

Turning Out Unlikely Voters? A Field Experiment In The Top-Two Primary*

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Abstract: Those who turn out in American primary elections are a small and unrepresentative subset of the population. Why do citizens forgo participation in nominating contests yet vote in general elections? We argue that limited contact lowers participation in primary elections. We present results from a randomized field experiment with near 150,000 letters in California's 2014 primary. Each letter went to one of the four million Californians who had participated in recent general elections but not in primaries. We find that a single letter increased turnout by 0.5 points from a base rate of 9.3 percent. This increase is more than twice the average effect calculated in a recent meta-analysis and represents a proportional increase of 5.4 percent. Our experiment shows that registrants who typically abstain from primaries - and who are thus often ignored by campaigns - can be effectively mobilized.

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A recurrent puzzle in American politics confounds campaigners every spring or summer of an election year: Why do so many of the citizens who turn out to vote in general elections not participate in primaries? This is surprising because primaries limit the choices on offer at the general election, and limited choices at the general election may limit the eventual representation of these voters. Given that many citizens undertake the cost to participate in general elections, why do they abstain from doing so in primary elections?

Low participation in primary elections may have serious consequences for the policies implemented by American government. Uneven participation in general elections is rued as a source of unequal representation (e.g., Brady, Verba, and Schlozman, 1995; Rosenstone and Hansen, 2003), yet the problem may be even more acute in primary elections with notably lower levels of turnout (e.g., McGhee, 2014). In California, for example, turnout among registered voters in federal general elections from 2000 to 2010 averaged 65.5% while turnout in federal primary elections averaged 40.8%. Some suggest that primary elections are implicated in the increasing divergence in voting patterns between Democrats and Republicans in Congress (e.g., Fiorina and Abrams, 2009; Jacobson, 2012).

Political reformers have tried to increase participation and representativeness in primary elections by way of institutional changes to the rules governing nomination contests. Prior scholarship has argued that more open primary formats should (Gerber and Morton, 1998) and do increase turnout (Jewel, 1984; Kenney, 1983, 1986), and found mixed effects of primary rules on the representativeness or moderation of primary electorates (Hedlund, Watts, and Hedge, 1982; Geer, 1986; Norrander, 1989; Kaufmann, Gimpel, and Hoffman, 2003). Today, much of the energy of reform groups has focused on the creation of “top-two” primaries in which any voter, regardless of party affiliation, can choose from among the full list of candidates, with the two leading vote-getters advancing to November (again, without regard to the candidate’s party affiliation). Reformers hoped to increase participation by opening primaries to a wider range of voters, even independents, and by giving voters of all partisan stripes a broader range of choices. Under these rules, primary turnout becomes even more critical. Because only two candidates, potentially from a single party, move

forward to the general election, the choices for the November electorate are limited by the choices of the primary electorate. Voters in the state of Washington and in California backed propositions to institute the new rules in 2008 and in 2010 respectively, with voters rejecting the reform in Arizona in 2012 and in Oregon in 2014.

In our view, the institutional hurdle that the top-two seeks to remove is but one of two potential explanations for lower turnout in primary elections than general elections. We argue that a more likely explanation for the limited participation in primaries is lack of mobilization activity in nominating elections. A decade of findings from randomized mobilization experiments demonstrates that canvassing and letters increase the turnout of the voters whom campaigns choose to target (e.g., Gerber and Green, 2000; Green and Gerber, 2008; Gerber, Green, and Larimer, 2008; Gerber, Green, and Nickerson, 2003). Of course, the extent of mobilization depends upon the extent of targeting from campaigns. At the general election, parties and affiliated interest groups are active in addition to the candidates' campaigns themselves. At the nomination stage, in contrast, parties tend to stay on the sidelines and mobilization activity, to the extent it occurs, comes only from the candidates themselves. With finite resources, candidates often target only those voters they expect to participate in the primary, which usually means those who have voted in past primaries.

Ignoring registrants without previous record of primary turnout is a common strategy according to candidates running for statewide office in California in 2014. In a telephone interview with the authors in June 2014, one candidate for Secretary of State explained that his mailers "were not going to people who don't vote in primaries." One of his opponents told us that he "sent out two direct mail pieces to Republican voters who turn out frequently in primaries." Contacting only previous primary voters may create a vicious cycle that leaves those who have not participated in the past on the sidelines.

Thus, an empirical question is whether voters who typically do not participate in primaries will, in fact, turn out if they are the target of a mobilization campaign, or whether they habitually abstain from primaries. In this paper, we first explore whether the institutional shift to a top-two primary alone leads to an immediate boost in participation. Then we analyze the impact of a randomized

get-out-the-vote (GOTV) experiment that we ran in California's 2014 top-two primary, asking whether institutional change coupled with mobilization can bring a new set of voters to the polls in primary elections, making these key contests more representative.

Answering the first question is relatively straightforward. While institutions like the top-two primary invite broader participation, the record so far shows that they do not guarantee an increase in primary turnout or a broadening of the types of voters who participate. In the first two elections in which it was implemented in California, for example, the top-two primary did not lead to increased participation. Turnout in the June primaries was 31.1% in 2012 and 25.1% in 2014, down from 28.2% and 33.3% in the 2008 and the 2010 congressional primaries (the former of which was held separately from the presidential primary). In the next section, we examine the composition of these electorates more closely to show that the top-two did not notably broaden the primary electorate along demographic or partisan lines. While the full impact of this reform will not become clear until campaigns adjust their mobilization tactics, to date the top-two reform has not delivered on its promise to increase participation in California.

To explore our argument that spurring new voters to participate in primaries requires contact, we then present results from a large-scale, block-randomized field experiment surrounding the June 3, 2014 top-two congressional primary election in California. We partnered with a non-partisan group to send letters to a random sample of 150,000 Californians who had voted in the 2012 general election but had not voted in any recent primary election. We sent each recipient one of three letters designed to test three types of contact that might increase participation.

We find that all three letters had similar effects on turnout for our target population of registrants who had not previously participated in primary elections. Our average treatment effect is about 0.5 percentage points, more than twice the average effect of 0.2 percentage points from a recent meta-analysis of 79 non-partisan mailings (Green, McGrath, and Aronow, 2013, p. 31).¹ With

¹ While this meta-analytic averages estimates non-partisan mail treatment effects across many different contexts, elections, and target populations, it seems a reasonable benchmark of comparison. Although our target population of non-primary voters likely has a lower propensity to vote than the average target populations in these studies, the meta-analysis of Enos, Fowler, and Vavreck (2014) found that GOTV treatments are *less* effective on lower-propensity relative to higher-propensity voters. This suggests that if our target population is low-propensity, the meta-analytic average of 0.2 may be an upper bound to a reasonable prior for the treatment effect in our study.

a base rate of turnout for this group in this election of 9.3 percent, our single-letter treatments had a proportional increase on turnout of about five percent. We then investigate heterogeneous treatment effects, which we had pre-registered, as well as persistence into the November general election.

The central result from our analysis is that these voters who would normally not be contacted by campaigns or election administrators because they hadn't previously voted in primaries ("low-propensity voters" in the parlance of campaigns) respond to simple informational letters to the same or even greater degree as citizens respond to such letters in general elections. Thus, while institutional reform to nomination procedures may be part of increasing participation in primary elections and making them more representative, our results suggest that new voters must be contacted and encouraged. Increasing participation in top-two primary elections may be especially important given the higher stakes under this nominating institution, but mobilizing still requires outreach.

This article makes three contributions. First, we use a rigorous, large, randomized experiment to show that citizens who do not participate in primary elections can be brought to the polls, the first such experiment to our knowledge in a top-two primary. We show that what appears to matter is contact of any kind, not specific information or motivations – at least those that we tested. This evidence explains why institutional reforms have not yet, by themselves, delivered on promises of reforming participation in primary elections, and suggests pathways for new efforts to translate institutional reform to individual behavior. Second, we use the large number of registrants sent treatment letters in our experiment to explore heterogeneity in treatment effects; we ask both whether a specific mobilizing message works better, and also whether certain types of registrant are more responsive to mobilization. While one might expect variation in responsiveness to these letters about the top-two institution by characteristics such as partisanship or age, we find quite limited heterogeneity. This suggests that what differentiates voters from non-voters is larger than what differentiates within the non-voter population.

Finally, and perhaps most importantly, our results show that the set of citizens who participate

in primaries is not a fixed group of the most politically active, to the exclusion of all others. Rather, the fact that we can bring to the polls registrants who normally abstain from primaries by sending each a single letter suggests that turnout in primaries is dynamic and contingent on context and campaign activity. It is reasonable to extend our results to believe that many more citizens might participate in primary elections if they are provided the information and motivation to do so, but that this information and motivation must be fostered. Few elite actors have had the motivation to foster broad participation in closed primary institutions but, especially under top-two primary systems, an avenue may now be open for entrepreneurial candidates and campaigns to create new coalitions of primary voters.

Our essay proceeds as follows. First, we detail the history of the top-two primary reform, its motivations and initial results. We then explore how turnout varied with the implementation of the top-two primary in California across individual-level characteristics such as age and party. We then present the theories of non-participation that motivated the design of our field experiment, present that design and the results. We explore whether the treatment effect varies with the type of individual receiving it or by the type of letter, and offer a discussion and concluding remarks.

A new institution ... but little change in participation

The most recent round of primary election reforms began when “blanket primaries” in Alaska, California, and Washington were struck down as unconstitutional with the Supreme Court’s *Democratic Party v. Jones* in 2000. After this decision, California instituted a “semiclosed” primary in which registered partisans would vote only in their party’s nominating contests, while independent voters could take part in a primary if and only if a party chose to let them in. Concerned that this left nonpartisans with no guaranteed voice, and that partisan primaries led to the election of ideologically extreme candidates, advocates proposed the top-two as a constitutionally permissible

solution to both problems.² Following the example of the state of Washington,³ California adopted the top-two through the 2010 Proposition 14, a measure placed on the ballot by legislators as part of a concession in budget negotiations with a moderate Republican lawmaker.

Advocates of the top-two made a clear prediction that it would lead to an increase in voter turnout, especially among those who were not registered with a major party and thus given a limited role under the existing system. For instance, during the campaign for Proposition 14, the nonprofit organization California Forward released a report by T. Anthony Quinn and R. Michael Alvarez with a press release under the headline “Primary Reform Could Boost Voter Participation.” It began, “According to a new report published by California Forward, a non-partisan ‘top two’ primary system would likely boost turnout of independent voters as well as voters registered with particular parties.”⁴ A prominent group supporting Proposition 14, the Independent Voter Project, claimed soon after its passage that, “The polarization that is fostered by low primary election voter turnout is reduced by Prop 14,” and that “the open primary will increase voter turnout.”⁵

Have the predictions of top-two advocates been realized?

As we presented above, turnout in the first two federal primaries held under the top-two institution in California was lower than in the two congressional primaries held prior to reform – 28.2% and 33.3% of registered voters in 2008 and 2010 under the old rules, compared to 31.1% and 25.1% in 2012 and 2014 under the top-two rules. We present here individual analysis that fails to provide evidence in support of its second goal, broadening the diversity of those who participate in primary elections. To do so, we analyze individual turnout decisions from the California statewide voter file. We use voter files produced relatively proximate to each election to minimize the influence

² The blanket primary was overturned in *Democratic Party v. Jones* because, as a nominating contest, it forced parties to associate themselves with voters from other parties or from no parties. Because the top-two does not select party nominees - it merely selects two November contestants, regardless of party, the US Supreme Court upheld its constitutionality in *Washington State Grange v. Washington Republican Party* 2008.

³ Washington voters backed the top two as Initiative Measure 872 in 2004, but because of a legal challenge by the state’s parties, it was not implemented until 2008.

⁴ See <http://www.cafwd.org/press-releases/entry/primary-reform-could-boost-voter-participation>.

⁵ See Peace, 2010, <http://ivn.us/2012/05/10/the-parties-are-afraid-of-californias-new-open-primary/#openprimary>.

of attrition on analysis of turnout.⁶ Each file contains the records for around 17 million registered voters in California, along with their history of turnout in recent elections and individual characteristics. We investigate how the predictors of turnout changed across these four primary elections under the old and new institutional rules.

We present in Figure 1 a slopeplot of turnout for different age and party groups in congressional primaries. The left side of the figure plots turnout under the old rules in the congressional primaries of 2008 and 2010, and the right side turnout under the top-two institution in 2012 and 2014. Each line connects turnout for the same group between the two institutions. For example, the line at the top of the plot connects the turnout of those aged 70 to 79 at the time of each election, which was 58.6 percent in 2008 and 2010 and declined to 56.4 percent in 2012 and 2014.

Figure 1 shows that in its early implementation the top-two primary has been accompanied by a decrease in turnout in all but one of these demographic groups. The only group to see an increase in turnout is the group of 18-19 year olds, moving from a rate of 7.0 percent to a rate of 8.2 percent. The overall decline in turnout under the top-two institution did not occur exactly the same across all groups, but is relatively constant. Two groups that were predicted by advocates to increase their participation in response to this reform – those registered with third parties or “no-party-preference” registrants (independents) who were not guaranteed a vote in any party’s primary before the move to the top-two – also show declines in turnout (18.9 to 15.9 and 17.7 to 17.2, respectively).⁷ Thus, not only did the top two generate an increase participation in the aggregate, it does not appear to have increased participation among those whom one might think would be most likely to respond.⁸

We view these results as descriptive motivation for our field experiment. Other factors that have changed over time in California are certainly relevant for changes in turnout, and we cannot attribute the drop in turnout to the causal impact of the top-two rules. What we can conclude is

⁶ We use files from August 2014, January 2013, January 2011, and March 2009 to characterize the four elections. As voter files are a snapshot, the 2014 file misses many of the records for those who turned out in older elections.

⁷ McGhee (2014) argues that while the top two did not appear to bring more voters to the polls, it did give the independent voters who participated a more consequential role in candidate contests.

