What Do I Need to Vote?
Bias in Information Provision by Local Election Officials

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Abstract

The adoption of voter identification (ID) requirements has raised concerns that these laws differentially reduce turnout among minorities. We use a field experiment to investigate one mechanism by which these laws could reduce turnout: differential information provision about voting requirements to minorities. We contact over 7,000 local election administrators in 48 states and observe that they provide different information about ID requirements to voters of different putative ethnicities. Emails sent from Latino aliases are significantly less likely to receive any response from local election officials than non-Latino white aliases and receive responses of lower quality. This raises concerns about the effect of voter ID laws on access to the franchise and about bias in the provision of information by local bureaucrats more generally.

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1 Introduction

Since 2002, twenty-one US states have adopted stricter identification (ID) requirements for voting. Proponents argue that the laws are necessary to prevent fraud and, in particular, to prevent undocumented immigrants from voting. Opponents of the laws argue that they will differentially reduce turnout among certain groups, especially poor and minority voters. Before the 2012 election, several voter ID laws were suspended by courts on these grounds, or were delayed under the Voting Rights Act. Empirical evidence of turnout effects from voter ID laws remains mixed (Alvarez et al. 2008, 2011, Ansolabehere 2009, Mycoff et al. 2009). But existing research suggests several mechanisms through which ID requirements may impact poor and minority turnout. First, poor and minority voters are less likely to have required forms of ID, which may lead them to abstain from voting (Barreto et al. 2007, 2009). Second, poll workers may be more likely to challenge poor or minority voters to show ID, leading more of these voters to be turned away (Atkeson et al. 2010, Cobb et al. 2012). Third, adopting new voting requirements may increase the complexity of the voting process, differentially impacting groups who face higher information costs learning about new procedures (Vercellotti and Andersen 2009).

We employ a large-scale, email-based field experiment, similar to Butler and Broockman (2011) and McClendon (2012), to explore this third mechanism. We test whether local election officials responsible for implementing voter ID laws provide different information about these requirements to Latino versus non-Latino white voters. We randomly assigned essentially all county or municipal election officials or commissions in 48 US states (over 7,000 officials) to receive emails from fictitious constituents with either putatively Latino or non-Latino white names asking questions about voting before the 2012 election. The emails were randomized to contain one of two texts: a specific question about voter ID or a control question measuring baseline differences in responsiveness.

Analyzing data from over 5300 replies, we find clear evidence of bias against Latinos in the responsiveness of local election officials. While our design does not allow us to identify specific mechanisms responsible for this bias, we show that emailers with Latino names were roughly five percentage points less likely to receive a reply to a question about voter ID requirements than non-Latino whites. This result is not driven by whether officials were

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1. Much debate about voter ID laws has focused on whether these laws prevent voting by undocumented immigrants or instead deter eligible Latino voters. Adding an African-American treatment condition would have reduced power for state level estimates substantially.
elected or appointed, or by small towns where officials might know all of their constituents. We also find that the replies Latino emailers do receive are less likely to convey accurate information about ID requirements. There is some evidence suggesting that this bias is greater in responses to questions about voter ID laws compared to a baseline question, but this is not statistically significant across model specifications.

The external validity of our experiment is strengthened by the realism of our design. First, Americans are increasingly likely to contact government officials via email, and Latinos are no exception to this trend. A 2009 poll found that of those Americans who reported contacting government within the last year, 60% had done so online or via email at least once (Smith et al. 2009). Among internet users, the proportion of whites, blacks, and Hispanics reporting online or email-based government interactions was similar (Smith 2010). Further, there is evidence that citizens regularly contact local officials, like the election administrators we email in this study. A 2007 poll found that 58% of Americans reported contacting a government office in the past year; of these, 27% said they contacted their local government, and 12% multiple levels of government, which could include local officials (Estabrook et al. 2007). Our study using email also proxies for other common forms of communication between constituents and local governments, such as letters and phone calls, where local officials may respond, or do so informatively, at their own discretion.

Moreover, while there is no data on the frequency with which voters contact local election officials specifically about voter ID laws, the electorate has limited knowledge about these requirements. A 2012 Pew survey found that only 82% of registered voters (and 76% of registered Latinos) living in states that required photo IDs knew about this requirement and 52% of registered voters (and 59% of registered Latinos) living in non-voter ID states erroneously thought they needed ID to vote (Lopez and Gonzalez-Barrera 2012). A more detailed 2012 survey of Pennsylvanians found that 34.3% of registered voters did not know of the existence of Pennsylvania’s new voter ID law and that nearly 12% incorrectly believed themselves to have the necessary identification (Barreto et al. 2012).

Our research makes three main contributions. First, we demonstrate that Latino voters face a higher barrier than non-Latino white voters to receiving information from the local election officials who implement election laws. Though this bias is not specifically confined to questions about voter ID, it has clear implications for democratic representation and minority turnout in states with new voter ID laws. Existing research has shown that changes in election procedures that increase information costs to voting can reduce turnout (Brady and McNulty 2011, McNulty et al. 2009). As election procedures change, eligible
Latino voters may receive lower quality information about new requirements and turnout declines may result. Second, we extend existing literature on the effects of voter ID laws by exploring a previously unexamined mechanism – the provision of information to potential voters before election day – through which these laws may differentially affect voters. Third, our experimental approach avoids the inferential pitfalls of observational studies of the effects of laws that are assigned non-randomly to states. In doing so, we add to a new literature using field experiments to study the behavior of public officials (Butler and Broockman 2011, Broockman 2013, McClendon 2012a, McClendon 2012b).

This paper proceeds as follows. In Section 2, we review existing literature on ethnic and racial bias in the provision of public services in the US before discussing research on voter ID laws. In Section 3, we introduce the hypotheses and design of our experiment, then present results in Section 4. Section 5 concludes with a discussion of the implications of our findings.

2 Existing Literature

2.1 Bias in Access to Public Services

Racial and ethnic discrimination in the provision of government services in the US reaches well into modern times. For example, in the case of Pigford v. Glickman, a federal court found that the Department of Agriculture had discriminated against black farmers applying for federal loans into the 1980s and 1990s (Cowan and Feder 2012). On the state and local level, accusations of discrimination have persisted despite the passage of civil rights legislation. Activists have complained of discrimination in jury selection, the hiring practices and behavior of police, and other areas of service provision (e.g., American Civil Liberties Union 2009, Equal Justice Initiative 2010). Recent evidence suggests that many state legislators discriminate in the quality of services provided to constituents with different ethnicities; Butler and Broockman (2011) finds that state legislators are more likely to respond to emails about voter registration from putatively white constituents than to putatively black constituents.

