

# Persistent Bias Among Local Election Officials

Blinded for Review

September 7, 2018

## **Abstract**

Results of an audit study conducted during the 2016 election cycle demonstrate that bias toward Latinos observed during the 2012 election has persisted. In addition to replicating previous results, we show that Arab/Muslim Americans face an even greater barrier to communicating with local election officials, though this study did not produce evidence of bias toward blacks. An innovation of our design allows us to measure whether emails were opened by recipients, which permits for the first time a test for the presence of implicit discrimination, which we find toward Arab/Muslim senders. We examine one explanation for the high level of bias toward this group, that bureaucrats are politically responsive. Our findings support this explanation: in geographies that cast more votes for the Republican candidate, discrimination against Arab/Muslim names was stronger.

**Word Count:** 3,900

Racial bias that limits access to the ballot threatens basic principles of democratic equality. One potential source of bias that has received little attention are the street level bureaucrats (Lipsky, 1980) who administer elections in the U.S. An audit study conducted during the 2012 U.S. election cycle showed these local election officials responded at significantly lower rates to inquiries from voters with putatively Latino, as opposed to white, surnames (White, Nathan and Faller, 2015). In this paper we report the results of a similar audit study performed during the 2016 election cycle. We find that the previously observed bias against Latinos is persistent. We also extend the previous study by testing the effects of two racial primes other than Latino. Voters with Arab/Muslim names received responses at significantly lower rates (11 percentage points) than whites, while black voters did not.

The two primary motivations for this study are to determine whether the previous finding of bias toward Latinos stands up to replication, and to examine whether this bias extends to blacks and Arab/Muslim Americans. In spite of the ample evidence of racial disparities in political participation (Hajnal and Lee, 2011; Abrajano and Alvarez, 2010; Hajnal and Abrajano, 2015; García-Bedolla and Michelson, 2012) and in every-day life (Bertrand and Mullainathan, 2004; Pierson et al., 2017), relatively little empirical work demonstrates the role of race in limiting access to the ballot in contemporary America (McNulty, Dowling and Ariotti, 2009; Pettigrew, 2016), and some claims in this area have aroused skepticism (Hajnal, Lajevardi and Nielson, 2017; Grimmer et al., 2018). The pervasive discrimination that blacks face in various arenas of American politics (Butler, 2014) suggest that this group could be at risk of bias in interacting with local election officials, though recent studies of political and bureaucratic responsiveness have failed to find bias toward blacks (Einstein and Glick, 2017; Gell-Redman et al., 2018). While there is also ample evidence of discrimination toward Arab and Muslim Americans (Gaddis and Ghoshal, 2015), this group has received comparatively less attention from scholars (Jamal and Naber, 2007; Panagopoulos, 2006). In an era of political rhetoric increasingly characterized by appeals to group identity, it is particularly important to understand how racially-motivated bias impacts the day-to-day mechanics of elections.

To seek evidence of bias, we focus on the thousands of local-level administrators charged with conducting elections in the United States. These bureaucrats are generally capable of exercising discretion in carrying out their job duties, which include responding to inquiries about the mechanics of voting and eligibility to participate in elections. Our core contention is that in exercising such discretion, street-level bureaucrats may be consciously or unconsciously influenced by the characteristics (e.g., race or partisanship) of individuals seeking public services (Lipsky, 1980; White, Nathan and Faller, 2015).

## Experiment Design

To determine the extent to which previously documented bias is persistent and extends to other racial groups, we conduct an email audit study of local election officials. The intended population of our study is all local election officials serving across all 50 U.S. states. Our intended sample comprises all such officials with publicly available email addresses and the analytic sample includes 6,439 local election officials from 44 states (Figure A1).<sup>1</sup>

The experimental stimulus consists of a single email sent to each local election official. All emails follow the same structure, greeting the official by name, referencing voter identification laws, and asking about the requirements to vote in the state corresponding to the official. Our design closely parallels White, Nathan and Faller (2015), but differs in that we send only messages that mention voter ID laws. Additionally, to minimize possible spillover issues, we create 27 variants of this request (SI section 2 and section 3).

Our experimental treatment is the putative identity of the email sender. In line with convention we expose officials to four distinct group identities by manipulating senders' names (Bertrand and Mullainathan, 2004; Bertrand and Duflo, 2017; Butler and Homola, 2017). Because the identities signaled in our treatments have elements which could be described as racial, ethnic, or religious, we refer to these generically as group identity treatments. To mitigate possible name effects, each group identity condition is signaled by 100 unique names. We check that the chosen names reliably prime ethnicity by conducting a manipulation check on Amazon's Mechanical Turk service where workers were asked to read sets of names and ascribe probabilities that a name belonged to a particular racial or ethnic group.<sup>2</sup> In total, we send 4,900 unique experimental conditions which combine variants of the contact language with treatment identities.

### Treatment assignment and implementation

We block treatment assignment on logged population density, two-party vote share in the 2012 presidential election, percent African American, percent Latino, percent of households with incomes below 150 percent of the federal poverty level,

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<sup>1</sup>We received Human Subjects approval from (*blind for review: two major research universities*) Human Subjects Committees. Both committees waived the requirement of informed consent. Additional implementation details are made available in the Supplemental Information. The study design, and pre-analysis plan were registered at Evidence in Governance and Politics. Data, code, and computing environments are available at *blind for review*.

<sup>2</sup>SI section 4 describes the procedure for choosing names, and section 15 provides the complete list of names.

and a dummy variable indicating whether a county was previously covered by Section 5 of the Voting Rights Act. Further details are provided in SI [section 5](#). Within each block we assign local election officials a racial condition and message version at random.

We sent 6,235 emails the morning of October 31, 2016, one email to each election official that was a part of the study.<sup>3</sup> Emails were sent from a privately registered domain, `ez-webmail.com`. Email addresses took the form of the senders' first initial, last name, and a two-digit string between 20 and 40. Accordingly, an official assigned to receive contact from a white voter, Daniel Nash, received a single email from `dnash24@ez-webmail.com`. To aid in identifying whether emails were received and opened by election officials, we include a 1x1 pixel image with a unique link – commonly referred to as a tracking pixel – in the email body. Upon opening the email, most email clients loaded the image from our server, providing a positive record that the email had been opened by a particular official. To maximize the probability that emails would not be collected by spam filters, we took steps to develop positive server reputation for the sending domain. Testing suggests these efforts were successful. In addition, we examined whether email content or sender name or address affected performance on client-side servers. We were unable to detect any difference in successful application of treatment in any of the conditions.

The choice to contact election officials eight days before the election is designed to make our study reflective of the real constraints on individuals seeking and providing information about voting requirements. To minimize the impact of our intervention on election officials' time, the specific request contained in the email is one that would require little effort to fulfill. Using data gathered via our mailing system, we estimate that the median time to compose and send a response to our email is three minutes, six seconds. We contend that any costs borne by public officials as a result of our intervention are counterbalanced by the benefits of uncovering persistent bias in electronic communications between constituents and local election officials.

Our pre-registered design uses a single outcome measure, GOTRESPONSE, coded 1 if an election official replied to our email prior to election day, and 0 otherwise (*Pre-analysis Plan Registered at EGAP, Blinded for Review*). We do not count auto-replies away messages, or bounces as valid replies. We further report exploratory analyses that use as an alternative outcome measure an indicator of whether or not a local election official opened the message, as well as exploratory sub-group analysis.

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<sup>3</sup>We also sent two waves of pilot email, 54 on October 26, 2016; and, 146 on October 28, 2016. For details, see SI [section 9](#).

Table 1: Response Rates by Experimental Condition

| Ethnic Cue        | White | Minority | Latino | Black | Arab  |
|-------------------|-------|----------|--------|-------|-------|
| Response Rate (%) | 61.3  | 56.6     | 58.4   | 61.4  | 50.1  |
| Standard Error    | 1.21  | 0.71     | 1.23   | 1.21  | 1.25  |
| N                 | 1,611 | 4,828    | 1,609  | 1,613 | 1,606 |

Notes: The *Minority* column includes all data from the *Latino*, *Black*, and *Arab* columns. Response rates and standard errors are reported in percentage terms.

## Results

Overall, 57.8 percent of the emails we sent received at least one reply from local election officials. While lower than the 67.7 percent response rate previously obtained from a similar sample (White, Nathan and Faller, 2015), this rate compares favorably with experiments on elected officials in the U.S., suggesting that our requests were taken at face value (Butler and Broockman, 2011).

Election officials respond at considerably lower rates when queries come from minority as opposed to white senders (difference in mean,  $\Delta\mu = -4.70$  percentage points, *Wilcox Rank-Sum*  $P < 2 \times 10^{-16}$ ). However, as we report in Table 1 responsiveness to minority senders is not uniformly lower. Nonparametric tests using white senders as the baseline find that a Latino name is sufficient to suppress the likelihood of a response by nearly 3 percentage points ( $\Delta\mu = -2.97$ ,  $P = 0.07$ ). Strikingly, an Arab/Muslim name lowers the likelihood of a response by greater than 11 percentage points ( $\Delta\mu = -11.3$ ,  $P < 1 \times 10^{-10}$ ). In contrast, black senders receive responses at a rate indistinguishable from white senders ( $\Delta\mu = 0.11$ ,  $P = 0.90$ ). Figure 1 (a) plots the Intent to Treat (ITT) causal effects of our treatments. Regression estimates with robust standard errors are reported in columns 1 and 2 of Table A6, and produce similar results.

Figure 1 (b) plots a precision weighted meta-analysis estimate (Gerber and Green, 2012, p. 361) that combines the results of our intervention with those previously reported (White, Nathan and Faller, 2015). These data, gathered in independent audits conducted over two election cycles, show that Latinos receive replies from local election officials at a rate 4.4 percentage points lower than whites ( $\Delta\mu = 4.4$ , precision weighted  $SE = 1.18$ ,  $P < 0.0001$ ).

While the persistence of the treatment of Latino senders in the 2012 and 2016 elections is remarkable, perhaps more striking is the finding that Arab/Muslim names suffer a penalty more than two times greater than the one produced by a Latino stimulus. One possible explanation for this finding is that names signaling

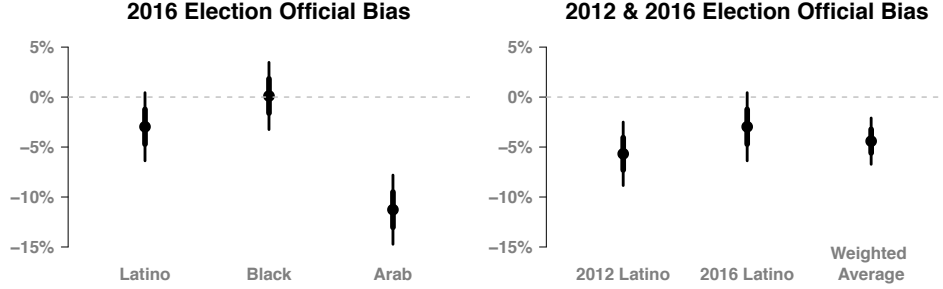


Figure 1: Points represent the ITT, the estimated difference in response rates to emails from the named identity, compared to the white response rate baseline. Thick bars report  $ITT \pm SE$ , thin bars report  $ITT \pm 1.96 * SE$ . All estimates are difference in means, except the *Weighted Average* which estimates a precision weighted difference (Gerber and Green, 2012) utilizing 2012 (White, Nathan and Faller, 2015) and 2016 Latino evidence.

this identity may stand out to election officials living in areas in which the population of Arab descent is very small. If this were the case, we might expect officials in counties with a low proportion of Arab Americans to be more likely to show bias. We find, however, that the proportion of Arab Americans does not moderate the treatment effect (Table A12, Model 3). Additional checks similarly fail to identify a pattern consistent with a theory of contact or proximity (Table A14).

### Evidence of implicit and explicit discrimination

Local election officials receiving our intervention demonstrate bias insofar as they respond differentially based only on the signal of group identity delivered through our treatments. This bias could be implicit, explicit, or both. Implicit bias is the result of an automatic evaluation (Greenwald, McGhee and Schwartz, 1998). Implicit bias is perhaps best defined with reference to its canonical measure, the implicit association test. In this test, subjects are exposed to labels of group identity along with terms or images meant to convey a positive or negative response. Subjects are asked to respond quickly to the stimuli presented to them, and the delay in response associated with certain item pairings captures the subject's level of implicit discrimination. Bias driven by a more conscious process is sometimes referred to as explicit (Devine, 1989).<sup>4</sup>

We argue that officials' choice to open an email is more reflective of implicit

<sup>4</sup>Explicit bias may also reflect what economists refer to as taste-based discrimination (Becker, 1957) though the conceptual distinction between the two concepts is not necessarily crisp.

discrimination. To see why this claim is plausible, consider how our experimental stimulus is delivered. Our correspondence appears first as an item in an email inbox, where it typically contains the name of the sender, the subject heading, and some portion of the body text. Officials are required to quickly scan this request from among the large number of other requests, categorize it mentally, and decide whether or not to open it. Though that need not be true for all respondents, on balance this high volume, low-attention task should be more reflective of an automatic process (Bertrand, Chugh and Mullainathan, 2005, p.96).

