Legislators from both parties were more responsive to co-partisans (by about 4.5 percentage points).
- Overall, Republican (but not Democratic) legislators replied less to the black alias (by 8.1 percentage points) and, while there is some limited evidence that part of this discrimination is strategic, much of it is not;
- Rather, this remaining discrimination appears to be almost entirely due to the racial composition of the parties, as Democratic and Republican whites discriminated against the black alias at nearly identical and significant levels (by 6.8 percentage points and 7.6 percentage points, respectively).
- Minority state legislators responded much more frequently to the black alias than to the white alias (by 16.5 percentage points overall).

While it is important to remember that our experiment only examines responsiveness to a request for help registering to vote and therefore does not necessarily indicate that legislators exhibit these patterns of behavior in other domains, each of these findings sheds light on ongoing debates both in political science and among institutional actors in American politics.

First, one of the arguments often advanced for increasing the number of minority legislators through mechanisms such as majority-minority legislative districts is that elected

officials better represent people with whom they share characteristics (Canon 1999). Similarly, previous research has suggested that black constituents participate in politics at higher rates when black legislators represent them because they believe black legislators are more responsive to their concerns (Griffin and Keane 2006). While there is ongoing debate about the effectiveness of some mechanisms designed to increase the number of minority elected officials (e.g. Cameron, Epstein, and O'Halloran 1996; Lublin 1999; Epstein and O'Halloran 1999; Gay 2007), our results provide direct support for the broader argument that how effectively minorities are represented *does* depend on the race of their representatives, regardless of their party.

Second, our results raise concerns that regardless of their party, the very legislators responsible for crafting the ways that citizens interact with nearly all American political institutions display a willingness to discriminate against minorities when they seek access to these institutions. Claims made that legislators may be willing to take action to suppress minority turnout (Brennan Center 2008; Barnes 2008) thus receive some support from our results; however, our results also indicate that white legislators of both parties, and not just Republicans, might be inclined to limit minority turnout for reasons unexplained by these groups' partisan preferences.

Finally, at the beginning of the paper we discussed the distinction between taste-based and statistical discrimination. In Table 2 we tested whether legislators' engaging in a specific form of statistical discrimination based on voters' perceived partisan preferences could explain their differential treatment in favor of the Jake alias. The results suggest that part but not all of the observed discrimination may be due to these strategic partisan considerations. In particular, Republican legislators were differentially favorable to Jake in the no partisanship conditions but continued to exhibit statistically significant levels of differential treatment in favor of Jake when



the voter's partisan preference was signaled. Further, the results by race of the legislator – with white legislators being more responsive to the Jake alias and minority legislators being more responsive to the DeShawn alias – seem to suggest that the observed differential treatment may have more to do with taste-based discrimination. That white legislators of both parties are just as likely to discriminate reinforces this interpretation. However, as noted before, we cannot control for all the potential factors that cause legislators to engage in statistical discrimination based on race. Future research may consider other important factors, including the likelihood that a voter turns out for elections.

Even though we cannot definitively differentiate between taste-based and statistical discrimination, our results have important implications for the state of racial equality in the United States. With some on the Supreme Court ready by all accounts to declare discrimination a fact of the past in the American political system, our experiment reveals the opposite – we found that legislators of every racial group engaged in significant levels of discrimination in favor of their racial group. Race still matters in American politics – both for elected officials and their constituents. While the election of Barack Obama as the United States' first black president is an auspicious development for race relations in America, our politics are still not colorblind.

## References

Altonji, Joseph G., and Rebecca Blank. 1999. "Race and Gender in the Labor Market." In *Handbook of Labor Economics Volume 3c*, eds. Orley C. Ashenfelter and David Card, 3144-3259. Elsevier Science B.V.