More generally, there is a large body of experimental research suggesting that individuals – even those who openly espouse equality and fairness – can display discriminatory behavior when making discretionary choices about other people. Experiments in areas as disparate as medical diagnosis and treatment, housing searches, and job applications find that otherwise identical whites and non-whites are treated differently (Ross et al. 2002, Green et al. 2007, Pager et al. 2009). For example, Bertrand and Mullainathan (2004) finds that job appli-
cants in Boston and Chicago with putatively white names receive 50% more callbacks than applicants with putatively black names, despite submitting identical resumes. Against this backdrop, it is plausible that local election officials may similarly discriminate in how they provide information to prospective voters.

2.2 Differential Impacts of Voter Identification Laws

Even if unintentional, discrimination in information provision to voters can make an already complicated and decentralized election administration system more difficult for minority voters to navigate, reducing turnout. Recent changes to voter ID laws often add to the complexity of the voting system, putting an additional burden on local election officials to convey information about new requirements to prospective voters. Since the passage of the federal Help American Vote Act of 2002 (HAVA), twenty-one states have adopted new ID requirements and thirty states currently ask for some type of ID on election day (National Conference of State Legislatures 2012). These laws fall on a spectrum from requiring a photo ID to laws that allow for non-photo IDs, with variation in the specific requirements. For example, whereas Texas’ proposed voter ID law would accept concealed handgun licenses but not student IDs, other “strict” photo ID laws permit student IDs.

To date, empirical attempts to assess the impact of voter ID laws have produced mixed results. These existing studies have examined different outcomes and used varying methodologies. For example, in elections between 2000 and 2006, Alvarez et al. (2008) and Mycoff et al. (2009) find no association between the strictness of voter ID laws and aggregate turnout on election day. Using individual-level panel data from the Current Population Survey, however, Alvarez et al. (2008) does find that the strictest ID restrictions depress turnout among low-income voters.

Hood and Bullock (2012) finds that Georgia’s ID law reduced turnout among those lacking ID, but shows no evidence of differential racial impacts. Mycoff et al. (2009), using survey data from the 2006 Cooperative Congressional Election Study (CCES), finds no effect of the strictness of voter ID laws on self-reported turnout.

A second approach looks at which groups lack relevant forms of ID. Barreto et al. (2007)

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2Many of these findings pertain to differential treatment of black and white Americans, but we think it is reasonable to expect a similar bias against Latinos.

3Even if new laws are designed to simplify the process, they increase complexity in the short term by changing rules that voters and election officials had become familiar with (Vercellotti and Andersen 2009). This is especially true in states where these laws are challenged in court and it is unclear whether they will be enforced in upcoming elections.

4Also see Alvarez et al. (2011).
uses exit polls from three states and finds that immigrant and minority voters are less likely to have various forms of ID. Similarly, Barreto et al. (2009) finds that black, low-income, and elderly voters in Indiana are significantly less likely to have required IDs. It is possible, however, that voters in these groups would obtain IDs if they were aware of a new requirement, and thus that voter ID laws would not limit participation.

A third approach examines which types of voters are actually asked to produce ID at polling stations. Ansolabehere (2009), using CCES data, finds that African-American and Latino voters were asked for ID at significantly higher rates than whites, but that only a tiny fraction were turned away for lack of ID. Similarly, Atkeson et al. (2010) finds that Hispanic voters were more likely than white voters to be asked for ID in New Mexico’s 2006 election. Cobb et al. (2012) finds that African-American and Latino voters were more likely than white voters to be asked for ID in Boston during the 2008 election, despite Massachusetts not having a voter ID law in place. This approach does not speak to the potential deterrence effects of voter ID laws on turnout, however. Voters may abstain from voting because they believe (perhaps incorrectly) that they do not have valid ID.

We depart from existing approaches by examining a new dependent variable: the provision of information to voters about voter ID requirements. This builds on evidence in Vercellotti and Andersen (2009) suggesting that negative turnout effects of voter ID laws are largest when states have recently implemented new laws, especially among Latino voters and other groups that may face steeper “learning curves” about new requirements.

3 Research Design

We use a field experiment to measure whether local election officials in 48 US states respond to email queries from fictitious constituents of different putative ethnicities at different rates or with information of differing qualities. We test three hypotheses about the provision of information on voter ID requirements to prospective voters. First, we predict overall bias in provision of information to prospective Latino voters compared with white voters (as cued through names), regardless of the voter ID requirements in a given state. Second, we hypothesize that “racialization” of voter ID laws leads to more bias in information provision when voter ID laws are specifically mentioned, compared to a baseline question. Third, we predict higher ethnic bias in the provision of information about voting requirements in those states that have adopted voter ID laws. In this section we present our hypotheses and design before describing how replies from local election officials are coded. We conclude with
a discussion of the ethics of the experiment.

3.1 Hypotheses

**H1 – Bias in responses against Latino emailers**: We hypothesize that election officials respond to emails from putatively Latino constituents at lower rates than to those from putatively white constituents. In addition to response rates, we expect bias against Latinos to manifest in less accurate, informative, or friendly responses. This prediction is consistent with the general evidence of racial and ethnic bias in the provision of discretionary services described in the previous section.

**H2 – Greater bias against Latino emailers in emails about voter ID laws**: We also expect that the focus on undocumented immigrants within discourse on voter ID has caused the laws to become “racialized,” leading to particular discrimination against Latino constituents when officials are responding to questions about voter ID compared to other subjects. This hypothesis is consistent with existing research suggesting that media coverage linking a policy to race or ethnicity causes people to associate the policy with their pre-existing biases, even when a policy does not have explicit racial or ethnic content (Valentino 1999, Gilens 2004, Tesler 2012).

