Continuing Evidence of Discrimination Among Local Election Officials *

D. Alex Hughes†[1], Micah Gell-Redman[2], Charles Crabtree[3], Natarajan Krishnaswami[1], Diana Rodenberger[1] and Guillermo Monge[1]

[1]School of Information, University of California, Berkeley
[2]Department of International Affairs, University of Georgia
[3]Department of Political Science, University of Michigan

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Abstract

Results of an audit study conducted during the 2016 election cycle demonstrate that the bias toward Latinos observed during the 2012 election has persisted. In addition to replicating previous results, we show that Arabs face an even greater barrier to communicating with local election officials. Our study did not produce evidence of discrimination against blacks, a finding that echoes the results of other recent studies. We examine one explanation for the high level of discrimination against Arabs, that bureaucrats are politically responsive. Our findings support this explanation. In geographies that cast more votes for the Republican candidate, discrimination against Arab names was stronger.

*We are thankful for the many helpful comments and suggestions from colleagues and participants at the 2017 Center for Experimental Social Science conference, the MPSA Experimental Approaches conference, and the State Politics and Policy Conference. All errors are our own. This research was approved by two university IRB, and was pre-registered at EGAP. All data files necessary to replicate the analysis have been made ready in a public repository. We would be pleased to make these files available to the reviewers, if they should so like.

†Corresponding Author, `d.alex.hughes@ischool.berkeley.edu`

Racial identity shapes who votes (Hajnal and Lee, 2011), which candidates they choose (Abrajano and Alvarez, 2010; Hajnal and Abrajano, 2015), and how campaigns seek to mobilize them (García-Bedolla and Michelson, 2012). Racial identity also shapes how voters are treated by the street-level bureaucrats responsible for conducting elections (Lipsky, 1980; Kimball and Kropf, 2006).[1] In the 2012 Presidential election, local election officials were significantly less responsive to requests from putative voters with Latino, as opposed to white, surnames (White, Nathan and Faller, 2015).

In this paper, we provide evidence that individuals from other racial groups may also experience bias when interacting with local election officials. We replicate the finding of bias toward Latinos in the 2012 election, which demonstrates the uncharacteristic robustness of this experimental result (Mullinix et al., 2015). Moreover, we show that Arab Americans received responses at a rate roughly 11 percentage points lower than whites. Discrimination against Arab names demonstrates a political logic; it is concentrated in areas that supported the Republican candidate in the 2016 presidential election. Constituents with African American names received responses at a rate indistinguishable from that of white voters, a finding that aligns with a recent pattern in the experimental literature.

MOTIVATION AND APPROACH

Bureaucrats capable of exercising discretion may be influenced by the characteristics (e.g., race or partisanship) of individuals seeking public services (for a review, see White, Nathan and Faller, 2015, pp. 131-2). Our two primary motivations are to determine whether the previous finding of bias toward Latinos stands up to replication, and to examine whether this bias extends to other racial groups. As a group at the center of debates over voter ID laws, Latinos could be at particularly high risk of bias. Other racial

[1] Local election officials are the elected or appointed men and women "responsible for the nuts and bolts of elections" (Kimball and Kropf, 2006). Individual assignment to these positions varies from state to state.

groups may be similarly vulnerable, however. Here we focus on two groups, African Americans and Arab Americans.

There can be little doubt that blacks face discrimination in various arenas of American political life (Butler, 2014). Nevertheless, recent studies of housing bureaucrats (Einstein and Glick, 2017) and elected officials (Gell-Redman et al., 2017) do not find evidence of discrimination against blacks.[2] This surprising pattern of experimental results leads to uncertain expectations regarding local election officials' responsiveness to blacks.

While there is ample evidence of discrimination against Arab Americans (Gaddis and Ghoshal, 2015), the experience of this group has received comparatively little attention from political scientists.[3] The anti-muslim rhetoric prevalent during the 2016 presidential campaign demonstrates that Arab Americans are often viewed as "others" whose exclusion from mainstream society could lead to bias from public officials.