⁸ In Supplemental Table S1, we present pooled multiple regression models predicting turnout using these data. The regression coefficients confirm the graphical presentation of Figure 1.

that, in its initial implementation in one state, the top-two primary has neither increased aggregate turnout in primary elections, nor has it appeared to broaden the partisan composition of those who do participate. In the next section, we present our field experiment testing whether contact of three different flavors increases participation in the top-two primary.

An experiment to mobilize primary turnout

The central question that our experiment is designed to answer is this: Are voters who typically abstain from primaries but have voted in general elections responsive to a mobilization message delivered in a single letter?⁹ Past research has shown that this sort of treatment applied to wider populations of voters in general elections brings, on average, a 0.2 percentage point increase in turnout (Green, McGrath, and Aronow, 2013, p. 31).

One argument is that voters who received our appeal are mobilized to a similar extent as general election voters, indicating that many registrants stay home due to lack of contact or knowledge about primary elections. Alternatively, the conventional wisdom of campaign professionals may hold: Attempting to mobilize voters who had not previously voted in a primary is a lost cause. To test this argument, we ran a randomized field experiment to estimate the causal effect of campaign contact on turnout among primary non-voters. We partnered with California Common Cause, a non-partisan non-profit organization dedicated to fostering transparency and accountability in political institutions, to deliver these letters on their letterhead. The main analysis of our experiment compares registrants that we sent a letter to against a control group without contact to test our hypothesis.

We also designed three variants of our treatment to explore the mechanisms through which mobilization, if it indeed occurs, takes place. All three letters deliver basic contact, but each is designed to test a different mechanism that might limit the influence of the top-two institution on individual turnout behavior. We note that these mechanisms are not necessarily specific to the top-two institution, but that the functioning of the top-two nomination system in many ways amplifies

⁹ Others have fielded experiments during primary campaigns, but usually with the goal of testing messages not specific to primary elections (e.g. Gerber, Green, and Larimer, 2008; Nickerson and Rogers, 2010; Panagopoulos, 2011).

their relevance.¹⁰ We now describe the theoretical motivation behind each mechanism.

The first mechanism we consider is that non-voters are in general not contacted by interested parties during the campaign, and are not otherwise urged to participate. They may not be attentive to advertising, perhaps even unaware that there is an upcoming election. Without contact, nothing drives them to the polls. As we noted above, many candidates and election professionals say that they do not waste resources on contacting voters that they do not expect to vote. Registrants without a record of participating in previous primaries may receive little or no communication about the primary election, and research consistently shows campaign contact of many kinds is an important influence on turnout (e.g., Caldeira and Patterson, 1982; Green and Gerber, 2008; Holbrook and McClurg, 2005).

If this mechanism is at work, then we will find that contact through our first letter, which provides basic information about the election and a reminder to vote, is just as effective as the more detailed letters described below. We present examples of all three letters in whole in the Supplemental Information, but in this section provide a brief description. The basic election information letter introduces the upcoming election date, poll hours, rules for absentee ballots (because a large proportion of our target population was registered to vote by mail), and encourages the registrant to participate. The other two letters include exactly the same text as the basic letter, but also include additional information intended to test one of other mechanisms.

Our second candidate mechanism is that non-voters may be confused about the new primary institution. California has changed its primary rules three times in the last 16 years and the top-two institution is anything but simple to understand. Especially for citizens who pay limited attention to political news and are on the margins of turning out or not election to election, these changes may be sufficiently confusing to deter participation. Further, many non-voters may not appreciate the greater importance of the top-two primary in generating the limited set of two choices in the general election. Our second treatment letter provides the same basic information as our first letter,

¹⁰ Our experiment targeted voters who had previously participated in general elections but not in primary elections. This suggests that two factors that might keep those who have never voted from participating, familiarity and comfort with the voting process (e.g., Gerber et al., 2013) or comfort with the political system broadly, were not impediments to voting for this set of registrants.

but also information on the functioning and importance of the top-two institution. It explains how the top-two primary works, starting a paragraph with “All Californians, regardless of party of registration, now have a voice in primary elections.” The paragraph references Proposition 14, and then gives a bullet list of important features of the reform: “All voters can now vote in the primary election”; “All candidates appear on one ballot”; “Candidates are not nominated by the party”; “Any voter can vote for any candidate”; “The two candidates with the most votes move on to the General Election in November.”¹¹ If voters have been deterred from turning out in past primaries because of confusion about their rules and their importance, they should turn out at the highest rates when they are sent this letter.

The third potential mechanism is that non-voters may not appreciate the low level of turnout in primary elections, and especially of other voters who share their preferences. Many non-voters may feel that the primary election is for others to worry about, and that they will engage at the general election. An alternative mechanism is lack of knowledge about low levels of primary turnout (often known as negative social norms), and specifically among partisan groups that seem particularly relevant in nominating elections. Our third treatment letter provides the same basic information as our first letter, but also information about the level of turnout in the 2012 congressional primary for each registrant’s party of registration, and encourages them to think about the importance of turnout for those who are like them. Specifically, the partisan identity letter added two paragraphs to the content of the informational letter. The first presented the rate of turnout for registrants of the same party as the recipient from the June 2012 top-two primary: “In the last California primary election, held in 2012, only [YY]% of voters like you registered [Party label] turned out to vote and make their voices heard. Voting in the primary election determines the candidates for the general election.” The turnout rates presented were 15.8 (registered with third parties), 18.5 percent (no party preference), 28.6 percent (Democrats), and 37.7 percent (Republicans). The second paragraph was a more standard encouragement to vote so that the voter’s voice be heard, “Your voice starts with your vote. As a voter, you help decide who will lead us.”

¹¹ This content was borrowed from other efforts by Common Cause and the Los Angeles City Clerk to explain the top-two primary in previous election outreach.

We designed the third letter with the goal of motivating individuals to participate through delivering information not only about the low level of turnout in the previous primary election, but also by an appeal to what may be part of their political identity, their party of registration. We suspected that registrants who had previously not voted in primaries might be more likely to do so when they learned how few who shared their partisan identity were participating in these elections. If a sense of the importance of participating in primaries as a partisan group is important to voters, then this third letter should provide the greatest stimulus to turnout.¹² We also note, however, that this letter includes a common exhortation common for civic duty treatments, and so the overall effect should be interpreted in this light.

Experimental design

To test to what extent basic contact and each of these potential factors of non-participation might mobilize registrants to primary elections, we ran our field experiment during the June 3, 2014 congressional primary election in California. The target population of our experiment was general election voters who had not recently participated in primary elections. To a random sample of such voters, we sent one of three treatment letters connected to the three theories of non-participation. Our research design is, first, to compare the effect of each letter on turnout to those randomly assigned to receive no letter, and, second, to compare the three letters to each other. This experiment evaluates two questions: can primary non-voters be brought to the polls by simple election communication? And, are some messages related to factors of non-participation more effective than others on these primary non-voters?

To implement our field experiment, we enumerated a target population from the March, 2014 California statewide voter file. We selected registrants who had voted in the 2012 presidential

¹² We were aware at the time of the design that presenting this descriptive social norm of low turnout may lead to a norm-consistent behavior of not voting (e.g., Cialdini, Reno, and Kallgren, 1990; Gerber and Rogers, 2009). We felt that level of turnout among the recipient's party of registration was a relevant fact, and absence of this knowledge might be keeping some potential voters at home. In fact, we find that those receiving this letter turn out at higher rates than control registrants, showing that this descriptive norm did not lead to lower turnout behavior on average. Of course, the norm-consistent behavior may depend on whether the level of turnout presented in the letter was higher or lower than the recipient's prior belief; if higher than expected, it might imply norm-consistent behavior is to vote, while if lower norm-consistent may be to abstain. Interestingly, we find heterogeneity in the effect of this letter, with larger effects on older registrants not registered as Democrats or Republicans.

general election, but who had no record of a vote in any 2012, 2010, or 2008 statewide primary election. These registrants did not vote in either the June 2008 primary for congressional and state offices (a relatively humdrum affair that attracted only 28% of registered voters) or in the February 2008 presidential primary (a galvanizing contest that drew the turnout of 58% of registrants). Our target population, then, is made up of November voters who have habitually abstained from primaries, even when the stakes are high and the contests close, as in 2008. We also removed records without a ZIP code as our treatments were sent by mail. Finally, we checked addresses of all those in our target population with a vendor who maintains lists of non-mailable addresses, residential moves, deaths, and incarcerations, and we removed from the target population any registrant who failed these checks. These selection criteria reduced the set of registrants from the full file's 17.65 million to our target population of 3.87 million registrants.¹³

Because we hypothesized that different parts of the population might be more hindered by different factors of non-participation, we assigned treatment via block randomization. We blocked on age, party, individual 2010 and 2008 general election vote history, district competitiveness, and whether or not a district had a plurality or voters from an ethnic or racial minority. These variables, along with our rates of treatment assignment, led to 382 blocks. Within each we assigned registrants to receive one of our three letters or a no-contact control condition.¹⁴

We assigned 149,596 of the 3.87 million in our target population to receive a single letter, sending twice as many of the top-two information and partisan identity letters as the election information letter. We contracted with a mail firm to send our letters on Common Cause letterhead and with Common Cause as the sender. Letters were mailed on Thursday, May 22 for zip codes in Northern California and on Friday, May 23 for zip codes in Southern California, as the letters were put in the mail from Southern California and sent with non-profit postage. With Memorial Day on Monday, May 26 and mail ballots having to be received on election day Tuesday, June 6, we sent letters early enough so that absentee voters would receive our treatment letter in time. With so

¹³ Full details of the number of cases removed at each of these stages is available in the Supplemental Information.

¹⁴ We collapsed blocks that were not of sufficient size for our assignment rates along with leftover cases from blocks of sufficient size into one leftover block where treatment was made by simple random assignment.

many California voters casting ballots through the mail – including a record 69% of all participating voters in June 2014¹⁵ – campaigns typically attempt to mobilize voters in time for them to mail a ballot that will arrive by election day. We followed this strategy in our mobilization efforts, and test letters sent to addressees in multiple cities in both Northern and Southern California arrived prior to the date that the ballot needed to be in the mail.

Our outcome of interest is whether or not the registrant cast a ballot in the primary election. We acquired the August, 2014 version of the statewide voter file and merged the records back to our target population. We successfully matched 98.8 percent of our target population to a corresponding record in the August file. To be conservative, we record turnout for those we do not match as zero and include those cases in all analysis. Turnout in the control group of this target population was 9.3 percent, compared to overall turnout in this election of 25.2 percent. This suggests we successfully targeted lower-propensity non-primary voters, though note that 9.3 percent turnout in the control shows that not having voted in a past primary before does not mean the registrant does not do so in the future.

Letters to primary non-voters mobilize them to vote

In this section, we present the results of the experiment, estimating the intent-to-treat effect of being assigned to receive each of the three letters, and subsequently explore whether that effect varies across individual characteristics. We find that each letter stimulated turnout by about 0.5 percentage points, moving it from a baseline of 9.3 percent in our control group to 9.8 percent for those in any of our treatment groups. Consistent with an argument that any contact is relevant for non-primary voters who are rarely contacted, we find this effect to be remarkably similar across the three letters and across various characteristics of individuals.

In Table 1, we present the basic results of the experiment. Each cell presents the rate of turnout for that group assigned to that condition in our experiment, along with a standard error and num-

¹⁵See the California Secretary of State's July 11, 2014 press release, "June Primary Results Certified, Showing Record-Low Turnout and Record-High Vote-by-Mail Rate," accessed at <http://www.sos.ca.gov/administration/news-releases-and-advisories/2014-news-releases-and-advisories/db14-057/> in August, 2015.

ber of observations. For example, the upper left cell shows that the 3.7 million registrants in our target population not sent any letter turned out at a rate of 9.3 percent in this election. The other four columns present results of the experiment for those registered decline-to-state or no-party-preference (which we will notate henceforth NPP), registered Democrat (DEM), registered Republican (REP), or registered with third or other parties (OTH). Registrants with these party preferences not sent letters turned out at rates of 8.6, 9.0, 10.6, and 9.2 percent, respectively. (Baseline turnout among Republicans may have been higher than for members of other parties because two credible candidates from that party - Neel Kashkari and Tim Donnelly - contested the governor's race, while Democratic incumbent governor Jerry Brown faced no serious opposition within his party.) The second row tabulates the turnout among these groups for the 149,596 registrants sent any of our three letters. Overall, those sent a letter turned out at a rate of 9.8 percent, yielding an average treatment effect of 0.5 percentage points. The average effect is relatively constant across parties, with difference of means estimates of 0.4, 0.4, 0.7, and 0.7.

Did one letter mobilize better than other letters?

The final three rows of Table 1 present the results for the three letters separately. The first column shows the uniformity of treatment effects, with all three letters increasing turnout among our target population to 9.8 percent.¹⁶ Looking across parties suggests modest heterogeneity. No-party-preference and other party registrants appear most responsive to the election info letter, while Republicans appear less responsive to the election info letter. Difference of proportions tests, however, do not yield statistically significant effects in any comparison, as we present in the note to the table.

In summary, our average treatment effects show that one letter sent to registrants who had not previously voted in primary elections increased their turnout in the 2014 June California primary election by 0.5 percentage points, a proportional increase of five percent. This effect size is twice

¹⁶ In Supplemental Table S2, we present OLS regressions to estimate the average treatment effect, which allow us to include randomization block fixed effects. In the first column we present the treatment effect estimates for the three letters: 0.50, 0.45, and 0.53. In column two, we include block fixed effects, and estimate identical treatment effects. In columns three and four, we test for difference between the three treatment letters, finding no statistically distinguishable difference with or without block fixed effects.

as large as the average effect of one mailer to voters in general elections. We interpret this effect as evidence that non-voters are available for mobilization to a similar degree to more consistent voters. We find this effect for all three of our letters, with no variation in the effect of specific mobilizing messages. The lack of large heterogeneity across our three messages suggests that any contact to those not normally mobilized can stimulate participation. In the next section, we test more thoroughly whether the effects vary by characteristics of the individual.