**H3 – Greater bias against Latino names in states with stricter voter ID laws**: We hypothesize that officials in states with stricter voter ID laws display larger bias in responsiveness against Latinos. We expect concerns about voting by non-citizens to be more salient in states with stricter laws. This may be because such concerns caused these states to adopt strict voter ID laws, or because exposure to greater debate about ID requirements has made these laws more “racialized.”

3.2 Experimental Set-up

To test these hypotheses, we adapted the experimental approach of Butler and Broockman (2011) and contacted every local official or election commission responsible for overseeing elections for each county or municipality at which elections are administered in 48 states. We sent emails to publicly available addresses through which these officials already field constituent requests. Some were email addresses of individual officials (e.g.,
john.doe@smithcounty.org), while others were general addresses of county or town election commissions (e.g., vote@smithcounty.org). This largely varies by state. Additionally, some local officials are partisan and locally elected, while others are civil servants. Results are analyzed from 46 of these states (N=6825); two states were dropped due to independence assumption violations.\footnote{In Virginia, separate emails forwarded to the same officials in some cases, meaning they received multiple treatment conditions simultaneously, violating the SUTVA (stable unit treatment value) assumption of no-spillover between units needed to identify effects (Rosenbaum and Rubin 1983). There were similar concerns about Minnesota, where officials indicated that they had communicated with each other about the emails in their first set of replies. We drop Virginia and Minnesota from all analysis.}

Table\[1\] lists sample sizes and voter ID law types by state.

Each local official or election commission received an email on September 4, 2012 with a randomly assigned text and sender name.\footnote{We sent a second wave of emails to these same officials on September 18, 2012 to increase the sample size, but have concerns about SUTVA violations within this round. Several officials in six additional states indicated in their replies that they had communicated about our emails. Therefore, data from the second round is not analyzed here. Please see the Supporting Information for combined results, which are robust to including the second set of emails.} The “voter ID email” asked a question about voter ID laws, while the “control email” asked a simpler and less-politicized question about voting in primary versus general elections. The control email text was adapted directly from the “Frequently Asked Questions” pages of several state websites and was meant to be a straightforward question that officials receive regularly. Asking both questions allows us to distinguish between baseline discrimination and discrimination specific to voter ID. We also varied the sender of the emails among four names, two that are putatively Latino (“José Martinez” and “Luis Rodriguez”) and two that are putatively non-Latino white (“Greg Walsh” and “Jake Mueller”).\footnote{We use two names per ethnicity to ensure that results are not due to idiosyncratic characteristics of any specific name. The non-Latino white names are taken directly from Butler and Broockman (2011) and Bertrand and Mullainathan (2004). We attempted to select names that unambiguously cued ethnicity. To test for consistency violations, we conducted a preliminary survey using Amazon’s Mechanical Turk (Berinsky et al. 2012). These names overwhelmingly (> 96%) cued the correct ethnicities. Within the pairs of Latino or non-Latino names, the two names each cued very similar characteristics on other dimensions, such as perceived level of education, income, race, age, and political engagement. Results of this pre-test are included in the Supporting Information.}

To send many emails in a short time window, we registered new domain names and created addresses using the names of our fictitious senders, such as lrodriguez@ajnmail.net. The text of the voter ID email was:

Hello,

I’ve been hearing a lot about voter ID laws on the news. What do I need to do...
Table 1: Number of Email Addresses Used by State

<table>
<thead>
<tr>
<th>State</th>
<th>No.</th>
<th>Unit</th>
<th>Law Type</th>
<th>State</th>
<th>No.</th>
<th>Unit</th>
<th>Law Type</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alabama</td>
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<td>Non-photo</td>
<td>Montana</td>
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<td>–</td>
<td>Nebraska</td>
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<td>County</td>
<td>HAVA</td>
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<td>County</td>
<td>Non-photo</td>
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<td>County</td>
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<td>Municipal</td>
<td>Photo</td>
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<td>HAVA</td>
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<td>33</td>
<td>County</td>
<td>HAVA</td>
</tr>
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<td>Municipal</td>
<td>Non-photo</td>
<td>New York</td>
<td>62</td>
<td>County</td>
<td>HAVA</td>
</tr>
<tr>
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<td>Non-photo</td>
<td>North Carolina</td>
<td>100</td>
<td>County</td>
<td>HAVA</td>
</tr>
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<td>Photo</td>
<td>North Dakota</td>
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</tr>
<tr>
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<td>Photo</td>
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<td>87</td>
<td>County</td>
<td>Non-photo</td>
</tr>
<tr>
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<td>–</td>
<td>–</td>
<td>Utah</td>
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<td>County</td>
<td>Non-photo</td>
</tr>
<tr>
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<td>Vermont</td>
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<td>HAVA</td>
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<td>HAVA</td>
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<td>–</td>
<td>–</td>
</tr>
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<td>39</td>
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<td>–</td>
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<td>23</td>
<td>County</td>
<td>HAVA</td>
</tr>
</tbody>
</table>

Total = 6825. †: 20 units from Massachusetts, Michigan, and Wisconsin were used in a pilot and dropped from the full experiment. *: Minnesota and Virginia were emailed, but not analyzed due to concerns about contamination. Law types are those in effect in September 2012, when our emails were sent, based on definitions from National Conference of State Legislatures (2012). The “photo” category includes any laws that request voters present photo ID; the “non-photo” category includes any laws that require any identification; the “HAVA” category includes states without regulations beyond those required by federal law.
to vote?

Thank you,

(Jose Martinez, Jake Mueller, Luis Rodriguez, or Greg Walsh)

The control email was:

Hello,

I’ve been wondering about this. Do you have to vote in the primary election to be allowed to vote in the general elections?

Thank you,

(Jose Martinez, Jake Mueller, Luis Rodriguez, or Greg Walsh)

In sum, there were eight treatment conditions (4 names x 2 email texts). We analyze the pairs of Latino or non-Latino names together, collapsing the study to four treatment conditions. These treatments were assigned within each state after pair matching the localities, following Imai et al. (2009). We merged demographic data from the 2010 US Census and 2005 American Community Survey (ACS) about each county or municipality to the list of official email addresses. We then used coarsened exact matching (Iacus et al. 2012) to create pairs of counties or municipalities within states that were most similar in terms of population density, income per capita, proportion black, proportion not Latino, and Democratic vote share in the 2008 presidential election. This increases the precision of the experiment by improving balance on observable covariates prior to randomization.

