Do Open Primaries Improve Representation?

An Experimental Test of California's 2012 Top-Two Primary*

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To improve representation and alleviate polarization among US lawmakers, many have promoted open primaries—allowing voters to choose candidates from any party—but the evidence that this reform works is mixed. To determine whether open primaries lead voters to choose ideologically proximate candidates, we conducted a statewide experiment just before California's 2012 primaries, the first conducted under a new top-two format. We find that voters failed to distinguish moderate and extreme candidates. As a consequence, voters actually chose more ideologically distant candidates on the new ballot, and the reform failed to improve the fortunes of moderate congressional and state senate candidates.

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According to a large body of research in American politics, citizens' political preferences are not polarized but their choices over who represents them are (Bafumi and Herron 2010; Fiorina, Abrams, and Pope 2005). Despite politicians and other party elites being more ideologically polarized than at any time in the past century (McCarty, Poole, and Rosenthal 2006; Bonica 2013), ordinary citizens still tend to claim to be ideological moderates and hold ideologically heterodox bundles of positions (Baldassari and Gelman 2008; Broockman 2014; Fiorina and Abrams 2009). On its face, such a divergence between voters' preferences and the choices on offer is potentially worrisome for democratic representation (Bafumi and Herron 2010; Fiorina and Abrams 2009). But the ramifications of elite polarization are potentially more far-reaching and disturbing. In the past four years, gridlock over routine nominations in the Senate spurred Democrats to "go nuclear" and change the filibuster rules. Partisan brinkmanship led Standard and Poor's to downgrade the nation's credit rating for the first time in history. And ideological warfare produced the first federal government shutdown in seventeen years. Since voters say they want effective governance above all else (Fiorina, Abrams, and Pope 2005), the rise of legislative intransigence as a consequence of rigid ideological divisions further signals a breakdown of representation.

Reforming primary elections to make them more "open" is one proposed solution for this worrisome state of affairs. Open primaries take several forms but generally allow voters to cast ballots for candidates of more than one party. The boldest recent reform is the top-two, or runoff, format. Designed to reduce partisan control of the nominating process, it places candidates with any party label (or no party label) on a single ballot, with the top two vote getters, regardless of party, then competing in the general election. On the assumption that voters prefer candidates more proximate to their own ideology and that in the aggregate they tend to prefer centrist policies, reformers theorize that these changes will benefit moderate candidates who, once elected, will be more willing to compromise. Proponents also argue that open primaries may increase participation by moderate voters (though see McCarty 2011, 365). This reasoning

is consistent with political scientists' models of primary systems (Aranson and Ordeshook 1972; Coleman 1971; 1972; McGann 2002) and was the pitch California voters heard in 2010 from Abel Maldonado, the author of the Top-Two Primaries Act that won 54% in a referendum and took effect in the June 2012 primaries.[1]

Many appear to have concluded that open primaries are likely to improve ideological congruence between voters and candidates, thereby yielding less ideologically extreme legislators (Burden 2004; Fiorina, Abrams, and Pope 2005; Kanthak and Morton 2001; Hacker and Pierson 2005; Mann and Ornstein 2012). Indeed, McCarty (2011, 363) observes, "It seems almost a logical certainty that opening primary elections to more nonpartisan and independent voters should have a moderating effect on politics by increasing the chance that moderate candidates get nominated." But for all the apparent certainty, empirical evidence is mixed.

On one side, a handful of studies find that open primaries do moderate political outcomes. For example, Members of Congress' (MCs) roll call votes from 1982-1990 appear to hew more closely to their districts' ideological leanings in states with semi-closed or open primaries than in states with closed party primaries (Gerber and Morton 1998). And, examining California's first attempt at primary reform in 1998, Gerber (2002) concludes that moderates were more likely to advance to the general election in state legislative races in 1998 than in 1996, controlling for other characteristics of the contests.

On the other hand, several studies fail to find that open primaries moderate politicians. Analyzing state legislators' roll call votes from 1996-2006, McGhee et al. (2014) conclude that open primaries are not associated with reduced legislative polarization at the state level. McGhee (2009) reaches similar conclusions about MCs' roll call voting. According to several studies, ideologically extreme congressional candidates fail to fare better in closed primaries compared to open primaries (Brady, Han, and Pope 2007; Hall and Snyder 2013).[2] In another study of the 1998 California blanket primaries, Bullock and Clinton

(2011) find that MCs elected in 1998 were on average no more moderate in their roll call voting than those elected in 1996 under the closed primary system, though MCs in competitive districts may have moderated slightly.