The American National Election Studies. 2005. *The 1948-2004 ANES Cumulative Data File* [dataset]. Stanford University and the University of Michigan [producers and distributors]. <<http://www.electionstudies.org>>

- Barnes, Robert. 2008. "High Court Upholds Indiana Law on Voter ID. 6-3 Ruling Calls Measure Reasonable to Fight Fraud." *The Washington Post*, April 29, A01.
- Bartels, Larry M. 1998. "Where the Ducks Are: Voting Power in a Party System." In *Politicians and Party Politics*, ed. John G. Geer. Baltimore, MD: The Johns Hopkins University Press, 43-79.
- Becker, Gary. 1957. *The Economics of Discrimination*. Chicago: University of Chicago Press.
- Bergan, Daniel E. 2009. "Does Grassroots Lobbying Work?" *American Politics Research* 37 (2): 327-352.
- Bertrand, Marianne, and Sendhil Mullainathan. 2004. "Are Emily and Greg More Employable than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination." *American Economic Review* 94 (4): 991-1013.
- Bobo, Lawrence. 2001. "Racial Attitudes and Relations at the Close of the Twentieth Century." In *America Becoming: Racial Trends and Their Consequences*, vol. 1, ed. Neil J. Smelser, William J. Wilson, and Faith Mitchell. Washington, DC: National Academy Press, 264-301.
- Brennan Center for Justice. 2008. "Brennan Center Criticizes Supreme Court Decision to Uphold Indiana Voter ID Law." Press Release, April 28.  
<[http://www.brennancenter.org/content/resource/brennan\\_center\\_criticizes\\_supreme\\_court\\_decision\\_to\\_uphold\\_indiana\\_voter\\_id/](http://www.brennancenter.org/content/resource/brennan_center_criticizes_supreme_court_decision_to_uphold_indiana_voter_id/)>.
- Cain, Bruce E., John Ferejohn, and Morris P. Fiorina. 1987. *The Personal Vote: Constituency Service and Electoral Independence*. Cambridge: Harvard University Press.
- Canon, David T. 1999. *Race, Redistricting, and Representation: The Unintended Consequences of Black Majority Districts*. Chicago: University of Chicago Press.

- Cameron, Charles, David Epstein, and Sharyn O'Halloran. 1996. "Do Majority-Minority Districts Maximize Substantive Black Representation in Congress?" *American Political Science Review* 90 (4): 794-812.
- Chattopadhyay, Raghendra., and Esther Duflo. 2004. "Women as Policy Makers: Evidence from a Randomized Policy Experiment in India." *Econometrica* 72 (5): 1409-43.
- Chavez, Linda. 1992. "Hispanic, Affirmative Action, and Voting." *Annals of the American Academy of Political and Social Science* 532: 75-87.
- Dahl, Robert A. 1956. *A Preface To Democratic Theory*. Chicago: University of Chicago Press.
- Dexter, Lewis A. 1964. "The Good Will of the Important People: More on the Jeopardy of the Interview." *Public Opinion Quarterly* 28 (4): 556-563.
- Epstein, David, and Sharyn O'Halloran. 1999. "Measuring the Electoral Impact of Majority-Minority Voting Districts." *American Journal of Political Science* 43 (2): 367-95.
- Fenno, Richard F. 1978. *Home Style: House Members in their Districts*. Boston: Little, Brown and Company.
- Fix, Michael and Margery A. Turner, eds. 1998. *A national report card on discrimination in America: The role of testing*. Washington, DC: Urban Institute Press.
- Fraga, Luis R. 1992. "Latino Political Incorporation and the Voting Rights Act." In *Controversies in Minority Voting: The Voting Rights Act in Perspective*, eds. Bernard Grofman and Chandler Davidson. Washington, DC: The Brookings Institution.
- Fryer, Roland G., Jr., and Steven D. Levitt. 2004. "The Causes and Consequences of Distinctively Black Names." *Quarterly Journal of Economics* 119(3): 767-805.
- Frymer, Paul. 1999. *Uneasy Alliances: Race and Party Competition in America*. Princeton.
- Gay, Claudine. 2007. "Legislating Without Constraints: The Impact of Constituency Preferences