The natural extension of this argument is that, once an official has opened an email, the determination to compose a reply requires additional conscious thought about the nature of the request and the identity of the sender before composing a response. In this sense, the decision to respond conditional on having opened is more reflective of explicit bias. While we claim this is a reasonable theoretical argument, the empirical foundation for it is suspect. Officials who open emails from white senders may be systematically different from those who open emails from non-white senders, and those differences may be correlated with an official's likelihood of ultimately responding to the email. Thus we take differential rates of opening our messages as causal evidence of implicit bias, while differential rates of response among those who open the email is an interesting, but non-causal quantity related to explicit bias.

In low-cost tasks where local elections officials are unlikely to be highly cognitively engaged, the pattern of email opens suggests that elections officials may be automatically screening requests from Arab/Muslim senders. Table 2, Columns 1 and 2, present estimates that a Minority sender identity in general ( $\Delta\mu = -2.6, SE = 1.5$ ), and an Arab/Muslim sender identity in particular ( $\Delta\mu = -6.9, SE = 1.8$ ) causes local elections officials to be less likely to open an email. Table 2, Columns 3 and 4, reports that even among those election officials who choose to open an email, minorities are less likely to receive a response, an effect that is mostly associated with Arab/Muslim senders. As explained above, by reporting the response rate among those officials who opened the message, we are conspicuously conditioning on a post-treatment outcome (Angrist and Pischke, 2008, pp.64-66). Nevertheless, the estimates for Arab/Muslim senders provide suggestive evidence that both implicit and explicit discrimination are at work; the first such finding in a field experiment of this type.

## The role of politics

Why would local election officials respond at such markedly lower rates when the sender's name denotes an Arab/Muslim identity? An important possible explanation is that officials' choices reflect the views of the political party most broadly

Table 2: Bias in two tasks: Opening and responding once opened

|                | TrackerHit  |               | GotResponse TrackerHit |                |
|----------------|-------------|---------------|------------------------|----------------|
|                | (1)         | (2)           | (3)                    | (4)            |
| Minority Cue   | −2.6 (1.5)* |               | −3.5 (2.6)             |                |
| Latino Cue     |             | −0.3 (1.8)    |                        | −0.3 (2.8)     |
| Black Cue      |             | −0.7 (1.8)    |                        | −0.9 (2.8)     |
| Arab Cue       |             | −6.8 (1.8)*** |                        | −10.1 (3.1)*** |
| Observations   | 6,095       | 6,095         | 2,628                  | 2,628          |
| Data Subset    | All         | All           | Opens Only             | Opens Only     |
| R <sup>2</sup> | 0.30        | 0.31          | 0.57                   | 0.58           |

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

*Notes:* Election officials open Arab/Muslim senders' questions less frequently. Conditional on opening an email, officials respond less frequently. Regression estimates with White Standard errors, among users with email addresses that report tracking pixel hits, reported as percentage points. *TrackerHit* is an indicator variable capturing whether the email was opened; *GotResponse* captures whether a valid response was received. Models (3) and (4) are conditional upon opening. All models include block fixed-effects.

supported in their area of jurisdiction. While it may seem self-evident that local officials would be responsive to partisan preference, models of bureaucratic behavior suggest otherwise. Results from the principle-agent framework suggest that in delegating authority to bureaucrats, politicians may protect some policy processes from political pressure (Miller and Whitford, 2016), and allow for a relatively high degree of bureaucratic autonomy (McCubbins and Schwartz, 1984). Nevertheless, numerous factors could lead to bureaucrats making politically responsive choices. In the case at hand, this could be due to the charged rhetoric aimed at racial and ethnic groups which marked the 2016 electoral campaign. In particular, the Republican presidential candidate in that election made high profile statements about Mexican immigrants (Staff, 2015) and muslim immigrants (Johnson, 2015), which were broadly interpreted as reflecting negative views of those groups.

Were the expressed views of the Republican candidate reflected in the behavior of local election officials working in jurisdictions that ultimately supported that candidate? Figure 2 shows conditional average treatment effects for each of the group identity primes with the white prime as the baseline for comparison. The results show the pronounced divergence of response rates to the white as opposed



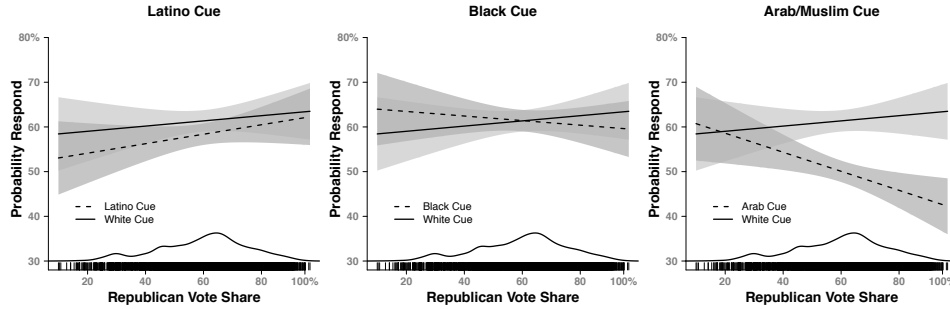


Figure 2: Predicted probability of receiving a response, reported for white vs. non-white names. The rug and density lines show data coverage across the vote-share range and grey envelopes are  $1.96 * SE$ . Predictions and SEs are from, [Table A15](#), Model (3).

to the Arab/Muslim cues as the Republican share of the two-party vote in the 2016 contest increases. Moving from a 41 to a 66 percent Republican vote share (the median among counties carried, respectively, by Democrats and Republicans) adds nearly 7 percentage points to the penalty assessed to Arab/Muslim names. Neither the black nor the Latino cues show a similar conditional effect ([Table A15](#)).

As is the case for any conditional average treatment effect, these results could be driven by other factors associated with partisan leaning. Given that Republican support was particularly high in rural areas, one important confounding factor could be the density of Arabs/Muslims people living in a jurisdiction, which could cause our treatments to be less believable to respondents. For two reasons, we believe this threat is not driving our results. First, the overall effect of the Arab/Muslim identity treatment is not conditional on the proportion Arab/Muslim population ([Table A13](#) and [Table A14](#)). Second, insofar as officials are using mental “maps” to determine the likelihood of receiving correspondence from a particular sender, such maps likely overestimate the proportion of ethnic minorities ([Wong et al., Forthcoming](#)).

If the reported effect of political partisanship is not an artifact of confounding, what does it reflect? As our results in the previous section demonstrate, election officials’ behavior could reflect bias rooted in officials’ explicit views of the world, or it could occur without officials’ conscious awareness. This ambiguity applies to the role of politics as well. Officials may have heard and absorbed the rhetoric of the presidential candidate, and therefore responded less to Arab/Muslim senders because they believed these individuals less likely to qualify (or perhaps deserve) to vote. Or officials may have previously held explicit or implicit views that cor-

respond with partisan preferences about whose access to the electoral process is the highest priority. In a more narrow sense, we are also unable to disentangle the two facets of our key identity treatment. Arab is both a national origin and ethnic category. Muslim is a religious identity. Names that imply both of these identities necessarily fail to separate the elements that could separately lead to bias. These nuanced questions are all potential grounds for future research that could be conducted outside the confines of an audit study.

### **Awareness of experiment**

We now consider the timing and perceived authenticity of our experimental stimulus. By sending emails on October 31, our intervention allowed 8 days for officials to respond to voter questions before the election. While this window is shorter than the 10-business-day window frequently used in audit studies (e.g. [Butler, 2014](#)), [Figure 3](#) shows that the reduced time window is not driving our reported effects. The preponderance of local election official responses were received in the first two days, and this clumping of responses close to the time of reception holds for all experimental conditions.

During the analysis phase of this project, it came to the researchers' attention that at least one other entity was pursuing a similar line of research, and that a limited number of public officials became concerned that audit studies might be underway. News reports claim that these concerns prompted the National Association of Secretaries of State (NASS) to alert its state branches, who in turn had the opportunity to alert individual officials. In sum, some of our experimental subjects may have become aware of the intervention. Such awareness could threaten our results, either by compromising independence between units, or by violating the exclusion restriction if minority names are more likely to raise suspicion than white names. Tests suggest that these threats are not leading to bias in our results. First, as we present in [Figure 3](#) (b) and (c), the systematic pattern of unresponsiveness to minority names appears rapidly and well before the reported NASS broadcast. Second, as we report in [Table A11](#), models that exclude states that witnessed interference between units, and models that censor response data at the time of the NASS broadcast both produce estimates very similar to our main results.

### **Conclusion**

Previous experimental evidence showed local election officials were less responsive to inquiries from Latinos, raising concerns about bias in the electoral process. Using a similar experimental design, we demonstrate the firm basis for these con-

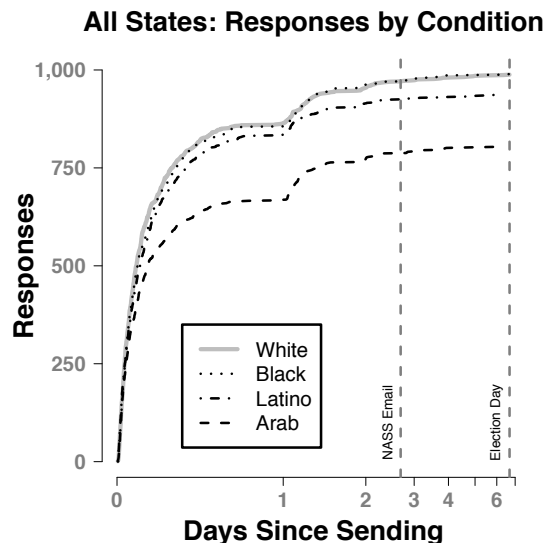


Figure 3: Rapidly slowing rates of response. The vertical axis plots the cumulative number of responses, split by group identity of sender; the horizontal axis plots time since sending. Election Day and NASS emails are noted with vertical dashed lines. Responses follow a clear diurnal rhythm, and patterns of bias appear rapidly.

cerns by replicating the initial finding. We also extend the results by testing for bias against other groups. Our results point to a number of open questions.

We encountered no evidence of bias from local election officials toward African Americans, making ours at least the third recent study to produce a similarly unexpected null finding (Einstein and Glick, 2017; Gell-Redman et al., 2018). These null findings do not imply the absence of discrimination toward blacks in any sphere of politics. Instead they reflect the behavior of public officials in the specific setting of electronic communication, which carries a distinct set of assumptions and heuristics. More pointedly, the null findings could be an artifact of the correspondence study method, in which name alone is used to signal group identity. The fact that multiple correspondence studies have failed to find discrimination may reinforce previous findings that images of African American faces are particularly powerful cues for eliciting prejudice (Eberhardt et al., 2004). In other words, additional cues (e.g., appearance) may be required to activate bias toward blacks, but not toward other groups.

Our intervention showed Arab/Muslim Americans to be markedly disadvantaged in their interactions with local election officials. This finding is particularly

salient given that it is not simply an artifact of Arab/Muslims being a relatively less numerous part of the electorate. Instead, we find that bias toward Arabs/Muslims is associated with support for the Republican candidate in the 2016 presidential election. While non-experimental, this result reflects an important prior finding of the literature on bureaucratic politics. In spite of the features of institutional design intended to protect against political influence (Miller and Whitford, 2016), bureaucrats may be influenced by their cultural and political surroundings (Berkman and Plutzer, 2010). As to what might be done to prevent bias in future interactions, our results suggest there is a role for interventions designed to limit the effects of implicit bias (Lau et al., 2015).

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# Appendix for Persistent Bias Among Local Election Officials

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# 1 Email Scraping

We collected email and personal contact information from local election officials by programmatically visiting state-maintained sites of local election official contact information.

We do not include the following states' local election officials in our assignment to treatment: Alaska, Hawaii, Maine, Maryland, Missouri, and New Jersey. We exclude Alaska because local election official jurisdictions were not mappable onto census area delineations for covariate data. We exclude Hawaii because a single board member represented each island, and the state did not provide individual email addresses for each island; rather, there was a single catch-all address. We do not include Maine, Missouri, or New Jersey because these states do not make email addresses of local election officials available. We do not include Maryland due to a clerical oversight.