Are certain types of people more responsive to the mobilization?

In Figures 2 and 3, we present heterogeneous treatment effects by party, age, and turnout history.¹⁷ Each point and confidence interval are derived from an OLS within fixed effects estimator, where the point is the coefficient estimate for that treatment and the confidence interval extends to plus or minus 1.96 standard errors. The first set of four points in Figure 2 plots the average treatment effects of being sent any letter, being sent election info, being sent top-two info, or being sent partisan turnout against being sent no letter. As the results above highlight, for the full sample these effects are around 0.5 percentage points for each letter with little apparent heterogeneity. The next set of four points presents the effects for Republicans, which show the suggestive evidence of a smaller effect on election info. The fourth and fifth set of points shows that no-party-preference and other party registrants appear to be more responsive to the election information letter than the other letters.

In Figure 3, we present heterogeneous treatment effects by groups of registrant age (the same age groupings used in our block randomization) and individual turnout history in recent general elections. We again include the overall effects to the far left for reference. There is some evidence that the letters were more effective for registrants aged 69+ and for those who did not vote in 2010 or 2008 than for registrants who voted in either or both of these elections (final two sets of effects to the right of the figure). Recall that all of our target population voted in the 2012 presidential general election.

¹⁷ We preregistered heterogeneous treatment effect analysis prior to receiving the results at <http://e-gap.org>, and also anticipated this analysis through the block randomization of our experimental design. We analyze heterogeneous treatment effects by two pre-registered district variables in a companion article.

In Table 2, we present OLS estimates of these heterogeneous treatment effects with each covariate of interest considered together in a multiple regression setting. We include the direct effect of each of these blocking variables, along with the interaction of each with an indicator for the registrant having been assigned to receive any of our three letters. In the first column we present the basic specification without heterogeneous effects, estimating a direct effect of being sent any letter of 0.5 percentage points. In the second column, we specify age as a linear term, and in columns three and four age as the seven categories we used in the block randomization. We find statistically significant larger treatment effects for registrants aged 59-68 and registrants who did not vote in the 2008 or 2010 general election (relative to the excluded category of 2008 or 2010 voters). For those aged 59-68, we estimate an average effect of 0.75 points, and the point estimate for those aged 69+ is 0.64. This effect for previous non-voters, however, falls in magnitude and below statistical significance when we control for the registrant's date of registration in column four (which adds variables measuring date of registration that we did not block upon). These interactions suggest this apparent effect of turnout history has more to do with how long the registrant has been registered at the address than their vote history.

Overall, the results in Table 2 suggest little variation in the treatment effect of any letter across types of people but for larger effects for older registrants. In Supplemental Table S3, we estimate heterogeneity across covariates for each letter separately. We again find little evidence of significant heterogeneity, though find large point estimates for (a) other party registrants receiving the election information letter [coefficient of 0.89]; (b) those aged 59-68 receiving the top-two letter or the partisan letter [0.69 and 0.71]; (c) those aged 29-38 receiving the partisan letter [0.69]; and (d) 69+ receiving the partisan letter [1.33], which is our largest observed treatment effect and the only in this set statistically significant at $p < .05$. Apparently the oldest set of registrants who haven't previously voted in primaries are particularly responsive to learning about low primary turnout among their partisan group.¹⁸

¹⁸ We show in Supplemental Figure S1 that the point estimate for the effect of the partisan letter is greater than 2 percentage points for those aged 71 to 78, and in Supplemental Figure S2 that the effect is especially driven by those registered no party preference, with a point estimate above 4 percentage points.

In summary, we find limited evidence of variation in treatment effects by characteristics of the individual. The main result is remarkably consistent across types of voters. That said, we find some evidence of a larger effect of our treatments among older registrants. These older registrants appear particularly responsive to the partisan turnout treatment letter, and we find evidence that older NPP registrants are most responsive. We also find some suggestive evidence that NPP and other party registrants were more responsive to the simple election information letter, which would be consistent with our argument that these registrants are rarely contacted by outside actors surrounding primary elections.

Treatment effects in 2014 general election

In Supplemental Information Section [E](#), we present all tables and figures for the treatment effect of our letters and variation by individual characteristics and letter for the 2014 November general election. While all of our treatments were designed to mobilize turnout to the top two primary election in June, registrants who learned more about the election process or engaged with the candidates may have been more likely to vote in the following general election. Although we do not estimate any average treatment effect in the general election, we do continue to find evidence that older registrants were especially responsive to our letters. In Supplemental Figures [S3](#) and [S4](#) and Supplemental Table [S6](#), we find that those aged 59 and older were about 0.5 points more likely to turn out in the 2014 general election when assigned to receive any of the three letters. The analysis of each letter separately suggests this is more driven by the top two and partisan letters than the election information.

The continued responsiveness of these older registrants in our view merits replication. On the one hand, it may be surprising to see that it is possible to teach older voters a new trick. On the other hand, the overall higher propensity of older voters to turn out in any election may make them more likely to be marginal with respect to turnout in this primary, and thus more responsive to mobilization appeals, the rationale laid out in Arceneaux and Nickerson (2009).

Discussion and conclusion

Despite the central place that elections hold in American government and representation, many citizens choose to abstain from participation. Why they do so in general elections has been a central question of political science for decades. Scholars have fielded thousands of surveys, run hundreds of experiments, and developed dozens of theories to understand why people do and do not vote in November. At the same time, much less attention has been devoted to studying turnout in elections of potentially equal import, primary elections. Because they feature lower turnout than general elections, and because the composition of primary electorates does not reflect the eligible electorate as a whole, primaries are the contests in which abstention may have its most profound political and policy effects. They merit the sustained focus of scholars studying mobilization, participation, and representation.

Primary elections have not escaped the attention of reform advocates, who have put propositions creating top-two primaries on the ballot in four states and successfully implemented it in two of them. Importantly, these institutional changes increase the stakes of primaries, because in many contests they fill the two positions in the general election with candidates from the same party. This makes primary turnout – which reformers expected to increase through the new rules – all the more vital. Our analysis shows that, at least in the first two rounds of the top-two primaries implementation in the nation’s largest state, institutional change alone has not led to increased turnout. Instead, we show that it has been accompanied by a drop in turnout during both 2012 and in 2014 for nearly every demographic group. California built a new primary that gave voters a broader field of candidates, but they did not come to take part in it.

Our field experiment in the June 3, 2014 primary tests whether mobilization combined with this institutional reform can help realize its promise of increasing primary turnout. By focusing on a spring contest, we add to the small set of experiments conducted in primary elections. Despite increasing focus in political science on primary elections in an age of polarized parties (e.g., Hill, N.d.; Hirano et al., 2010; Jacobson, 2012; McGhee et al., 2014), most field experiments on turnout have focused on general elections or on general mechanisms in local and primary elections.

We implement novel messages for our field experiment specific to primary elections and top-two primary elections.

We also find – in contrast to conventional political wisdom – that mobilization can be as effective for voters who regularly skip primaries as it is for registrants in general elections. Even though (or perhaps because) campaigns typically do not target registrants who have not participated in recent primaries, we find that they are even more responsive to receiving a letter than voters typically are in general elections, increasing their turnout by 0.5 percentage points. This adds to a growing body of work that looks at the marginal impact of mobilization appeals on registrants with varying propensities to vote (e.g., Arceneaux, Mullin, and Kousser, 2012; Arceneaux and Nickerson, 2009; Enos, Fowler, and Vavreck, 2014). It is also analogous to recent work on the participation of ex-felons (Gerber et al., 2014), because our results show that mobilizing a group that is often ignored by campaigns can lead to a significant relative increase in participation by this segment of the electorate – in our case, 5% – relative to the group’s baseline level of turnout. One group in particular within our study population, those who do not register with a major party, exhibits the lowest baseline levels of turnout. These registrants, whose participation has been the focus of recent reform efforts such as the top-two primary, particularly deserve further study.

That said, campaigns with limited resources must likely evaluate the cost-effectiveness of targeting each group. Given our estimated treatment effect for sending any letter, the costs of producing the mailing, postage, and acquisition and cleaning of voter file, we calculate a cost per vote of \$48.55. This is about 25% less than normal cost per vote of treatments delivered by mail (Green and Gerber, 2008, p. 72). Even though we find that targeting previous non-voters can be effective in absolute terms for turnout, candidates likely vary in their ability to capture the votes of this group of potential voters when they do vote. It may still be more cost-effective for some candidates to target higher-propensity voters who are their supporters. However, our results highlight that for some candidates, these non-primary voters may be available to add to their coalition.

Regarding theories of political participation, we have shown that among the set of voters who are willing to bear the cost of voting in general elections but have not voted in recent primary con-

tests, there is some subset that is available for mobilization through simple contact. This suggests that part of what leads to lower turnout in primary elections is lower campaign activity. Just as in general elections it appears citizens are more likely to vote when asked to do so. Our treatments are probably a lower bound on these effects. A single letter on plain white paper from a non-partisan interest group is a pretty mild treatment compared to other GOTV appeals that primary campaigns might employ.

The results of our study suggest that voters who abstain from primaries are too often ignored by primary campaigns. These voters can be mobilized as effectively as voters in general elections, with our treatment effect actually larger than usual for a non-partisan mailer. Although the implementation of a top-two primary alone did not boost turnout, the combination of institutional reform and targeted mobilization can increase participation in America's primaries. Though our mobilization experiment was conducted through a nonpartisan organization, we think our results should encourage entrepreneurial candidates who seek to create new coalitions of primary voters. Such a strategy may be especially effective in places with top-two primaries. It may be a combination of institutional reform, incentives for candidates, and traditional campaign outreach that brings participation in primary elections to a level commensurate with their importance to modern representative American democracy.

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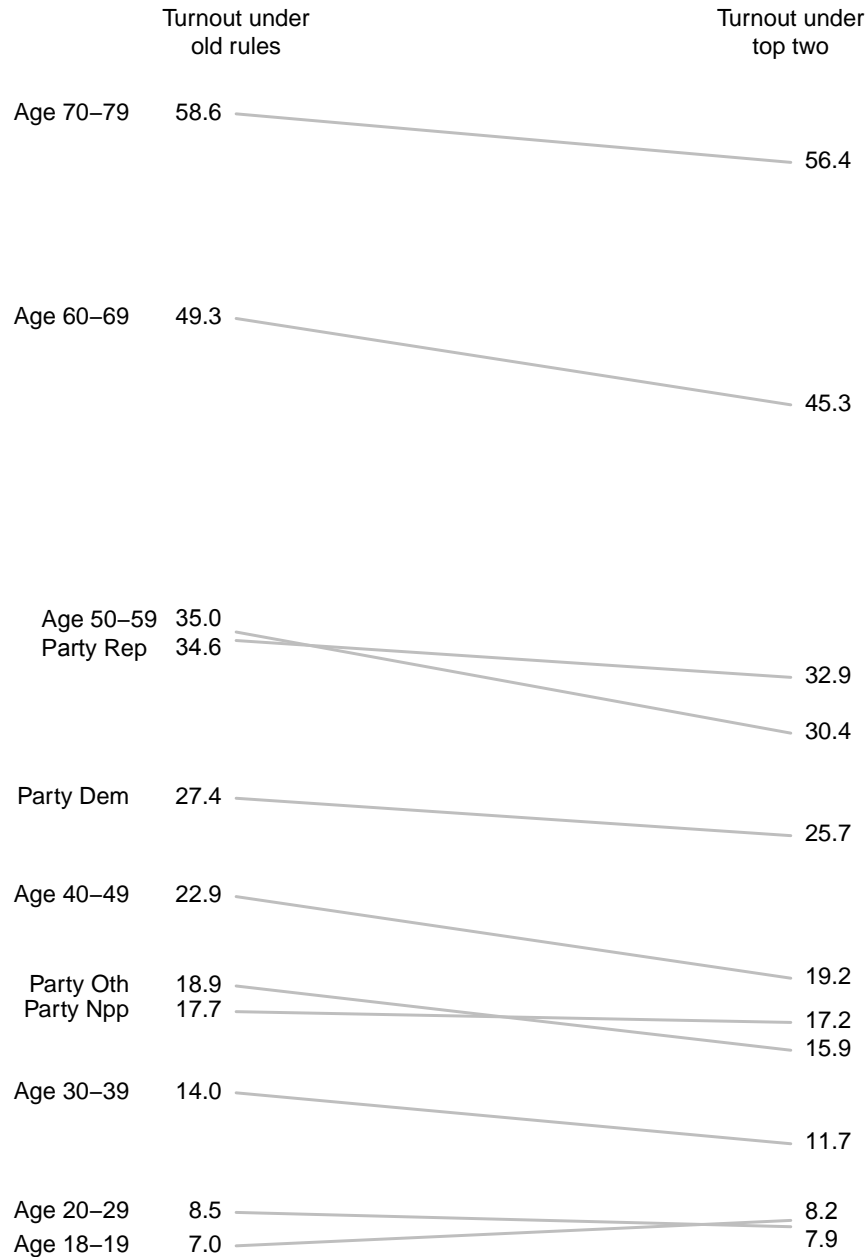
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Figure 1: Turnout by group in top two and traditional primaries in California, 2008 to 2014 (non-experimental)