Each pair of counties was randomly assigned to receive one of the email texts with equal probabilities. The ethnicities of the email senders (Latino, non-Latino) were randomized within each pair with equal probabilities, such that one of the units received an email from a Latino name and the other did not. In states with odd numbers of units, the least similar unit was randomly assigned to a treatment condition on its own. After matching, balance for both treatments (text and ethnicity of emailer) improved, but remained imperfect, because

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10 There are not substantively different outcomes within the pairs of Latino or non-Latino names.

11 We match on population density and income per capita as predictors of capacity and professionalization of local officials, which should affect overall response rates. We match on the proportion of a county’s population that is African-American or Latino as predictors of the racial and ethnic diversity of an area. This should predict the plausibility of our treatment and the possibility of an election official herself being a member of a minority group. Finally, we match on Democratic vote share in the last presidential election to account for partisan responses to potential Latino voters.
of the small number of observations within some states (see Supporting Information). For this reason, we use regression adjustment to control for these covariates in the analyses below rather than looking only at differences in means across treatment conditions.

3.3 Coding of Responses

We received over 5300 replies to these emails from at least 4557 local election officials. Although some replies came back from different addresses than had been emailed, we can match all but 19 replies back to the officials to which the emails were sent. The 19 unidentified emails are dropped from all analyses. We use human coding to measure the accuracy of responses.

Using a dataset of voter ID requirements in each state when the emails were sent, we used double-blind human coding to assess the accuracy of each of 2549 replies to the voter ID email. The names of our fictitious emailers were removed and research assistants were not told the research questions. Research assistants assigned each email to one of several categories based on the match between the reply and each (anonymized) state’s requirements: “Absolutely accurate,” meaning the reply contained the full and accurate information about voter ID requirements in the state; “General or narrow, but accurate,” meaning the reply was non-specific or overly narrow, but did not contain inaccurate information; “Non-informative,” meaning the reply did not attempt to answer the question; or “Inaccurate,” meaning the reply contained explicitly inaccurate information about requirements. Research assistants also noted whether each reply had a “friendly” tone, indicated by use of common salutations or well wishes (i.e., “Have a good day”). Examples of each type of email are provided in the Supporting Information. Each reply to the voter ID email was initially coded by two research assistants; replies for which the two coders disagreed were sent to a

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12The multiple emails per official most commonly involved an automatically generated “out of the office” message followed later by a substantive reply. Where multiple replies were sent by the same local election official, we collapse these emails into a single observation with the accuracy value of the most accurate of the component emails.

13Voter ID requirements in some states changed in the final months before the 2012 election due to litigation and court rulings. We code accuracy with respect to the laws in each state as of the date our email was sent (Sept. 4). The dataset of requirements used in our coding process is available on request.

14“Absolutely accurate” emails include replies that provided links to state websites with the official instructions about voting requirements, even if these requirements were not in the body of the email.

15For example, an email saying only “bring an ID when you come to vote,” is considered too general. An email saying only “bring your driver’s license when you come to vote” where many other forms of ID are also accepted is considered too narrow. Neither type of email is explicitly inaccurate.
We find similar patterns when using only absolute accuracy and when collapsing all types of accurate responses into a single category, giving us confidence that our results are not an artifact of coding choices.

Coding the control email was straightforward because it had a single, simple answer; no state requires voting in primaries to vote in general elections. After removing emails that bounced back for incorrect addresses and auto-reply messages, we took a random sample of 100 replies to the control question and found that all were accurate. Based on this sample, we reduced the cost of further coding by considering all replies to control emails that were not bounce backs or “out of office” messages as “absolutely accurate.”

### 3.4 Ethical Considerations

We recognize that there may be objections to conducting an experiment on public officials. These concerns likely take four forms: (1) that we used deception; (2) that consent for participation was not obtained; (3) that local officials were exposed to possible harm by participation; and/or (4) that the experiment risked denying public officials’ services to real constituents. We address these objections here.

This experiment follows standard practice in experimental social science research on racial or ethnic bias, such as the housing, job application, and state legislator studies described in Section 2, in which a minor degree of deception is used and consent is not obtained in advance (McClendon 2012a). Both are necessary to make valuable inferences about behavior that otherwise would be unobservable. Our study was approved in advance by Harvard University’s Institutional Review Board (IRB). The deception was minimal. If the officials replied with a question to our fictitious constituent, we sent no response (to avoid “leading on” the official). If the officials had been informed that the emails came from researchers or asked for their consent, they likely would have behaved differently, rendering the study useless.

Regarding the third objection, there was no harm to local officials from the experiment.

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On the first pass, the two research assistants disagreed on some aspect of the coding in 37% (955) of the emails. 69% (661) of disagreements regarded accuracy; the remainder were on other variables. 77% (739) of disagreements were resolved by the third coder. In 216 emails, all three coders disagreed about at least one variable. The authors then (single-) blind coded these emails, with identifying information for treatment conditions removed, agreeing on the coding for 126 (58%). This left 90 emails for which we could not determine the accuracy, coded as “Ambiguous.” All results below are robust to dropping these observations (see Supporting Information).
We are unable to make inferences about discrimination by individuals.\footnote{There are many possible reasons an individual official did not reply to an email; bias is only observable in aggregate patterns across emails sent to many officials.} Moreover, the email addresses used were made publicly available specifically to field similar queries from voters. We attempted to ask questions that local election officials already answer in the course of their duties. Many responses to the voter ID email that were coded as “absolutely accurate” involved simply pasting a link to a state website. “Absolutely accurate” responses to the control email frequently contained only the word “No.” This last point speaks to the fourth objection – that responding to our emails overburdened election officials and risked denial of services to real constituents. We believe this is implausible. Replying to these emails should not have taken more than several minutes. The good we were requesting – information – is non-rival; supplying it to our fictitious constituents does not prevent it from being provided to others.\footnote{Unlike the field experiments described in Humphreys (2011), our study does not involve randomizing the distribution of some sort of benefit to participants, which must be withheld from a control group.} Our emails were also sent months in advance of the election to avoid distracting officials during a peak period.