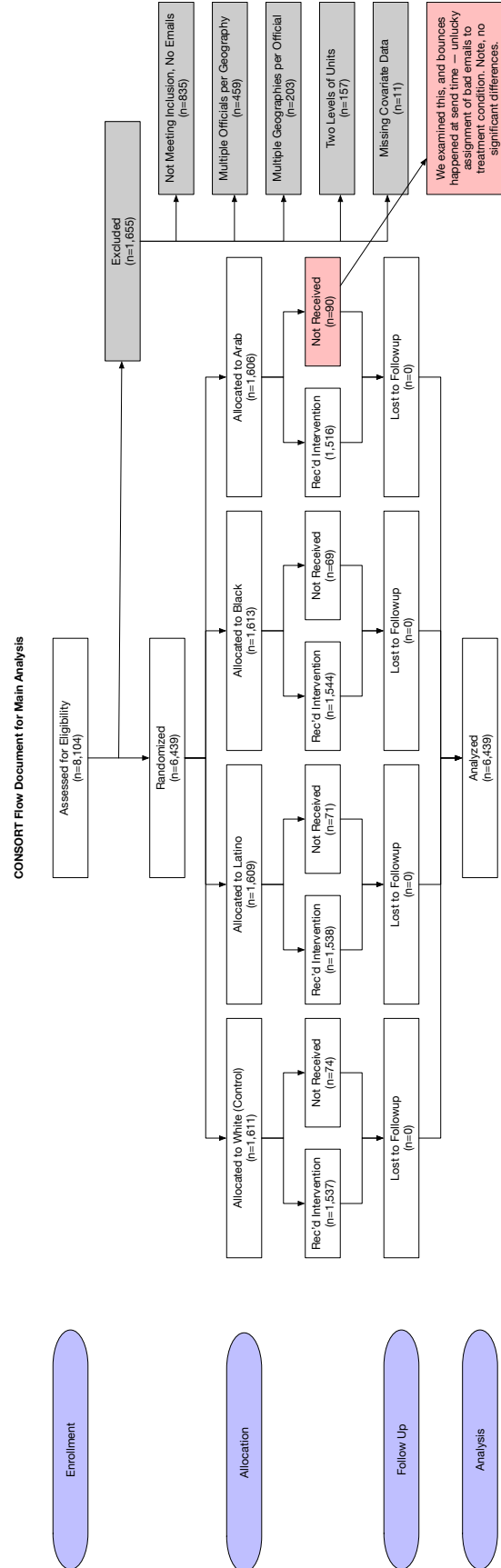
We report other individual officials that were excluded from randomization, as well as reasons for these exclusions in [Table A1](#). Local election officials were excluded from the study for concerns related to spillover, or multiple local election officials overseeing a single jurisdiction. All determinations were made prior to randomization. [Figure A1](#) reports the Consort enrollment and randomization chart for this project.

Table A1: Local Election Officials excluded prior to randomization

Attrition by Study Exclusion Criteria

| Exclusion Criteria Category   | Exclusion Criteria Details  | Number of deleted registrars or units of treatment (n) | Number of subjects remaining in cohort after exclusion (N) |
|---|---|--|--|
| Initial Count   | Registrars from whom we collected public information  |  | 8104   |
| Two levels of units per state   | <b>County and municipality</b>  |  |  |
|   | Delete registrars at county level - Wisconsin   | (72)   | 8032   |
|   | Delete registrars at county level - Michigan  | (83)   | 7949   |
|   | <b>State and county</b>   |  |  |
|   | Delete registrars at state level - Delaware   | (2)  | 7947   |
| Missing emails  | Delete registrars at county level with no email address - California, Idaho, Indiana, Maine, Missouri, Mississippi, New York, Pennsylvania              | (652)  | 7295   |
|   | Delete registrars at municipality level with no email address - Connecticut, Michigan, New Hampshire, Rhode Island, Wisconsin                           | (183)  | 7112   |
| Multiple registrars per unit of treatment   | Randomly select one registrar per county and delete remaining duplicates:   |  |  |
|   | Alabama   | (3)  | 7109   |
|   | Arkansas  | (19)   | 7090   |
|   | Connecticut   | (79)   | 7011   |
|   | Louisiana   | (15)   | 6996   |
|   | New Hampshire   | (4)  | 6992   |
|   | Keep registrar with name and delete registrar with no name - Nevada   | (2)  | 6990   |
|   | Keep registrar with job title "County Director" and delete registrar with job title "Deputy County" - Delaware  | (6)  | 6984   |
|   | Keep registrar with job title "City Clerks" and delete registrars with job title "Town Clerks" - Michigan   | (68)   | 6916   |
|   | For registrars with no job title, randomly select one and delete remaining duplicates - Michigan  | (33)   | 6883   |
|   | Randomly select registrar based on ranking of job title (1- "city clerk", 2- "town clerk", 3- "village clerk"), delete remaining duplicates - Wisconsin | (230)  | 6653   |
| Spillover - Registrars responsible of multiple units of treatment or registrars sharing email address | Randomly select one county, delete remaining counties for each registrar:   |  |  |
|   | Georgia   | (155)  | 6498   |
|   | Hawaii  | (3)  | 6495   |
|   | Michigan  | (31)   | 6464   |
|   | New York  | (4)  | 6460   |
|   | South Dakota  | (2)  | 6458   |
|   | West Virginia   | (1)  | 6457   |
|   | Wisconsin   | (7)  | 6450   |
| Missing data  | Unable to assign to treatment due to missing covariate data   | (11)   | 6439   |
| Total   |   | (1665)   | 6,439  |

Figure A1: CONSORT Document



## 2 Mailer Content

Unlike [White et al. \[2015\]](#), we did not vary whether the local election official receives a request directly related to voter identification. Because previous results establish that prejudicial behavior occurred almost exclusively in response to emails related to voter identification, we focus only on requests of that type.

To minimize the chance that local elections officials would become aware of the study, we took care to develop many versions of email language. In particular, all content that we mailed was a variant of a simple, three sentence paragraph that took the form: (1) Preamble; (2) Question One; (3) Question two.

By asking the same question in multiple ways, we achieve greater certainty that the resulting behavior is a response to the main causal variable of interest, the race of the putative voter, rather than any idiosyncratic feature of our request. [Table A2](#) presents the different values for the preamble and the two questions. These elements were combined at random, to produce 27 variants of the message text delivered to local officials. These variants were scored by 171 humans for “*clarity*”, “*warmth*” and “*appropriateness*”. Data resulting from these evaluations suggest that the language variants would not be evaluated differently by readers.

As an example, one particular realization of our stimulus might draw the first cue each section, forming the email:

Dear <John Adams>,

I have been hearing quite a bit about identification rules on the news. Do the changes affect <California>? I was wondering what I need to bring with me to vote?

Thank you,

<Daniel Nash>

| Cue Type   | Cue Text  |
|------------|---|
| Preamble   | I have been hearing quite a bit about identification rules on the news. |
| Preamble   | I have heard a lot on the news about identification.                    |
| Preamble   | The news has talked a lot about identification rules.                   |
| Question 1 | Do the changes affect <b>state</b> ?                                    |
| Question 1 | Are these changes happening in <b>state</b> ?                           |
| Question 1 | Do these affect <b>state</b> ?  |
| Question 2 | I was wondering what I need to bring with me to vote?                   |
| Question 2 | I was wondering if I need to bring anything specific with me to vote?   |
| Question 2 | Is there anything specific I need to bring to vote?                     |

Table A2: Features manipulated for random assignment of messages to registrars of voters.

### 3 No Question Effects

In the following models, we report that the causal effects are invariant to including question fixed effects.

Table A3

|  | <i>Dependent variable:</i> |                      |
|--|----------------------------|----------------------|
|  | GotResponse                |                      |
|  | (1)                        | (2)                  |
| Minority                                 | −0.047***<br>(0.014)       |                      |
| Latino                                   |                            | −0.030*<br>(0.017)   |
| Black                                    |                            | −0.0001<br>(0.017)   |
| Arab                                     |                            | −0.111***<br>(0.017) |
| Question Fixed Effect                    | Yes                        | Yes                  |
| Observations                             | 6,439                      | 6,439                |
| R <sup>2</sup>                           | 0.006                      | 0.013                |
| Adjusted R <sup>2</sup>                  | 0.002                      | 0.009                |
| Residual Std. Error                      | 0.493 (df = 6411)          | 0.492 (df = 6409)    |
| <i>Note:</i> *p<0.1; **p<0.05; ***p<0.01 |                            |                      |

## 4 Name Selection

In this appendix, we describe our approach to selecting the names of constituents. Our intent in choosing names from population lists was to eliminate the possibility of any name-based confounds to be responsible for differences in the behavior of local elections officials.

The most common practice in this literature selects a small number of names – frequently one or two for each racial/ethnic group – to send to recipients. For example, [Butler and Broockman \[2011\]](#) select the names Jake Muller and DeShawn Jackson to signal race/ethnicity of the sender; [White et al. \[2015\]](#) select the names Jake Muller and Greg Walsh to signal non-Latino white senders and José Martinez and Luis Rodriguez to signal Latino senders. We have two primary concerns with this choice of names. In a very narrow sense, the reuse of the name Jake Muller between studies may lead to a generalized awareness of the name as an experimental prime for an audit study. More generally, this is an example of a name-based confound that might lead subjects’ responses to be caused by a particular facet of the name rather than general traits about the racial/ethnic group. Selecting a large number of names greatly ameliorates this concern. Importantly, though, it is not simply the increase in the number of names that eliminates confounds; instead, it is the broad basis of names that removes the confound.

In line with previous work on election official responsiveness, we exclusively use male names [[White et al., 2015](#)]. Using names from a single gender reduces the variance in output that is not associated with race or ethnicity signals, increasing the efficiency of the experimental design. This stands in contrast to work that uses both male and female names [[Einstein and Glick, 2017](#)]. Whereas [Einstein and Glick](#) believe the possibility of gender-race discrimination interactions are important for tests of their theory, there is no such theoretical motivation in our work, and so we simplify the design to improve efficiency.

We draw white first names from the social security administration’s records of births in Oregon in 1990. We utilize a list of distinctly African American names to produce our Black first names [[Fryer and Levitt, 2004](#)]. Latino names are sourced from New York City baby names for children born between 2011 and 2014. Finally Arab/Muslim names were sourced from a list of common names (<http://www.behindthename.com/names/usage/arabic/>). Our intent with using this varied set of name sources was twofold. First and foremost, we wanted to generate plausible first names as an experimental stimulus. Second, we took care to ensure that the list of names we utilized was unlikely to match other name lists used in name-based audit studies.

We generate non-Hispanic White, Black, and Latino surnames from a US Census list of the 1000 most commonly occurring surnames [[Word et al., 2008](#)]. This dataset provides information about the distribution of racial and ethnic groups by each surname. For example, among individuals with the most commonly occurring surname, Smith, the census data identifies that 73% identify as non-Hispanic White, 22% identify as Black, and 1.5% identify as Hispanic. To select names, we set minimum levels within each category. For a surname to be chosen as a white surname, more than 70% with that name needed claim a non-Hispanic White identity. For a surname to be chosen as a black surname, 30% or more of people with that surname needed claim a black identity; for Latino surnames



we set this threshold at 60%. We note that this choice was made to produce what were, in our estimation, names that strongly signaled racial/ethnic group, without utilizing the *most* common surnames associated with these groups.

Arab/Muslim names, and indeed demographic and health statistics are difficult to identify [Al-Sayed et al., 2010]. Consequently, we sourced surnames from <http://surnames.behindthename.com/names/usage/arabic>. This site does not provide frequency counts for names, so we assigned a uniform probability to each name being assigned.

With the set of first and last names created, we join the names together to produce a *given name* and *surname* pair that signals senders’ racial/ethnic identities.

After constructing and curating a list of names to be sent as racial and ethnic primes, we recruited a set of workers through Amazon’s *Mechanical Turk* (mTurk) worker platform. We paid mTurk workers a small amount to guess the probability that a particular name was of one or another ethnic group. Specifically, for each of 25 randomly selected names (from the set of  $\approx 400$ ) we asked workers to estimate their confidence (ranging from 0 percent to 100 percent) that an individual with a given name belonged to a particular racial or ethnic group.

As an example – the example we used in training workers for the mTurk task – we provided the name Yao Ming, a famous Chinese basketball player who played in the American NBA for 8 seasons. If a subject were certain that the name Yao Ming was a member of the Asian racial or ethnic group, the worker would place a certainty of 100 with this group. If the worker were mostly certain – for example 90 percent certain – that the name Yao Ming belonged to the Asian racial or ethnic group, she would place a 90 with that group and the remaining 10 percent certainty with other group(s) she thought the name may belong.

The results of this task are reported in [section 15, Table A16](#).

## 5 Blocking

In this appendix, we describe our blocking strategy. One concern when conducting experiments is that we might be unlucky in our randomization. In order to account for this possibility, we created blocks of local election officials which are nested in districts with similar population characteristics. An additional benefit to blocking is that it enables higher-powered comparisons by reducing baseline differences in the potential outcomes to treatment and control. While valid causal inference is possible without blocking, a well-designed blocking scheme provides increased statistical power by comparing alike units.

We block on measures that are likely to predict whether a voting official will respond to (a) any form of contact and (b) forms of contact from minority voters. Specifically, we block on the population density of districts, proportion of the district that is below 150 percent of the federal poverty line, the proportion Black, the proportion Latino, President Obama’s margin of victory in the 2012 Presidential Election, and whether the district was previously covered by §5 of the VRA. We note that although there are very likely to be other factors that also influence whether a local election official responds to a query for information randomization cuts all ties with these factors.

Our blocking data was most commonly measured at the county level – e.g. county electoral returns. However, the relevant electoral area addressed by a local election official may, or may not also be a county. In some states local election officials execute elections across multiple counties; in other states local elections officials represent a single county; while in still others officials might work at the municipal level. When our blocking features were more geographically broad than the area covered by a local election official, we apply the county level values to the municipal level. When our blocking features were more narrowly measured than the political geography covered by an official, we simply average the county-level measurements. Specific details are available in the notebooks that accompany this work.

Blocking was implemented via the `blockTools` package written by Ryan Moore (Moore 2012.) Blocks of size four were created using an ‘optimalGreedy’ blocking algorithm. The algorithm begins by identifying the best pair of individual units to place in a single block, then identifies the best additional unit to include in that block, until the specified magnitude of the block is reached. It repeats the process until all units are blocked. We did not permit blocks from being formed between units in different states.

Experimental blocking is commonly understood as creating large groups such that units *within* groups are more similar than units *between* groups. This approach can be extended to the creation of more blocks that are of smaller size. Indeed, the limiting case is the matched pair design, in which each group contains only one treatment and one control unit.