We test for the presence of bias toward all three of these groups by conducting 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 sample comprises all such officials with publicly available email addresses. In total, 6,439 local election officials from 44 states, or 94 percent of the local election official universe, received our intervention.[4]

Our experimental stimulus consists of a single email sent from a putative voter to each local election official. All emails follow the same structure, greeting the official by name, referencing voter identification requirements, and asking about the requirements to vote in the state corresponding to the official.[5] In order to minimize possible spillover issues, we create 27 variants of this request, described in Appendix B and Appendix C.

To examine the extent of bias among local election officials, we vary the race of the

[2]In one treatment arm of a study of private landlords, Guess, Fang and Humphreys (2017) also fail to detect discrimination against blacks at standard levels of statistical significance.

[3]Exceptions include Jamal and Naber (2007), and Panagopoulos (2006).

[4]Details of the our implementation appear in Appendix A.

[5]White, Nathan and Faller (2015) test for bias in both a generic email and an email related to voter identification. To maintain power while introducing additional racial and ethnic treatments, we focus exclusively on emails that mention voter ID laws.

email sender.[6] In line with convention we expose officials to four distinct racial identities by manipulating senders' names (Bertrand and Mullainathan, 2004; Bertrand and Duflo, 2016; Butler and Homola, 2017). To mitigate possible name effects, each racial condition is signaled by 100 unique names. Using many names avoids the possibility that any treatment effect could be driven by characteristics other than race associated with a particular name. Appendix D describes the procedure for choosing names, and Appendix O provides the complete list of names.

Treatment assignment was blocked on logged population density, Democratic 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, and a dummy variable indicating whether a county was previously covered by Section 5 of the Voting Rights Act.[7] Local election officials were assigned a randomly selected message version and a randomly selected name corresponding to the assigned racial condition. Combining the large number of names with treatment language variants permitted us to send 4,900 unique experimental conditions to election officials.

Emails were delivered to election officials on the morning of October 31, 2016.[8] We chose to contact election officials eight days before the election 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, we chose a request 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.[9]. The costs borne by officials must be bal-

[6]The identities signaled in our treatments have elements that could be described as racial or ethnic. Following Sen and Wasow (2016), we use race as shorthand for both race and ethnicity.

[7]Appendix E contains details about the blocking scheme, including descriptive statistics for the blocking covariates.

[8]We conducted two pilot tests using the same randomization and content. The first pilot was run in Minnesota – chosen because it was excluded from White, Nathan and Faller (2015) – and the second in the western states of Washington, Oregon, California, and Nevada. Final analyses use all states because there is no evidence for differential behavior. See Appendix I. Results are robust to exclusion of these pilot states. Additional details about delivery timing are provided in Appendix J.

[9]See Appendix K

anced against the benefits of learning that discrimination appears to be a persistent and widespread feature of electronic communications between constituents and local election officials.

Our outcome measure, GotResponse, is coded 1 if an election official replied to our email prior to election day, and 0 otherwise.[10] We do not count auto-responses as replies and record only the first email response received.

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 reported in White, Nathan and Faller (2015), this rate compares favorably with experiments on elected officials in the U.S. (e.g., 56.5 percent in Butler and Broockman (2011)), suggesting that our requests were taken at face value.

Election officials respond at considerably lower rates when queries come from minority as opposed to white senders (difference in mean ($\Delta\mu$) $= -4.70$ percent, *Wilcox Rank-Sum* $P < 2 \times 10^{-16}$). However, 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 ($\Delta\mu = -2.97$ percent, $P = 0.07$). Strikingly, an Arab name lowers the likelihood of a response by greater than 11 percentage points ($\Delta\mu = -11.3$ percent, $P < 1 \times 10^{-10}$). In contrast, Black senders receive responses at rates that are indistinguishable from white senders ($\Delta\mu = 0.11$ percent, $P = 0.90$).

