Who gets to vote? New evidence of discrimination among local election officials *

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Abstract

We replicate the results of a recent audit study and confirm that local election officials are significantly less responsive to requests for information from putative voters with Latino names than those with "white" names. We extend the original study by experimentally testing for bias toward Arab and African Americans, and find that Arab American names received responses at a rate roughly 11 percentage points lower than whites. Surprisingly, constituents with African American names received responses at a rate indistinguishable from that of white constituents. These new results demonstrate both persistence of and unexpected variation in bias among public officials.

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How does group identity affect political participation? As the 2016 general election illustrated, few questions are more central to contemporary American democracy. 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). In a recent paper, White, Nathan and Faller (2015), henceforth and without derogation WNF, contribute to our understanding of race in American politics by providing evidence of bias in an unlikely, and largely unexplored arena — among the so-called "street-level" bureaucrats (Lipsky, 1980) responsible for fielding electronic inquiries about voting requirements. They find that local officials were significantly less responsive to requests from putative voters with Latino surnames than putative voters with "white" surnames.[1]

In this paper, we successfully replicate WNF's main finding, using a similar experimental design. We find that Latinos received responses from local officials at a rate three percentage points lower than whites, recovering an estimate of this parameter that is similar to WNF's in magnitude and dispersion. We also extend WNF's findings by experimentally testing for the extent of bias again Arab and African Americans. Arab American names received responses at a rate roughly 11 percentage points lower than whites, and this bias is not confined to areas in which Arab constituents are relatively less common. In surprising contrast to a large literature on discrimination among political elites (e.g., Butler, 2014), constituents with African American names received responses at a rate indistinguishable from that of white voters.

In addition to replicating previous findings and discovering new evidence of bias in the electoral process, we investigate whether political institutions moderate the extent of these biases. We see mixed evidence for institutional effects. While a law mandating provision of Spanish-language voting material is associated with lower bias toward Latinos, jurisdictions previously covered under the (now defunct) Section 5 of the Voting Rights Act did not

[1]Racial categories appear without quotation marks in the remainder of the paper.

respond differently to our treatments.

Our paper makes two primary contributions. First, we successfully replicate a prior experimental finding of discriminatory behavior among public officials. Our replication is particularly important in light of recent studies showing that many experimental findings fail to replicate (Mullinix et al., 2015; Coppock, 2016), and given the centrality of voting procedures to the democratic process. Second, we show significant and unexpected variation in bias by local election officials toward different racial groups. Our results raise a number of important questions for future scholarship on discrimination in the electoral process.

Hypotheses

A substantial literature suggests that bureaucrats capable of exercising discretion may be influenced by the characteristics (e.g., race, partisanship) of individuals seeking public services (for a review, see White, Nathan and Faller, 2015, pp.131-2). Our paper preserves the core aim of WNF – to test this hypothesis by determining whether emails emanating from "putatively Latino constituents" elicit different behavior in local election officials than emails associated with white constituents. We make two modifications to the previous design, however, with the goal of extending their findings. First, while WNF tested for bias in both a generic email and an email related to voter identification, we focus exclusively on emails that mention voter ID laws.[2] Second, we extend the test of bias to cover non-white racial groups other than Latinos.

Our study design uses four distinct racial identities: the baseline category (white), the category for which WNF previously detected discrimination (Latino), and two additional categories (African and Arab American), that allow us to determine whether bias in local election officials applies beyond the scope of the initial discovery. The choice of four total signals of race balances an interest in determining the scope conditions of WNF's findings

[2]This choice allowed us to introduce additional experimental conditions without shrinking the sample size upon which the replication is based and thereby reducing our ability to replicate the original findings.