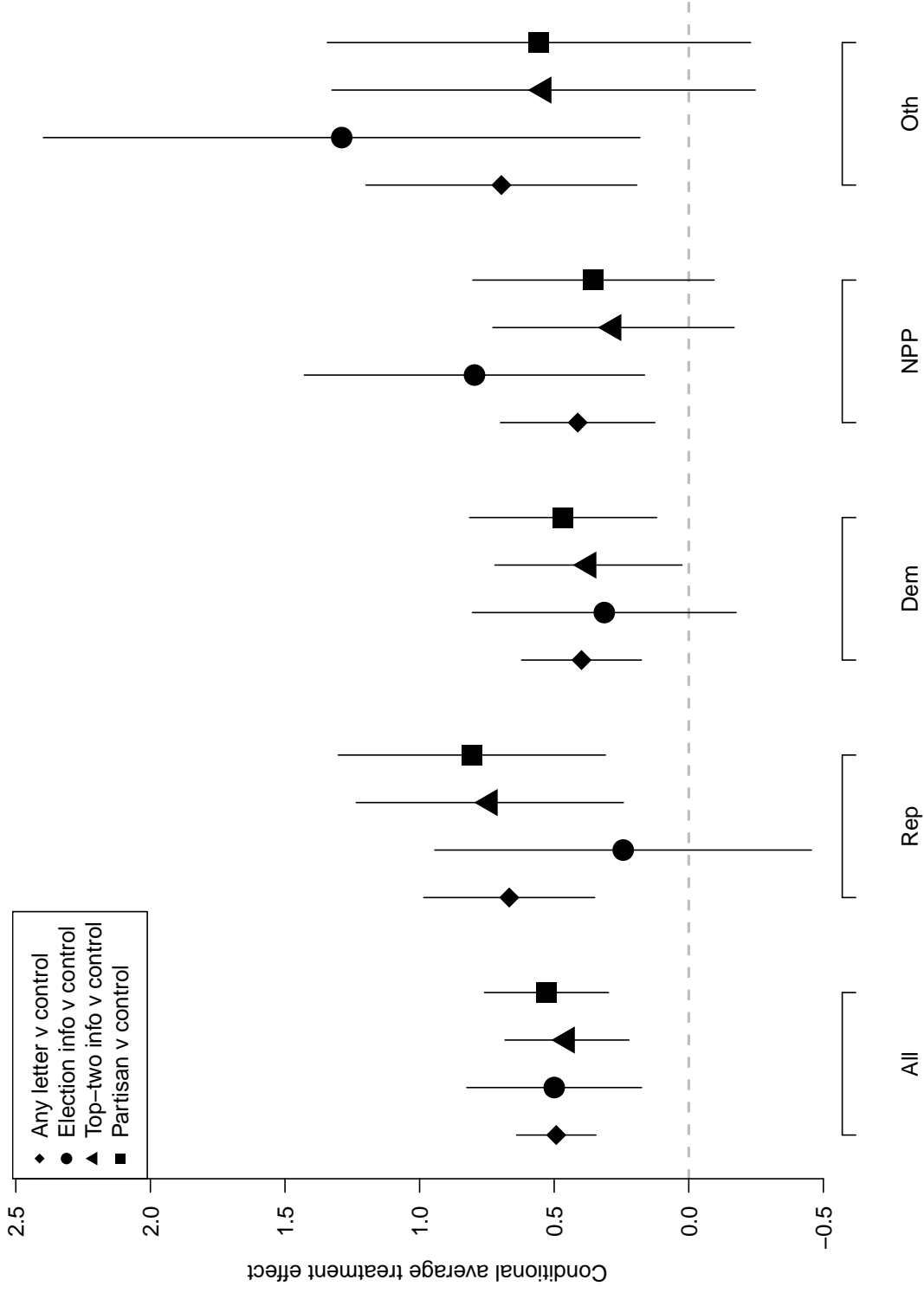
Note: Data are aggregated from individual observations from the California voter file. Turnout in left column is averaged across congressional primaries in 2008 and 2010, in right column for congressional primaries in 2012 and 2014. Groups are not mutually exclusive (e.g. Party Dem includes Age 50-59 and vice versa).

Table 1: Turnout in 2014 primary by treatment assignment and party (experimental results)

| | All | NPP | DEM | REP | OTH |
|---------------|--------------|--------------|--------------|---------------|---------------|
| Control | 9.3 (0.0) | 8.6 (0.0) | 9.0 (0.0) | 10.6 (0.0) | 9.2 (0.1) |
| | 3,722,672 | 919,917 | 1,584,950 | 898,297 | 319,508 |
| Any Letter | 9.8 (0.1) | 9.0 (0.1) | 9.4 (0.1) | 11.3 (0.2) | 9.9 (0.3) |
| | 149,596 | 36,959 | 63,733 | 36,095 | 12,809 |
| Election Info | 9.8 (0.2) | 9.4 (0.3) | 9.3 (0.3) | 10.9 (0.4) | 10.5 (0.6) |
| | 29,885 | 7,381 | 12,733 | 7,211 | 2,560 |
| Top-two Info | 9.8 (0.1) | 8.9 (0.2) | 9.4 (0.2) | 11.3 (0.3) | 9.8 (0.4) |
| | 59,854 | 14,789 | 25,500 | 14,441 | 5,124 |
| Partisan | 9.8 (0.1) | 8.9 (0.2) | 9.5 (0.2) | 11.4 (0.3) | 9.8 (0.4) |
| | 59,857 | 14,789 | 25,500 | 14,443 | 5,125 |

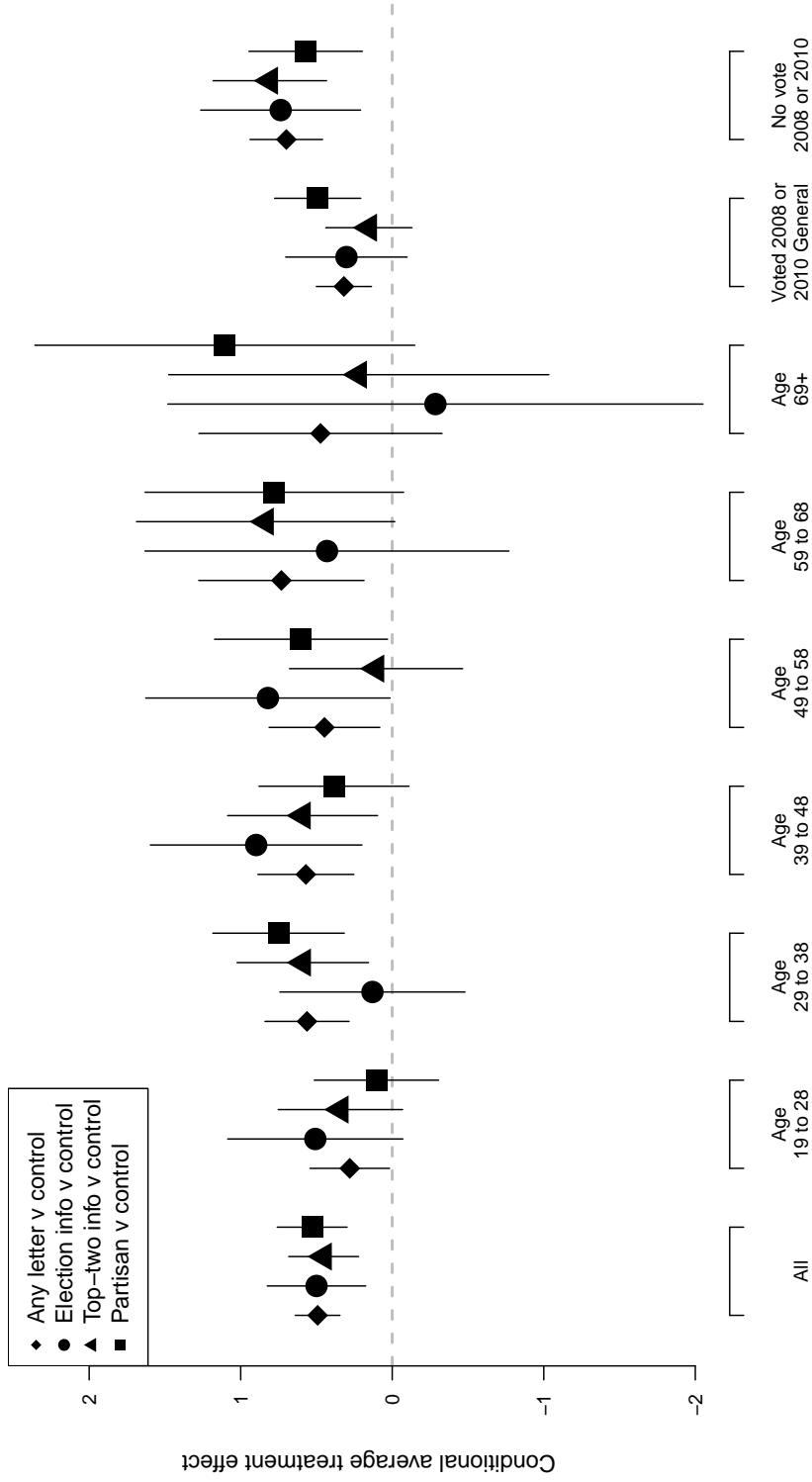
Note: Cell entries are turnout for that party (column) and treatment assignment (row) for the target population in our field experiment. Standard errors in parentheses and cell counts below. NPP=no party preference or decline to state; DEM=Democrat; REP=Republican; OTH=Other/third-party. Of 12 two-way difference of proportion tests comparing each individual letter to each other for each partisan group, 0 are significant at $p < .05$, two-tailed.

Figure 2: Heterogeneous treatment effects by party



Note: Each point is the coefficient from an OLS regression of turnout on an indicator for that treatment assignment for that subgroup of the target population, with randomization block fixed effects. Confidence intervals extend to ± 1.96 standard errors.

Figure 3: Heterogeneous treatment effects by age and previous turnout



Note: Each point is the coefficient from an OLS regression of turnout on an indicator for that treatment assignment for that subgroup of the target population, with randomization block fixed effects. Confidence intervals extend to ± 1.96 standard errors.

Table 2: OLS estimates of heterogeneous treatment effects, any letter

| | (Direct effect) | (Age linear) | (Age binned) | (Age binned) |
|-----------------------------------|-----------------|---------------|---------------|---------------|
| Intercept | 6.27* (0.03) | 6.29* (0.03) | 2.33* (0.04) | 6.52* (0.05) |
| Any letter | 0.50* (0.08) | 0.20 (0.14) | -0.14 (0.21) | 0.12 (0.25) |
| Age | 0.26* (0.00) | 0.26* (0.00) | | |
| Abstain 08 and 10 | 5.84* (0.03) | 5.82* (0.03) | 5.42* (0.03) | 1.72* (0.04) |
| Party NPP | -0.08* (0.04) | -0.08* (0.04) | -0.06 (0.04) | 0.18* (0.04) |
| Party REP | 1.34* (0.04) | 1.33* (0.04) | 1.42* (0.04) | 1.72* (0.04) |
| Party OTH | 0.50* (0.05) | 0.49* (0.06) | 0.44* (0.06) | -0.26* (0.06) |
| Any letter*Age | | 0.01 (0.01) | | |
| Any letter*Abstain 08 and 10 | | 0.43* (0.16) | 0.46* (0.16) | 0.30 (0.19) |
| Any letter*Party NPP | | 0.04 (0.19) | 0.04 (0.19) | 0.04 (0.19) |
| Any letter*Party REP | | 0.29 (0.19) | 0.30 (0.19) | 0.30 (0.19) |
| Any letter*Party OTH | | 0.28 (0.28) | 0.28 (0.28) | 0.22 (0.28) |
| Age 29-38 | | | 1.79* (0.04) | 2.33* (0.04) |
| Age 39-48 | | | 3.56* (0.05) | 4.58* (0.05) |
| Age 49-58 | | | 5.53* (0.05) | 6.86* (0.05) |
| Age 59-68 | | | 10.24* (0.06) | 11.61* (0.06) |
| Age 69+ | | | 15.04* (0.07) | 16.43* (0.07) |
| Any letter*Age 29-38 | | | 0.39 (0.23) | 0.39 (0.23) |
| Any letter*Age 39-48 | | | 0.43 (0.24) | 0.45 (0.24) |
| Any letter*Age 49-58 | | | 0.31 (0.25) | 0.34 (0.25) |
| Any letter*Age 59-68 | | | 0.59* (0.28) | 0.63* (0.28) |
| Any letter*Age 69+ | | | 0.48 (0.36) | 0.52 (0.36) |
| Registered prior to 08 | | | | -4.38* (0.05) |
| Registered prior to 10 | | | | -2.51* (0.05) |
| Any letter*Registered prior to 08 | | | | 0.38 (0.26) |
| Any letter*Registered prior to 10 | | | | -0.65* (0.27) |
| N | 3855411 | 3855411 | 3855411 | 3855411 |
| R ² | 0.02 | 0.02 | 0.03 | 0.03 |
| adj. R ² | 0.02 | 0.02 | 0.03 | 0.03 |
| Resid. sd | 28.70 | 28.70 | 28.69 | 28.57 |

Standard errors in parentheses

* indicates significance at $p < 0.05$

Note: Dependent variable is turnout in that election (100=yes, 0=no). OLS regression coefficients with standard errors in parentheses. Heterogeneous effects separated by each treatment letter in Supplemental Table S3.

Supplemental Information

Turning Out Unlikely Voters? A Field Experiment In The Top-Two Primary

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September 18, 2015

Contents

| | |
|--|-----------|
| A Construction of the target population | 2 |
| B Blocking | 2 |
| C Details of letters mailed | 3 |
| D Additional tables and figures | 4 |
| E Treatment effects for 2014 general election | 10 |
| F Example letters | 19 |

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A Construction of the target population

In this section, we describe the process for obtaining and cleaning the set of observations that make up our target population.

The California Secretary of State maintains the publicly available California registered voter file, pursuant to Title 2, Division 8, Chapter 2, Section 20108.53. This file lists the names, addresses, stated party preference, and past voting history for all registered voters in the state of California for all Federal and State elections. Municipal elections are not specifically identified in this document. We used the April, 2014 version of the voter file to construct our target population, which contains 17,651,518 rows and 73,598,901 unique voting events.