Figure 1 (a) plots the causal effects of our treatment.[11] Regression estimates with robust standard errors are reported in columns 1 and 2 of Table 1, and produce similar

[10] Another dependent variable that might be examined is the tone (or some other attribute) of the email response. We focus here on the response rate to avoid the complications associated with computing valid treatment effects for tone.

[11] Table A5 presents these estimates in tabular format.

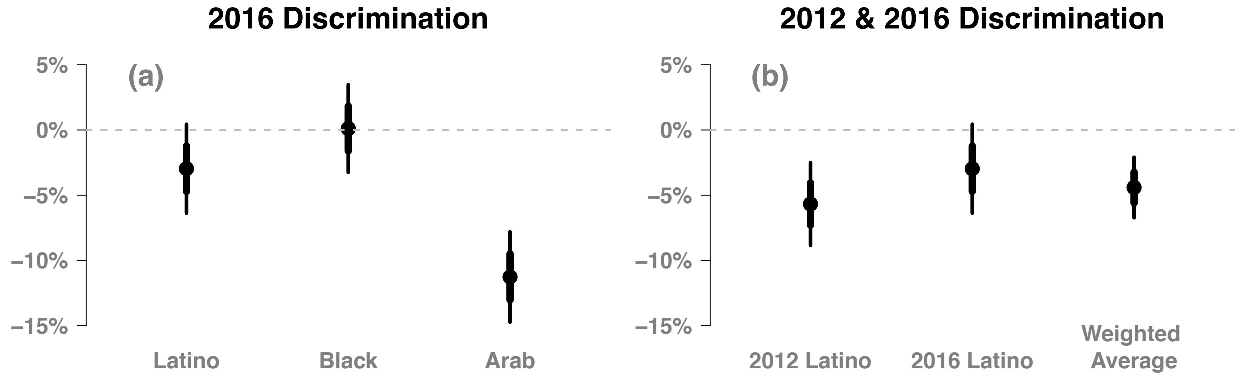

Figure 1: Points represent estimated difference in response rates between named category and white response rate (ITT). Thick bars report $ITT \pm SE$, thin bars report $ITT \pm 1.96 * SE$. Estimates are from difference in means estimator and precision-weighted difference in means.

results.[12] 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 of 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 (precision weighted $SE = 1.18$).

While the robustness of this experimental finding is remarkable, perhaps more striking is the finding that Arab 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 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 1, Model 3). Additional checks, reported in Table A14, similarly fail to identify a pattern consistent with a theory of contact or proximity.

Rather than a simple byproduct of Arab names being uncommon, it appears that the lower response rate to Arabs is linked with the contours of political partisanship in the

[12]Results are similar when estimated using maximum likelihood models with gaussian, logit, or probit link functions (Appendix H).

Table 1: Estimates of Discrimination

	GotResponse			
	(1)	(2)	(3)	(4)
Minority Cue	$-4.7(1.3)^{***}$			
Latino Cue		-3.0 (1.6)*	-2.8 (1.8)	-5.4 (6.8)
Black Cue		0.2 (1.7)	0.3 (1.8)	6.1 (6.8)
Arab Cue		-11.3 (1.6)***	-10.9 (1.8)***	5.0 (6.6)
Pct. Arab			1.7 (2.5)	
Pct. Arab × Arab Cue			-1.7 (2.7)	
R Vote Share				-0.31 (0.13)**
R Vote Share × Arab Cue				-0.27 (0.11)**
Observations	6,439	6,439	6,406	6,406
R^2	0.330	0.337	0.337	0.340

Notes: OLS regression estimates with White standard errors. Dependent variable is receiving a response from the election official. *Minority Cue* combines non-white cues; *Latino, Black, & Arab Cues* are signaled by name. All models include block fixed-effects. Coefficients are reported as percentages. *p<0.1; **p<0.05; ***p<0.01

2016 election. As column 4 of Table 1 indicates, election officials in localities with a higher Republican vote share were less responsive overall, and particularly less responsive to Arabs. This conditional effect is visualized in Figure 2, which shows the pronounced divergence of response rates to the white as opposed to the Arab cues as the Republican share of the two-party vote 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 names.