In [Table A4](#) we report the results of our blocking strategy. In brief, blocking and subsequent randomization succeeded.

Table A4

|   | ethnic_cue | Mean Density | Mean Income | Mean Black | Mean Latino | Mean Obama | Mean VRA |
|---|------------|--------------|-------------|------------|-------------|------------|----------|
| 1 | White      | 1.860        | 0.044       | 0.043      | 0.055       | -0.063     | 0.120    |
|   |            | 0.019        | 0.001       | 0.003      | 0.003       | 0.007      | 0.008    |
| 2 | Latino     | 1.850        | 0.045       | 0.043      | 0.055       | -0.061     | 0.117    |
|   |            | 0.020        | 0.001       | 0.003      | 0.003       | 0.007      | 0.008    |
| 3 | Black      | 1.850        | 0.045       | 0.044      | 0.056       | -0.060     | 0.120    |
|   |            | 0.020        | 0.001       | 0.003      | 0.003       | 0.007      | 0.008    |
| 4 | Arab       | 1.840        | 0.045       | 0.043      | 0.054       | -0.065     | 0.118    |
|   |            | 0.020        | 0.001       | 0.003      | 0.003       | 0.007      | 0.008    |

*Notes.* Standard errors are reported beneath variable means

## 6 Nonparametric Results

This table produces the non-parametric, difference in means between the white, minority, latino, black and Arab name-cues. As we report in [Figure 1](#), minority, latino and Arab names receive responses at rates lower than white names. There is no detectable difference between the response rates of black and white names.

Table A5: Response Rates by Experimental Condition

| <b>Ethnic Cue</b> | <b>White</b> | <b>Minority</b> | <b>Latino</b> | <b>Black</b> | <b>Arab</b> |
|-------------------|--------------|-----------------|---------------|--------------|-------------|
| Response Rate (%) | 61.3         | 56.6            | 58.4          | 61.4         | 50.1        |
| Standard Error    | 1.21         | 0.71            | 1.23          | 1.21         | 1.25        |
| N                 | 1,611        | 4,828           | 1,609         | 1,613        | 1,606       |

*Notes.* The *Minority* column includes all data from the *Latino*, *Black*, and *Arab* columns. Response rates and standard errors are reported in percentage terms.

## 7 Fixed Effects Models

Table A6 presents linear probability models estimating the same causality quantites as reported in Figure 1 in the main body of the paper, though we provide more information in this Appendix. Models 1 and 2 estimate the causal effect of voter contact sent by non-white voters (model 1) and specific racial and ethnic classes of voters (model 2), but without including block-specific fixed effects. Models 3 and 4 estimate these same relationships, but include block fixed effects. Models 1 and 2 estimate robust (HC3) standard errors; models 3 and 4 estimate robust standard errors as constructed in the `lfe`, version `lfe_2.5-1998`.

We note that, while all models reported herein use *HC3* standard errors, we obtain substantively similar results when using Bell-McCaffery small-sample standard errors recommended by Lin and Green [2015].

In Model 1, we estimate that the local election officials respond to 61.3 percent of the emails they received from white voters. Emails received from racial and ethnic minority voters received a response at a rate 4.7 percent lower than this baseline: 56.6 percent of emails sent by minority names received a local election official response. Model 3, estimates the same relationship, but de-means the estimates within each block. The estimate of the causal relationship between sending an email as a minority voter rather than a white voter does not change substantively, although the blocking does improve the efficiency of the estimator.

In Models 2 and 4 we examine whether different racial and ethnic minority groups are treated differently by the local election officials. We find evidence to support this hypothesis. Models that do (Model 4) and do not (Model 2) include block fixed effects both find that emails from a Latino voter are 3.0 percent less likely to receive a response than emails sent from a white voter. In contrast, emails sent from Black voters are treated very similarly as emails sent from white voters. The estimate of the causal relationship is very nearly zero ( $\beta = 0.1$  percent), and is roughly  $1/30$  the magnitude of the latino effect. As such, this estimate does not provide evidence in support of the hypothesis that black voters are treated differently than white voters when they contact their local elections officials.

The lack of a causal effect for the Black cue stands in stark contrast to the difference in the response rate to Arab voters. In both Models 2 and 4 we estimate Americans with Arab names receive a response from elections officials at a rate 11.3 percent lower than the baseline response rate.

Table A6: Causal Estimates

|                | GotResponse          |                       |                      |                       |
|----------------|----------------------|-----------------------|----------------------|-----------------------|
|                | (1)                  | (2)                   | (3)                  | (4)                   |
| Minority       | −4.700***<br>(1.410) |                       | −4.710***<br>(1.330) |                       |
| Latino         |                      | −2.970*<br>(1.730)    |                      | −2.990*<br>(1.630)    |
| Black          |                      | 0.110<br>(1.720)      |                      | 0.167<br>(1.650)      |
| Arab           |                      | −11.300***<br>(1.740) |                      | −11.300***<br>(1.630) |
| Constant       | 61.300***<br>(1.210) | 61.300***<br>(1.210)  |                      |                       |
| Block FE       | No                   | No                    | Yes                  | Yes                   |
| Observations   | 6,439                | 6,439                 | 6,439                | 6,439                 |
| R <sup>2</sup> | 0.002                | 0.009                 | 0.330                | 0.337                 |

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## 8 Robust to Link Function

While OLS estimators are unbiased estimates of the causal effect under this research design, we demonstrate that the choice of link function in a general linear model does not meaningfully alter estimates. In [Table A7](#) and [Table A8](#), we use a maximum likelihood approach to estimating these models, first with a gaussian link function, but also with logit and probit functions.

Table A7: Robust to Logit and Probit Specification

|  | <i>Dependent variable:</i> |                      |                      |
|--|----------------------------|----------------------|----------------------|
|  | GotResponse                |                      |                      |
|  | <i>normal</i>              | <i>logistic</i>      | <i>probit</i>        |
|  | (1)                        | (2)                  | (3)                  |
| Minority                                 | −0.047***<br>(0.014)       | −0.194***<br>(0.059) | −0.121***<br>(0.037) |
| Intercept                                | 0.613***<br>(0.012)        | 0.461***<br>(0.051)  | 0.288***<br>(0.032)  |
| Observations                             | 6,439                      | 6,439                | 6,439                |
| Log Likelihood                           | −4,589.000                 | −4,379.000           | −4,379.000           |
| Akaike Inf. Crit.                        | 9,183.000                  | 8,762.000            | 8,762.000            |
| <i>Note:</i> *p<0.1; **p<0.05; ***p<0.01 |                            |                      |                      |

Table A8: Robust to Logit and Probit Specification

|  | <i>Dependent variable:</i> |                      |                      |
|--|----------------------------|----------------------|----------------------|
|  | GotResponse                |                      |                      |
|  | <i>normal</i>              | <i>logistic</i>      | <i>probit</i>        |
|  | (1)                        | (2)                  | (3)                  |
| Latino                                   | −0.030*<br>(0.017)         | −0.124*<br>(0.072)   | −0.077*<br>(0.045)   |
| Black                                    | 0.001<br>(0.017)           | 0.005<br>(0.072)     | 0.003<br>(0.045)     |
| Arab                                     | −0.113***<br>(0.017)       | −0.459***<br>(0.072) | −0.286***<br>(0.045) |
| Intercept                                | 0.613***<br>(0.012)        | 0.461***<br>(0.051)  | 0.288***<br>(0.032)  |
| Observations                             | 6,439                      | 6,439                | 6,439                |
| Log Likelihood                           | −4,567.000                 | −4,356.000           | −4,356.000           |
| Akaike Inf. Crit.                        | 9,141.000                  | 8,721.000            | 8,721.000            |
| <i>Note:</i> *p<0.1; **p<0.05; ***p<0.01 |                            |                      |                      |



## 9 Pilot Inclusion

We piloted our delivery and intake engineering in two separate pilots. The first, executed in Minnesota, was initially met with technical implementation issues – we received server information that no emails from our system were being delivered to local election official addresses. We addressed this issue, and, because our forensics determined that it would not be possible for officials to be aware of our first pilot, we re-ran this pilot and were successful on this follow-up attempt. To ensure that our engineering was not only a Minnesota-specific success, we ran a second pilot in the Western states of Washington, Oregon, California, and Nevada. We chose these states because of their relatively small local election official population (233 total local election officials), and their distance from other large local election official areas.

As we report in [Table A9](#) and [Table A10](#), neither including nor excluding these pilot states from the analysis changes the substance nor the interpretation of the core results. As well, there is no evidence that the causal effect is different in pilot or non-pilot states.

Table A9: Robust to Pilot Exclusion

|  | <i>Dependent variable:</i> |                      |                      |
|--|----------------------------|----------------------|----------------------|
|  | GotResponse                |                      |                      |
|  | (1)                        | (2)                  | (3)                  |
| Minority Cue                             | −0.047***<br>(0.014)       | −0.046***<br>(0.014) | −0.046***<br>(0.014) |
| Pilot                                    |                            |                      | 0.120*<br>(0.065)    |
| Minority Cue * Pilot                     |                            |                      | −0.034<br>(0.076)    |
| Constant                                 | 0.613***<br>(0.012)        | 0.609***<br>(0.013)  | 0.609***<br>(0.013)  |
| Include Pilot                            | Yes                        | No                   | Yes                  |
| Observations                             | 6,439                      | 6,206                | 6,439                |
| R <sup>2</sup>                           | 0.002                      | 0.002                | 0.003                |
| Adjusted R <sup>2</sup>                  | 0.002                      | 0.001                | 0.003                |
| <i>Note:</i> *p<0.1; **p<0.05; ***p<0.01 |                            |                      |                      |

Table A10: Robust to Pilot Exclusion

|                         | <i>Dependent variable:</i> |                      |                      |
|-------------------------|----------------------------|----------------------|----------------------|
|                         | GotResponse                |                      |                      |
|                         | (1)                        | (2)                  | (3)                  |
| Latino Cue              | −0.030*<br>(0.017)         | −0.030*<br>(0.018)   | −0.030*<br>(0.018)   |
| Black Cue               | 0.001<br>(0.017)           | 0.005<br>(0.018)     | 0.005<br>(0.018)     |
| Arab Cue                | −0.113***<br>(0.017)       | −0.112***<br>(0.018) | −0.112***<br>(0.018) |
| Pilot                   |                            |                      | 0.120*<br>(0.065)    |
| Latino Cue * Pilot      |                            |                      | 0.021<br>(0.093)     |
| Black Cue * Pilot       |                            |                      | −0.107<br>(0.092)    |
| Arab Cue * Pilot        |                            |                      | −0.013<br>(0.093)    |
| Constant                | 0.613***<br>(0.012)        | 0.609***<br>(0.012)  | 0.609***<br>(0.012)  |
| Include Pilot           | Yes                        | No                   | Yes                  |
| Observations            | 6,439                      | 6,206                | 6,439                |
| R <sup>2</sup>          | 0.009                      | 0.009                | 0.010                |
| Adjusted R <sup>2</sup> | 0.008                      | 0.009                | 0.009                |

Note: \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

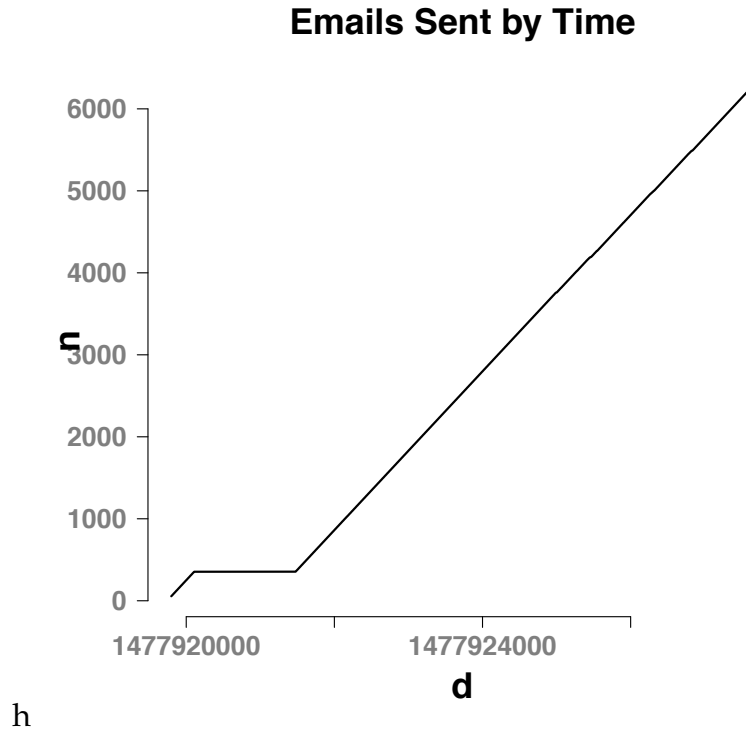


Figure A10: The number of emails sent is marked on the y-axis, and the time (in UNIX seconds, in the UNIX epoch) are plotted on the x-axis. Note the 30 minute gap in sending. Here, we waited to ensure that emails were making it to officials' inboxes, before green-lighting the remainder of the production email run.