Table 1: Comparison of Hypotheses

White, Nathan and Faller (2015)		Replication	
WNF1	Bias in response to Latino emails	–	*No Replication Attempted*
WNF2	Bias in response to Latino emails referencing voter ID	**H1**	Bias in response to minority emails referencing voter ID
WNF3	HTE: Strict ID laws × Latino	**H2**	HTE: Strict ID laws × minority
Exploratory	HTE: VRA × Latino	–	*No Replication Attempted*
Exploratory	HTE: §5 VRA × Latino	**H3**	HTE: Previous §5 VRA× minority
Exploratory	HTE: §203 VRA × Latino	**H4**	HTE: §203 VRA× minority

Notes. H1, H2, H3, and H4 preserve the white vs. Latino comparison in White, Nathan and Faller but extend the design to test responsiveness to black and Arab minority groups. HTE refers to the heterogenous treatment effect estimated by interacting the treatment with the specified covariate.

with the need to preserve sufficient statistical power to replicate their core result. The literature on discretionary bureaucratic actors leads to the theoretical expectation that all of these minority groups will be subject to bias, and we adopt that generic expectation in our own hypotheses.[3] Finally, we reproduce two tests that WNF conducted to determine whether bias was greater in states with strict voter ID laws or in places covered under Sections 5 or 203 of the Voting Rights Act (henceforth, §5, §203 VRA, respectively). The precise correspondence between our hypotheses and those of WNF appears in Table 1.

We test our hypotheses by conducting an email audit study of local election officials. The intended population of our study is all local (county or municipal) election officials serving across all 50 U.S. states. Our sample comprises all such officials with publicly available email addresses.[4] In total, 6,439 local election officials from 44 states, or 94 percent of the registrar universe (White, Nathan and Faller, 2015; Kimball and Kropf, 2006), received our intervention.

Our experimental stimulus consists of an email sent from a putative voter to each local election official. All emails follow the same structure, greeting the official by name, refer-

[3]While Einstein and Glick (2017) find that public housing bureaucrats do not exhibit bias against blacks, this non-discriminatory behavior stands in stark contrast to established expectations. As we discuss later, however, their results support our own.

[4]To collect the email addresses of these officials, we scraped the data from the relevant website of each state. Details of the implementation and results of this process appear in Appendix A.

encing voter identification requirements, and asking about the requirements to vote in the state corresponding to the official. 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 elected officials, we vary the race of the email sender. In line with convention we expose officials to four distinct racial identities by manipulating senders' names (Butler and Broockman, 2011; Butler, 2014; White, Nathan and Faller, 2015; Bertrand and Duflo, 2016; Bertrand and Mullainathan, 2004). 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 not only by race, but by other characteristics associated with a particular name, for example socio-economic status, education, or other idiosyncratic features. Appendix D describes the procedure for choosing names, and Appendix E provides the complete list of names.

Assignment to racial treatments was based on blocked quadruples of same-state registrars with similar jurisdiction-level demographic traits. Treatment assignment was blocked on population density (logged), margin of victory for Barack Obama 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 covered by §5 of the VRA.[5] Within each blocked quadruple, we assign each official to one racial condition, randomly drawing a name from that condition and a message version. 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.[6] Our outcome measure, GotResponse, is coded 1 if an election official replied to our email prior

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

[6] 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.

Table 2: 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.

to election day, and 0 otherwise. 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 elections officials. While lower than the rate reported in WNF (67.7 percent), this rate compares favorably with experiments on elected officials in the US (e.g., 56.5 percent in Butler and Broockman (2011)), providing evidence that our requests were taken at face value.

As we report in Table 2, response rates differ across experimental conditions. 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 registrar response by nearly 3 percent ($\Delta\mu = -2.97$ 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, $P = 0.90$). These estimates are presented in Figure 1 (a). Regression estimates and robust standard errors, reported in Table 3, produce similar results.[7]

[7] These regression estimates are unbiased estimators of causal effects (Wooldridge, 2010; Angrist and Pischke, 2008), though in Appendix H, we note that estimating maximum likelihood models with gaussian, logit, or

Table 3: Causal Estimates

	GotResponse			
	(1)	(2)	(3)	(4)
Minority Cue	−4.7(1.4)***		−4.7 (1.3)***	
Latino Cue		−3.0 (1.7)*		−3.0 (1.6)*
Black Cue		0.1 (1.7)		0.2 (1.7)
Arab Cue		−11.3 (1.7)***		−11.3 (1.6)***
Constant	61.3(1.2)***	61.3 (1.2)***		
Block FE	No	No	Yes	Yes
Observations	6,439	6,439	6,439	6,439
R^2	0.002	0.01	0.3	0.3