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.

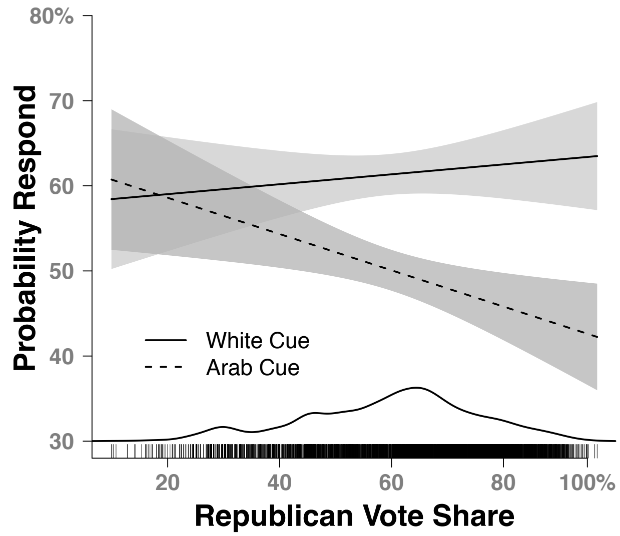

Figure 2: Predicted probability of response, reported for white vs. arab names. The rug and density lines show data coverage across the vote share range. Predictions are from Model 3 in Table A15 reported in the Appendix.

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.

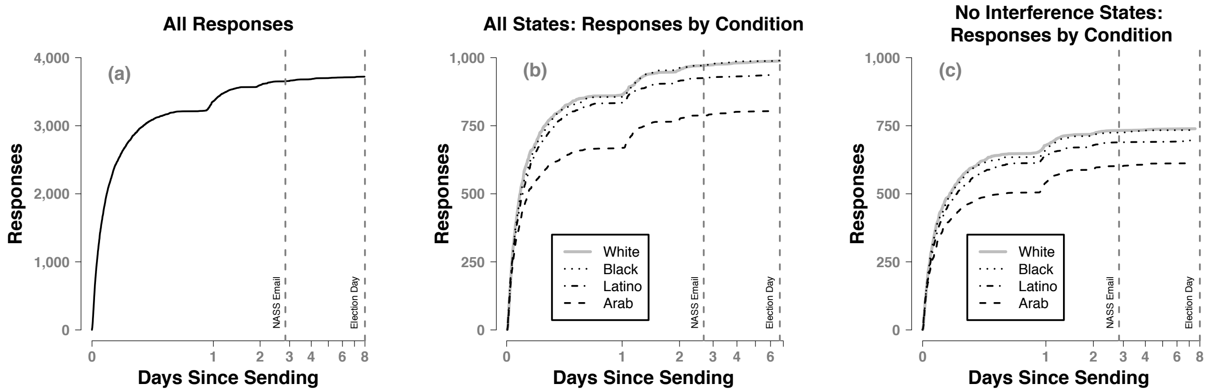

Figure 3: Rapidly slowing rates of response. In all plots, the x-axis is reported in logged time since sending. Election Day and NASS emails are noted with vertical dashed lines. Plot (a) shows all responses for all states; (b) shows all states' responses by experimental condition; (c) excludes states with documented interference and pilot states.

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 concerns 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., 2017). One possible explanation for these findings is that public officials are not biased toward blacks. Our preferred explanation, however, is based on the fact that all of these studies use correspondence methods, in which a name alone is used to signal race. The fact that multiple correspondence studies have failed to find discrimination may indicate that additional cues (e.g., appearance) are required to activate bias toward blacks, but not toward other groups.