A key piece of this research is the focus on voters who have previously voted in a general election, but not in a primary election. We apply population filters to create a sampling frame. Specifically, we limit the sampling frame to those who:

1. Have a reported ZIP code. This reduces the entries in our dataframe by 227 from 17,651,518 to 17,651,291.
2. Voted in the 2012 general election. This reduces the entries in our dataframe by 5,417,707 from 17,651,291 to 12,233,584.
3. Did not vote in a 2012, 2010, or 2008 primary election. This reduces the entries in our dataframe by 8,077,598 from 12,233,584 to 4,155,986.
4. Were not matched (by a private vendor) to records indicating they had moved, been incarcerated, or died. This reduces entries in our dataframe from 4,155,986 to 3,872,268.

All together the population filters reduce the number of records from **17,651,518** to **3,872,268**. After removing records of ineligible voters and verifying the address of eligible voters, the vendor standardized all addresses to U.S. Post Office bulk rate standards.

B Blocking

One concern in experimental designs is that pre-treatment randomization will “fail” leading to covariate imbalance and an inability of researchers to draw clean causal inference. This concern is magnified when populations of interest might respond to treatment differently and the analyst is motivated to estimate heterogeneous treatment effects. Blocking on observable covariates mitigates this concern by sorting all possible recipients into groups (blocks) and then randomly assigning treatment within each block. This way, there is no risk for pre-treatment imbalance on observable covariates. Moreover, because the assignment to treatment was orthogonal to covariates and the covariates are by design balanced, researchers can work with each block as a separate experiment, which allows greater ability to make theoretically motivated comparisons within and between population sub-groups.

We block on the following variables:

1. *Age*: Recorded in 7 intervals, 18-28, 29-38, 39-48, 49-58, 59-68, 69+, and age missing.¹

¹ Because our target population required voting in the 2012 election, no elector aged 18 in 2014 was eligible to be included.

2. *Party Preference*: Recorded in 4 groups, Republican, Democrat, Decline to State or No Party Preference, or Other/third party.
3. *2010 & 2008 General Election Vote History*: Vote history was drawn from the voter file.
4. *District Competitiveness*: We calculated district competitiveness using data downloaded from the Federal Election Commission at ftp.fec.gov/FEC/data.fec.gov/candidate_disbursement/all_house_senate.csv. After downloading this data, we subset to include only records of spending that occurred after the most recent election. We classified congressional districts as competitive when two or more candidates in that district spent more than \$100,000 dollars in the current election cycle prior to the primary. This criterion led to the following districts classified as competitive: 03, 07, 10, 12, 17, 25, 26, 31, 33, 36, 45, 52.
5. *Majority Minority District*: We identify majority minority districts as those districts where a plurality of the districts' residents are non-white or non-first language English. These criteria led to the following districts classified as majority minority: 19, 25, 27, 30, 31, 32, 39, 47, 48, 49, 51, 52, 53, 56, 57, 58, 59, 63, 69, 80.²

We applied randomization within blocks defined by the unique intersection of this set of variables. For blocks too small to apply our targeted rates of treatment assignment, and for leftover cases due to rounding in larger blocks, we created a “leftover” block, in which we made treatment assignments by simple random sampling.

C Details of letters mailed

In this section, we present basic details of the letters mailed. Actual copies of the letters are included at the end of the Supplemental Information.

- Election information Letter.
All of the information contained in the election information letter is also contained in each of the treatment letters. The opening paragraph reminds the recipient that there is an election upcoming on June 3, and expresses enthusiasm for the recipient to cast a vote. The second content paragraph reminds the recipient of the laws surrounding voting by mail, and the third paragraph provides the recipient with information about how to address questions. All of the language in this letter, as well as all of the letters was drawn directly from the California Secretary of State website and was also reviewed by managers at California Common Cause.
- Top-two information Letter.
The top-two information letter contains all the content of the election information letter, but also contains two additional elements. In the first additional paragraph, we provide the recipient with the information that following the Top Two Candidate Open Primary Act of 2010 (Prop 14) all Californians can now participate in primary elections. In the second additional element, we include a bulleted list of the changes placed into effect by Prop 14. The contents of this information were borrowed from other efforts by Common Cause and

² The two blocking categories of competitiveness and majority-minority districts are not analyzed in this paper, but will be in a subsequent manuscript.

the Los Angeles City Clerk to explain the top-two primary in previous election outreach. Specifically, we note that:

- All voters can now vote in the primary election.
 - All candidates appear on one ballot.
 - Candidate are not nominated by the party.
 - Any voter can vote for any candidate.
 - The two candidates with the most vote move on to the General Election in November.
- **Partisan Identity Letter.**
The partisan identity letter contains all the content of the election information letter, but also two additional paragraphs. In the first additional paragraph, we provide the recipient of the letter with information about turnout rates of co-partisans in the 2012 California Presidential Election primary. This information was calculated using the most current California voter file, the same file that was used for the mailing list. In the second paragraph, we include an appeal to the recipient to turn out to vote.

D Additional tables and figures

In Table S1, we present our observational analysis of turnout under California’s top-two primary. The dependent variable is whether or not the registrant voted in that election. The first specification controls for party of registration, and shows the average rate of turnout for registrants with each of these parties across the four primary elections analyzed. In the second column, we include a dummy variable for the latter two elections held under the top-two rules, whose point estimate of -1.5 shows that, on average, registrants were 1.5 percentage points less likely to vote under the top-two system. In the third column, we interact the top-two indicator with each of the parties of registration, showing how turnout by party changes across the two institutions. All four party groupings turn out at lower rates, but with a notably larger effect for other party registrants and a notably smaller effect for no party preference/decline to state registrants.

In Table S2, we present OLS regressions to estimate the average treatment effect, which allow us to include randomization block fixed effects. These block fixed effects nest controlling for the interactions of each of our blocking variables. In the first column we present the treatment effect estimates for the three letters: 0.50, 0.45, and 0.53. In column two, we include block fixed effects, and estimate identical treatment effects. In column three, we estimate treatment effects for the target population aged 24 and older, the subset of the electorate who was eligible to vote in all of the primary elections in our selection criteria. In columns four and five, we test for difference between the three treatment letters, finding no statistically distinguishable difference with or without block fixed effects.

In Table S3, we extend the analysis from the main body in Table 2. We estimate heterogeneity across covariates for each treatment letter separately, rather than for any letter as in Table 2. We again find little evidence of significant heterogeneity, though find large point estimates for (a) other party registrants receiving the election information letter [coefficient of 0.89]; (b) those aged 59-69 receiving the top-two letter or the partisan letter [0.69 and 0.71]; (c) those aged 29-38 receiving the partisan letter [0.69]; and (d) 70+ receiving the partisan letter [1.33], which is our largest observed treatment effect and the only in this set statistically significant at $p < .05$. Apparently

Table S1: Model of turnout in top two and traditional primaries in California, 2008 to 2014 (non-experimental)

| | (Base) | (DID) | (DID) |
|-------------------|------------|------------|------------|
| Top two primary | | -1.50* | |
| | | (0.01) | |
| Party REP | 33.78* | 34.52* | 34.60* |
| | (0.01) | (0.01) | (0.01) |
| Party DEM | 26.55* | 27.31* | 27.41* |
| | (0.01) | (0.01) | (0.01) |
| Party NPP | 17.43* | 18.21* | 17.69* |
| | (0.01) | (0.01) | (0.02) |
| Party OTH | 17.08* | 17.99* | 18.86* |
| | (0.02) | (0.02) | (0.04) |
| Top two*Party REP | | | -1.68* |
| | | | (0.02) |
| Top two*Party DEM | | | -1.70* |
| | | | (0.02) |
| Top two*Party NPP | | | -0.49* |
| | | | (0.02) |
| Top two*Party OTH | | | -2.93* |
| | | | (0.05) |
| N | 66,002,372 | 66,002,372 | 66,002,372 |
| R ² | 0.276 | 0.276 | 0.276 |

Standard errors in parentheses

* indicates significance at $p < 0.05$

Note: Data are individual observations from the California voter file. Dependent variable is turnout in that election (100=yes, 0=no). OLS regression coefficients with standard errors in parentheses. Year is a linear term for time. Top two primary takes the value of 1 in 2012 and 2014, 0 in 2010 and 2008.

Table S2: OLS estimates of treatment effects from experiment

| | (All, no FEs) | (All, w FEs) | (Age 24+, w FEs) | (Letters, no FEs) | (Letters, w FEs) |
|--------------------------|---------------|--------------|------------------|-------------------|------------------|
| Intercept | 9.31* | | | 9.81* | |
| | (0.02) | | | (0.17) | |
| Treatment: Election info | 0.50* | 0.50* | 0.46* | | |
| | (0.17) | (0.17) | (0.18) | | |
| Treatment: Top-two info | 0.45* | 0.45* | 0.44* | -0.05 | -0.05 |
| | (0.12) | (0.12) | (0.13) | (0.21) | (0.21) |
| Treatment: Partisan | 0.53* | 0.53* | 0.57* | 0.03 | 0.03 |
| | (0.12) | (0.12) | (0.13) | (0.21) | (0.21) |
| N | 3872268 | 3872268 | 3464044 | 149596 | 149596 |
| R ² | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| adj. R ² | 0.00 | 0.00 | 0.00 | -0.00 | -0.00 |
| Resid. sd | 29.09 | 28.53 | 28.78 | 29.73 | 29.11 |

Standard errors in parentheses

* indicates significance at $p < 0.05$

Note: Dependent variable is turnout in that election (100=yes, 0=no). OLS regression coefficients with standard errors in parentheses. Models indicating fixed effects have block fixed effects (within estimator). Columns four and five compare placebo letter to information and identity letters only.

the oldest set of registrants who haven't previously voted in primaries are particularly responsive to learning about low primary turnout among their partisan group. We show in Figure S1 that the point estimate for the effect of the partisan letter is greater than 2 percentage points for those aged 71 to 78, and in Figure S2 that the effect is especially driven by those registered no party preference, with a point estimate above 4 percentage points.

Table S3: OLS estimates of heterogeneous treatment effects, separate effects by letter