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

One possible explanation for the high degree of bias shown to Arab Americans 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. We find, however, that discrimination toward Arab names does not vary with the proportion of Arab Americans within counties (see full results in Appendix L, Tables 16 and 17).[8]

By preserving the core design of the initial experiment, we are able to perform a precision weighted meta-analysis that combines the results of our intervention with those of WNF (Gerber and Green, 2012, p.361). As we report in Figure 1 (b), the precision-weighted estimate of the Latino treatment effect combining 2012 and 2016 data permits a stable, precise estimate of the response penalty paid by purported voters with Latino names. These individuals receive replies from registrars 4.4 percent less frequently than voters with white names (*precision weighted* $SE = 1.18$).

In addition to the main effects of our treatment, we consider whether institutions influence the responsiveness of local election officials to inquiries from racial minorities. One

probit link functions does not change the estimate or interpretation.

[8]This null result could also be interpreted as evidence against the hypothesis that officials who experience increased contact with Arab-Americans should exhibit less bias.

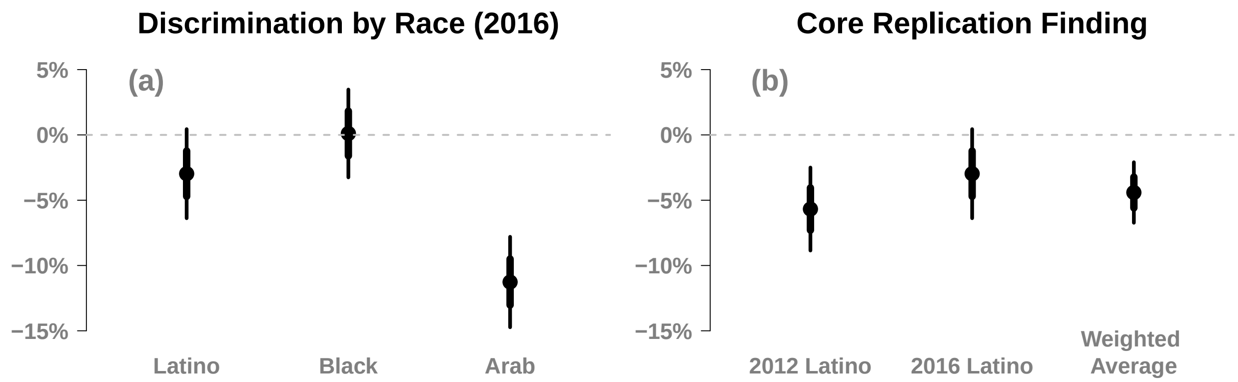

Figure 1: Points represent estimated difference in response rates between named category and white response rate (ITT). Thick bars report $ITT \pm SE$, thin bars report $ITT \pm 1.96 * SE$. Estimates are from difference in means estimator and precision-weighted difference in means. The weighted average is the precision weighted average of 2012 and 2016 Latino evidence.

potentially important source of institutional variation is the presence of "voter ID" laws, which impose more or less strict requirements on voters to establish their identities before casting a ballot. On the one hand, the presence of such laws could make election officials more concerned about voting by non-citizens, and thus less responsive to minority requests. On the other hand, voter ID requirements could cause local officials to view voting by non-citizens as less likely in the first place, and thus decrease the incentive to be less responsive to minority requests. Like WNF, we find no evidence that our experimental stimulus caused different registrar responses conditional on the voter ID laws in force.[9]

We also consider the importance of two sections of the Voting Rights Act: §5, which required preclearance before implementing changes to voting procedures, and §203, which requires provision of voting materials in the native language of applicable minority groups, in practice often Spanish speakers. Insofar as they institutionalize some form of monitoring of official behavior toward minorities in general (in the case of §5) or Latinos (in the case of §203), these provisions might diminish bias. WNF present exploratory evidence showing discrimination was less likely in jurisdictions covered by either of these provisions, or by

[9]Additional details and associated regression tables are reported in Appendix M.