Whatever its explanation, the lack of discrimination against blacks makes it all the more remarkable that our intervention showed Arab Americans to be so markedly disadvantaged in their interactions with local election officials. This finding is particularly salient given that it is not simply an artifact of Arabs being a relatively less numerous part of the electorate. Instead, we find that bias toward Arabs 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 – bureaucrats are influenced by their cultural and political surroundings (Berkman and Plutzer, 2010). In other words, when it comes to service provision in the electoral arena, both race and place matter.

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Online Appendix for
"Continuing Evidence of Discrimination Among Local Election Officials"

(Not for publication.)

CONTENTS

A Email Scraping **A2**

B Mailer Content **A4**

C No Question Effects **A6**

D Name Selection **A7**

E Blocking **A8**

F Nonparametric Results **A11**

G Fixed Effects Models **A12**

H Robust to Link Function **A14**

I Pilot Inclusion **A16**

J Email Send Timing **A18**

K Time to Response **A19**

L No Damage from Spillover **A21**

M Limited District Characteristic Heterogeneity **A23**

N Political Heterogenity **A27**

O Names and Assessment of Racial and Ethnic Group **A29**

A 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. After collecting this local election contact information we donated the codebase to a registered 501(c)(3) nonprofit organization whose mission is to increase voters' access to the polls.

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.

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	**County and municipality**		
	Delete registrars at county level - Wisconsin	(72)	8032
	Delete registrars at county level - Michigan	(83)	7949
	State and county		
	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
	Winsconsin	(7)	6450
Missing data	Unable to assign to treatment due to missing covariate data	(11)	6439
	Total	**(1665)**	**6,439**

B Mailer Content

Unlike White, Nathan and Faller (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 restulting 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 **state**?
Question 1	Are these changes happening in **state**?
Question 1	Do these affect **state**?
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.

C No Question Effects

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

Table A3

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

D Name Selection

In this appendix, we describe our approach to selecting the names of putative constituents. Our primary source for data on names came from the NYC census. We initially chose the NYC census list to avoid possibly capturing American regional patterns in naming conventions. In particular, our early motivation was to identify white names that did not strongly indicate heritage from a particular geographic region. However, upon piloting these names, we realized that the choice of NYC to mitigate white-regional naming conventions led to a broader pattern: the racial and ethnic minority names identified as the most prevalent in NYC are quite different than the prevalence of these names in broader America. As a result, we drew on a large number of sources to create these composed names.

In line with previous work on election official responsiveness, we exclusively use male names (White, Nathan and Faller, 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.

Because NYC seemed to have a particular set of non-hispanic/white names that were not general to the rest of the country – likely because of the high concentration of names of Jewish and Eastern European descent – we used data from the Social Security Administration to generate a list of popular white names.

As well, upon review we realized that the most common Black names in NYC may prime a racial or ethnic identity that is not African American. As a result, we utilize a list of distinct African American names Fryer 2004.

Arabic names (both for first and last names) were not available from the NYC name website. In fact, few data sources appear to contain this information. We used the names at `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.

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 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 Appendix O, Table A16.

E 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.

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 simliar than units *between* groups. This approach can be extend 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

F 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

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.

G Fixed Effects Models

Table A6 presents the same results as Table 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 here that, while all models reported herein use *HC3* standard errors, we obtain substantively similar results when using Bell-McCaffery small-sample standard errors 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^{***}		-4.710^{***}	
	(1.410)		(1.330)	
Latino		-2.970^{*}		-2.990^{*}
		(1.730)		(1.630)
Black		0.110		0.167
		(1.720)		(1.650)
Arab		-11.300^{***}		-11.300^{***}
		(1.740)		(1.630)
Constant	61.300^{***}	61.300^{***}		
	(1.210)	(1.210)		
Block FE	No	No	Yes	Yes
Observations	6,439	6,439	6,439	6,439
R^2	0.002	0.009	0.330	0.337
Note:		*p<0.1; **p<0.05; ***p<0.01		