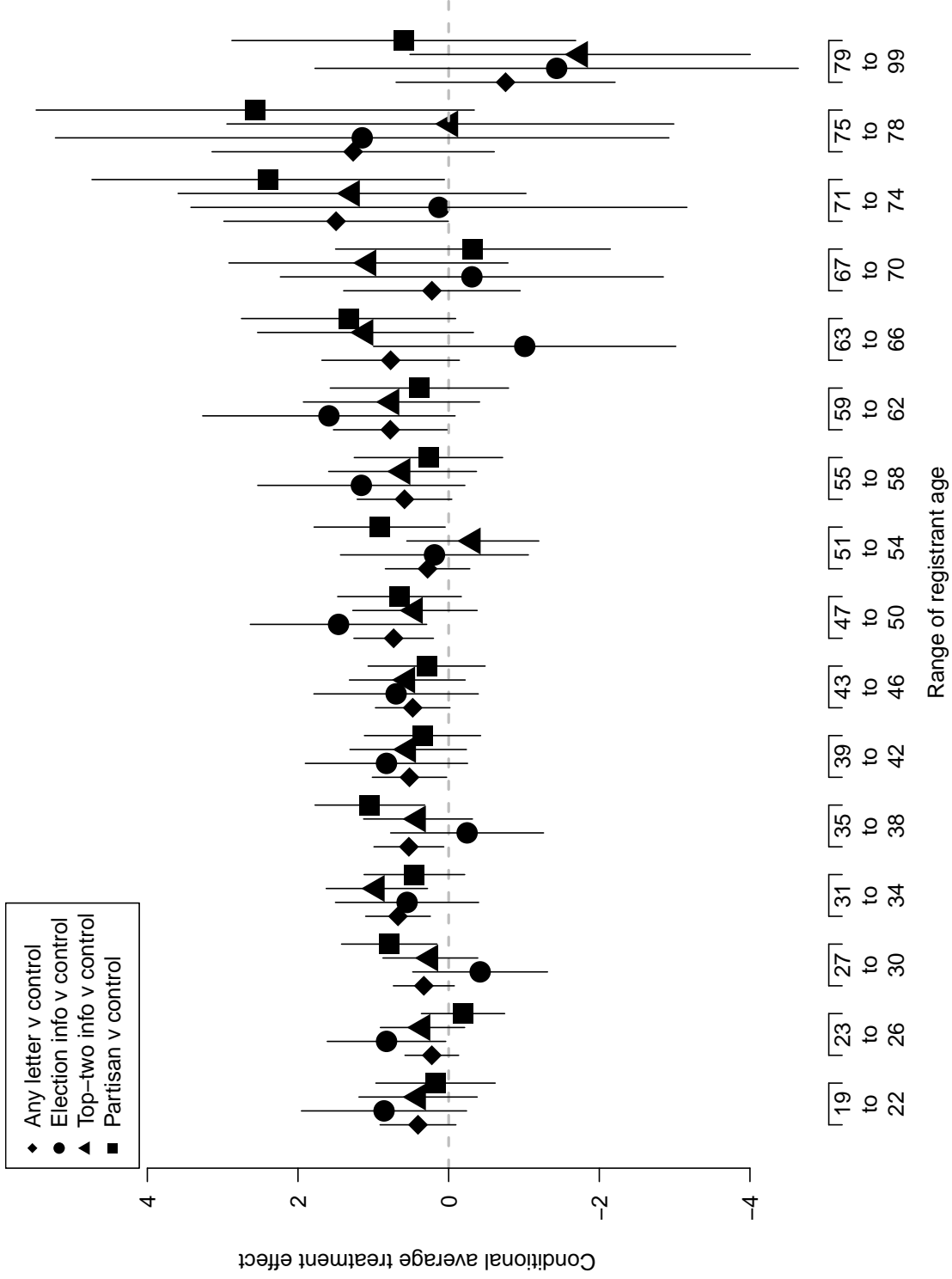
| | (Direct effect) | (Age linear) | (Age binned) |
|---|-----------------|---------------|---------------|
| Intercept | 6.27* (0.03) | 6.29* (0.03) | 6.52* (0.05) |
| Election info letter | 0.51* (0.17) | 0.13 (0.30) | 0.13 (0.56) |
| Top-two info letter | 0.45* (0.12) | 0.05 (0.21) | 0.05 (0.39) |
| Partisan letter | 0.54* (0.12) | 0.39 (0.21) | 0.17 (0.40) |
| Age | 0.26* (0.00) | 0.26* (0.00) | |
| Abstain 08 and 10 | 5.84* (0.03) | 5.82* (0.03) | 1.72* (0.04) |
| Party NPP | -0.08* (0.04) | -0.08* (0.04) | 0.18* (0.04) |
| Party REP | 1.34* (0.04) | 1.33* (0.04) | 1.72* (0.04) |
| Party OTH | 0.50* (0.05) | 0.49* (0.06) | -0.26* (0.06) |
| Election info letter*Age | | 0.00 (0.01) | |
| Age*Top-two info letter | | 0.00 (0.01) | |
| Age*Partisan letter | | 0.02* (0.01) | |
| Election info letter*Abstain 08 and 10 | | 0.40 (0.34) | 0.36 (0.43) |
| Top-two info letter*Abstain 08 and 10 | | 0.69* (0.24) | 0.50 (0.30) |
| Partisan letter*Abstain 08 and 10 | | 0.18 (0.24) | 0.08 (0.31) |
| Election info letter*Party NPP | | 0.51 (0.42) | 0.49 (0.42) |
| Top-two info letter*Party NPP | | -0.08 (0.30) | -0.08 (0.30) |
| Partisan letter*Party NPP | | -0.07 (0.30) | -0.07 (0.30) |
| Election info letter*Party REP | | -0.01 (0.43) | -0.04 (0.43) |
| Top-two info letter*Party REP | | 0.43 (0.30) | 0.46 (0.30) |
| Partisan letter*Party REP | | 0.30 (0.30) | 0.32 (0.30) |
| Election info letter*Party OTH | | 0.89 (0.63) | 0.88 (0.63) |
| Top-two info letter*Party OTH | | 0.13 (0.44) | 0.03 (0.44) |
| Partisan letter*Party OTH | | 0.12 (0.44) | 0.09 (0.44) |
| Age 29-38 | | | 2.33* (0.04) |
| Age 39-48 | | | 4.58* (0.05) |
| Age 49-58 | | | 6.86* (0.05) |
| Age 59-68 | | | 11.61* (0.06) |
| Age 69+ | | | 16.43* (0.07) |
| Registered prior to 08 | | | -4.38* (0.05) |
| Registered prior to 10 | | | -2.51* (0.05) |
| Election info letter*Age 29-38 | | | -0.20 (0.50) |
| Election info letter*Age 39-48 | | | 0.57 (0.52) |
| Election info letter*Age 49-58 | | | 0.57 (0.55) |
| Election info letter*Age 59-68 | | | 0.23 (0.63) |
| Election info letter*Age 69+ | | | -0.49 (0.78) |
| Age 29-38*Top-two info letter | | | 0.42 (0.36) |
| Age 39-48*Top-two info letter | | | 0.54 (0.37) |
| Age 49-58*Top-two info letter | | | 0.13 (0.39) |
| Age 59-68*Top-two info letter | | | 0.82 (0.44) |
| Age 69+*Top-two info letter | | | 0.30 (0.56) |
| Age 29-38*Partisan letter | | | 0.65 (0.36) |
| Age 39-48*Partisan letter | | | 0.29 (0.37) |
| Age 49-58*Partisan letter | | | 0.45 (0.39) |
| Age 59-68*Partisan letter | | | 0.65 (0.44) |
| Age 69+*Partisan letter | | | 1.25* (0.56) |
| Election info letter*Registered prior to 08 | | | 0.38 (0.58) |
| Top-two info letter*Registered prior to 08 | | | -0.30 (0.41) |
| Partisan letter*Registered prior to 08 | | | 1.04* (0.41) |
| Election info letter*Registered prior to 10 | | | -0.56 (0.60) |
| Top-two info letter*Registered prior to 10 | | | -0.22 (0.42) |
| Partisan letter*Registered prior to 10 | | | -1.12* (0.42) |
| N | 3855411 | 3855411 | 3855411 |
| R ² | 0.02 | 0.02 | 0.03 |
| adj. R ² | 0.02 | 0.02 | 0.03 |
| Resid. sd | 28.70 | 28.70 | 28.57 |

Standard errors in parentheses

* indicates significance at $p < 0.05$

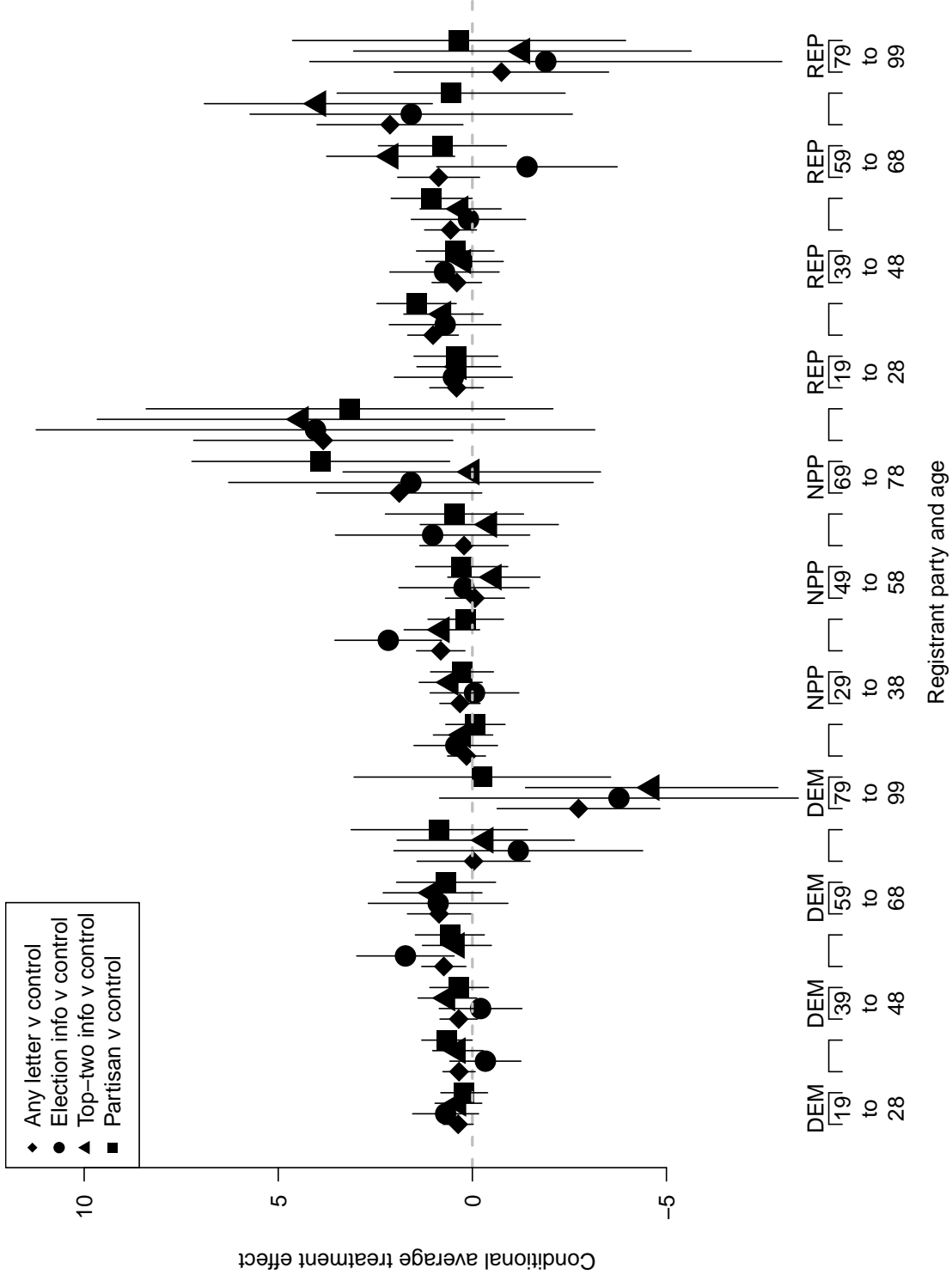
Note: Dependent variable is turnout in that election (100=yes, 0=no). OLS regression coefficients with standard errors in parentheses.

Figure S1: Heterogeneous treatment effects by small age ranges



Note: Each point is the coefficient from an OLS regression of turnout on an indicator for that treatment assignment for that subgroup of the target population, with randomization block fixed effects. Confidence intervals extend to ± 1.96 standard errors.

Figure S2: Heterogeneous treatment effects by small age cross party



Note: Each point is the coefficient from an OLS regression of turnout on an indicator for that treatment assignment for that subgroup of the target population, with randomization block fixed effects. Confidence intervals extend to ± 1.96 standard errors.

E Treatment effects for 2014 general election

In this section, we reproduce the tables and figures from the primary election, instead analyzing treatment effects and their variation for turnout in the November 2014 general election. We matched 96.5% of our target population to records in the January 2015 voter file with turnout history; all non-matches are set to abstention in the general. Table S4 presents the difference in mean turnout in the 2014 general election by treatment assignment. The letters have no effect on general election turnout on average. No differences within party are statistically significant when Bonferroni corrected for multiple comparisons. In Table S5, we produce OLS estimates with block fixed effects, and again find no average treatment effect.

In Figures S3 and S4, we present treatment effects by party, age, and turnout history. The treatment effect on registrants aged 59 and above is close to statistically significant. We specify the turnout history differently for these treatment effects because it is a general election rather than a primary. The effects by turnout history are suggestive that the letters are more effective on non-midterm voters (2008 voters and non-2010 voters) than for midterm voters.

In Table S6, we present OLS estimates of these heterogeneous treatment effects with each covariate of interest considered together in a multiple regression setting. We do find evidence of variation in the effect by age. While most ages show little influence of being assigned to receive any letter, those aged 59-68 turn out about 0.5 points higher when assigned to receive any letter, and those aged 69+ about 0.4 points higher.

In Table S7, we estimate variation in average effects across covariates for each treatment letter separately, rather than for any letter as in Table S6. The treatment effects on those aged 59 and above for any letter appear to be driven by the top-two letter and the partisan letter. We show in Figure S5 that the conditional average treatment effect of the partisan letter is greater than 2 percentage points for those aged 63 to 66 and aged 71 to 78, and that the effect of the top-two letter is close to one point for those aged 59 to 74. The points in Figure S6 suggest that the effect is especially driven by those registered no party preference.

Table S4: Turnout in 2014 general by treatment assignment and party (experimental results)

| | All | NPP | DEM | REP | OTH |
|---------------|---------------|---------------|---------------|---------------|---------------|
| Control | 27.4 (0.0) | 26.1 (0.0) | 26.5 (0.0) | 30.7 (0.0) | 26.1 (0.1) |
| | 3,722,672 | 919,917 | 1,584,950 | 898,297 | 319,508 |
| Any Letter | 27.4 (0.1) | 26.0 (0.2) | 26.4 (0.2) | 31.0 (0.2) | 26.0 (0.4) |
| | 149,596 | 36,959 | 63,733 | 36,095 | 12,809 |
| Election Info | 27.4 (0.3) | 26.4 (0.5) | 26.4 (0.4) | 30.2 (0.5) | 26.6 (0.9) |
| | 29,885 | 7,381 | 12,733 | 7,211 | 2,560 |
| Top-two Info | 27.3 (0.2) | 25.8 (0.4) | 26.4 (0.3) | 31.1 (0.4) | 25.0 (0.6) |
| | 59,854 | 14,789 | 25,500 | 14,441 | 5,124 |
| Partisan | 27.5 (0.2) | 26.0 (0.4) | 26.4 (0.3) | 31.2 (0.4) | 26.8 (0.6) |
| | 59,857 | 14,789 | 25,500 | 14,443 | 5,125 |

Note: Cell entries are turnout for that party (column) and treatment assignment (row) for the target population in our field experiment. Standard errors in parentheses and cell counts below. NPP=no party preference or decline to state; DEM=Democrat; REP=Republican; OTH=Other/third-party. Of 12 two-way difference of proportion tests comparing each individual letter to each other for each partisan group, 1 are significant at $p < .05$, two-tailed. Applying a Bonferroni correction for the number of tests, 0 are significant at $p < 0.0042$, two-tailed.