## 10 Email Send Timing

In this appendix, we describe the timing of sending our emails. Emails were delivered in waves over a few hours to officials in the sample. We decided against emailing all local election officials at the same time to reduce the chance of unexpected results due to technical errors and to reduce possible spillover effects. We also considered emailing local election officials over a period of multiple days. Ultimately, we were concerned that the likelihood of differential response rates on different days outweighed the benefits to spreading email messages across several days. Note the 30 minute gap in sending. Here, we waited to ensure that emails were making it to officials' inboxes, before green-lighting the remainder of the production email run. We determined that our stimulus was making it to election officials inboxes when we received replies from officials in several states.

## 11 Time to Response

In this appendix, we consider how much time was required for local election officials to respond to our email. To do so, we merge tracker hits from our server with the time that we received an email reply. The tracker hit records when a registrar opened the email, and the response effectively records when the task is complete.

We take some care in computing this, because election-official-side email clients handle our tracker hits differently. In particular, some email clients “cache” a version of our image on their own servers to speed up the loading of images in emails. When this occurs, we do not receive reliable information about when an email was opened.

We work around this problem by including only the *first* load that occurs on our sever. Not only does this preclude problems with individuals’ email clients, but at the same time we believe it also represents a conservative (long) estimate of the time to complete the task.

As we plot in [Figure A10](#), the task that we set before election officials did not require a substantial amount of time. Of those responses that we received, and have valid data for, the median time to respond was fewer than three minutes. It is, however, important to note that we neither have information about the time to respond for officials who do not respond to our stimulus, nor for officials whose email clients prohibit us from gathering reliable data.

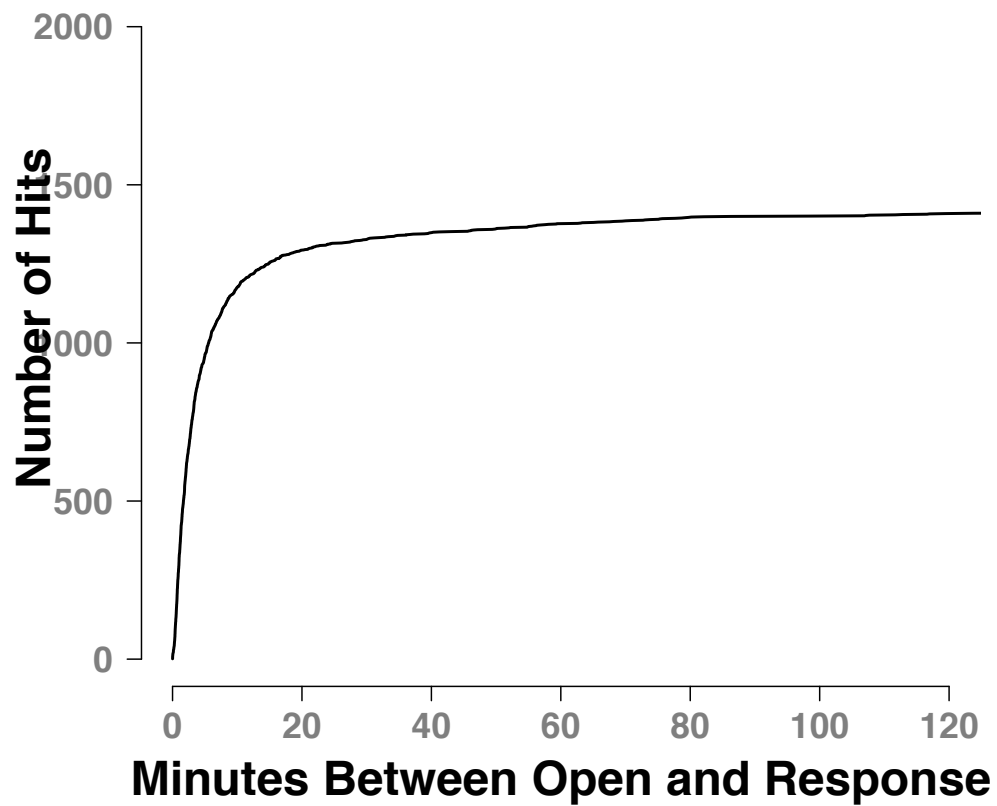


Figure A10: On the x-axis are the minutes elapsed since the first time the local election officials opened our stimulus, until the time that we received a response from that election official. On the y-axis are the cumulative number of responses that have been received in that duration of time.

## 12 No Damage from Spillover

After we collected outcome data, we learned that election officials in some states were suspicious about the emails, and contacted their state organization who, in turn, contacted the national organization. As well, we came to learn that at least one other research team was pursuing a substantively similar project, using the domain registered by [White et al. \[2015\]](#).

While we would have preferred that participants not realize that they were being studied, we do not think that their knowledge of the intervention undermines our inferences. This is because local election officials state that when they were unsure of an email’s legitimacy, they simply chose not to respond. While this would depress response rates, lowering overall responsiveness to our email prompt, it would not invalidate the *causal* estimates that we seek unless this decreased response rate were also shaped by the sender name.

To examine whether this notification seems to have affected the willingness of elections officials to respond, here we estimate a number of Cox proportional hazard (duration) models. We choose this model class because they are unbiased and efficient in the presence of censored data. In particular, this model type permits us to estimate models that use the pre-registered end date of observation, as well as the timing of the NASS clerk email as the end date of observation. As we report in [Table A11](#), the coefficients estimated in all models are highly stable.

Table A11: Cox Proportional Hazards Models

|                | (1)                | (2)                | (3)                | (4)                | (5)                | (6)                | (7)                | (8)                |
|----------------|--------------------|--------------------|--------------------|--------------------|--------------------|--------------------|--------------------|--------------------|
| Minority Cue   | -0.13***<br>(0.04) | -0.14***<br>(0.04) | -0.13***<br>(0.04) | -0.13***<br>(0.04) |                    |                    |                    |                    |
| Latino Cue     |                    |                    |                    |                    | -0.10*<br>(0.05)   | -0.10*<br>(0.05)   | -0.08*<br>(0.05)   | -0.07<br>(0.05)    |
| Black Cue      |                    |                    |                    |                    | -0.02<br>(0.05)    | -0.03<br>(0.05)    | -0.01<br>(0.04)    | -0.02<br>(0.04)    |
| Arab Cue       |                    |                    |                    |                    | -0.29***<br>(0.05) | -0.29***<br>(0.05) | -0.31***<br>(0.05) | -0.30***<br>(0.05) |
| Data Subset    | Clean              | Clean              | All                | All                | Clean              | Clean              | All                | All                |
| Censoring Date | Election           | Clerk              | Election           | Clerk              | Election           | Clerk              | Election           | Clerk              |
| Observations   | 4,548              | 4,548              | 6,435              | 6,435              | 4,548              | 4,548              | 6,435              | 6,435              |
| R <sup>2</sup> | 0.002              | 0.002              | 0.002              | 0.002              | 0.01               | 0.01               | 0.01               | 0.01               |

Notes. Cox proportional hazards models. Outcome is converting from no response to response. *Clean* data subset are states without known spillover, and exclude pilot data. *All* data subset includes all states' data. Two censoring points are estimated. *Election* is the pre-registered censoring date at election day; *Clerk* places the censoring date at the time of the NASS email notification. \* p<0.1; \*\* p<0.05; \*\*\* p<0.01

## 13 Limited District Characteristic Heterogeneity

In the following models, reported in [Table A12](#) and [Table A13](#) and [Table A14](#), we examine whether officials' response to treatment is different conditional on characteristics of their district. In particular, one hypothesis is that officials who preside over jurisdictions that hold a relatively large share of minority voters may be more likely to respond to a question about voting from minority voters. Indeed, as we show in [Table A12](#) and [Table A13](#), while there is little change in the responsiveness of election officials as the proportion of voters in that jurisdiction becomes increasingly black (shown in *Model (2)* and *Model (3)* in both [Table A12](#) and [Table A13](#)), as we report in *Model (1)* in [Table A12](#) and [Table A13](#), there is some evidence that officials' responsiveness changes as the proportion of Latinos in a jurisdiction increases.

The distribution of Arab Americans is somewhat distinct from the distribution of blacks and Latinos. Indeed, data from the current CPS suggests that just 8 percent of U.S. counties have no Latino population, and 25 percent have no black population. In contrast, fully half of the counties in the U.S. have no residents who identify with an Arab heritage. Thus, it is possible that the lack of variation in the `pct_arab` population variable has made it mechanically impossible for a regression to detect a heterogeneous treatment effect.

To examine whether this is possible, we rescale the percent of Arab population into a three-level factor variable in the following way:

- For geographies that have zero Arab population, we code the rescaled variable as 0;
- Among geographies that have at least one person who identified an Arab heritage, we make a split at the median of percent of Arab population.

This indicator splits the Arab population into three categories. The first category covers the 50 percent of U.S. counties with no Arab population. The second covers the 25 percent of U.S. counties whose Arab population is below the median value for those counties in which any Arabs live. The third category covers the remaining 25 percent of counties whose Arab population is above this median.

As we report in [Table A14](#) even after rescaling the data in this way, there is still no evidence that local election officials who work in districts with larger Arab populations treat requests from Arab names differently.



Table A12

|                           | <i>Dependent variable:</i> |                             |                      |
|---------------------------|----------------------------|-----------------------------|----------------------|
|                           | GotResponse                |                             |                      |
|                           | (1)                        | (2)                         | (3)                  |
| Minority                  | −0.052***<br>(0.015)       | −0.048***<br>(0.015)        | −0.044***<br>(0.015) |
| Percent Latino            | −0.241<br>(0.236)          |                             |                      |
| Percent Latino × Minority | 0.093<br>(0.143)           |                             |                      |
| Percent Black             |                            | −0.163<br>(0.230)           |                      |
| Percent Black × Minority  |                            | 0.013<br>(0.133)            |                      |
| Percent Arab              |                            |                             | 1.580<br>(2.440)     |
| Percent Arab × Minority   |                            |                             | −1.270<br>(2.530)    |
| Observations              | 6,439                      | 6,439                       | 6,406                |
| R <sup>2</sup>            | 0.330                      | 0.330                       | 0.329                |
| Adjusted R <sup>2</sup>   | 0.104                      | 0.103                       | 0.101                |
| <i>Note:</i>              |                            | *p<0.1; **p<0.05; ***p<0.01 |                      |

Table A13

|                         | <i>Dependent variable:</i> |                      |                      |
|-------------------------|----------------------------|----------------------|----------------------|
|                         | GotResponse                |                      |                      |
|                         | (1)                        | (2)                  | (3)                  |
| Latino                  | −0.049***<br>(0.019)       | −0.026<br>(0.018)    | −0.028<br>(0.018)    |
| Black                   | 0.013<br>(0.019)           | −0.003<br>(0.018)    | 0.003<br>(0.018)     |
| Arab                    | −0.121***<br>(0.019)       | −0.113***<br>(0.018) | −0.109***<br>(0.018) |
| Percent Latino          | −0.227<br>(0.233)          |                      |                      |
| Percent Latino × Latino | 0.345**<br>(0.167)         |                      |                      |
| Percent Latino × Black  | −0.199<br>(0.174)          |                      |                      |
| Percent Latino × Arab   | 0.138<br>(0.168)           |                      |                      |
| Percent Black           |                            | −0.173<br>(0.234)    |                      |
| Percent Black × Latino  |                            | −0.098<br>(0.162)    |                      |
| Percent Black × Black   |                            | 0.119<br>(0.166)     |                      |
| Percent Black × Arab    |                            | 0.008<br>(0.156)     |                      |
| Percent Arab            |                            |                      | 1.680<br>(2.460)     |
| Percent Arab × Latino   |                            |                      | −0.850<br>(2.780)    |
| Percent Arab × Black    |                            |                      | −0.657<br>(2.770)    |
| Percent Arab × Arab     |                            |                      | −1.740<br>(2.670)    |
| Block FE                | Yes                        | Yes                  | Yes                  |
| Observations            | 6,439                      | 6,439                | 6,406                |
| R <sup>2</sup>          | 0.339                      | 0.337                | 0.337                |

Note:

A25

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

Table A14

|                               | <i>Dependent variable:</i> |                      |
|-------------------------------|----------------------------|----------------------|
|                               | GotResponse                |                      |
|                               | (1)                        | (2)                  |
| Minority Cue                  | −0.040**<br>(0.021)        |                      |
| Latino Cue                    |                            | −0.020<br>(0.025)    |
| Black Cue                     |                            | 0.004<br>(0.026)     |
| Arab Cue                      |                            | −0.106***<br>(0.025) |
| 1-50pct Arab                  | 0.073**<br>(0.032)         | 0.073**<br>(0.032)   |
| 51-100pct Arab                | 0.082**<br>(0.034)         | 0.081**<br>(0.034)   |
| Minority Cue * 1-50pct Arab   | −0.026<br>(0.035)          |                      |
| Minority Cue * 51-100pct Arab | 0.005<br>(0.036)           |                      |
| Latino Cue * 1-50pct Arab     |                            | −0.024<br>(0.043)    |
| Black Cue * 1-50pct Arab      |                            | −0.004<br>(0.043)    |
| Arab Cue * 1-50pct Arab       |                            | −0.050<br>(0.043)    |
| Latino Cue * 51-100pct Arab   |                            | −0.008<br>(0.044)    |
| Black Cue * 51-100pct Arab    |                            | −0.0004<br>(0.044)   |
| Arab Cue * 51-100pct Arab     |                            | 0.026<br>(0.044)     |
| Block FE                      | Yes                        | Yes                  |
| Observations                  | 6,439                      | 6,439                |
| R <sup>2</sup>                | 0.332                      | 0.340                |
| Adjusted R <sup>2</sup>       | 0.107                      | 0.115                |

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

## 14 Political Heterogeneity

The models in [Table A15](#) present estimates for how the effect of our treatment differs conditional on political sentiment. There is no evidence that districts that voted at greater rates for the Republican candidate in the 2012 general election treated our experimental stimulus differently (see models (1) and (2)). However, in models (3) and (4) we report a persistent, non-experimental finding that districts that favored the Republican candidate in the 2016 election were significantly less likely to respond to inquiries from Arab names. Indeed, for every percent increase in Republican vote share, our models estimate that election officials in that district were 0.25 percent less likely to respond to requests from an Arab name. [Figure 2](#) in the main text plots this relationship.