- on Policy Representation in Majority-Minority Districts.” *Journal of Politics* 69 (2): 442-56.
- Griffin, John D., and Michael Keane. 2006. “Descriptive Representation and the Composition of African American Turnout.” *American Journal of Political Science* 50 (4): 998-1012.
- Griffin, John D. and Brian M. Newman. 2007. “The Unequal Representation of Latinos and Whites.” *Journal of Politics* 69 (4): 1032-1046.
- Grose, Christian R. 2005. “Disentangling Constituency and Legislator Effects in Legislative Representation: Black Legislators or Black Districts?” *Social Science Quarterly* 86 (2): 427-443.
- Grose, Christian R., Maruice Mangum, and Christopher Martin. 2007. “Race, Political Empowerment, and Constituency Service: Descriptive Representation and the Hiring of African-American Congressional Staff.” *Polity* 39 (4): 449-478,
- Hajnal, Zoltan L. 2009. “Who Loses in American Democracy? A Count of Votes Demonstrates the Limited Representation of African Americans.” *American Political Science Review* 103 (1): 37-57.
- Hall, Richard L. 1996. *Participation in Congress*. New Haven, CT: Yale University Press.
- Kinder, Donald R. and Cindy D. Kam. 2009. *Us Against Them: The Ethnocentric Foundations of American Opinion*. Chicago.
- Lublin, David I. 1999. *The Paradox of Representation*. Princeton, NJ: Princeton University Press.
- Pager, Devah and Lincoln Quillian. 2005. “Walking the Talk? What Employers Say Versus What They Do.” *American Sociological Review* 70 (3): 355-380.
- Pitkin, Hanna. 1967. *The Concept of Representation*. Berkeley: University of California Press.

- Putnam, Robert D. 1993. *Making Democracy Work: Civic Traditions in Modern Italy*. Princeton, NJ: Princeton University Press.
- Squire, Peverill. 2007. "Measuring State Legislative Professionalism: The Squire Index Revisited." *State Politics and Policy Quarterly* 7 (2): 211–227.
- Swain, Carol M. 1993. *Black Faces, Black Interests: The Representation of African Americans in Congress*. Cambridge, MA: Harvard University.
- Thernstrom, Abigail. 1987. *Whose Votes Count? Affirmative Action and Minority Voting Rights*. Cambridge, MA: Harvard University Press.
- Thernstrom, Stephan, and Abigail Thernstrom. 1997. *America in Black and White: Our Nation, Indivisible*. New York: Simon and Schuster.
- Verba, Sidney. 2003. "Would the Dream of Political Equality Turn out to Be a Nightmare?" *Perspective on Politics* 1 (4): 663-679.
- Whitby, Kenny J. 1997. *The Color of Representation: Congressional Behavior and Black Constituents*. Ann Arbor: University of Michigan Press.
- Word, David L., Charles D. Coleman, Robert Nunziata, and Robert Kominski. n.d. "Demographic Aspects of Surnames from Census 2000." *Technical Report for the U.S. Census Bureau*. <<http://www.census.gov/genealogy/www/surnames.pdf>>.
- Young, Iris M. 1990. *Justice and the Politics of Difference*. Princeton, NJ: Princeton University Press.

## Box 1. Email Sent to State Legislators

From: *[Treatment Name]*  
To: **[Legislator's Email Address]**  
Subject: A Question on Registering to Vote

Dear **[Representative/Senator]** **[Legislator's Last Name]**,

My name is *[Treatment Name]* and I'm trying to figure out how to register to vote for the upcoming election. I heard that the voter registration deadline is soon.

Who should I call in order to register? Also, is there anything special I need to do when I register so that I can vote in future *[{blank}/Democratic/Republican]* primary elections?

Thanks,  
*[Treatment Name]*

Note: Bolded items were manipulated across emails. Items in italics were assigned randomly based on the treatment group.

Table 1. Overall Effect Sizes – Does Jake Receive More Replies Than DeShawn?

	No Partisanship Signal	Republican Signal	Democratic Signal	Party Differential	
DeShawn	55.3%	54.3%	57.3%	-2.9%	Combined <b>-0.9%</b> <i>(p=0.61)</i>
Jackson	N=806	N=810	N=812	<i>(p=0.23)</i>	
Jake	60.5%	56.4%	55.3%	1.1%	
Mueller	N=812	N=820	N=799	<i>(p=0.31)</i>	
Race	<b>-5.1%*</b>	-2.1%	1.9%		
Differential	<i>(p=0.04)</i>	<i>(p=0.39)</i>	<i>(p=0.43)</i>	Combined Effect <b>-0.1% (p=0.95)</b>	

Notes: The first column supplies the response rates when partisanship was not signaled while the second and third columns, respectively, supply the response rates when the Republican and Democratic partisan signals were included in the emails. The last row in each section gives the difference in the response rates between the DeShawn and Jake aliases for that particular partisan signal. These values are calculated so that positive values indicate a differential treatment in favor of DeShawn and negative values a differential treatment in favor of Jake. The last row gives the combined race differential when pooling the observations for which partisanship was signaled. The second to last column gives the difference between the response rates between the Republican and Democratic partisan signals for that particular alias, while the last column pools the party differential for both the Jake and DeShawn aliases. Positive values in these columns indicate differential treatment in favor of the Republican signal while negative values indicate differential treatment in favor of the Democratic signal. P-values (two-tailed) are reported below the coefficients. ^Sig. at the 0.10 level (two-tailed), \*Sig. at the 0.05 level (two-tailed), \*\*Sig. at the 0.01 level (two-tailed).