§203 alone (White, Nathan and Faller, 2015, Figure 2, panels A and C, respectively).[10] This provocative finding took on greater interest when the Supreme Court effectively nullified §5 in a 2013 decision.[11] As we report in Appendix N, Table 21, we find no evidence that elections officials in previously §5 VRA counties showed more or less bias in response to our intervention. We do, however, find that discrimination toward Latino senders decreases in areas covered by §203, and that no similar effect holds for other minority groups (see Appendix O, Table 22).

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 2 shows that the reduced time window is not driving our reported effects. The preponderance of registrar 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 2 (b) and (c), the systematic pattern of unresponsiveness to

[10] In our view, the institutional environment specified by the combination of §5 and §203 is theoretically ambiguous, so we focus instead on replicating the results for each provision separately. Page 138 of WNF presents evidence of a null effect of the treatment within §5 jurisdictions, which is distinct from a differential effect of treatment conditional on §5.

[11] *Shelby County v. Holder*, 570 U.S. ___ (2013).

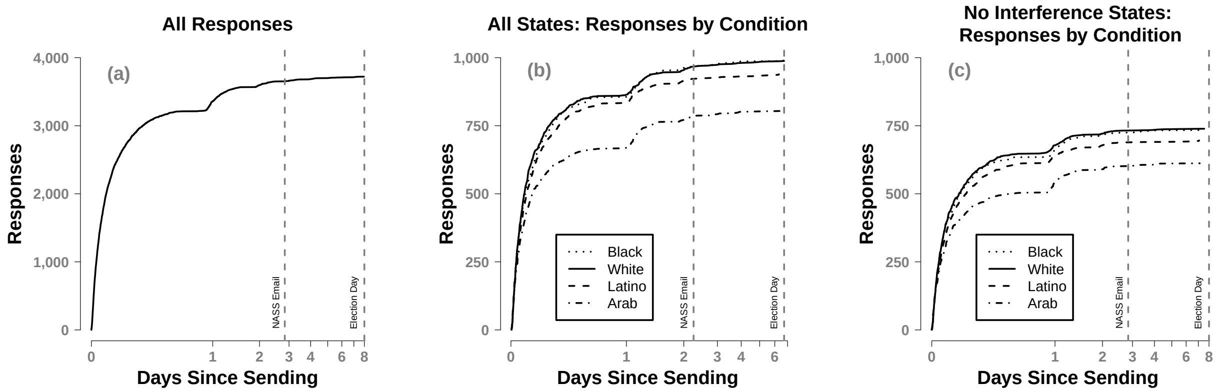

Figure 2: Rapidly slowing rates of response. In all plots, the x-axis is reported in logged time since sending. Election Day and NASS emails are noted with vertical dashed lines. Plot (a) shows all responses for all states; (b) shows all states' responses by experimental condition; (c) excludes states with documented interference and pilot states.

minority names appears rapidly and well before the reported NASS broadcast. Second, as we report in Table 15, 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 that demanded further investigation. Using a similar experimental design, we are able to replicate and extend these findings. 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 second recent study to produce this unexpected null finding (Einstein and Glick, 2017). Considering the presence of bias toward blacks in so many aspects of American political and social life, its absence from this this stage of the electoral process is indeed surprising. Could it be that, for this racial identity and in this setting, a name by itself is insufficient to cue the stereotypes that motivate discrimination?

In light of this lack of discrimination against blacks, it is 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. Rather than simply increased presence, might a different type of contact be required to lower the steep barriers to communication between local election officials and Arab-Americans?

Our replication and extension of the finding of bias on the part of local election officials invites the question of whether this discrimination influences political participation. While existing research shows that even seemingly small obstacles may have significant impacts on voter turnout (McNulty, Dowling and Ariotti, 2009), only through further scholarship can we learn whether access to information about registration affects who votes or who wins.