Table A15

|                                  | <i>Dependent variable:</i> |                   |                     |                     |
|----------------------------------|----------------------------|-------------------|---------------------|---------------------|
|                                  | GotResponse                |                   |                     |                     |
|                                  | Romney Voteshare           |                   | Trump Voteshare     |                     |
|                                  | (1)                        | (2)               | (3)                 | (4)                 |
| Latino Cue                       | −0.084<br>(0.070)          | −0.066<br>(0.067) | −0.059<br>(0.070)   | −0.054<br>(0.068)   |
| Black Cue                        | 0.078<br>(0.070)           | 0.088<br>(0.067)  | 0.066<br>(0.070)    | 0.061<br>(0.067)    |
| Arab Cue                         | −0.018<br>(0.070)          | −0.011<br>(0.067) | 0.050<br>(0.070)    | 0.050<br>(0.068)    |
| 2012 R. Vote Share               | 0.215**<br>(0.091)         | −0.057<br>(0.173) |                     |                     |
| Latino Cue × 2012 R. Vote Share  | 0.102<br>(0.128)           | 0.068<br>(0.123)  |                     |                     |
| Black Cue × 2012 R. Vote Share   | −0.144<br>(0.128)          | −0.163<br>(0.123) |                     |                     |
| Arab Cue × 2012 R. Vote Share    | −0.178<br>(0.127)          | −0.191<br>(0.122) |                     |                     |
| 2016 R. Vote Share               |                            |                   | 0.058<br>(0.081)    | −0.306**<br>(0.136) |
| Latino Cue × 2016 R. Votes Share |                            |                   | 0.048<br>(0.114)    | 0.041<br>(0.110)    |
| Black Cue × 2016 R. Vote Share   |                            |                   | −0.109<br>(0.113)   | −0.099<br>(0.109)   |
| Arab Cue × 2016 R. Vote Share    |                            |                   | −0.271**<br>(0.114) | −0.269**<br>(0.110) |
| Intercept                        | 0.499***<br>(0.050)        |                   | 0.579***<br>(0.050) |                     |
| Block Fixed Effects              | No                         | Yes               | No                  | Yes                 |
| Observations                     | 6,439                      | 6,439             | 6,438               | 6,438               |
| R <sup>2</sup>                   | 0.012                      | 0.338             | 0.010               | 0.340               |

Note:

\*p&lt;0.1; \*\*p&lt;0.05; \*\*\*p&lt;0.01

## 15 Names and Assessment of Racial and Ethnic Group

Table A16: Name Score Table

| Name                 | Ethnic Cue | Mean White | Mean Latino | Mean Black | Mean Arab |
|----------------------|------------|------------|-------------|------------|-----------|
| Daniel Nash          | White      | 97.6       | 0.9         | 1          | 0         |
| Mathew Roberts       | White      | 95         | 0           | 3.7        | 0         |
| Alex Steele          | White      | 94.6       | 0.4         | 5          | 0         |
| Nicholas Austin      | White      | 94.6       | 0.4         | 4.6        | 0         |
| Zachary Fitzpatrick  | White      | 94.3       | 0.7         | 4.1        | 0         |
| Christopher Schmidt  | White      | 93.7       | 0.1         | 3.4        | 0.1       |
| Ryan Thompson        | White      | 93.1       | 0           | 6.2        | 0         |
| Timothy Bartlett     | White      | 93         | 0           | 6          | 0         |
| Corey Kennedy        | White      | 93         | 0           | 7          | 0         |
| Garrett Riddle       | White      | 92.9       | 0.4         | 6.6        | 0         |
| Austin Walsh         | White      | 92.4       | 0.3         | 5.8        | 0         |
| Christopher Rogers   | White      | 92.1       | 0           | 7.9        | 0         |
| Jacob Gates          | White      | 92         | 0           | 6.7        | 0         |
| Kyle Caldwell        | White      | 92         | 0           | 6          | 0         |
| Matthew Pratt        | White      | 91.4       | 0           | 8.6        | 0         |
| Joseph Mayer         | White      | 91.3       | 0           | 8.7        | 0         |
| Ian Thornton         | White      | 90.5       | 0           | 9.5        | 0         |
| Scott Sherman        | White      | 89.5       | 0.2         | 8.8        | 0         |
| Daniel Horn          | White      | 89.3       | 0           | 2.5        | 0         |
| Zachary Proctor      | White      | 89         | 0           | 7.5        | 0         |
| Brandon Hart         | White      | 88.8       | 0           | 11.2       | 0         |
| Nathan Brewer        | White      | 88.3       | 0           | 2.8        | 0         |
| Garrett Allen        | White      | 87.5       | 0.6         | 11.9       | 0         |
| John Miller          | White      | 87.3       | 0           | 10.9       | 0         |
| Robert Peterson      | White      | 87.2       | 0           | 11.7       | 0         |
| Dylan Garrett        | White      | 86.9       | 0           | 7.5        | 0         |
| Michael Quinn        | White      | 86.7       | 0           | 13.3       | 0         |
| Justin Kramer        | White      | 86.4       | 0           | 8.2        | 0         |
| Robert Todd          | White      | 86.1       | 0.4         | 12.1       | 0         |
| Travis Roberts       | White      | 85.7       | 0.7         | 10.7       | 0         |
| Richard Bowers       | White      | 85.7       | 1.3         | 6.7        | 0         |
| Jason Gillespie      | White      | 85.4       | 0.4         | 7.1        | 0         |
| Garrett Miller       | White      | 85.3       | 0           | 14.7       | 0         |
| Kyle Thompson        | White      | 84.4       | 0           | 15         | 0         |
| Dustin Lawson        | White      | 84.2       | 0           | 15.3       | 0         |
| Sean Cooper          | White      | 84.1       | 0           | 15.3       | 0         |
| James McPherson      | White      | 83.2       | 0           | 14.6       | 0         |
| Brandon Pierce       | White      | 83.2       | 0.5         | 14.7       | 0         |
| John Gregory         | White      | 83         | 2.9         | 10.2       | 0         |
| David Cochran        | White      | 82.9       | 0           | 17.1       | 0         |
| Seth Rodgers         | White      | 82.9       | 0.7         | 6.4        | 1.4       |
| Christopher Anderson | White      | 82.9       | 0.2         | 16.8       | 0         |
| Tyler Reeves         | White      | 82.5       | 0.4         | 12.9       | 0         |
| Justin McIntyre      | White      | 82.5       | 5.6         | 6.4        | 0         |
| Matthew Moore        | White      | 82.4       | 0.7         | 16.6       | 0.1       |
| Stephen Peterson     | White      | 81.9       | 0           | 16.2       | 0         |
| Kyle French          | White      | 81.8       | 0.9         | 13.6       | 0         |
| Timothy Middleton    | White      | 81.4       | 0           | 17.7       | 0         |

|                    |        |      |      |      |      |
|--------------------|--------|------|------|------|------|
| Ian Smith          | White  | 81.3 | 0    | 18.7 | 0    |
| Tyler Larson       | White  | 81.1 | 0    | 18.9 | 0    |
| Gregory Leblanc    | White  | 80.8 | 0.4  | 11.5 | 1.5  |
| Ryan Chapman       | White  | 80.7 | 0.2  | 16.8 | 0    |
| William Humphrey   | White  | 80.6 | 0    | 19.4 | 0    |
| Justin Mullins     | White  | 80.5 | 0    | 11.4 | 0    |
| Joshua Burke       | White  | 80.4 | 0    | 14.2 | 0    |
| Jacob Haas         | White  | 80   | 0    | 2.2  | 0    |
| Levi Wolfe         | White  | 80   | 0    | 0    | 0    |
| Kevin Patterson    | White  | 80   | 0    | 19.1 | 0    |
| Jeremy Short       | White  | 79.6 | 0    | 18.7 | 0    |
| Cody Lang          | White  | 79.4 | 0    | 3.1  | 0    |
| Taylor Long        | White  | 79   | 0    | 17.7 | 0    |
| Zachary Bailey     | White  | 78.8 | 0    | 12   | 0    |
| Michael White      | White  | 77.8 | 0    | 16.7 | 0    |
| Jeffrey Phillips   | White  | 77.1 | 0.4  | 21.7 | 0    |
| Travis Miller      | White  | 77.0 | 0    | 23.0 | 0    |
| Brian Bennett      | White  | 76.9 | 0    | 19.4 | 1.2  |
| Robert Cochran     | White  | 76.4 | 2.3  | 12.7 | 4.5  |
| Michael Hendrix    | White  | 76.2 | 0    | 17.9 | 0    |
| Travis Osborn      | White  | 75.4 | 0.8  | 7.1  | 0    |
| Michael Boyer      | White  | 75.3 | 0    | 15.3 | 1.3  |
| Travis Collins     | White  | 75   | 0    | 24.3 | 0    |
| Christopher Hebert | White  | 74.7 | 0.7  | 22.7 | 0    |
| Samuel Peters      | White  | 74.5 | 0    | 18.2 | 0    |
| Shane Page         | White  | 74.4 | 1.2  | 24.4 | 0    |
| Jeffrey Fox        | White  | 74.4 | 0.8  | 8.1  | 0    |
| Anthony Underwood  | White  | 73.8 | 0    | 23.8 | 0    |
| Justin Lyons       | White  | 73.5 | 6.7  | 18.0 | 0    |
| Michael Rose       | White  | 71.9 | 3.8  | 23.1 | 0    |
| Devin Foster       | White  | 71   | 0    | 27   | 0    |
| Joshua Clark       | White  | 70   | 0    | 5    | 0    |
| Jordan Rogers      | White  | 69.7 | 0    | 21.6 | 0    |
| Joseph Graves      | White  | 68.8 | 0    | 17.8 | 6.2  |
| Robert Reed        | White  | 68.2 | 1.7  | 10.2 | 16.7 |
| Tyler Murray       | White  | 67.3 | 2    | 24   | 1.3  |
| James Marsh        | White  | 66.9 | 1.2  | 13.8 | 0    |
| Travis Frye        | White  | 66.8 | 0    | 24.1 | 0    |
| Cameron Young      | White  | 65.6 | 0    | 23.7 | 0    |
| Stephen Sherman    | White  | 64.6 | 0    | 26.9 | 0    |
| Benjamin Wood      | White  | 64   | 0    | 14.5 | 0    |
| Eric Murray        | White  | 61   | 0    | 29   | 0    |
| Andrew Allen       | White  | 60.9 | 0    | 28.4 | 0    |
| Austin Hall        | White  | 59.5 | 0    | 24.1 | 1.8  |
| Samuel Wood        | White  | 55.8 | 0    | 44.2 | 0    |
| Marcus McFarland   | White  | 55.5 | 0    | 44.5 | 0    |
| Michael Lang       | White  | 55.5 | 2.7  | 12.3 | 0    |
| Samuel Hopkins     | White  | 51.2 | 0    | 34.6 | 1.7  |
| Brandon Estes      | White  | 50.8 | 36.6 | 11.6 | 0    |
| Sean Watts         | White  | 40.4 | 1.8  | 50.7 | 1.4  |
| Jordan Smith       | White  | 39.6 | 0    | 50.4 | 0    |
| Jose Hanson        | White  | 9.5  | 77.5 | 12.5 | 0    |
| Jose Cruz          | Latino | 0    | 100  | 0    | 0    |
| Jorge Castro       | Latino | 0    | 100  | 0    | 0    |