Table 2. Response Rates by the Treatment Name, the Partisanship Signal, and Legislators' Party

(a) Republican Legislators					
	No partisanship	Republican	Democrat	Party Differential	
DeShawn	58.9%	58.0%	54.0%	4.0	Combined <b>4.3<sup>^</sup></b> <b>(p=0.10)</b>
Jackson	N=360	N=362	N=361	(p=0.28)	
Jake	67.0%	63.1%	58.5%	4.6	
Mueller	N=364	N=366	N=357	(p=0.21)	
Race	<b>-8.1*</b>	-5.1	-4.5		
Differential	<b>(p=0.02)</b>	<b>(p=0.16)</b>	<b>(p=0.22)</b>	Combined Effect <b>-4.8<sup>^</sup> (p=0.06)</b>	
(b) Democratic Legislators					
	No partisanship	Republican	Democrat	Party Differential	
DeShawn	52.4%	51.3%	59.9%	-8.5**	Combined <b>-5.1*</b> <b>(p=0.03)</b>
Jackson	N=446	N=448	N=451	(p=0.01)	
Jake	55.1%	51.1%	52.7%	-1.6	
Mueller	N=448	N=454	N=442	(p=0.63)	
Race	<b>-2.7</b>	0.2	7.2*		
Differential	<b>(p=0.42)</b>	<b>(p=0.94)</b>	<b>(p=0.03)</b>	Combined Effect <b>3.7 (p=0.11)</b>	

Notes: The first column supplies the response rates when partisanship was not signaled while the second and third columns, respectively, supply the response rates when the Republican and Democratic partisan signals were included in the emails. The next to last row in each section then gives the difference in the response rates between the DeShawn and Jake aliases for that particular partisan signal. These values are calculated so that positive values indicate a differential treatment in favor of DeShawn and negative values a differential treatment in favor of Jake. The last row gives the combined race differential when pooling the observations for which partisanship was signaled. The second to last column in each section gives the difference between the response rates between the Republican and Democratic partisan signals for that particular alias, while the last column pools the party differential for both the DeShawn and Jake aliases. Positive values in these columns indicate differential treatment in favor of the Republican signal while negative values indicate differential treatment in favor of the Democratic signal. P-values (two-tailed) are reported below the coefficients. <sup>^</sup>Sig. at the 0.10 level (two-tailed), \*Sig. at the 0.05 level (two-tailed), \*\*Sig. at the 0.01 level.