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Supplementary Information for "Who gets to vote? New evidence of discrimination among local election officials"

(Not for publication.)

Contents

A Email Scraping

In this appendix, we describe the email address collection process. We created a separate scraper for each state webpage. These scrapers fall into three different categories.

Non-dynamic (static) webpages: Consisting of all the webpages that presented a single list (or similar somewhat structured text) with all the registrars and their contact information.

PDF and raw documents: Consisting of the extraction of information of raw documents, such as pre-formatted spreadsheets or pdf documents.

Navigating webpages: Consisting on all those pages that needed navigation (either through selection of a dropdown list, interactive links from an index list or a clickable map) in order to display the county's registrar contact information.

Each of these categories was processed within a different Jupyter Notebook file, in order to make use of similarities in the structure of the scraping script for pages with consistent structure.

Both the static and the non-static webpages were scraped using Python scripts making use of the python binding (http://selenium-python.readthedocs.io/) of the selenium framework (http://www.seleniumhq.org/), a web driver that allows for programmatic operation of a web browser. For performance the browser used by the script was a standard headless browser frequently used for similar tasks, PhantomJS (http://phantomjs.org/).

The PDF and raw document extraction was more varied and relied on a more manual labor. Excel spreadsheets were manipulated directly through pandas (http://pandas.pydata.org/). PDF documents were first processed in Tabula (http://tabula.technology/) with which individual tables were identified and extracted into groups of csv's, and these csv's were then read and processed using pandas as well.

Table 4 presents a list of the states by the type of data provided by the state.

We do not include the following states' registrars in our assignment to treatment: Alaska, Hawaii, Maine, Maryland, Missouri, and New Jersey. We exclude Alaska because registrar

Table 4: Summary of States by Scraping Method Used. *Montana appears twice since emails were extracted from both in order to check data accuracy. Maine, New Jersey and Missouri do not appear in our lists as their webpages did not provide email information.*

Static Scraping	**Dynamic Scraping**	**Downloadable**
Arizona	Alaska	Arkansas
Alabama	Georgia	Colorado
California	Illinois	Connecticut
Delaware	Indiana	Montana
Florida	Kentucky	New Hampshire
Hawaii	Michigan	New York
Idaho	Minnesotta	Wisconsin
Iowa	Missouri	Wyoming
Kansas	Mississippi	
Louisiana	Pennsylvania	
Massachusetts	Rhode Island	
Montana	Virginia	
North Carolina		
North Dakota		
Nebraska		
New Mexico		
Nevada		
Ohio		
Oklahoma		
Oregon		
South Carolina		
South Dakota		
Tennessee		
Texas		
Utah		
Vermont		
Washington		
West Virginia		

jurisdictions were not mappable onto borough, city-borough, 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 registrars available. We do not include Maryland due to a clerical oversight.

We report other registars that were excluded from randomization, as well as reasons for these exclusions in Table 5. Most of these exclusions are for reasons related to concerns over spillover, or multiple registrars overseeing a single jurisdiction. All determinations were made prior to randomization.

Table 5: Registrars 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

In this appendix, we list the questions used in our emails to local election officials.

Unlike WNF, we did not vary whether the local election official receives a request directly related to voter identification. Because WNF establish that prejudicial behavior occurred almost exclusively in responses to emails related to voter identification, we focus only on requests of that type. The three randomly assigned elements of the email text all deliver questions related to voter identification and its impact on voting. The purpose of varying these elements is to avoid introducing confounding that could arise if a particular question wording induces higher or lower response rates. 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. 6 presents the different values for each of two preambles and a question. These elements were combined at random, to produce 27 variations of the message text that local officials receive.

For example, one particular realization of this cue might draw the first cue from each of 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,`

`<John Smith>`

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 6: 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 7

	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 possiblity 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 and Levitt, 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 ≈ 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 E, Table 8.

E Names and Assessment of Racial and Ethnic Group

Table 8: 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

F 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 registrars 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 registrar responds to a query for information randomization cuts all ties with these factors.

Blocking was implemented via the 'blockTools' package (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.