|                     |        |     |      |      |     |
|---------------------|--------|-----|------|------|-----|
| Cesar Marquez       | Latino | 0   | 100  | 0    | 0   |
| Jose Gutierrez      | Latino | 0   | 100  | 0    | 0   |
| Juan Campos         | Latino | 0   | 100  | 0    | 0   |
| Saul Gonzalez       | Latino | 0   | 100  | 0    | 0   |
| Miguel Salazar      | Latino | 0   | 100  | 0    | 0   |
| Jesus Perez         | Latino | 0   | 100  | 0    | 0   |
| Diego Velazquez     | Latino | 0   | 100  | 0    | 0   |
| Fernando Hernandez  | Latino | 0   | 100  | 0    | 0   |
| Juan Ramos          | Latino | 0   | 99.6 | 0    | 0   |
| Jose Valdez         | Latino | 0   | 99.6 | 0.4  | 0   |
| Edwin Vasquez       | Latino | 0.6 | 99.4 | 0    | 0   |
| Gerardo Escobar     | Latino | 0.8 | 99.2 | 0    | 0   |
| Esteban Herrera     | Latino | 0   | 99.2 | 0    | 0   |
| Jose Mendez         | Latino | 0   | 98.2 | 0.7  | 0   |
| Luis Gomez          | Latino | 1.1 | 97.9 | 0.5  | 0   |
| Fernando Acosta     | Latino | 1.1 | 97.8 | 0    | 0   |
| Adriel Hernandez    | Latino | 0.8 | 97.3 | 1.2  | 0   |
| Aldo Garcia         | Latino | 0   | 97.3 | 0    | 0   |
| Jaime Gonzalez      | Latino | 1.4 | 97.1 | 1.4  | 0   |
| Alejandro Rodriguez | Latino | 0   | 96.9 | 3.1  | 0   |
| Emilio Gonzalez     | Latino | 0.4 | 96.8 | 2.1  | 0   |
| Esteban Contreras   | Latino | 2.3 | 96.6 | 0    | 0   |
| Daniel Valdez       | Latino | 0   | 96.2 | 1.2  | 0   |
| Enrique Lopez       | Latino | 3.8 | 96.2 | 0    | 0   |
| Camilo Lopez        | Latino | 1.1 | 96.1 | 0    | 0   |
| Miguel Barrera      | Latino | 0.7 | 95.7 | 1.8  | 0   |
| Angel Ruiz          | Latino | 2   | 95.5 | 0.5  | 0   |
| Roberto Reyes       | Latino | 0   | 95   | 5    | 0   |
| Edwin Santiago      | Latino | 5.4 | 94.6 | 0    | 0   |
| Angel Navarro       | Latino | 0   | 94.4 | 5.6  | 0   |
| Ricardo Gomez       | Latino | 0.7 | 94.3 | 0.3  | 0   |
| Marvin Lopez        | Latino | 3.6 | 92.7 | 2.7  | 0   |
| Alejandro Ibarra    | Latino | 0.4 | 92.7 | 2.7  | 0   |
| Jesus Hernandez     | Latino | 1.3 | 92.3 | 1.7  | 1.3 |
| Emilio Cabrera      | Latino | 7.7 | 92.3 | 0    | 0   |
| Cristian Ramirez    | Latino | 1.2 | 92.2 | 0    | 0   |
| Jesus Martinez      | Latino | 2.1 | 92.1 | 1.4  | 1.4 |
| Julio Morales       | Latino | 0.4 | 92.1 | 0    | 7.1 |
| Adan Perez          | Latino | 2.5 | 91.5 | 0    | 0   |
| Angel Maldonado     | Latino | 3.8 | 91.2 | 0    | 0   |
| Darwin Gonzales     | Latino | 4.2 | 90.8 | 4.6  | 0   |
| Daniel Garcia       | Latino | 2.1 | 90.7 | 6.4  | 0   |
| Esteban Jimenez     | Latino | 0   | 90.4 | 1.9  | 0   |
| Alberto Mendoza     | Latino | 0.7 | 90   | 1.4  | 0   |
| Edgar Garcia        | Latino | 9   | 90   | 1    | 0   |
| Miguel Rubio        | Latino | 0   | 89.1 | 9.1  | 0   |
| Pablo Escobar       | Latino | 5.6 | 88.9 | 0    | 5.6 |
| Luis Martinez       | Latino | 0   | 88.9 | 11.1 | 0   |
| Carlos Villarreal   | Latino | 1.9 | 88.8 | 0.8  | 0   |
| Luis Gonzalez       | Latino | 3.3 | 88.3 | 0    | 0   |
| Jean Lopez          | Latino | 7.9 | 88.2 | 2.6  | 0   |
| Carlos Ramos        | Latino | 1.4 | 88.2 | 0    | 0   |
| Juan Perez          | Latino | 2.5 | 86.7 | 10.8 | 0   |
| Ricardo Garza       | Latino | 5.8 | 86.7 | 1.7  | 1.7 |



|                   |        |      |      |      |      |
|-------------------|--------|------|------|------|------|
| Manuel Padilla    | Latino | 0    | 86.4 | 0    | 4.3  |
| Miguel Rodriguez  | Latino | 1.8  | 86.4 | 0.9  | 0    |
| Angel Pineda      | Latino | 5    | 85   | 1.2  | 1.2  |
| Luis Moreno       | Latino | 2.5  | 84.6 | 0    | 0    |
| Iker Martinez     | Latino | 3.2  | 83.9 | 1.1  | 0.7  |
| Edgar Cardenas    | Latino | 8.7  | 83.7 | 1.7  | 0    |
| Edwin Hernandez   | Latino | 11.1 | 83.5 | 3    | 0.5  |
| Mario Chavez      | Latino | 3.6  | 82.1 | 1.4  | 1.4  |
| Johan Estrada     | Latino | 8.3  | 80.7 | 0.9  | 0.7  |
| Jefferson Sanchez | Latino | 9.3  | 80.7 | 9.3  | 0    |
| Johan Garcia      | Latino | 11.7 | 80.6 | 3.9  | 0    |
| Emiliano Lopez    | Latino | 1.7  | 80   | 1.7  | 1.7  |
| Erick Hernandez   | Latino | 13.8 | 79.4 | 5.3  | 0    |
| Giovani Herrera   | Latino | 14.2 | 79.2 | 0    | 1.7  |
| Luis Padilla      | Latino | 3.5  | 78.8 | 1.9  | 0    |
| Randy Munoz       | Latino | 14.5 | 78.8 | 0    | 0    |
| Jadiel Rodriguez  | Latino | 1.7  | 78.8 | 15.8 | 0.4  |
| Brayan Estrada    | Latino | 2.8  | 78.2 | 9.5  | 1    |
| Erik Rodriguez    | Latino | 7.7  | 78.2 | 0.5  | 0    |
| Erick Suarez      | Latino | 13.5 | 76.9 | 2.7  | 1.5  |
| Maximo Flores     | Latino | 9.7  | 76.1 | 3.2  | 0    |
| Yaniel Campos     | Latino | 1.2  | 74.4 | 5.9  | 1.2  |
| Miguel Trevino    | Latino | 0.9  | 72.6 | 5    | 0    |
| Yair Fuentes      | Latino | 0    | 69.5 | 4.1  | 18.2 |
| Matias Murillo    | Latino | 4.8  | 69   | 1    | 6    |
| Anderson Guerrero | Latino | 18.8 | 68.8 | 2.5  | 1.2  |
| Edwin Castaneda   | Latino | 21.1 | 68.2 | 0    | 0    |
| Kenny Rodriguez   | Latino | 27.1 | 67.4 | 0.9  | 1.2  |
| Damian Martinez   | Latino | 13.7 | 66.8 | 18.2 | 0    |
| Januel Aguilar    | Latino | 7.2  | 66.1 | 8.3  | 1.7  |
| Noel Torres       | Latino | 22.3 | 65.9 | 11.8 | 0    |
| Ismael Romero     | Latino | 5.8  | 60.4 | 4.2  | 24.6 |
| Derick Torres     | Latino | 21.8 | 59.5 | 13.2 | 1.8  |
| Julius Salazar    | Latino | 8.8  | 58.4 | 2.2  | 8.8  |
| Angel Ponce       | Latino | 14.2 | 52.8 | 19.2 | 1.1  |
| Thiago Zamora     | Latino | 2    | 52.5 | 6.5  | 6    |
| Junior Delgado    | Latino | 15   | 50.4 | 30   | 0    |
| Kenny Lozano      | Latino | 35.4 | 45.7 | 8.9  | 0    |
| Jael Calderon     | Latino | 13.3 | 44   | 29.3 | 0    |
| Darwin Guzman     | Latino | 26.0 | 42.4 | 17.4 | 0.7  |
| Edwin Zuniga      | Latino | 12.7 | 38.7 | 22.7 | 3.3  |
| Byron Salazar     | Latino | 34.2 | 31.5 | 24.6 | 6.9  |
| Jean Barrera      | Latino | 45   | 23   | 5    | 2    |
| Jefferson Ponce   | Latino | 55.9 | 0.5  | 28.2 | 0    |
| DeShawn Jackson   | Black  | 2.4  | 0    | 97.6 | 0    |
| Tyrone Brown      | Black  | 1.2  | 1.7  | 96.7 | 0    |
| DeShawn Harris    | Black  | 2.9  | 0.3  | 96.7 | 0    |
| DeShawn Brown     | Black  | 2.1  | 0    | 96.7 | 0    |
| Darius Thomas     | Black  | 2.5  | 0    | 96.2 | 1.2  |
| DeAndre Jackson   | Black  | 1.4  | 0.8  | 96.1 | 0    |
| Jamal Jones       | Black  | 1.8  | 0    | 95.4 | 0    |
| DeShawn Glover    | Black  | 4    | 1    | 95   | 0    |
| Tyrone Thomas     | Black  | 3.9  | 0.6  | 94.7 | 0    |
| Terrell Turner    | Black  | 4.4  | 0    | 94.4 | 0    |

|                      |       |      |     |      |      |
|----------------------|-------|------|-----|------|------|
| Darnell Jackson      | Black | 5.7  | 0   | 94.3 | 0    |
| Terrell Watkins      | Black | 5    | 0.8 | 93.1 | 0.4  |
| Trevon Williams      | Black | 7.1  | 0   | 92.9 | 0    |
| Darius Haynes        | Black | 6    | 0.7 | 92.7 | 0    |
| DeAndre Wilkins      | Black | 5.3  | 0.3 | 92.3 | 0    |
| Darnell Haynes       | Black | 7.5  | 1.1 | 91.4 | 0    |
| DeShawn Ware         | Black | 5.4  | 0   | 91.2 | 0    |
| DeAndre Scott        | Black | 5.8  | 0.4 | 91.2 | 0    |
| Trevon Johnson       | Black | 0.9  | 0   | 90.9 | 0    |
| Tyrone Jones         | Black | 9.2  | 0   | 90.8 | 0    |
| Jalen Washington     | Black | 6.9  | 0   | 90.8 | 0    |
| Darius Davis         | Black | 9.3  | 0   | 90.7 | 0    |
| Darnell Alexander    | Black | 8.3  | 0.5 | 90.4 | 0    |
| DeShawn Anthony      | Black | 3.5  | 0   | 90   | 0    |
| Demetrius Jackson    | Black | 10   | 0   | 90   | 0    |
| Darnell Davis        | Black | 11.8 | 0   | 88.2 | 0    |
| Terrell Davis        | Black | 10.9 | 0   | 88.2 | 0.9  |
| Jamal Coleman        | Black | 7.5  | 0.5 | 88   | 4    |
| Tyrone Johnson       | Black | 8.5  | 0   | 87.7 | 0    |
| Darius Washington    | Black | 11.8 | 0.6 | 87.6 | 0    |
| Marquis Harris       | Black | 6.5  | 5   | 87   | 0    |
| Malik Johnson        | Black | 5.5  | 0   | 86.4 | 6.4  |
| Maurice Brown        | Black | 13.8 | 0   | 86.2 | 0    |
| Tyrone Harris        | Black | 11.5 | 0.3 | 85.5 | 0    |
| DeShawn Johnson      | Black | 13.6 | 0   | 85   | 0    |
| DeAndre Davis        | Black | 12.7 | 1   | 85   | 0    |
| Terrell Ware         | Black | 6    | 1.8 | 84.5 | 1.8  |
| Andre Harris         | Black | 13.1 | 1.5 | 84.2 | 0    |
| Jamal Williams       | Black | 10.5 | 1.1 | 84.2 | 1.1  |
| Darnell Mitchell     | Black | 15.4 | 0   | 83.9 | 0    |
| Darnell Carter       | Black | 10.3 | 0   | 83.8 | 0    |
| Terrance Terrell     | Black | 13.5 | 1.2 | 83.5 | 0    |
| Terrell Scott        | Black | 12.5 | 0.2 | 83   | 0    |
| Terrance Johnson     | Black | 17.5 | 0   | 80.8 | 0    |
| Andre Johnson        | Black | 19.3 | 0.2 | 80.4 | 0    |
| Terrell Washington   | Black | 12.3 | 0   | 80.3 | 0    |
| Demetrius Johnson    | Black | 14.5 | 0.5 | 79.1 | 0    |
| Darryl Willis        | Black | 20   | 0   | 79   | 0    |
| Dominique Richardson | Black | 18.4 | 2.7 | 78.9 | 0    |
| Darius Miles         | Black | 20.5 | 0.5 | 78.6 | 0    |
| Darius Willis        | Black | 13   | 0   | 78.3 | 0    |
| Dominique Brown      | Black | 16.2 | 0   | 77.2 | 0    |
| Darius Bryant        | Black | 20   | 1.1 | 77.2 | 0    |
| Trevon Grant         | Black | 20   | 1.7 | 77.1 | 0    |
| Trevon Henry         | Black | 20.6 | 2.1 | 76.8 | 0    |
| Reginald Brown       | Black | 13   | 8.5 | 76.5 | 0    |
| Marquis Williams     | Black | 15   | 0.8 | 75.7 | 0    |
| Dominique Walker     | Black | 21.8 | 1.6 | 75.5 | 0    |
| Malik Hawkins        | Black | 15.9 | 0.3 | 75.3 | 8.3  |
| Tyrone Dorsey        | Black | 25   | 0   | 75   | 0    |
| Terrance Robinson    | Black | 16   | 0.2 | 73.8 | 0    |
| Darius Byrd          | Black | 20.4 | 0   | 73.5 | 0    |
| Malik Williams       | Black | 0.3  | 0.8 | 73.3 | 19.7 |
| Jalen Walker         | Black | 27.1 | 0   | 72.3 | 0    |