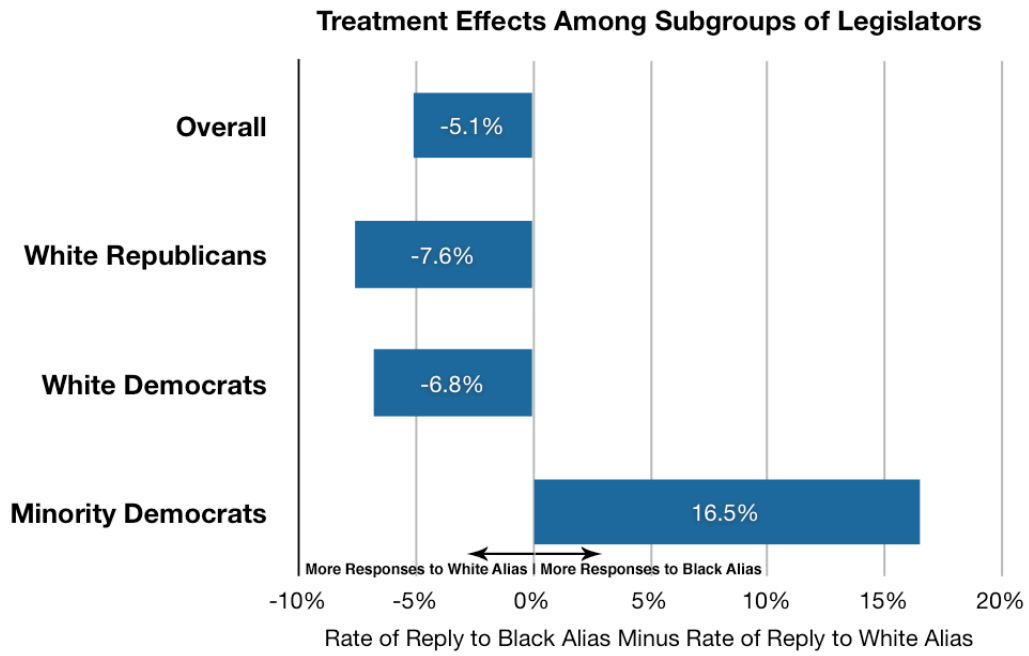


Table 3. Response Rates by the Experimental Condition and the Legislators' Party and Race

Legislator Party:	Democratic Legislators		Republican Legislators	
Legislator Race:	Whites	Minorities	Whites	Minorities
DeShawn Jackson	54.3%	45.9%	59.3%	44.4%
	N=348	N=98	N=351	N=9
Jake Mueller	61.2%	29.4%	66.9%	75.0%
	N=363	N=85	N=356	N=8
<b>Race Differential</b>	<b>-6.8<sup>^</sup></b>	<b>16.5*</b>	<b>-7.6*</b>	<b>-30.6</b>
	<i>(p=0.07)</i>	<i>(p=0.02)</i>	<i>(p=0.04)</i>	<i>(p=0.23)</i>

Notes: The first group supplies the response rates among Democratic legislators while the second group supplies response rates among Republican legislators. Within each group, the first column presents the results for white legislators of that party while the second column presents the results for minority legislators. The last row in each of these column gives the difference in the response rates between the DeShawn and Jake aliases for that particular partisan signal. These values are calculated so that positive values indicate a differential treatment in favor of DeShawn and negative values a differential treatment in favor of Jake. Positive values in these columns indicate differential treatment in favor of the Republican signal while negative values indicate differential treatment in favor of the Democratic signal. P-values (two-tailed) are reported below the coefficients. <sup>^</sup>Sig. at the 0.10 level (two-tailed), \*Sig. at the 0.05 level (two-tailed), \*\*Sig. at the 0.01 level (two-tailed).

Figure 1. Heterogeneous levels of differential treatment among subgroups of legislators



FINAL REVIEW

## Appendix

This appendix reports the results of regression results meant to provide a robustness check for the results in Tables 1-3. These regression results are presented in Table 4. In all cases, the dependent variable is whether the legislative office responded to the email that was sent (1=responded, 0=did not respond). Each model is estimated using OLS regression and includes the following control variables: whether the legislator was up for reelection in 2008, whether they were part of their state's Senate or upper chamber, the population and median household income of the legislators district (as reported by the Census), Census data on the percent white and percent black of the population in the district, the Squire index of legislative professionalism for the state, and whether the state is located in the South.

The relevant treatment effects that correspond to those presented in Tables 1-3 are bolded and given at the bottom of Table 4. As these results show, all of the findings from Tables 1-3 continue to hold even when controlling for these other factors.

Finally, there are a few control variables that may be of theoretical interest to other researchers that were consistently statistically significant predictors across the various regression models. First, as might be expected, legislators running for re-election were between 7 and 9 percentage points more likely to respond. Legislators from more affluent districts were also more likely to respond – a \$10,000 increase in a district's median household income was associated with a 3.2 to 3.8 percentage point greater likelihood of reply. Finally, legislators from the South were between 8 and 18 percentage points less likely to respond. Among the variables that were not statistically significant, the insignificance of the Squire index of state legislative professionalism (Squire 2007) was surprising.