4 Experimental Results

We find clear evidence in support of our first hypothesis of overall bias against Latinos in response rates and accuracy, but do not find consistent support for our second and third hypotheses. In Section 4.1, we report results for our main outcome of interest: the response rate of local election officials to emails sent by people of different ethnicities. In Section 4.2, we present evidence that conditional on having replied at all, replies to Latino emailers are of lower quality.\footnote{Sections 4.1 and 4.2 follow a pre-analysis plan, available on request.} Although we cannot identify a specific causal mechanism for bias, in Section 4.3 we exclude several possibilities, narrowing the range of plausible mechanisms. In Section 4.4, we report a series of robustness tests.

Summary statistics are reported in Table 2 broken out by treatment condition.\footnote{Summary statistics by state are in the Supporting Information.} The summary statistics provide evidence of bias; for example, Latino emailers receive responses at lower rates and are less likely to receive absolutely accurate responses than non-Latino emailers. Because our randomization did not achieve perfect balance, however, we prefer parametric approaches that control for locality characteristics that may affect response rates.

We use three different modeling approaches, each of which involves a trade-off between
Table 2: Summary Statistics, All States

<table>
<thead>
<tr>
<th></th>
<th>Voter ID Email</th>
<th></th>
<th>Control Email</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Latino</td>
<td>Non-Latino</td>
<td>Latino</td>
<td>Non-Latino</td>
</tr>
<tr>
<td>States</td>
<td>46</td>
<td>46</td>
<td>46</td>
<td>46</td>
</tr>
<tr>
<td>Emails sent</td>
<td>1662</td>
<td>1664</td>
<td>1683</td>
<td>1683</td>
</tr>
<tr>
<td>Incorrect address / bounce</td>
<td>64</td>
<td>67</td>
<td>48</td>
<td>61</td>
</tr>
<tr>
<td>Response rate</td>
<td>1036 (64.8%)</td>
<td>1126 (70.5%)</td>
<td>1184 (72.4%)</td>
<td>1211 (74.6%)</td>
</tr>
<tr>
<td>“Accurate, absolutely”</td>
<td>523 (50.5%)</td>
<td>619 (55.0%)</td>
<td>~ 1184 (100%)</td>
<td>~ 1211 (100%)</td>
</tr>
<tr>
<td>“Accurate, general or narrow”</td>
<td>190 (18.3%)</td>
<td>203 (18.0%)</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>“Non-informative”</td>
<td>272 (26.3%)</td>
<td>248 (22.0%)</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>“Inaccurate”</td>
<td>13 (1.3%)</td>
<td>10 (0.9%)</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>“Ambiguous”</td>
<td>38 (3.7%)</td>
<td>45 (4.0%)</td>
<td>–</td>
<td>–</td>
</tr>
<tr>
<td>“Friendly”</td>
<td>665 (64.2%)</td>
<td>762 (67.8%)</td>
<td>–</td>
<td>–</td>
</tr>
</tbody>
</table>

“Incorrect address” refers to emails that either immediately were “returned to sender” or to which we received a reply informing us that the email was addressed to an incorrect recipient. Refer to text for other definitions.

viewing states as separate or borrowing information from states with large sample sizes to improve estimates for states with small sample sizes. First, we run separate regressions for each state. ID laws and other features of the electoral administrative system vary by state and may affect overall responsiveness (intercept) and the size or direction of treatment effects (slope). Separate regressions allow us to produce unique estimates for each state, but these models are underpowered – even if effects existed, we would not find statistically significant results in many states (Table 1). Second, we run multi-level models in which coefficients for the effect of the Latino name and the intercept vary by state, while coefficients for the control variables are pooled across all states. This estimates different treatment effects for every state, including those with small sample sizes, but borrows information across states (Gelman and Hill 2007). Third, we run a single fully pooled regression model with state fixed effects. This allows intercepts for each state to vary, but estimates a single treatment effect for the entire country, driven more heavily by states with larger sample sizes. We prefer multi-level models as a compromise between the limitations of the first and third approaches, but report results from all three models for comparison.

As a first cut, we look at the general responsiveness and accuracy of local election officials, before analyzing the treatment effect of a Latino name. Overall, local officials were

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21This also reduces heterogeneity in our estimates of effect sizes, by pulling estimates towards the overall sample mean, while still allowing effects to vary by state. These models are estimated using the arm package in R.
significantly more likely to respond to the control email versus the voter ID email and were more likely to do so accurately. This is consistent with the control email being easier to answer than the voter ID email. We also find that local officials were more likely to respond accurately to the voter ID email in states without voter ID laws than those with these laws, where an accurate answer is more complicated (see Supporting Information).

4.1 Response Rate

We find strong evidence supporting our first hypothesis that the response rate to Latino names is lower than the response rate to non-Latino names. Table 3 reports results for fully pooled and partially pooled (multi-level) logistic regression models in which the outcome is whether a local official replied to our email. In Table 3 columns (1) and (2) show the results for response to both emails, while columns (3) and (4) restrict the sample to the voter ID email. In each case, Latino names are significantly less likely to receive responses than non-Latino names, even after controlling for locality characteristics.

These effects are substantively large. Panel A of Figure 1 plots the simulated first difference in the predicted probability of a response to a Latino versus non-Latino name for both email texts (Models (1) and (2)). Responses to Latino names are three-and-a-half to four percentage points less likely than to non-Latino white names. In addition, results from logistic regression models estimated separately for each state are consistent with our hypothesis, although small sample sizes within states result in large standard errors and less statistical significance. Overall, Latino names are associated with negative coefficients for 34 of the 46 states for which we estimate treatment effects. Figure 2 plots the simulated first differences in response rates between Latino and non-Latino names to voter ID questions by state for partially pooled and unpooled models.