Studies on primary reform thus have produced mixed findings, but they have also relied solely on cross-sectional or pretest-posttest observational designs, which have well-known shortcomings for causal inference (Campbell and Stanley 1963). Consequently, inferences from these studies about how primary reforms affect polarization have limited certainty. To overcome these limitations, we turned to an experiment. In a large survey of registered California voters conducted just prior to the June 6 California primary, we randomly assigned half the sample to vote with a ballot identical to the one they would see in the actual top-two primary (treatment), and the other half to the traditional ballot they would have seen had the referendum (Proposition 14) failed (control). With this design, we assess whether the reform led voters to choose candidates closer to their claimed ideologies—that is, whether the reform improved proximity voting—and whether it helped moderate candidates for the US Congress and the California State Senate.

The survey in which we conducted the experiment is one of the most comprehensive studies of congressional primaries in a state, enabling analysis of a range of factors potentially related to the success or failure of primary reform. We therefore begin the article not with the experimental results themselves but with an empirical analysis of whether the assumptions reformers often make are plausible. In particular, we examine whether district electorates are indeed more moderate than partisan primary electorates, and whether voters have the knowledge necessary to pick proximate (and therefore often moderate) candidates.

Previewing the findings, voters appear to know so little about the candidates' positions that, even if they wanted to, they could not intentionally cast a ballot for their district's moderate candidates. They distinguish ideologically between Democrats and Republicans, but do not distinguish between candidates

within party. In fact, voters often perceive Tea Party candidates, "Occupy" candidates, and genuine moderates as equally centrist. As one might therefore expect, voters failed to choose more proximate candidates under the top-two format in our experiment. To a reformist's chagrin, this suggests that voters lack the knowledge to incentivize centrism in open primaries for Congress and California State Senate. More broadly, this lack of finer-grained information about candidate ideology runs contrary to assumptions frequently made in spatial models of legislative elections, and suggests limits to citizens' ability to hold politicians accountable for ideological extremism even with open primary reform.

The article now proceeds as follows. First, we describe the experiment and the data we collected on candidates. Second, we bring these data to bear on the most critical assumptions underlying reformers' hopes. Third, we analyze the data at the level of individual voters, focusing on whether the reform led them to choose more ideologically proximate candidates. Fourth, we explore alternative interpretations and look for evidence of the reform's effectiveness among subgroups of voters. Fifth, we analyze the experimental data at the candidate level to determine whether the top-two reform improved the vote share of moderate candidates. Sixth, we extend the analysis from congressional races to state senate races. Finally, we conclude by discussing the implications of our negative findings as well as their limitations for a longer-term assessment of the new ballot form.

Experiment and Data

In the 10 days before the 2012 California primary, we polled 4599 registered California voters recruited through Survey Sampling International (SSI). Although not a probability sample, the participants represent the population on party registration, ideological self-placement, and other key demographic variables. The survey's results on vote choice also closely mirror the results of the actual election.[3]

The top-two ballot can only help moderate candidates when they appear on the ballot and compete against more extreme candidates. Accordingly, about one month before the election, we classified

districts into three categories: (1) no chance the reform could help a moderate, (2) a slight chance it could help, (3) a better than slight chance it could help. We based these decisions largely on whether at least one viable moderate faced at least one (more) extreme candidate who was also viable, taking into account the district's partisan registration and electoral history. We conducted the ballot experiment in the 34 of California's 53 congressional districts that fell into categories 2 and 3. In these districts, we considered 130 of the 238 candidates who ran as viable (based on the authors' pre-primary assessments of previous election results, endorsements, media coverage, and money raised).[4] Our analysis mostly focuses on viable candidates in the 20 category 3 races, which we call "best-case districts" (but the results are the same for all candidates; see Table 3). These contests had 110 candidates, 58 of whom we considered viable.