H 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

	Dependent variable:		
	GotResponse		
	normal	*logistic*	*probit*
	(1)	(2)	(3)
Minority	-0.047^{***}	-0.194^{***}	-0.121^{***}
	(0.014)	(0.059)	(0.037)
Intercept	0.613^{***}	0.461^{***}	0.288^{***}
	(0.012)	(0.051)	(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
Note:			*p<0.1; **p<0.05; ***p<0.01

Table A8: Robust to Logit and Probit Specification

	Dependent variable:		
	GotResponse		
	normal	*logistic*	*probit*
	(1)	(2)	(3)
Latino	-0.030^{*}	-0.124^{*}	-0.077^{*}
	(0.017)	(0.072)	(0.045)
Black	0.001	0.005	0.003
	(0.017)	(0.072)	(0.045)
Arab	-0.113^{***}	-0.459^{***}	-0.286^{***}
	(0.017)	(0.072)	(0.045)
Intercept	0.613^{***}	0.461^{***}	0.288^{***}
	(0.012)	(0.051)	(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
Note:	*p<0.1; **p<0.05; ***p<0.01		

I 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.

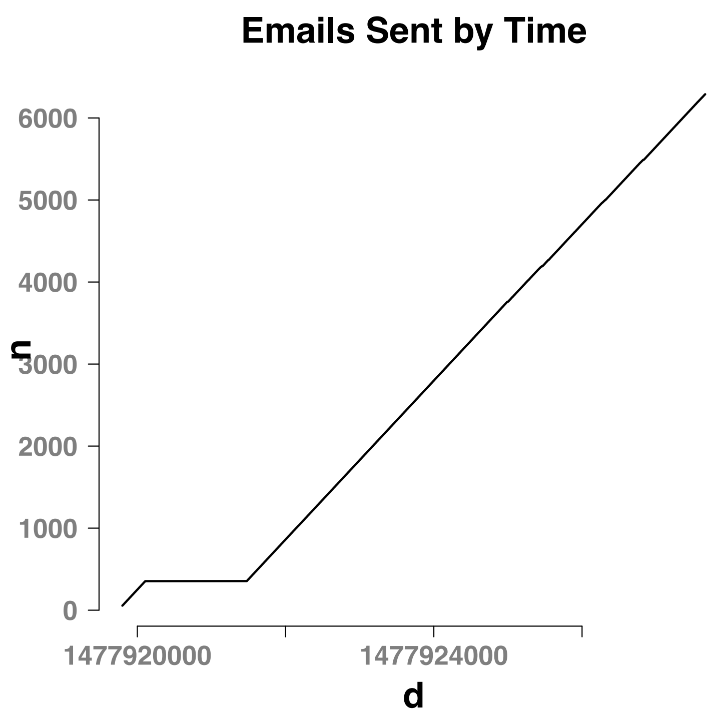
As we report in Table A9 and Table A10, neither including nor excluding these pilot states from the analysis changes the substance nor the interpretation of the core results. As well, there is no evidence that the causal effect is different in pilot or non-pilot states.

Table A9: Robust to Pilot Exclusion

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

Table A10: Robust to Pilot Exclusion

	Dependent variable:		
	GotResponse		
	(1)	(2)	(3)
Latino Cue	-0.030^{*}	-0.030^{*}	-0.030^{*}
	(0.017)	(0.018)	(0.018)
Black Cue	0.001	0.005	0.005
	(0.017)	(0.018)	(0.018)
Arab Cue	-0.113^{***}	-0.112^{***}	-0.112^{***}
	(0.017)	(0.018)	(0.018)
Pilot			0.120^{*}
			(0.065)
Latino Cue * Pilot			0.021
			(0.093)
Black Cue * Pilot			-0.107
			(0.092)
Arab Cue * Pilot			-0.013
			(0.093)
Constant	0.613^{***}	0.609^{***}	0.609^{***}
	(0.012)	(0.012)	(0.012)
Include Pilot	Yes	No	Yes
Observations	6,439	6,206	6,439
R^2	0.009	0.009	0.010
Adjusted R^2	0.008	0.009	0.009
Note:			*p<0.1; **p<0.05; ***p<0.01



h

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.