In a second set of models, we find limited support for our second hypothesis that the gap between response rates to non-Latinos and Latinos is larger for questions about voter ID laws than a less “racialized” election-related question. Columns (5) and (6) of Table 3 show the results of fully and partially pooled models with an interaction between indicators for Latino name and the voter ID email text. The interaction term is not statistically significant in

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22The table reports the fixed effect, or average coefficient value for the Latino Name variable across all states in the partially pooled models (Gelman and Hill 2007).
23We simulate 1000 predicted probabilities for each observation in the data, following the approach in Hanmer and Kalkan (2013) for all first differences and predicted probabilities reported in the paper.
24Because of small sample sizes, interaction models for each state are not informative (results not shown).
Table 3: Response Rate By Ethnicity, Fully and Partially Pooled Models

<table>
<thead>
<tr>
<th>Model Type:</th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sample:</td>
<td>Both Emails</td>
<td>Both Emails</td>
<td>Voter ID</td>
<td>Voter ID</td>
<td>Both Emails</td>
<td>Both Emails</td>
</tr>
<tr>
<td>Latino Name</td>
<td>$-0.20^{***}$</td>
<td>$-0.21^{***}$</td>
<td>$-0.26^{***}$</td>
<td>$-0.28^{***}$</td>
<td>$-0.13$</td>
<td>$-0.13$</td>
</tr>
<tr>
<td></td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>(0.08)</td>
<td>(0.08)</td>
<td>(0.08)</td>
<td>(0.08)</td>
</tr>
<tr>
<td>Voter ID Email</td>
<td>$-0.32^{***}$</td>
<td>$-0.32^{***}$</td>
<td>(0.06)</td>
<td>(0.06)</td>
<td>$-0.25^{**}$</td>
<td>$-0.25^{**}$</td>
</tr>
<tr>
<td>% Not Latino (County/Town)</td>
<td>0.50</td>
<td>0.57</td>
<td>0.75</td>
<td>0.85·</td>
<td>0.50</td>
<td>0.56</td>
</tr>
<tr>
<td></td>
<td>(0.44)</td>
<td>(0.39)</td>
<td>(0.62)</td>
<td>(0.51)</td>
<td>(0.44)</td>
<td>(0.39)</td>
</tr>
<tr>
<td>Obama Vote, 2008 (County/Town)</td>
<td>$-0.69^{*}$</td>
<td>$-0.54^{*}$</td>
<td>$-0.33$</td>
<td>$-0.21$</td>
<td>$-0.69^{*}$</td>
<td>$-0.54^{*}$</td>
</tr>
<tr>
<td>% Black (County/Town)</td>
<td>$-0.24$</td>
<td>$-0.59$</td>
<td>$-0.51$</td>
<td>$-0.67$</td>
<td>$-0.25$</td>
<td>$-0.57$</td>
</tr>
<tr>
<td></td>
<td>(0.44)</td>
<td>(0.37)</td>
<td>(0.59)</td>
<td>(0.47)</td>
<td>(0.44)</td>
<td>(0.37)</td>
</tr>
<tr>
<td>ln(Per Capita Income) (County/Town)</td>
<td>$0.19^{***}$</td>
<td>$0.20^{***}$</td>
<td>$0.15^{***}$</td>
<td>$0.17^{***}$</td>
<td>$0.19^{***}$</td>
<td>$0.20^{***}$</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.02)</td>
<td>(0.04)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>ln(Population Density) (County/Town)</td>
<td>$0.20^{***}$</td>
<td>$0.18^{***}$</td>
<td>$0.18^{***}$</td>
<td>$0.16^{***}$</td>
<td>$0.20^{***}$</td>
<td>$0.18^{***}$</td>
</tr>
<tr>
<td></td>
<td>(0.02)</td>
<td>(0.02)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.02)</td>
<td>(0.02)</td>
</tr>
<tr>
<td>(Intercept)</td>
<td>$-1.29$</td>
<td>$-1.67$</td>
<td>$-1.67$</td>
<td>$-1.30^{**}$</td>
<td>$-1.30^{**}$</td>
<td>$-1.30^{**}$</td>
</tr>
<tr>
<td></td>
<td>(0.47)</td>
<td>(0.61)</td>
<td>(0.61)</td>
<td>(0.47)</td>
<td>(0.47)</td>
<td>(0.47)</td>
</tr>
</tbody>
</table>

State Fixed Effects: $Y \quad -- \quad Y \quad -- \quad Y \quad --$

$N \quad 6442 \quad 6442 \quad 3188 \quad 3188 \quad 6442 \quad 6442$

$*** p < 0.001, ** p < 0.01, * p < 0.05, \cdot p < 0.1.$ Logistic regression coefficients with standard errors in parentheses. Partially pooled models have varying intercepts by state and allow the coefficient on Latino Name and its interaction with the email text indicator to vary by state, following Gelman and Hill (2007).
Figure 1: First Differences in Overall Response Rates: Panel (A) shows simulated differences between the non-Latino and Latino names in the overall predicted probability a local official responds to either email text, with 95% confidence intervals, based on the models in columns 1 and 2 of Table 3. Panel (B) shows first differences in response rate between non-Latino and Latino names broken down by each email text based on columns 5 and 6 of Table 3.
Figure 2: *First Differences in Response Rates by State*: Panel (A) shows simulated first differences and 95% confidence intervals for the state level effect of Latino name on responses to the voter ID email, based on individual state-level logistic regressions. States are ordered from left to right by sample size (see Table 1). Estimates are underpowered in states with small sample sizes. Panel (B) shows simulated first differences by state of the effects of the Latino name on response rate using the partially pooled multi-level model. In panel (B) there is estimated bias against Latino names for essentially all states.
either model. Panel B of Figure 1 shows the simulated first differences to Latino and non-Latino white names for each email text separately. Bias against Latino emailers in responses to the voter ID email text is estimated to be approximately three percentage points larger than in the control email, but the confidence intervals of these estimates overlap. In sum, while our point estimates are consistent with our second hypothesis and suggest that bias against Latino emailers may be higher for the voter ID email, this difference is not statistically significant.

Next, we find no evidence supporting our third hypothesis that states with stricter voter ID laws exhibit greater differences in response to Latinos versus non-Latinos. Testing this final hypothesis involves a non-experimental comparison of treatment effect sizes across states with different laws that were assigned endogenously. Furthermore, we have small sample sizes of effect estimates by law type (e.g., only 11 states with photo ID laws), such that we should only find evidence confirming our third hypothesis if the difference between states with and without these laws is very large. In two additional specifications, we include indicators in our fully pooled and partially pooled models for each state’s type of law, and interact these with the Latino name variable. We find no statistically significant interaction between the type of voter ID law and the Latino name variable on response rate (see Supporting Information).