Table 9

	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

G Fixed Effects Models

Table 10 presents the same results as Table 3 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 registrars 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 registrar 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 registrars. 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 10: 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 11 and Table 12, we use a maximum likelihood approach to estimating these models, first with a gaussian link function, but also with logit and probit functions.

Table 11: 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 12: 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 registrar 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 registrar population (233 total registrars), and their distance from other large registrar areas.

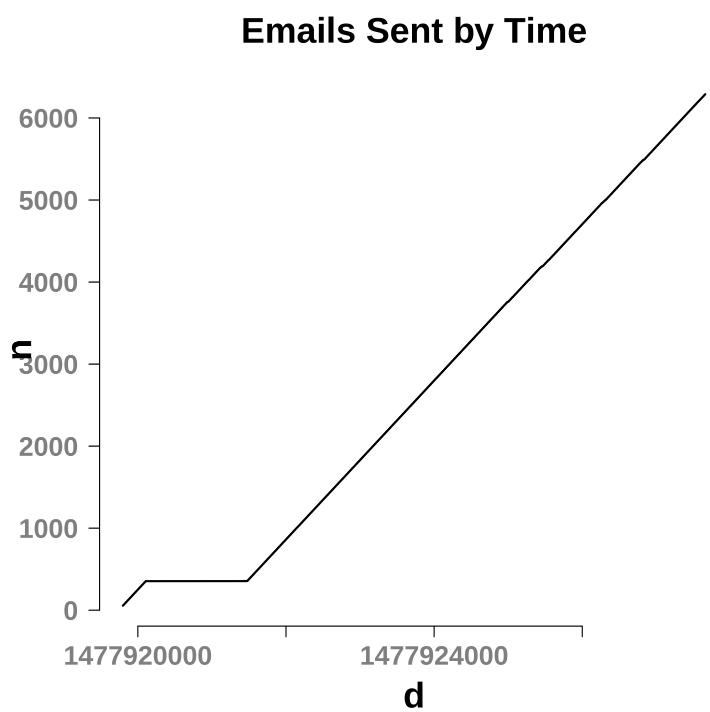
As we report in Table 13 and Table 14, neither including nor excluding these pilot states from the analysis changes the substance nor the interpretation of the core results. As well, there is no evidence that the causal effect is different in pilot or non-pilot states.

Table 13: 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 14: 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 3: 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 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 registrars 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 registrars 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.

K 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 15, the coefficients estimated in all models are highly stable.

Table 15: 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. $^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

L Limited District Characteristic Heterogeneity

In the following models, reported in Table 16 and Table 17, 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 a voter of that class. Indeed, as we show in Table 16 and Table 17, while there is little change in the responsiveness of elections officials as the proportion of voters in that jurisdiction becomes increasingly black (shown in *Model (2)* and *Model (3)* in both Table 16 and Table 17, as we report in *Model (1)* in Table 16 and Table 17, there is some evidence that officials' responsiveness changes as the proportion of voter in a jurisdiction becomes increasingly Latino.

Table 16

	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 17

	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)
Observations	6,439	6,439	6,406
R^2	0.339	0.337	0.337
Adjusted R^2	0.115	0.113	0.110
Note:	*p<0.1; **p<0.05; ***p<0.01		

M No Voter ID Heterogeneity

In this appendix, we examine whether treatment effects are conditioned by the type of voter identification law in force. To perform this test, we interact indicators of the type of voter ID laws that are in effect with treatment indicators.

We collect data about the type of Voter ID rules that are in place from the National Conference of State Legislators, available at http://www.ncsl.org/research/elections-and-campaigns/voter-id.aspx This creates four categories of voter ID classification; to these four we add a fifth super-category that includes all areas classified as strict regardless of whether they required a photo ID or not.