To determine whether moderate candidates benefit from the top-two primary, we need to identify the moderates. Here, we employ multiple measures. To learn about participants' perceptions of the candidates, we asked participants to rate the ideology of their districts' candidates using a 7-point scale after they reported their intended vote choice. While these ratings allow us to answer questions about voters' knowledge of the candidates, they may fail to reliably measure candidate ideology because voters tend to project their own positions onto their preferred candidates (Markus and Converse 1979; Granberg and Brent 1980; Dalager 1996; Merrill, Grofman, and Adams 2001; cf. Krosnick 1990), a tendency that appears markedly in these data.[5] We therefore assess candidate ideology by averaging four other measures. First, prior to fielding the survey, the research team visited candidates' websites, scoured media coverage of the races, and attempted to rate the candidates with the 7-point ideology scale. Second, we hired 204 politically knowledgeable Mechanical Turk workers to visit websites for viable candidates in the 20 category 3 districts and rate those candidates on the 7-point scale.[6] Third, we use Campaign Finance scores (CFscores), which map candidates into an ideological space based on the sources of campaign

contributions (Bonica 2014). Fourth, we make use of Project Vote Smart's database of candidates' positions, which reflects candidate surveys and imputed positions from candidate statements, and which we transform into an ideology measure using a unidimensional item response theory (IRT) model. We standardized the means and variances of these four measures to zero and one, respectively, and averaged them into a single index (no listwise deletion). They correlate reasonably well for Democratic candidates, less well for Republicans.[7] Each of these measures suffers from potential drawbacks, including substantial measurement error and missing data. We believe averaging these four is the most defensible approach for measuring candidate ideology, and the average score correlates with representatives' ideologies as measured with roll-call votes (first-dimension DW-NOMINATE scores) at 0.61 for Republican incumbents and 0.73 for Democratic incumbents (Poole and Rosenthal 1997). But our findings are similar when we use each item individually, as we show below (see Table 2). We also use this 4-item index to evaluate the accuracy of voters' perceptions by comparing candidates' scores to voters' own placements of the candidates.

The Logic and Assumptions behind Open Primary Reform

According to their advocates, open primaries should improve ideological representation because they allow voters to choose candidates who agree with them ideologically regardless of party. For example, they allow moderate Republicans to vote for a moderate Democratic candidate or allow an independent to vote for a moderate Republican candidate. There are, however, several reasons why voters may fail to cast ballots for moderate candidates in open primaries. And, if voters succeed in doing so, there are also reasons why moderates may not end up winning. In this section, we briefly examine the logic behind reformers' hopes, focusing on four potential obstacles: voter ignorance, strategic voting, voter extremity, and the problem of more than two candidates.

Do Voters Know Enough to Vote for Ideologically Proximate Candidates?

The first obstacle is voter ignorance. To opt for ideologically proximate candidates on an open ballot, voters must first have some sense for the candidates' ideologies. Do voters know enough about the candidates to make such judgments? Can they distinguish, for example, moderate liberals from extreme liberals? To our knowledge, no large-scale survey has investigated this question in congressional primaries. For general elections, however, studies yield worrisome results: citizens do, on average, see incumbent House and Senate Democrats as more liberal than Republicans, but they generally are unable to distinguish ideological differences *within* parties (Franklin 1991; Snyder and Ting 2002; Tausanovitch and Warshaw 2013). If people cannot distinguish the views of House candidates in general elections, it seems unlikely they could do so in primary elections, when information tends to be scarcer (Moore 1987). Since our survey asked participants to place candidates on the same 7-point ideology scale as they placed themselves, we can shed light on this critical assumption about voters' knowledge.

For advocates of primary reform, the results are discouraging. Voters, we find, know little about primary candidates' ideologies beyond what they can infer from party labels. Indeed, many participants would not even venture a guess about candidates' ideologies, especially when asked about challengers. In the 20 best-case districts (category 3), participants rated 30% of incumbents and 54% of viable challengers with a "don't know" or blank response. When participants did rate the ideology of candidates, they generally hit wide of the mark—often very wide of the mark. In perhaps the most striking example, participants from the 24th District failed to view Abel Maldonado, the moderate Republican who spearheaded the top-two reform, as appreciably more centrist than his Tea Party opponent, Chris Mitchum. As one of the few Republican lawmakers to break the Taxpayer Protection Pledge, Maldonado faced a backlash from the right, culminating in the local GOP's decision to endorse the inexperienced Tea Party candidate Chris Mitchum in the primary. But while Santa Barbara's conservative political elite observed

gaping ideological differences between the two candidates, most constituents in the district failed to do so. They placed the two candidates at almost the same position on the ideological continuum, with Maldonado at 5.21 and Mitchum at 5.26, despite Maldonado raising more than $1.5 million during his primary campaign to advertise his more moderate views (FEC data, June 2010).[8]