4.2 Characteristics of Responses

We also investigate whether, conditional on receiving a response, the nature of responses varies between emails to Latino and non-Latino aliases. We focus only on responses to the voter ID email because these varied in their accuracy, as described in Section 3.3. Figure 3 presents both fully pooled and partially pooled estimates for: absolute accuracy, non-informativeness, and friendliness of responses.

The first panel of Figure 3 shows first differences between Latino and non-Latino emailers in the receipt of absolutely accurate responses. Latino emailers are less likely to receive an absolutely accurate response. In state-specific logistic regressions, 25 of 45 states’ estimated first differences are negative, but only two of these state-level models have 95% confidence intervals that exclude zero.

A key reason Latino emailers received fewer absolutely accurate responses appears to be

\footnote{Full model specifications and regression tables appear in the Supporting Information.}

\footnote{The total number of states drops to 45 here because we received no replies to the voter ID email from Delaware.}
that local officials asked Latino emailers for more information, rather than answering their question directly. Panel 2 of Figure 3 displays simulated first differences between Latino and non-Latino emails on non-informativeness. Latino emailers are estimated to be nearly 5% more likely to receive a non-informative response compared to non-Latino emailers. In state-specific logistic regressions, 23 of 45 states have positive estimated first differences (with Latinos less likely to receive informative responses, as above), but none of these state-level estimates attain statistical significance ($p \leq .05$).

In addition to the content of email responses, we examine whether the tone or style of responses differs to Latino and non-Latino emailers. Two-thirds of all responses to the voter ID question were coded as friendly. Panel 3 of Figure 3 shows first differences between Latino and non-Latino aliases for friendliness. Latino emailers are estimated to be several percentage points less likely to receive a friendly response, but this result falls short of standard levels of statistical significance in both the fully and partially pooled specifications. In state-specific logistic regressions, 26 of 45 states had negative simulated first differences, none of which reached statistical significance ($p \leq .05$). Taken together, Figure 3 still suggests differential

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27For example, non-informative responses involved asking our emailer for his address or asking the emailer to come into the office at a later date.
treatment of Latino and non-Latino emailers, even after conditioning on response rate.

### 4.3 Potential Mechanisms

The bias we observe in responsiveness could result from several plausible mechanisms. Public officials may not respond because they believe that Latinos are either unlikely to live in their districts or unlikely to support these officials politically. Either could be seen as “statistical discrimination,” with bias emerging from possibly rational beliefs about differences between Latino and non-Latino constituents (Altonji and Blank 1999, Butler and Broockman 2011). Alternatively, our results could be due to “taste-based discrimination,” resulting from deeper seated prejudices. While our design does not allow us to directly distinguish between these mechanisms, a series of additional specifications suggest that several plausible statistical discrimination mechanisms are insufficient to account for our results.

Under a statistical discrimination mechanism, bias in response rates could result from differences in the plausibility of our emails to different local officials. For example, public officials may respond less to emailers they rationally believe to be less likely to live in their communities. We test whether the difference in response rates to Latino versus non-Latino names depends on the percent of a municipality or county’s population that is Latino. Figure 4 plots estimated differences in response rates to Latino and non-Latino names across both email texts, by percent of the population in the municipality or county that is not Latino. We plot the simulated first difference in the probability of a response to a Latino or non-Latino name for areas with population greater than 88% not Latino (the 10th percentile in Latino population in our data). We restrict the sample to avoid unwarranted extrapolation; we do not have enough observations with large Latino populations to determine whether Latinos experience less bias in places with very high Latino populations. Estimates for localities in the highest 10% of our sample by Latino population are statistically indistinguishable from zero, with wide confidence intervals. While Figure 4 shows that bias in

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28 While this seems intuitive, officials who have more discriminatory attitudes towards Latinos (taste-based discrimination) might also be particularly likely to work in communities where few Latinos live.

29 Results are for a partially pooled (multilevel) model that interacts the Latino Name and % Not Latino variables. The regression table is in the Supporting Information.

30 Note that because our units of observation are counties or municipalities, heavily Latino areas, such as Los Angeles County, are only a single observation in our data. In addition, because we are following the approach to simulating first differences in Hammer and Kalkan (2013), it is not possible to extend the plot to values of the non-Latino population for which there is no observed data, leaving much of Figure 4 blank to the left of 88%.
response rates increases as the population becomes more non-Latino, there is still statistically significant bias against Latinos in places where as much as 10% of the population is Latino and officials plausibly interact regularly with Latino constituents. Figure 4 also does not alleviate normative concerns about discrimination; approximately 7.6 million Latinos, as measured in the 2010 Census, live in the locations included in Figure 4.

A similar statistical discrimination mechanism would hold if our findings for responsiveness are driven by officials in small towns or counties who know most of their constituents. Officials in these areas may not respond to an email from Luis Rodriguez or José Martinez because they do not believe that our emailer is their constituent. In contrast, officials in larger towns or counties likely interact regularly with constituents they have never met before. To address this concern, we re-estimate all of the results in Sections 4.1 and 4.2, dropping the municipalities in the bottom 10th percentile of our sample in terms of total population. Dropping these 646 observations has no substantive effect; all treatment effect estimates of the Latino name remain as before (see Supporting Information), indicating that our results cannot be explained by the suspicions of officials in particularly small towns or counties. Moreover, our non-Latino white surnames, Mueller and Walsh, are actually much less common in the US population than Martinez and Rodriguez. As of the 2000 Census, Rodriguez was the ninth most common surname in the entire country; Martinez was eleventh (Word et al. 2008). While these names are not evenly distributed, many clerks in small towns likely do not have constituents named Mueller (469th most common) or Walsh (267th most common) either.