Table S5: 2014 General OLS estimates of treatment effects from experiment

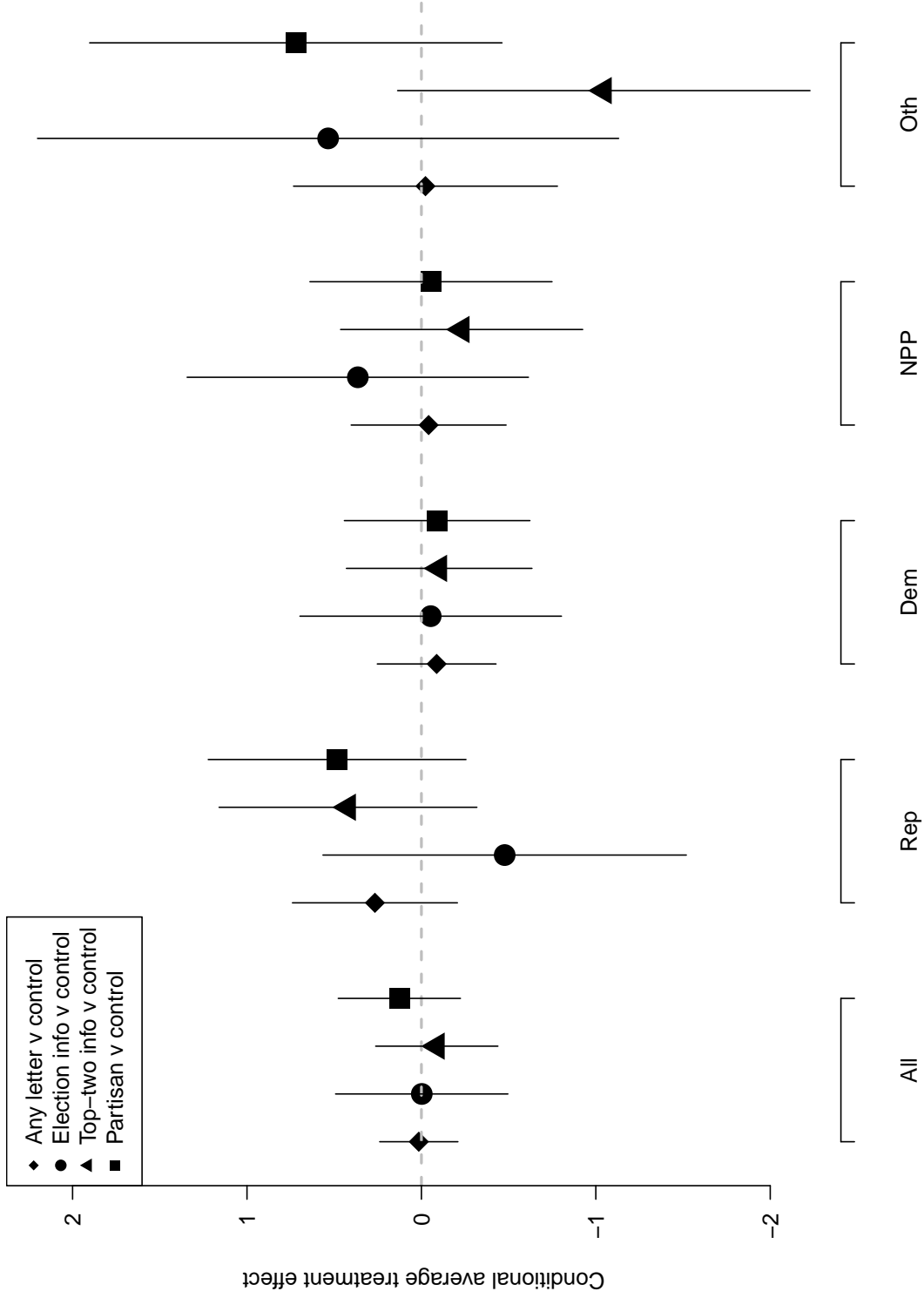
| | (All, no FEs) | (All, w FEs) | (Age 24+, w FEs) | (Letters, no FEs) | (Letters, w FEs) |
|--------------------------|---------------|--------------|------------------|-------------------|------------------|
| Intercept | 27.36* | | | 27.35* | |
| | (0.02) | | | (0.26) | |
| Treatment: Election info | -0.01 | -0.00 | 0.06 | | |
| | (0.26) | (0.25) | (0.27) | | |
| Treatment: Top-two info | -0.09 | -0.09 | -0.01 | -0.09 | -0.09 |
| | (0.18) | (0.18) | (0.19) | (0.32) | (0.31) |
| Treatment: Partisan | 0.12 | 0.13 | 0.17 | 0.13 | 0.13 |
| | (0.18) | (0.18) | (0.19) | (0.32) | (0.31) |
| N | 3872268 | 3872268 | 3464044 | 149596 | 149596 |
| R ² | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 |
| adj. R ² | -0.00 | -0.00 | -0.00 | -0.00 | -0.00 |
| Resid. sd | 44.58 | 43.39 | 43.89 | 44.59 | 43.31 |

Standard errors in parentheses

* indicates significance at $p < 0.05$

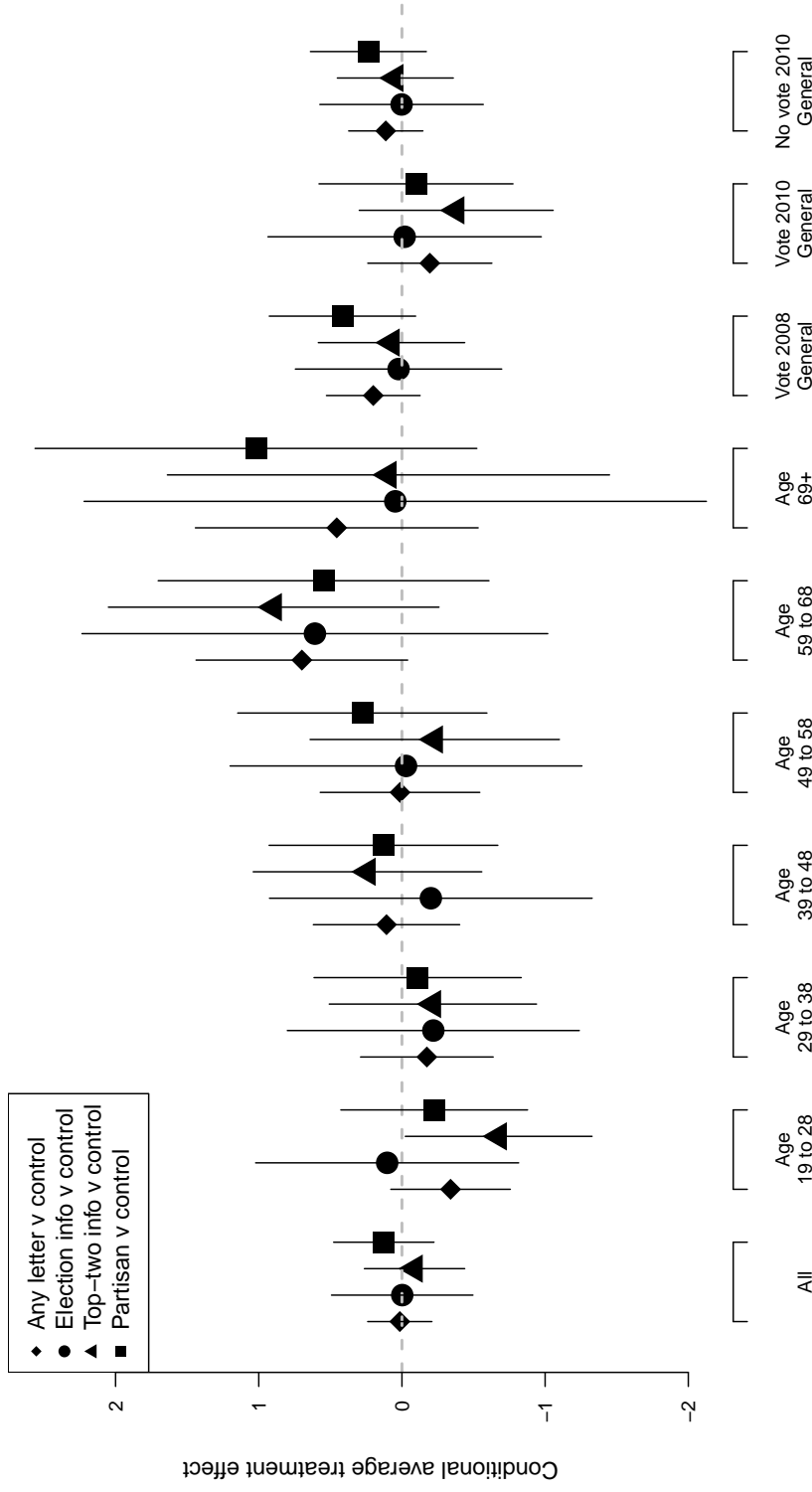
Note: Dependent variable is turnout in that election (100=yes, 0=no). OLS regression coefficients with standard errors in parentheses. Models indicating fixed effects have block fixed effects (within estimator). Columns three and four compare placebo letter to information and identity letters only.

Figure S3: 2014 General Heterogeneous treatment effects by party



Note: Each point is the coefficient from an OLS regression of turnout on an indicator for that treatment assignment for that subgroup of the target population, with randomization block fixed effects. Confidence intervals extend to ± 1.96 standard errors.

Figure S4: 2014 General Heterogeneous treatment effects by age and previous turnout



Note: Each point is the coefficient from an OLS regression of turnout on an indicator for that treatment assignment for that subgroup of the target population, with randomization block fixed effects. Confidence intervals extend to ± 1.96 standard errors.

Table S6: 2014 General OLS estimates of heterogeneous treatment effects, any letter

| | (Direct effect) | (Age linear) | (Age binned) | (Age binned) |
|-----------------------------------|-----------------|---------------|---------------|---------------|
| Intercept | 25.51* (0.04) | 25.51* (0.04) | 16.29* (0.06) | 22.89* (0.08) |
| Any letter | 0.01 (0.12) | -0.12 (0.21) | -0.47 (0.32) | -0.51 (0.39) |
| Age | 0.44* (0.00) | 0.44* (0.00) | | |
| Abstain 08 and 10 | 2.08* (0.05) | 2.08* (0.05) | 2.13* (0.05) | -3.81* (0.06) |
| Party NPP | 0.07 (0.06) | 0.07 (0.06) | 0.02 (0.06) | 0.41* (0.06) |
| Party REP | 3.32* (0.06) | 3.31* (0.06) | 3.32* (0.06) | 3.79* (0.06) |
| Party OTH | 0.93* (0.08) | 0.93* (0.09) | 0.90* (0.09) | -0.22* (0.09) |
| Any letter*Age | | 0.02* (0.01) | | |
| Any letter*Abstain 08 and 10 | | 0.05 (0.24) | 0.02 (0.24) | 0.11 (0.30) |
| Any letter*Party NPP | | 0.11 (0.29) | 0.11 (0.29) | 0.08 (0.29) |
| Any letter*Party REP | | 0.30 (0.30) | 0.32 (0.30) | 0.29 (0.30) |
| Any letter*Party OTH | | 0.17 (0.44) | 0.15 (0.44) | 0.13 (0.44) |
| Age 29-38 | | | 5.94* (0.07) | 6.85* (0.07) |
| Age 39-48 | | | 10.16* (0.07) | 11.85* (0.07) |
| Age 49-58 | | | 13.33* (0.07) | 15.55* (0.07) |
| Age 59-68 | | | 19.79* (0.09) | 22.08* (0.09) |
| Age 69+ | | | 21.79* (0.11) | 24.12* (0.11) |
| Any letter*Age 29-38 | | | 0.17 (0.35) | 0.13 (0.35) |
| Any letter*Age 39-48 | | | 0.44 (0.36) | 0.41 (0.37) |
| Any letter*Age 49-58 | | | 0.34 (0.38) | 0.31 (0.38) |
| Any letter*Age 59-68 | | | 1.04* (0.43) | 1.02* (0.44) |
| Any letter*Age 69+ | | | 0.87 (0.54) | 0.85 (0.55) |
| Registered prior to 08 | | | | -8.11* (0.08) |
| Registered prior to 10 | | | | -3.05* (0.08) |
| Any letter*Registered prior to 08 | | | | 0.43 (0.40) |
| Any letter*Registered prior to 10 | | | | -0.28 (0.41) |
| N | 3855411 | 3855411 | 3855411 | 3855411 |
| R ² | 0.02 | 0.02 | 0.02 | 0.03 |
| adj. R ² | 0.02 | 0.02 | 0.02 | 0.03 |
| Resid. sd | 44.05 | 44.05 | 44.03 | 43.84 |

Standard errors in parentheses

* indicates significance at $p < 0.05$

Note: Dependent variable is turnout in that election (100=yes, 0=no). OLS regression coefficients with standard errors in parentheses. Heterogeneous effects separated by each treatment letter in Supplemental Table S7.