To examine the extent of voter ignorance of candidates' ideology, Figure 1 plots participants' perceptions of Democratic and Republican candidates against the 4-item average ideological scores. It plots these relationships separately by candidate party and by incumbency, including the best-fit line (least squares regression) to describe the relationship between perceptions and reality in each subplot, the 45° line as a reference point, and the R^2s that measure the goodness of fit for each regression. Figure 1 reports on the 109 viable major party candidates in our survey.[9] It shows that voters did see Democrats as more liberal and Republicans as more conservative (the study showed party labels on the ballot), but were largely unable to distinguish moderates from extremists within each party. If they had done so, we would see a positive slope similar to the 45° line, indicating that the more conservative the candidate, the more conservative the rating assigned by participants. For incumbents, we see some sign of the expected upward slope, but most of the variation appears to be noise, with R^2s below 0.25. Voters also arguably see incumbents from both parties as more moderate than they really are, with mean ratings varying only between about 2.2 and 3.8 for Democrats and 4.0 and 6.0 for Republicans.

Although voters seem to have an inkling of where incumbents stand, they appear to lack any awareness of challengers' ideology. Their perceptions of non-incumbents from both parties fail to correlate with reality (as measured by the 4-item index score). The R^2s for the best-fit lines in Figure 1 (right plots) are essentially zero, a result that holds up when we disaggregate by both participant and candidate party affiliation. We therefore see little signal in these perceptions, just noise.

(Figure 1 here.)

Voters' knowledge of the candidates' positions appears so limited that a simple party indicator predicts beliefs about candidate ideology better than does the 4-item measure (or any component of that measure). When we regress participants' perceptions of candidate ideology on the party indicator (following Snyder and Ting 2003), we find a slightly better fit (larger R^2) than when we regress it on the 4-item average. This pattern holds across a variety of specifications. This result is consistent with previous findings on voters' inability to accurately locate representatives ideologically within parties (Franklin 1991; Snyder and Ting 2003; Tausanovitch and Warshaw 2013). Our contribution here is to show that this pattern holds for primary candidates as well.

Are Voters Voting Strategically?

Besides ignorance, voters may not choose ideologically proximate candidates on the top-two ballot because of strategic voting. This can take many forms but one of the most problematic for primary reform is raiding. Instead of voting for the candidate who most closely shares a voter's ideology (sincere voting), voters "raid" when they cross over to vote for weaker candidates of the other party (strategic voting), hoping their action will ultimately help their own party's chances in the general election. Research suggests that strategic voting of this kind is rare in primaries (Hedlund 1977; Ranney 1968, 1972; Alvarez 1997; Sides, Cohen, and Citrin 2002), a result we confirm in a later section (in addition to addressing another form of strategic voting, hedging).

If Voters Do Vote for Proximate Candidates, Will It Help Moderates?

If voters do shift towards ideologically proximate candidates on the top-two ballot, will this help moderate candidates win? Not necessarily. First, voters need to be moderates themselves. A body of research supports this assumption, finding that voters are generally ideologically moderate (Fiorina, Abrams, and Pope 2005; Ansolabehere, Rodden, and Snyder 2006; Levendusky and Pope 2011; though see

Broockman 2014 for a methodological critique). The median voters in California congressional districts also tend to be moderate, as measured by constituent policy preferences (Kousser, Phillips, and Shor 2013), a finding we replicate with participant self-reported ideology. Even in California districts with reputations for extremity, such as the 23rd (Central Valley) being conservative or the 13th (Berkeley and Oakland) being liberal, median voters are moderate. Moreover, we find that Democratic candidates encountered median voters more moderate than their closed-primary counterparts in 11 of the 20 best-case districts, while Republican candidates did so in all 20.[10]

Even if voters have the necessary knowledge, vote sincerely (not strategically), and prefer moderate candidates, moderate candidates still may not benefit. Formal models of open primaries and multi-candidate races fail to yield consistent predictions about the ideology of winners, even in moderate districts (Chen and Yang 2002; Cooper and Munger 2000; Cox 1987; Oak 2006). Open primaries may fail to favor moderate candidates for many reasons, but one of the more straightforward is that they allow for more than two candidates. With more than two contestants, the winning platform is not necessarily that of the median voter. In fact, the key to victory in such races is not where candidates locate on an ideological spectrum relative to voters, but how close they are to other candidates (Tullock 1967).