Table 18: NCSL Voter ID Law in Force in 2016

	Photo ID	**Non-Photo ID**
Strict	Georgia	Arizona
	Indiana	North Dakota
	Kansas	Ohio
	Mississippi	
	Tennessee	
	Virgina	
	Wisconsin	
Non-Strict	Alabama	Alaska
	Florida	Arkansas
	Idaho	Colorado
	Louisana	Connecticut
	Michigan	Deleware
	Rhode Island	Hawaii
	South Dakota	Kentucky
	Texas	Missouri
		Montana
		New Hampshire
		North Carolina
		Oklahoma
		South Carolia
		Utah
		Washington

Results from these models are reported in Table 19 and Table 20. There is little evidence to support a hypothesis that the effect of receiving a minority email functioned differently

Table 19: No Voter ID Heterogeneity

	Dependent variable:				
	GotResponse				
	(1)	(2)	(3)	(4)	(5)
Nonstrict, Non Photo	0.001				
	(0.035)				
Nonstrict, Photo		-0.070^{***}			
		(0.027)			
Strict, Non Photo			0.073		
			(0.081)		
Strict, Photo				0.016	
				(0.026)	
All Strict					0.023
					(0.026)
Minority Cue	-0.051^{***}	-0.044^{***}	-0.050^{***}	-0.047^{***}	-0.052^{***}
	(0.015)	(0.017)	(0.014)	(0.017)	(0.018)
Nonstrict, Non Photo * Minority Cue	0.027				
	(0.040)				
Nonstrict, Photo * Minority Cue		-0.009			
		(0.031)			
Strict, Non Photo * Minority Cue			0.129		
			(0.093)		
Strict, Photo * Minority Cue				-0.0005	
				(0.030)	
All Strict * Minority Cue					0.013
					(0.030)
Constant	0.613^{***}	0.635^{***}	0.612^{***}	0.608^{***}	0.605^{***}
	(0.013)	(0.015)	(0.012)	(0.015)	(0.015)
Observations	6,439	6,439	6,439	6,439	6,439
R^2	0.002	0.007	0.005	0.002	0.003
Adjusted R^2	0.002	0.006	0.004	0.001	0.002
Residual Std. Error (df = 6435)	0.494	0.492	0.493	0.494	0.493
F Statistic (df = 3; 6435)	4.290^{***}	15.000^{***}	10.300^{***}	4.130^{***}	5.900^{***}
Note:				*p<0.1; **p<0.05; ***p<0.01	

among elections officials operating in districts with more strict voter IDs.

Table 20: No Voter ID Heterogeneity

	Dependent variable:				
	GotResponse				
	(1)	(2)	(3)	(4)	(5)
Nonstrict, Non Photo	0.001				
	(0.035)				
Nonstrict, Photo		-0.070^{***}			
		(0.026)			
Strict, Non Photo			0.073		
			(0.081)		
Strict, Photo				0.016	
				(0.026)	
All Strict					0.023
					(0.026)
Latino Cue	−0.026	−0.034	-0.034^{*}	-0.036^{*}	-0.043^{**}
	(0.019)	(0.021)	(0.018)	(0.021)	(0.021)
Black Cue	0.002	−0.003	−0.001	−0.001	−0.004
	(0.019)	(0.021)	(0.018)	(0.021)	(0.021)
Arab Cue	-0.129^{***}	-0.096^{***}	-0.116^{***}	-0.104^{***}	-0.109^{***}
	(0.019)	(0.021)	(0.018)	(0.021)	(0.021)
Nonstrict, Non Photo * Latino	−0.027				
	(0.049)				
Nonstrict, Non Photo * Black	−0.006				
	(0.049)				
Nonstrict, Non Photo * Arab	0.115^{**}				
	(0.049)				
Nonstrict, Photo * Latino		0.014			
		(0.037)			
Nonstrict, Photo * Black		0.012			
		(0.037)			
Nonstrict, Photo * Arab		−0.053			
		(0.037)			
Strict, Non Photo * Latino			0.170		
			(0.113)		
Strict, Non Photo * Black			0.086		
			(0.113)		
Strict, Non Photo * Arab			0.132		
			(0.113)		
Strict, Photo * Latino				0.020	
				(0.037)	
Strict, Photo * Black				0.006	
				(0.037)	
Strict, Photo * Arab				−0.027	
				(0.037)	
All Strict * Latino					0.037
					(0.036)
All Strict * Black					0.015
					(0.036)
All Strict * Arab					−0.012
					(0.036)
Constant	0.613^{***}	0.635^{***}	0.612^{***}	0.608^{***}	0.605^{***}
	(0.013)	(0.015)	(0.012)	(0.015)	(0.015)
Observations	6,439	6,439	6,439	6,439	6,439
R^2	0.011	0.015	0.012	0.009	0.010
Adjusted R^2	0.009	0.014	0.011	0.008	0.009
Residual Std. Error (df = 6431)	0.492	0.491	0.491	0.492	0.492
F Statistic (df = 7; 6431)	9.820^{***}	13.700^{***}	11.200^{***}	8.600^{***}	9.390^{***}
Note:					*p<0.1; **p<0.05; ***p<0.01