Table S7: 2014 General OLS estimates of heterogeneous treatment effects, separate effects by letter

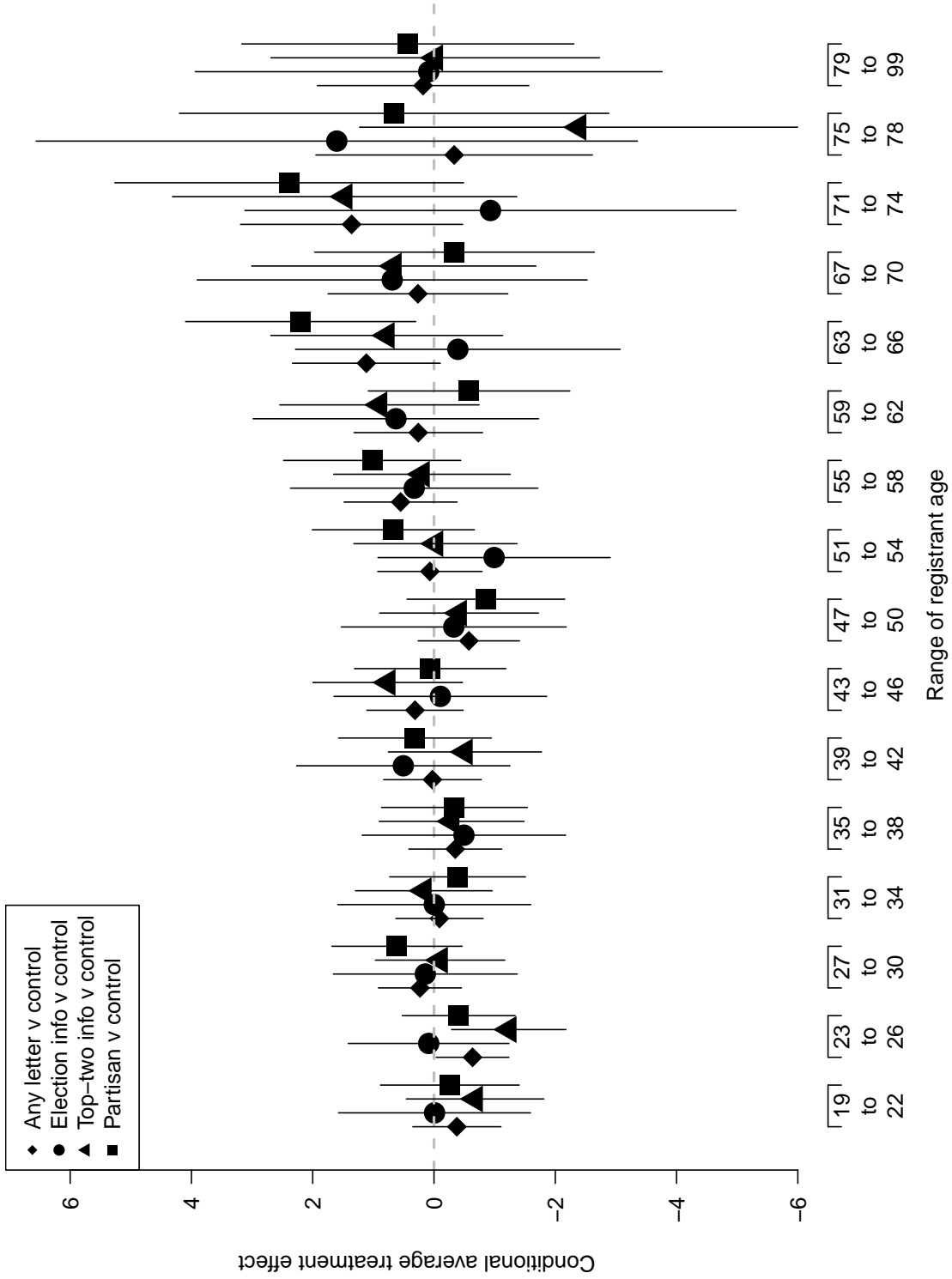
| | (Direct effect) | (Age linear) | (Age binned) |
|---|-----------------|---------------|---------------|
| Intercept | 25.51* (0.04) | 25.51* (0.04) | 22.89* (0.08) |
| Election info letter | -0.00 (0.26) | -0.06 (0.46) | 0.48 (0.85) |
| Top-two info letter | -0.08 (0.18) | -0.21 (0.33) | -1.06 (0.60) |
| Partisan letter | 0.12 (0.18) | -0.06 (0.33) | -0.46 (0.61) |
| Age | 0.44* (0.00) | 0.44* (0.00) | |
| Abstain 08 and 10 | 2.08* (0.05) | 2.08* (0.05) | -3.81* (0.06) |
| Party NPP | 0.07 (0.06) | 0.07 (0.06) | 0.41* (0.06) |
| Party REP | 3.32* (0.06) | 3.31* (0.06) | 3.79* (0.06) |
| Party OTH | 0.93* (0.08) | 0.93* (0.09) | -0.22* (0.09) |
| Election info letter*Age | | 0.01 (0.02) | |
| Age*Top-two info letter | | 0.03* (0.01) | |
| Age*Partisan letter | | 0.02 (0.01) | |
| Election info letter*Abstain 08 and 10 | | -0.03 (0.53) | -0.35 (0.66) |
| Top-two info letter*Abstain 08 and 10 | | 0.24 (0.37) | 0.48 (0.47) |
| Partisan letter*Abstain 08 and 10 | | -0.11 (0.37) | -0.04 (0.47) |
| Election info letter*Party NPP | | 0.48 (0.65) | 0.47 (0.65) |
| Top-two info letter*Party NPP | | -0.06 (0.46) | -0.12 (0.46) |
| Partisan letter*Party NPP | | 0.10 (0.46) | 0.07 (0.46) |
| Election info letter*Party REP | | -0.40 (0.66) | -0.37 (0.65) |
| Top-two info letter*Party REP | | 0.45 (0.46) | 0.45 (0.46) |
| Partisan letter*Party REP | | 0.49 (0.46) | 0.48 (0.46) |
| Election info letter*Party OTH | | 0.65 (0.96) | 0.58 (0.96) |
| Top-two info letter*Party OTH | | -0.84 (0.68) | -0.90 (0.68) |
| Partisan letter*Party OTH | | 0.93 (0.68) | 0.94 (0.68) |
| Age 29-38 | | | 6.85* (0.07) |
| Age 39-48 | | | 11.85* (0.07) |
| Age 49-58 | | | 15.55* (0.07) |
| Age 59-68 | | | 22.08* (0.09) |
| Age 69+ | | | 24.12* (0.11) |
| Registered prior to 08 | | | -8.11* (0.08) |
| Registered prior to 10 | | | -3.05* (0.08) |
| Election info letter*Age 29-38 | | | -0.25 (0.77) |
| Election info letter*Age 39-48 | | | -0.31 (0.81) |
| Election info letter*Age 49-58 | | | -0.04 (0.84) |
| Election info letter*Age 59-68 | | | 0.68 (0.96) |
| Election info letter*Age 69+ | | | -0.05 (1.20) |
| Age 29-38*Top-two info letter | | | 0.41 (0.55) |
| Age 39-48*Top-two info letter | | | 0.90 (0.57) |
| Age 49-58*Top-two info letter | | | 0.47 (0.60) |
| Age 59-68*Top-two info letter | | | 1.53* (0.68) |
| Age 69+*Top-two info letter | | | 0.98 (0.85) |
| Age 29-38*Partisan letter | | | 0.05 (0.55) |
| Age 39-48*Partisan letter | | | 0.28 (0.57) |
| Age 49-58*Partisan letter | | | 0.34 (0.59) |
| Age 59-68*Partisan letter | | | 0.69 (0.68) |
| Age 69+*Partisan letter | | | 1.19 (0.85) |
| Election info letter*Registered prior to 08 | | | 1.15 (0.88) |
| Top-two info letter*Registered prior to 08 | | | 0.12 (0.62) |
| Partisan letter*Registered prior to 08 | | | 0.38 (0.62) |
| Election info letter*Registered prior to 10 | | | -1.54 (0.92) |
| Top-two info letter*Registered prior to 10 | | | 0.18 (0.65) |
| Partisan letter*Registered prior to 10 | | | -0.12 (0.65) |
| N | 3855411 | 3855411 | 3855411 |
| R ² | 0.02 | 0.02 | 0.03 |
| adj. R ² | 0.02 | 0.02 | 0.03 |
| Resid. sd | 44.05 | 44.05 | 43.84 |

Standard errors in parentheses

* indicates significance at $p < 0.05$

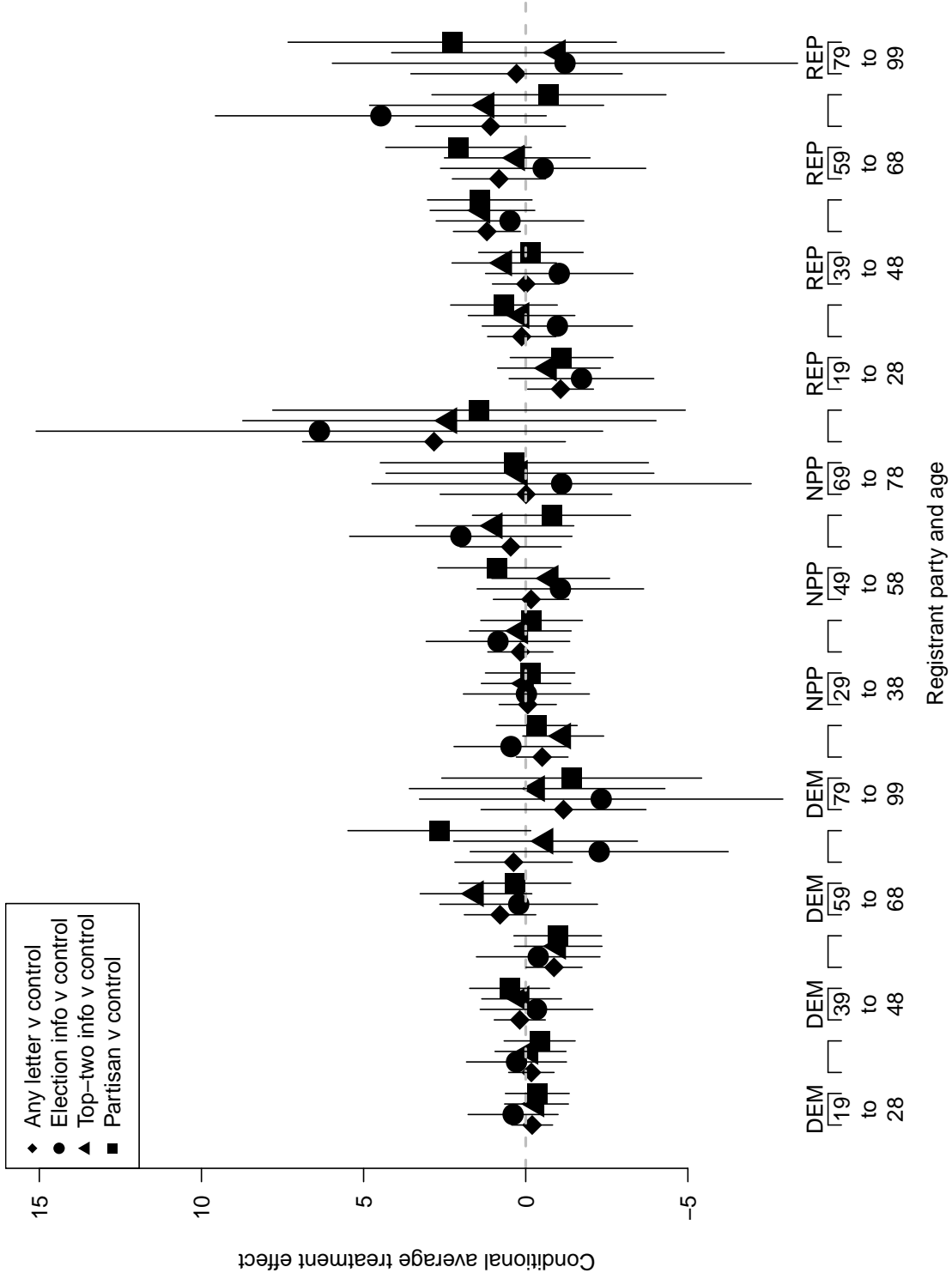
Note: Dependent variable is turnout in that election (100=yes, 0=no). OLS regression coefficients with standard errors in parentheses.

Figure S5: 2014 General Heterogeneous treatment effects by small age ranges



Note: Each point is the coefficient from an OLS regression of turnout on an indicator for that treatment assignment for that subgroup of the target population, with randomization block fixed effects. Confidence intervals extend to ± 1.96 standard errors.

Figure S6: 2014 General Heterogeneous treatment effects by small age cross party



Note: Each point is the coefficient from an OLS regression of turnout on an indicator for that treatment assignment for that subgroup of the target population, with randomization block fixed effects. Confidence intervals extend to ± 1.96 standard errors.

F Example letters

In the next three pages, we present a sample of each of our three treatment letters. The first letter is the election information letter, the second the top-two information letter, and the final an example of the partisan identity letter.



www.commoncause.org

JULIA ROBERTS
123 HOLLYWOOD BLVD
LOS ANGELES, CA 90001

Dear Registered Voter,

You are currently a registered voter in the State of California. *This letter is to remind you that a Primary Election will be held on Tuesday, June 3, 2014.* Polls will be open from 7 AM to 8 PM on Election Day. Don't forget to vote!

Please vote on June 3!

If you choose to vote by mail, your ballot must be received on or before Election Day. Alternatively, you may drop it off in person at any polling place within your county on Election Day. If you receive a mail ballot but you would like to vote in-person, you must bring your blank mail ballot with you to your polling location or you will have to vote provisionally.

If you have any questions about the voting process, please visit the official Secretary of State website (<http://www.ca.sos.gov>) or call your County Registrar of Voters. We hope you will vote in the upcoming June election!

Sincerely,

Kathay Feng, Executive Director
California Common Cause



www.commoncause.org

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LOS ANGELES, CA 90001

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You are currently a registered voter in the State of California. *This letter is to remind you that a Primary Election will be held on Tuesday, June 3, 2014.* Polls will be open from 7 AM to 8 PM on Election Day. Don't forget to vote!

All Californians, regardless of party of registration, now have a voice in primary elections. Voters passed Proposition 14, the Top Two Candidates Open Primary Act, in 2010, which changed the primary election process for congressional, statewide, and legislative races. Proposition 14 declared that, "No voter shall be denied the right to vote for the candidate of his or her choice ... based upon his or her disclosure or nondisclosure of party preference." Important changes to be aware of are that:

- All voters can now vote in the primary election.
- All candidates appear on one ballot.
- Candidates are not nominated by the party.
- Any voter can vote for any candidate.
- The two candidates with the most votes move on to the General Election in November.

Please vote on June 3!

If you choose to vote by mail, your ballot must be received on or before Election Day. Alternatively, you may drop it off in person at any polling place within your county on Election Day. If you receive a mail ballot but you would like to vote in-person, you must bring your blank mail ballot with you to your polling location or you will have to vote provisionally.

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Sincerely,

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In the last California primary election, held in 2012, only 18.5% of voters like you registered as No Party Preference turned out to vote and make their voices heard. Voting in the primary election determines the candidates for the general election. I hope you will take advantage of this opportunity to make your voice heard.

Your voice starts with your vote. As a voter, you help decide who will lead us. You make your voice heard on important issues that affect the future of our state and nation. Every time you exercise your precious right to vote, our democracy grows stronger.

Please vote on June 3!

If you choose to vote by mail, your ballot must be received on or before Election Day. Alternatively, you may drop it off in person at any polling place within your county on Election Day. If you receive a mail ballot but you would like to vote in-person, you must bring your blank mail ballot with you to your polling location or you will have to vote provisionally.

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