Top-Two Primary Experiment

As the above discussion makes clear, the argument that open primaries will improve ideological representation is not as straightforward as it seems. To examine whether voters do indeed shift towards ideologically proximate candidates and whether the top-two reform ultimately favors moderate candidates, we turn to our experiment. As noted above, we randomly assigned participants to one of two conditions at the beginning of the survey: the new top-two ballot (treatment) or a closed ballot (control). In the experiment, participants assigned to the treatment condition could vote for any candidate running in their

district.[11] In the control condition, participants could only choose candidates from the party they registered with, although independent voters could choose to vote in the Democratic primary and 62% did so.[12]

Before presenting the results, we note that the counterfactual this experiment examines—a closed ballot in 2012—is not quite the right counterfactual to ascertain the overall effect of the reform. Ideally, we would compare the open ballot in 2012 to the closed ballot in 2010. Since the new ballot rules may have attracted different candidates, such as more moderate candidates (Rogowski 2013), and changed incumbents' reelection strategies, our experiment could underestimate the benefits of the open primary reform. Our experiment is nevertheless informative about the key mechanism underlying reform—that voters will shift to moderate candidates when they have the opportunity. Without this shift, candidates have no incentive to appeal to the center (to enter the primaries as moderates or reposition as moderates).[13]

Individual-Level Proximity Results for Congressional Elections

We first test the clear, individual-level prediction that participants in the open-primary condition vote for candidates who are closer to their own self-stated ideologies than do participants in the closed-primary condition. Since most voters identify themselves as moderate, voting for proximate candidates also implies voting on average for moderate candidates. We believe that analyzing proximity is the most revealing of several possible individual-level analyses testing reformers' expectations, and we find similar results with other tests (e.g., comparing how often treated and control participants voted for their districts' most moderate candidate).[14]

To measure ideological proximity between participants and candidates, we calculate the absolute value of the difference between a participant's 7-point ideological self-placement and his or her chosen candidate's ideological score, called the city-block measurement of proximity. We again measure ideology with the 4-item average of: our ratings MTurk ratings, CFscores, and scaled Project Vote Smart items, but

the results are similar with each item individually (see Table 2). The 4-item ideology index is scaled 1-7, so our city block measure of proximity ranges from 0 (a vote for an ideologically identical candidate) to 6. Since we use participants' self-reported ideology, the sample size drops by about 5% because of individuals who failed to self-report (see the Appendix for details on missing data). We code our treatment variable 0 for the closed ballot condition and 1 for the top-two ballot condition.

We begin by presenting the results visually—comparing voters' opportunities to select an ideologically proximate candidate to their actual choices—for the 20 best-case races. As reformers hoped, the top-two primary did improve the potential for proximity voting. Figure 2a plots the distribution of (city-block) distances between voters and the candidates ideologically closest to them, by experimental condition (using a kernel density estimator). It shows that participants in the top-two condition could select candidates closer to their self-stated ideology than those in the closed-ballot condition. This is an important result. It reveals that, at least in one sense, the reform worked—it gave voters the opportunity to pick more proximate candidates. Moreover, it did so in every one of the 20 best-case races,[15]

On the other hand, voters failed to take advantage of this opportunity, as Figure 2b reveals. This figure presents the distribution of actual proximity voting—the absolute distance of vote choice from self-placement—in the two conditions. Voters in the top-two condition are in fact not more but less ideologically proximate to their chosen candidates than those in the closed primary condition. Comparing Figures 2a and 2b, we see that the potential gain from the top-two format went unrealized, a surprising finding.

To summarize these results, Table 1 presents the individual-level bivariate regression of actual proximity voting (the x-axis in Figure 2b) on the treatment indicator variable. It includes district indicator variables (that is, fixed effects for districts), so that the analysis only examines the experiment's effect within districts, not between districts. It confirms that, at least in its first test in June 2012, California's primary reform failed to improve proximity voting. Rather, it appears to slightly increase the distance

between voters and their chosen candidates by an average of 0.15 points on the 7-point scale (95% confidence interval 0.01 to 0.29) in the best-case districts (column 1), a result that is similar across all districts (column 2). Of course, ideological proximity is only one reason to vote for a candidate, but it is striking that the reform worsened proximity voting despite its potential to improve it. In the next section, we show that this result is robust to coding and measurement decisions.