Another avenue for statistical discrimination may come from the political incentives of local election officials. As discussed in Section 3.2, some of the email addresses for local election officials were for non-partisan civil servants, while others were for personal addresses of elected local politicians, such as elected county and town clerks. Of the 46 states analyzed above, in 19 emails went to civil servants, in 19 emails went to elected officials (though they may still have been answered by civil servants working for the elected officials), and in 8 states

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31 Also see http://www.census.gov/genealogy/www/data/2000surnames/.
32 Local officials may have seen that emailers were not registered in the voter registration file. For this to offer an explanation for the results, officials would need to be (a) significantly more likely to find the non-Latino (first and last) names than Latino names, and (b) be less likely to respond to an email when the emailer is not registered. Yet, there are likely many localities where no one of these specific names is registered. Moreover, we received many replies from officials to both Latino and non-Latino names in which the officials indicated they knew the emailer was not registered.
Figure 4: First Differences in Overall Response Rates by Percent of Population that is Non-Latino. Simulated differences in the response rate between Latino and non-Latino names with 95% confidence intervals at different values of percent non-Latino in each locality. Nearly 7.6 million Latinos live in the areas of the United States shown on the x-axis of this plot.
the email addresses were mixed among both types. As discussed in Butler and Broockman (2011), partisan elected officials may have political incentives to reply to constituents of different ethnicities at different rates, or with differing accuracy, if ethnicity serves as a heuristic for a constituent’s partisanship. Similar incentives do not hold for civil servants who gain no advantage from providing different information to different constituents. To test for this, we re-estimate the models for response rate and accuracy in Sections 4.1 and 4.2 and find no interaction effects between the electoral status of officials in a state and the Latino name treatment. The effect of the Latino treatment remains as before when controlling for the elected status of local officials (see Supporting Information).

### 4.4 Robustness Tests

We also conduct a series of additional tests that demonstrate the robustness of our findings to different modeling choices. First, there may be concerns that our treatment effect estimates inappropriately borrow information across states with different voter ID law types or from different geographic regions. We re-estimate the results from the fully pooled and partially pooled models for response rate and accuracy in new specifications in which we either only pool states with or without voter ID laws or only pool within geographic regions. While the smaller samples lead to larger standard errors, the majority of results remain signed in the same directions with the exception of fully pooled models for the South for response rate and the West for accuracy (see Supporting Information). In addition, we re-estimate the partially pooled multi-level models from Section 4.1, but add a third level of clustering – either geographic region or law type – at which intercepts and effect estimates are also allowed to vary. These new partially pooled models are substantively consistent with the results reported in Section 4.1 (see Supporting Information).

Second, because of differences across states in the level at which elections are administered – municipal versus county – several states have very large sample sizes. There may be

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33 We are unable to identify the partisanship of individual officials within these states, however. The states with a mix of elected and unelected email recipients were California, Massachusetts, Michigan, New Hampshire, North Dakota, Oregon, Texas, and Washington.

34 Also see Burden et al. (2010, 2011) for research on differences in the behavior of elected versus unelected local election officials.

35 In these models, we collapse election official status to a binary variable by state indicating whether at least some of the emailed officials in that state were elected.

36 When pooling by regions we use four regional classifications: West, Midwest, East, and South. Definitions are in the Supporting Information.
concern that our results are being driven primarily by one of these large sample states. We re-estimate each of the models for response rate and accuracy reported in Section 4.1 and 4.2, sequentially dropping each of the seven states with the largest sample sizes. All results are robust to dropping each of these states (see Supporting Information).

Third, as shown in Table 2, 240 emails bounced back because of an incorrect email address. One benefit of a pair matched design (discussed in Section 3.2) is that if any observation does not receive the assigned treatment, the other observation in the pair can be dropped while maintaining balance (Imai et al. 2009). Our regression models already control for covariates, but we also drop the other half of each pair in which an observation was not treated due to a bounce-back and re-estimate the results above. This reduces our overall sample size by 215 observations, but all findings remain as before (see Supporting Information).

5 Conclusions

We conducted a large-scale field experiment testing public officials’ responsiveness to requests for information about voting from constituents of different ethnicities. This design allows us to directly study the public officials tasked with providing citizens election-related information and to isolate the effect of ethnicity on their actual performance. We find strong evidence that public officials are less responsive to questions from putatively Latino emailers. In addition, the responses that Latino emailers did receive to voter ID questions were less likely than responses to non-Latino emailers to be absolutely accurate, and were more likely to be non-informative.

We also hypothesized that public officials would exhibit stronger bias against Latinos in response to voter ID questions than in response to a baseline question. We find limited evidence in favor of this hypothesis, since the estimated bias is larger for voter ID questions, but is not statistically significantly distinguishable from a control text. In addition, we expected that bias in response to voter ID questions would be greatest in the states with strict voter ID laws, but we did not find evidence in support of this hypothesis. While our design does not allow us to directly distinguish among potential mechanisms for the bias we observe, we provide evidence suggesting that statistical discrimination mechanisms such as the political incentives of local officials or differences in the plausibility of our treatment in

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37These are Connecticut, New Hampshire, Vermont, Texas, Massachusetts, Michigan, and Wisconsin.
different places cannot explain our results.

These findings have important implications for debates about voter ID laws, and indeed for any changes to voting requirements or elections administration. The behavior of the local officials tasked with implementing reforms has been shown to affect the impact of new electoral rules (Burden et al. 2010, 2011) and previous research has also shown that changes to voting procedures can decrease turnout by increasing the information costs to voting (Brady and McNulty 2011, McNulty et al. 2009, Vercellotti and Andersen 2009). Our results indicate that changes to existing voting regulations are likely to differentially increase information costs for Latino voters because public officials are less responsive to their inquiries than to non-Latinos. That we find officials respond at lower rates to Latinos even when asked a question requiring a single word answer (“No”) suggests that public officials can be biased even on topics not associated with ethnicity in public discourse and for which the costs of response are exceptionally low. While local election officials interact with constituents by means beyond email, email is an increasingly common form of constituent interaction (Smith 2009, Smith et al. 2010), and similar bias can occur in other forms of communication where local officials have discretion to reply, or to reply accurately (such as with letters, phone calls, etc.). Future reforms to electoral rules should include serious efforts to disseminate information to voters of all ethnic and socio-economic groups to offset potential bias in information provision at an individual level.

More broadly, while previous research has used audit-style field experiments to measure discrimination in the constituency service of partisan politicians (Butler and Broockman 2011), we focus on discrimination in the performance of many non-partisan local civil servants. In addition to elections, local civil servants have discretionary control over the provision of many goods and services controlled by local governments in the US – from trash collection and snow plowing to the management of welfare offices. Our results suggest that similar ethnic or racial biases may affect the quality of services delivered in these other areas. Future research can expand the use of similar experimental methods to examine the presence of bias in service delivery in many other aspects of local administration in the US. This research can encourage policy changes aimed at reducing the impacts of such discrimination.
References