N No §5 VRA Heterogeneity

In this appendix, we examine whether treatment effects are conditioned by whether an area was covered under the (now defunct) §5 of the VRA. To perform this test, we interact prior §5 coverage with our treatment indicators. As we report in Table 21, we find no evidence that these political institutions constrained discrimination.

VRA status was gathered from the Department of Justice website, available at https://www.justice.gov/crt/jurisdictions-previously-covered-section-5.

Table 21: HTE of Prior VRA Status

	Dependent variable:			
	GotResponse			
	(1)	(2)	(3)	(4)
Minority	−0.047***	−0.045***		
	(0.014)	(0.015)		
Latino			−0.029*	−0.033*
			(0.017)	(0.018)
Black			0.001	0.007
			(0.017)	(0.018)
Arab			−0.112***	−0.110***
			(0.017)	(0.018)
VRA County?	0.091***	0.100***	0.091***	0.100***
	(0.019)	(0.038)	(0.019)	(0.038)
Minority * VRA County?		−0.012		
		(0.044)		
Latino * VRA County?				0.030
				(0.053)
Black * VRA County				−0.048
				(0.053)
Arab * VRA County?				−0.017
				(0.053)
Constant	0.602***	0.601***	0.602***	0.601***
	(0.012)	(0.013)	(0.012)	(0.013)
Observations	6,439	6,439	6,439	6,439
R^2	0.005	0.005	0.012	0.013
Adjusted R^2	0.005	0.005	0.012	0.012
Note:	*p<0.1; **p<0.05; ***p<0.01			

O § 203 Heterogeneity

While there was scant evidence in support of a differential treatment effect based on registrars presiding in §5 VRA covered areas, there is evidence that suggests that in districts where there are relatively high proportions of voters who do not speak English – i.e. districts that meet the rubric for §203 coverage under the VRA – that querying elections officials from matched ethnic identities leads to higher response rates. As we report in Table 22, while election officials' response rates are lower on average (by about 14 percent) in counties that are covered by §203, in those counties that quality for §203 under Hispanic language minority groups, registrars are *in fact* more responsive. Indeed, comparing a county that was not covered by §203 and was sent a white name and a county that was covered by §203 and was sent a Latino name, our models estimate that the latter is responded to at a rate approximately 10 percent higher than the purportedly white voter.

Table 22: Difference on 203 Coverage

	Dependent variable:	
	GotResponse	
	(1)	(2)
§203 Covered	-0.138^{*}	-0.140^{*}
	(0.074)	(0.074)
Minority Cue	-0.050^{***}	
	(0.014)	
§203 Covered × Minority Cue	0.110	
	(0.083)	
Latino Cue		-0.038^{**}
		(0.017)
Black Cue		0.004
		(0.017)
Arab Cue		-0.116^{***}
		(0.017)
§203 Covered × Latino Cue		0.277^{***}
		(0.098)
§203 Covered × Black Cue		-0.116
		(0.103)
§203 Covered × Arab Cue		0.130
		(0.102)
Observations	6,438	6,438
R^2	0.055	0.064
Adjusted R^2	0.048	0.057
Residual Std. Error	0.482 (df = 6391)	0.480 (df = 6387)
Note:		*p<0.1; **p<0.05; ***p<0.01