(Figure 2 and Table 1 here.)

Alternative Interpretations and Robustness

Given the earlier findings about voters' misperceptions of candidate ideology, voters may fail to support moderate or more proximate candidates on the open ballot because they simply do not know which candidates are moderate or proximate. Before we reach that conclusion, however, we must rule out several alternative interpretations.

(Table 2 here.)

One concern is that participants may have behaved unusually in our closed-ballot condition because of its artificiality. In particular, if participants were aware of the new ballot format and planned to take advantage of it by voting for a candidate of another party, they may have been confused when they failed to see that candidate on the experimental ballot in the closed-ballot condition, and picked candidates randomly or chosen the don't know option. To investigate this possibility, we asked participants after the vote choice and ideology questions, "Had you heard about the new ballot format before this survey?" Only about half of the participants said they were aware of the change. Table 2 presents evidence that awareness did not affect outcomes. It only reports the key coefficient from the model in Table 1, which is from the regression of city block proximity of vote choice on treatment assignment. As its second and third rows show, the top-two format increased ideological distance by a similar amount among those who were and were not aware of the ballot change.[16] We can also address this concern by examining "don't know"

responses to the vote question. Participants who did not see their preferred candidate on the ballot in the closed condition may have chosen the “don’t know” response at higher rates when asked about voting. If so, we should see more “don’t know” responses in the closed condition, but we do not. The “don’t know” response rate is the same (33%) in the two groups.

Another concern is that turnout in this primary was light and our sample may over-represent individuals who failed to vote and who may therefore be less likely to vote based on ideology and to recognize and reward moderate candidates on the open ballot. Several facts mitigate such concerns. First, we only interviewed registered voters. Second, we exclude from analyses voters who said they would not vote. Third, candidates’ vote share in the survey’s open ballot condition closely matched actual election results. Finally, the results remain the same when we limit the analysis to individuals who said they would definitely vote and to the most politically knowledgeable individuals, as shown in Table 2.[17] Most importantly, if actual voters are more partisan and ideologically extreme, they should be even less likely to cross over and vote for moderate candidates, not more so. To the extent that our survey includes nonvoters, which it undoubtedly does, their presence may work against the finding.

The presence of No Party Preference (NPP) candidates on the ballot for the first time could also potentially explain the failure of the open ballot to reduce ideological distance. Voters may have attributed greater centrism to these candidates simply because of their non-partisan label even when they were not actually centrist. As shown in Table 2’s next row, we find, however, that proximity worsened significantly in districts without NPP candidates.

This raises a related concern: the top-two reform may have led many candidates to adopt more moderate positions than they would have in a closed primary (Rogowski 2013). As a consequence, candidates may have already been unusually proximate to voters in the closed condition, resulting in a ceiling effect—i.e., voters could not choose more proximate candidates on the top-two ballot. However, this

conjecture is inconsistent with the evidence. First, viable candidates held a diverse range of positions. Second, the reform appears not to have changed the distribution of candidate positions compared to the previous primary. Third, as Figure 2 shows, voters had the potential to select far more proximate candidates on the top-two ballot, inconsistent with a ceiling. Finally, a proximity ceiling should not lead voters to make less proximate choices on the top-two ballot, but they do.[18]

Strategic Voting: Hedging and Raiding

Yet another possible interpretation is that ideological distance may increase with the top-two format, not because voters are making poor decisions, but because they are strategically voting for more distant candidates. One such form of insincere voting is called hedging: when voters' proximate choices have little chance of winning the election, voters may vote strategically for more distant candidates. In particular, Republicans in California's many Democratic-leaning districts may choose to vote for a Democratic candidate in the top-two condition because the Republican candidates are unlikely to finish in the top two or win the general election—likewise for Democratic voters in the handful of Republican leaning districts. They may also cross over simply to participate in a competitive contest (Alvarez and Nagler 2002; Kousser 2002; Salvanto and Wattenberg 2002). If hedging is common, the open ballot may worsen proximity not because of voters' ignorance about candidate ideology, but because they are strategically choosing to vote for candidates who happen to be more distant from themselves ideologically.