Table 4. OLS Regression Results for Predicting Legislative Responsiveness: Robustness Check of Tables 1-3

Independent Variable	No Partisan Signal		Partisan Signal		No Partisan Signal	
	Table 1	Table 2	Table 2	Table 2	Table 3	Table 3
<i>Randomized Treatments</i>						
DeShawn Treatment	-0.052* (0.024)	-0.023 (0.032)	-	0.031 (0.023)	-0.077** (0.025)	0.062^ (0.035)
Republican Treatment	N/A	N/A	-0.048* (0.023)	-	N/A	N/A
<i>Interaction Terms</i>						
Republican Legislator*DeShawn	-	-0.065 (0.047)	-	-0.077* (0.034)	-	-0.030 (0.049)
Republican Legislator*Republican Treatment	N/A	N/A	0.092** (0.034)	-	N/A	N/A
Minority Legislator*DeShawn	-	-	-	-	0.205** (0.072)	0.192** (0.075)
<i>Control Variables</i>						
Republican Legislator	0.065* (0.026)	0.097** (0.035)	-0.050* (0.025)	0.034 (0.025)	0.065* (0.026)	0.080* (0.035)
Minority Legislator	-0.181** (0.048)	-0.182** (0.047)	-0.118** (0.035)	-0.119** (0.035)	-0.292** (0.061)	-0.286** (0.062)
Up for Re-election	0.090** (0.027)	0.090** (0.027)	0.069** (0.020)	0.069** (0.020)	0.094** (0.027)	0.093** (0.027)
District Population (100,000s)	0.001 (0.017)	0.001 (0.017)	-0.005 (0.012)	-0.006 (0.012)	0.003 (0.017)	0.002 (0.017)
Median HH Inc. (\$10,000s)	0.038** (0.009)	0.038** (0.009)	0.032** (0.007)	0.031** (0.007)	0.039** (0.009)	0.038** (0.009)
Senator	0.048 (0.030)	0.048 (0.030)	0.070** (0.022)	0.071** (0.022)	0.050^ (0.030)	0.050^ (0.030)
Squire Index	-0.017 (0.114)	-0.017 (0.114)	-0.062 (.080)	-0.063 (0.080)	-0.026 (0.114)	-0.026 (0.114)
South	-0.179** (0.031)	-0.179** (0.031)	-0.076** (0.021)	-0.076** (0.021)	-0.179** (0.030)	-0.179** (0.030)
District White %	-0.099 (0.120)	-0.099 (0.120)	-0.064 (0.086)	-0.068 (0.086)	-0.095 (0.120)	-0.096 (0.120)
District Black %	0.050 (0.132)	0.050 (0.132)	-0.266** (0.092)	-0.268** (0.092)	0.065 (0.132)	0.064 (0.132)
Constant	0.490** (0.122)	0.476** (0.122)	0.519** (0.088)	0.485** (0.092)	0.495** (0.122)	0.488** (0.122)
N	1618	1618	3241	3241	1618	1618
R <sup>2</sup>	0.082	0.083	0.055	0.055	0.087	0.087
<i>Relevant Treatment Effects</i>						
<b>All Legislators:</b>	<b>-0.052*</b>					
<b>DeShawn Treatment – Jake Treatment</b>	<b>(0.024)</b>					
<b>Republican Legislators:</b>		<b>-0.088*</b>		<b>-0.046^</b>		
<b>DeShawn Treatment – Jake Treatment</b>		<b>(0.035)</b>		<b>(0.025)</b>		
<b>Democratic Legislators:</b>		<b>-0.023</b>		<b>0.031</b>		
<b>DeShawn Treatment – Jake Treatment</b>		<b>(0.032)</b>		<b>(0.023)</b>		
<b>Republican Legislators:</b>			<b>0.044^</b>			
<b>Republican Signal – Democratic Treatment</b>			<b>(0.025)</b>			
<b>Democratic Legislators:</b>			<b>-0.048*</b>			
<b>Republican Signal – Democratic Treatment</b>			<b>(0.023)</b>			
<b>Minority Legislators:</b>					<b>0.127^</b>	<b>0.130^</b>
<b>DeShawn Treatment – Jake Treatment</b>					<b>(0.067)</b>	<b>(0.067)</b>
<b>White Legislators:</b>					<b>-0.077**</b>	<b>-0.062^</b>
<b>DeShawn Treatment – Jake Treatment</b>					<b>(0.025)</b>	<b>(0.035)</b>

Notes: The dependent variable is whether the legislative office responded to the email. All models estimated via OLS Regression. ^Sig. at the 0.10 level, \*Sig. at the 0.05 level, \*\*Sig. at the 0.01 level [All two-tailed].