J 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.

K 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 registar 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.

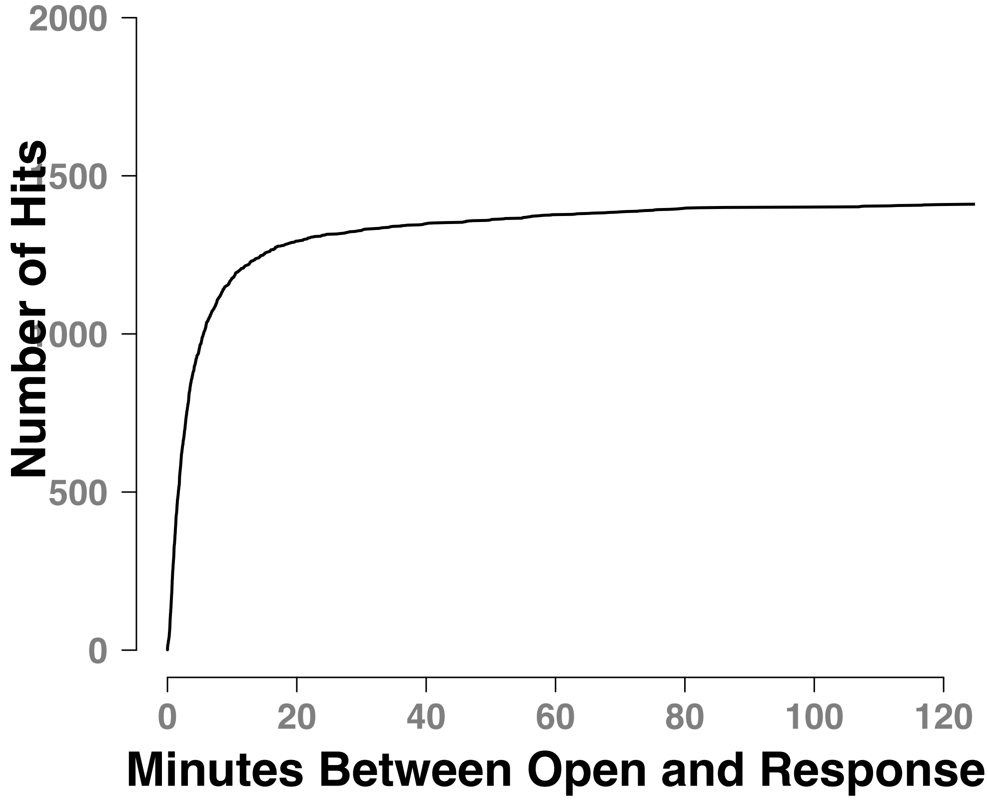


Figure A10: On the x-axis are the minutes elapsed since the first time the local election officials opened our stimulus, until the time that we received a response from that election official. On the y-axis are the cumulative number of responses that have been received in that duration of time.

L 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, Nathan and Faller (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 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^{***}	-0.14^{***}	-0.13^{***}	-0.13^{***}				
	(0.04)	(0.04)	(0.04)	(0.04)				
Latino Cue					-0.10^{*}	-0.10^{*}	-0.08^{*}	-0.07
					(0.05)	(0.05)	(0.05)	(0.05)
Black Cue					-0.02	-0.03	-0.01	-0.02
					(0.05)	(0.05)	(0.04)	(0.04)
Arab Cue					-0.29^{***}	-0.29^{***}	-0.31^{***}	-0.30^{***}
					(0.05)	(0.05)	(0.05)	(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^2	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 estimted. *Election* is the pre-registered censoring date at election day; *Clerk* places the censoring date at the time of the NASS email notification. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