To see whether hedging lies behind our key finding, Table 2 shows the open-ballot effect by two variables that likely predict hedging: whether district partisanship corresponds with participant partisanship and whether the district has an incumbent. The estimates provide some support for hedging—the top-two ballot does worsen proximity more in districts where party registration leans against the participant or in districts with an incumbent—but they show that hedging does not explain our key finding. Even in districts

where we would expect less hedging (e.g., open races), we still fail to find that the open ballot improved proximity.

Another way we examine whether hedging drives these results is to look at voters' choices when they do cross over and vote for a candidate of the other party. If they are hedging, we should see them generally voting for the most proximate candidate from the other side (of course, this assumes they know the candidate positions, which seems unlikely given the above findings). When a moderate Republican crosses over to vote for a Democrat, for example, she should tend to select the most moderate Democrat. To investigate this, similar to the logic of Figure 2, we compare the minimum possible proximity to an out-party candidate to the actual proximity of vote choices made by the 16.2% of participants who crossed over. We find that crossover voters chose candidates over half a point more distant from their own self-reported ideology than they could have, on average ($p < .001$). Indeed, just 52.7% of crossover voters selected the most proximate candidate from the other party.

Of course, ideological distance may be large for crossover voters because of another form of strategic voting that we have already discussed: raiding. Voters raid when they attempt to sabotage the other side by voting for its weakest candidate. Raiding seems likely to be rare in this primary. Studies have generally found little evidence of it in a variety of elections (Hedlund 1977; Ranney 1968, 1972; Alvarez 1997), including the 1998 California blanket primary (Sides, Cohen, and Citrin 2002). Moreover, raiding should be rarer in this type of open primary than in other types, such as blanket primaries, because voters lack a guarantee that their party's candidates will move on to the general election, and so raiding could jeopardize their own party's chances (Sinclair 2013). Nevertheless, we searched extensively for evidence of raiding but found no clear sign of it. In fact, when voters cross over, they tend to vote for the same candidates as their out-party peers. Of course, strategic voting can take many other forms. However, given

these results and the low salience and low information nature of the primary, substantial strategic voting seems unlikely.[19]

Robustness to Measurement and Coding Decisions

In the remainder of Table 2, we show that the tendency of the top-two ballot to increase ideology-vote distance (worsen proximity) is robust to measurement and coding decisions: it holds using each of the component ratings of the four-item average (author ratings, Mechanical Turk ratings, CF scores, Project Vote Smart ratings), and when using a factor score from all four ratings based on principal component factor analysis.[20] It holds when we calculate proximity with Euclidean distance instead of absolute distance. It holds when we calculate ideology-vote proximity with the average respondent placement of candidates. The only estimate where the sign becomes negative, the expected direction, is when we calculate proximity with respondents' own placement of the candidate they voted for, though the coefficient is imprecisely estimated (the sample size decreases here because we exclude respondents who failed to place the candidate they voted for). Given voters' tendency to project their own ideology on to candidates, we are surprised by this weak result. Finally, the main finding is robust to weighting the survey data to 2012 Census Current Population Survey data for California.[21] In the next section, we show that this result holds up among many subgroups.

Did the Reform Help Among Subgroups?

According to this experiment, the top-two primary failed to improve proximity voting and help moderate candidates as reformers expected. So far, we have focused on the average effect of the top-two ballot on candidate vote share and individual voters' choices, which is ultimately what matters for reform. We now briefly look for glimmers of reformers' hopes among certain types of voters or districts. As we show in Table 3, the open ballot condition failed to clearly improve proximity among ideologically moderate voters or

among voters who lack a party affiliation. It also failed to do so in districts that are closely divided along partisan lines or that have a large number of independent voters (Bullock and Clinton 2011). In these districts, moderate candidates may have a chance, and so voters may be more willing to cross party lines to vote for them, but we fail to find that they do. Additionally, although one might expect the size of the field to affect the top-two format's ability to make a difference—the number of candidates could influence positioning (Osborne 1995; Cox 1987), or a larger field could increase the informational demands on voters (Lau, Andersen, and Redlawsk 2008). We find no such pattern. Finally, we find similar results when we include all non-viable candidates. Overall, these analyses yield little reason for optimism about the success of the top-two primary in achieving its purported goal in 2012.

(Table 3 here.)