|                     |       |      |      |      |      |
|---------------------|-------|------|------|------|------|
| Trevon Scott        | Black | 25.8 | 0    | 71.7 | 0    |
| Maurice Miles       | Black | 25.2 | 0.5  | 71.5 | 0    |
| Malik Mitchell      | Black | 6.7  | 0    | 71   | 14   |
| Jamal Johnson       | Black | 6    | 0    | 71   | 3    |
| Xavier Brown        | Black | 16.2 | 6.9  | 70.3 | 0    |
| Dominique Jones     | Black | 22.7 | 4.5  | 70   | 0    |
| DeAndre Mathis      | Black | 16.3 | 3.7  | 69.7 | 0    |
| Maurice Davis       | Black | 29   | 0.6  | 69.4 | 0    |
| Terrell Thomas      | Black | 8.3  | 8.3  | 69.2 | 8.3  |
| Reginald Coleman    | Black | 33.3 | 0    | 66.7 | 0    |
| Jalen Neal          | Black | 20   | 0    | 65.8 | 0    |
| Jalen Harris        | Black | 17.8 | 2.8  | 65   | 0    |
| Maurice Thomas      | Black | 27   | 1.3  | 64.3 | 0    |
| Darryl Brooks       | Black | 28.9 | 7.1  | 62.1 | 0    |
| Reginald Davis      | Black | 39.2 | 0    | 60.8 | 0    |
| Malik Robinson      | Black | 14.4 | 0    | 60.6 | 18.9 |
| Marquis Mitchell    | Black | 17.7 | 3.1  | 60.4 | 0    |
| Terrance Woods      | Black | 39.3 | 0    | 60.4 | 0    |
| Jalen Johnson       | Black | 10   | 0    | 60   | 3.3  |
| Demetrius Fields    | Black | 23.5 | 2.4  | 60   | 0    |
| Dominique Simmons   | Black | 27.7 | 11.2 | 59.6 | 0    |
| Jalen Thomas        | Black | 26.8 | 4.5  | 59.5 | 0    |
| Darryl Watkins      | Black | 39.1 | 0    | 57.7 | 0    |
| Jalen Carter        | Black | 36   | 0    | 57.5 | 0    |
| Xavier Scott        | Black | 37.8 | 0.6  | 56.7 | 3.3  |
| Xavier Willis       | Black | 20.7 | 20   | 56.4 | 0    |
| Willie Davis        | Black | 40   | 1    | 56   | 0    |
| Malik Neal          | Black | 16.3 | 0    | 55.8 | 14.2 |
| Xavier Brooks       | Black | 28.1 | 0.8  | 55   | 0    |
| Dominique Alexander | Black | 30.6 | 12.1 | 55   | 0    |
| Willie Brown        | Black | 37.8 | 0.4  | 54.8 | 0.9  |
| Darryl Williams     | Black | 28   | 0    | 54.5 | 0    |
| Willie Jones        | Black | 39   | 2.5  | 54.5 | 0    |
| Willie Williams     | Black | 43.3 | 0    | 54.3 | 0    |
| Dominique Matthews  | Black | 34.7 | 8.8  | 53.5 | 0    |
| Andre Miles         | Black | 35.8 | 9.2  | 52.3 | 0    |
| Xavier Davis        | Black | 44   | 0.3  | 49   | 0    |
| Darryl Brown        | Black | 44.4 | 0.6  | 47.8 | 0    |
| Darryl Davis        | Black | 53.2 | 0    | 45   | 0    |
| Willie Singleton    | Black | 46.2 | 0    | 43.8 | 0    |
| Reginald Turner     | Black | 45   | 5.6  | 40.8 | 0    |
| Jalen Holmes        | Black | 33.6 | 0    | 40.5 | 0    |
| Darryl Walker       | Black | 57.3 | 0.7  | 40   | 0    |
| Willie Nixon        | Black | 71.4 | 0    | 13.6 | 0    |
| Basir Albaf         | Arab  | 0    | 0    | 0    | 99.2 |
| Botros Ahmed        | Arab  | 0    | 0    | 0    | 98.4 |
| Sami El-Amin        | Arab  | 0    | 0    | 1.7  | 97.8 |
| Salah Darzi         | Arab  | 0    | 0    | 2.2  | 97.8 |
| Abd El-Mofty        | Arab  | 0    | 0.5  | 0.9  | 97.7 |
| Sharif Abdullah     | Arab  | 0    | 0    | 2.9  | 97.1 |
| Shahnaz Hussain     | Arab  | 0    | 0    | 0    | 96.8 |
| Duha El-Amin        | Arab  | 0    | 0    | 1.5  | 95.8 |
| Shams El-Amin       | Arab  | 0.1  | 0.1  | 3.3  | 95.6 |
| Ibrahim El-Hashem   | Arab  | 0    | 0    | 1.8  | 95.5 |

|                     |      |      |     |      |      |
|---------------------|------|------|-----|------|------|
| Mahdi Albaf         | Arab | 0    | 0   | 1.8  | 94.7 |
| Bakr Abdullah       | Arab | 0    | 0   | 0    | 94.5 |
| Husain Sultan       | Arab | 0    | 0   | 0    | 94.4 |
| Sajjad Ahmed        | Arab | 0.6  | 0   | 1.2  | 94.1 |
| Fayiz Muhammad      | Arab | 0    | 0   | 1    | 94   |
| Ghassan Ahmed       | Arab | 6.2  | 0   | 0    | 93.8 |
| Ghayth Abdullah     | Arab | 0    | 0   | 4.7  | 93.6 |
| Ramadan Muhammad    | Arab | 0    | 0   | 4.4  | 93.3 |
| Maalik El-Ghazzawy  | Arab | 0    | 0   | 1.9  | 93.1 |
| Hafeez Saab         | Arab | 0    | 0   | 3    | 93   |
| Tarik El-Amin       | Arab | 0    | 0   | 5    | 93   |
| Abbas Abdullah      | Arab | 0    | 0   | 4.2  | 92.9 |
| Imad Zaman          | Arab | 0    | 0   | 1.4  | 92.9 |
| Mohammed Ahmed      | Arab | 0    | 0   | 3.8  | 92.5 |
| Jabr Hussain        | Arab | 5.9  | 0   | 1.8  | 92.4 |
| Hikmat Ahmad        | Arab | 1.2  | 0   | 0    | 92.2 |
| Bahadur Abdullah    | Arab | 0.7  | 0   | 0    | 92.1 |
| Al-Amir Bousaid     | Arab | 0    | 0   | 0.3  | 92.1 |
| Shadi Bousaid       | Arab | 0    | 0   | 0    | 91.7 |
| Jalal El-Amin       | Arab | 0    | 0   | 1.9  | 91.5 |
| Nasim Abdullah      | Arab | 0    | 0   | 2.6  | 90.9 |
| Salil Albaf         | Arab | 2.1  | 0   | 0.7  | 90.7 |
| Hakim Ajam          | Arab | 0    | 0   | 8.7  | 90.7 |
| Boulos Amjad        | Arab | 1.2  | 3.8 | 1.9  | 90.6 |
| Baqir Ali           | Arab | 3.3  | 0   | 0.8  | 89.2 |
| Mohammed Boulos     | Arab | 0    | 0   | 11.2 | 88.8 |
| Bahij Nejem         | Arab | 0    | 0   | 0.9  | 88.6 |
| Zahi El-Mofty       | Arab | 0    | 0   | 0.7  | 88.6 |
| Gafar Hakim         | Arab | 0    | 0   | 2.9  | 88.6 |
| Hussein Darzi       | Arab | 0.6  | 1.8 | 3.2  | 88.2 |
| Basir Muhammad      | Arab | 0    | 2.1 | 8.6  | 88.2 |
| Sa'Di Albaf         | Arab | 0    | 6.7 | 3.7  | 88   |
| Mukhtar Amjad       | Arab | 0.5  | 0   | 6.5  | 87.8 |
| Tahir El-Amin       | Arab | 0    | 4.6 | 2.4  | 87.6 |
| Yuhanna El-Amin     | Arab | 0    | 0   | 6.2  | 86.9 |
| Aamir Abujamal      | Arab | 0    | 0   | 0.8  | 86.7 |
| Husain El-Mofty     | Arab | 10.9 | 0   | 0.9  | 86.4 |
| Fadl Nejem          | Arab | 0    | 0   | 0    | 85.7 |
| Halim Zaman         | Arab | 0    | 0   | 2    | 85.5 |
| Imran Hakim         | Arab | 7.7  | 1.5 | 1.5  | 85.4 |
| Samir Abdurashid    | Arab | 0    | 0   | 1.1  | 84.6 |
| Ihsan El-Mofty      | Arab | 0    | 0   | 0    | 84.5 |
| Tarek Saqqaf        | Arab | 0.7  | 0   | 6    | 84   |
| Abdul-Aziz El-Mofty | Arab | 0    | 0   | 1.6  | 83.2 |
| Wadud Hakim         | Arab | 1.2  | 0   | 13.8 | 82.5 |
| Shukri Saqqaf       | Arab | 0    | 0   | 3.8  | 82.3 |
| Yaser Karimi        | Arab | 0    | 0   | 3.2  | 81.6 |
| Fakhri Ali          | Arab | 0.1  | 0   | 5.3  | 80.8 |
| Nabil Saab          | Arab | 0.6  | 0   | 7.8  | 80.6 |
| Ziauddin Muhammad   | Arab | 0    | 0   | 1.2  | 80   |
| Rayyan Albaf        | Arab | 0    | 0   | 5    | 79.3 |
| Rasul Ajam          | Arab | 0    | 0.3 | 1.5  | 78.8 |
| Nour El-Ghazzawy    | Arab | 1.5  | 0   | 3.1  | 78.5 |
| Rifat Alfarsi       | Arab | 0    | 0   | 6.7  | 78.3 |

|                    |      |     |      |      |      |
|--------------------|------|-----|------|------|------|
| Sajjad El-Amin     | Arab | 0   | 0    | 5    | 78.3 |
| Sa'Di El-Ghazzawy  | Arab | 0.7 | 0    | 8    | 77.3 |
| Fayiz Samara       | Arab | 1.5 | 0    | 2.3  | 76.2 |
| Aali Hussain       | Arab | 0   | 11.1 | 1.1  | 75   |
| Imran Mohammed     | Arab | 1.1 | 0    | 6.7  | 74.4 |
| Nizar Kader        | Arab | 0   | 0    | 2.8  | 73.9 |
| Jaffer Bousaid     | Arab | 6.9 | 0    | 1.2  | 73.8 |
| Jafar Sultan       | Arab | 0.3 | 0    | 17.6 | 73.2 |
| Shafiq Samara      | Arab | 0.9 | 0    | 16.8 | 73.2 |
| Fayiz Nejem        | Arab | 0   | 0.3  | 2.6  | 72.4 |
| Salim Kader        | Arab | 0   | 0    | 10.4 | 72.1 |
| Wafi Sultan        | Arab | 0   | 0    | 3.7  | 71.6 |
| Husni Zaman        | Arab | 0   | 0    | 18   | 71.3 |
| Adam Ahmad         | Arab | 7.4 | 5.2  | 7.4  | 71.0 |
| Khaled Samara      | Arab | 0   | 3.3  | 14.7 | 70   |
| Rasheed Zaman      | Arab | 2.7 | 0.7  | 22.7 | 70   |
| Fakhri El-Mofty    | Arab | 1.8 | 0.3  | 12.9 | 68.8 |
| Sameer Sultan      | Arab | 6.2 | 0    | 9.6  | 68.5 |
| Guda El-Mofty      | Arab | 0   | 11   | 7.5  | 66.5 |
| 'Abbas Nagi        | Arab | 0   | 0    | 15.5 | 65   |
| Adnan El-Mofty     | Arab | 0   | 0    | 8.3  | 64.2 |
| Zaki Karim         | Arab | 1.1 | 0    | 20.3 | 63.9 |
| Mis'Id El-Ghazzawy | Arab | 0   | 0    | 0    | 63.3 |
| Nurullah Nejem     | Arab | 0   | 1.1  | 10.8 | 61.9 |
| Latif El-Mofty     | Arab | 0.5 | 3.2  | 29.2 | 61.6 |
| Safi Boulos        | Arab | 0.4 | 7.7  | 0.4  | 61.5 |
| Tayeb Kader        | Arab | 3.8 | 0    | 21.8 | 59.8 |
| Waheed Bousaid     | Arab | 1.5 | 0    | 14.4 | 58.5 |
| Mansoor Amirmoez   | Arab | 0   | 21.2 | 5.6  | 58.1 |
| Dawud Karim        | Arab | 0   | 1.2  | 35.6 | 52.9 |
| Tal' At Tawfeek    | Arab | 7.1 | 0    | 20   | 46.4 |
| Murtaza Nagi       | Arab | 0.4 | 0.7  | 4.6  | 42.5 |
| Ayman Amirmoez     | Arab | 0   | 28.1 | 0    | 41.9 |
| Rusul Samara       | Arab | 1.8 | 5.9  | 14.5 | 41.4 |
| Rais Nagi          | Arab | 0   | 0.1  | 1.9  | 40   |
| Wafi Kader         | Arab | 2.5 | 0    | 23.8 | 33.8 |

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