M 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

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

Table A13

	Dependent variable:		
	GotResponse		
	(1)	(2)	(3)
Latino	-0.049^{***}	-0.026	-0.028
	(0.019)	(0.018)	(0.018)
Black	0.013	-0.003	0.003
	(0.019)	(0.018)	(0.018)
Arab	-0.121^{***}	-0.113^{***}	-0.109^{***}
	(0.019)	(0.018)	(0.018)
Percent Latino	-0.227		
	(0.233)		
Percent Latino × Latino	0.345^{**}		
	(0.167)		
Percent Latino × Black	-0.199		
	(0.174)		
Percent Latino × Arab	0.138		
	(0.168)		
Percent Black		-0.173	
		(0.234)	
Percent Black × Latino		-0.098	
		(0.162)	
Percent Black × Black		0.119	
		(0.166)	
Percent Black × Arab		0.008	
		(0.156)	
Percent Arab			1.680
			(2.460)
Percent Arab × Latino			-0.850
			(2.780)
Percent Arab × Black			-0.657
			(2.770)
Percent Arab × Arab			-1.740
			(2.670)
Block FE	*Yes*	*Yes*	*Yes*
Observations	6,439	6,439	6,406
R^2	0.339	0.337	0.337
Note:			$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Table A14

	Dependent variable:	
	GotResponse	
	(1)	(2)
Minority Cue	−0.040**	
	(0.021)	
Latino Cue		−0.020
		(0.025)
Black Cue		0.004
		(0.026)
Arab Cue		−0.106***
		(0.025)
1-50pct Arab	0.073**	0.073**
	(0.032)	(0.032)
51-100pct Arab	0.082**	0.081**
	(0.034)	(0.034)
Minority Cue * 1-50pct Arab	−0.026	
	(0.035)	
Minority Cue * 51-100pct Arab	0.005	
	(0.036)	
Latino Cue * 1-50pct Arab		−0.024
		(0.043)
Black Cue * 1-50pct Arab		−0.004
		(0.043)
Arab Cue * 1-50pct Arab		−0.050
		(0.043)
Latino Cue * 51-100pct Arab		−0.008
		(0.044)
Black Cue * 51-100pct Arab		−0.0004
		(0.044)
Arab Cue * 51-100pct Arab		0.026
		(0.044)
Block FE	*Yes*	*Yes*
Observations	6,439	6,439
R^2	0.332	0.340
Adjusted R^2	0.107	0.115
Note:		$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

N Political Heterogenity

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

	Dependent variable:			
	GotResponse			
	OLS	*felm*	*OLS*	*felm*
	(1)	(2)	(3)	(4)
Latino Cue	−0.084	−0.066	−0.059	−0.054
	(0.070)	(0.067)	(0.070)	(0.068)
Black Cue	0.078	0.088	0.066	0.061
	(0.070)	(0.067)	(0.070)	(0.067)
Arab Cue	−0.018	−0.011	0.050	0.050
	(0.070)	(0.067)	(0.070)	(0.068)
2012 R. Vote Share	0.215**	−0.057		
	(0.091)	(0.173)		
Latino Cue × 2012 R. Votes Share	0.102	0.068		
	(0.128)	(0.123)		
Black Cue × 2012 R. Vote Share	−0.144	−0.163		
	(0.128)	(0.123)		
Arab Cue × 2012 R. Vote Share	−0.178	−0.191		
	(0.127)	(0.122)		
2016 R. Vote Share			0.058	−0.306**
			(0.081)	(0.136)
Latino Cue × 2016 R. Votes Share			0.048	0.041
			(0.114)	(0.110)
Black Cue × 2016 R. Vote Share			−0.109	−0.099
			(0.113)	(0.109)
Arab Cue × 2016 R. Vote Share			−0.271**	−0.269**
			(0.114)	(0.110)
Intercept	0.499***		0.579***	
	(0.050)		(0.050)	
Block Fixed Effects	No	Yes	No	Yes
Observations	6,439	6,439	6,438	6,438
R^2	0.012	0.338	0.010	0.340
Note:	*p<0.1; **p<0.05; ***p<0.01			

O 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
Dariel 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
Dariel 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 Abdulrashid	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