Candidate-Level Results for Congressional Elections

In this section, we test whether moderate candidates received a larger vote share in the top-two ballot condition than in the control ballot condition. Given that voters failed to shift towards ideologically proximate candidates, moderate candidates seem unlikely to benefit. Nevertheless, testing whether they do is important in part because it closely captures the hopes of reformers. It also provides us with yet another robustness check, further addressing potential concerns about posttreatment bias in self-reported ideology scores (since this analysis is at the candidate-level, we do not rely on self-reported ideology).

In this analysis, the outcome variable is the vote share for moderate candidates. To avoid potential biases, however, we calculate this difference in vote share—the treatment effect—separately by voter party registration over all viable candidates in the primary.[22] (Since voters cannot vote for out-party candidates in the control condition, we assign a value of zero vote share in these cases.) The unit of analysis is therefore vote share for each candidate from voters from each party (Democratic, Republican, and independent/other), so candidates appear three times in the data set.[23] Given that we only conducted the

experiment in 34 districts and that a reasonable share of participants did not report an intended vote choice, the sample size drops considerably from the 4599 voters initially surveyed (see the Appendix for details on the sample size and missing data).

Figure 3 presents this analysis for the 20 best-case districts (category 3), although the results are similar for all districts, as we show below. The vertical axis depicts the difference in vote share between the open and closed ballot conditions for each candidate (after removing the main effects for the six types of party and voter registration), while the horizontal axis places candidates from most extreme to most moderate, determined by folding our 4-item ideology measure (so extreme liberal and extreme conservative are now coded 1 and moderates continue to be coded 4). If the reform helped moderate candidates, we should see an upward slope in the scatterplot—that is, we should see the more moderate candidates receiving more votes under the open ballot than under the closed ballot. Instead, however, we find a downward slope. After recoding candidate moderation to 0-1, we estimate the slope at -0.021, which implies that an extreme candidate, located at 1 or 7 on the ideology scale, would experience a 2.1% loss in vote share were she to relocate at 4 with the transition from the closed primary to the top-two primary. Table 4 presents the regression of the difference in vote share between ballot formats on candidates' moderateness, the same best-fit line shown in the plot. It shows that the slightly negative slope we find is imprecisely estimated (95% confidence interval -0.17 to 0.14), so it could be consistent with a more positive or negative effect. The regressions include dummy variables for voter party registration interacted with candidate party (fixed effects) and we cluster the standard errors at the candidate level. In short, the experimental results fail to support the hypothesis that the top-two format helped moderate candidates.

We repeated these analyses in all 34 districts where we conducted the ballot experiment. We would expect the open ballot to benefit moderates even less in this larger set of districts, but we actually find a slightly positive coefficient of 0.039 (see Table 4, column 2), though again, it is imprecisely estimated.

(Figure 3 and Table 4 here.)

Finally, we may fail to find that the open ballot helped moderates because these centrists competed against each other in some cases. We test this possibility by repeating the analysis from Table 4 after combining the vote shares of similarly positioned candidates within districts. (For example, if two candidates from the same district had ideological scores of 3.8 and 4.2, we would round their ideologies to 4.0 and combine their vote shares.) We continue to fail to find an effect after rounding ideology scores to the nearest half-point and full-point. We also investigated whether the top-two ballot helped moderates finish in the top two, but found no sign that it did—if anything, extreme candidates fared better on the open ballot. In sum, we find scant evidence that the top-two ballot reduced polarization by helping moderate candidates in 2012.[24]

Results in State Senate Races

Are the reported results for congressional contests consistent in down-ticket races? The evidence from the June 2012 California state senate primaries indicates the same outcomes. In fact, voters knew even less about candidates in these races than congressional candidates, and voters randomly assigned to the top-two condition again failed to support moderate candidates at higher rates or to vote for more proximate candidates than those assigned to the closed ballot. And as yet another hurdle for the reform, voters appeared more hesitant to vote for out-party candidates in these races: just 6% of partisans crossed over, compared to 16% in house races. These results provide evidence of the reform's apparent failure in 2012 across multiple electoral contexts.[25]

Discussion and Conclusion

Pundits and scholars frequently assert that closed primary elections contribute to ideological polarization in legislatures across America (e.g., Nivola and Galston 2006). As a cure for polarization and its effects on legislative behavior—gridlock and paralysis—many advocate open primaries, arguing that this institutional

reform will improve ideological congruence between representatives and the represented, and thus yield more moderate legislators. For this logic to hold, however, so too must multiple assumptions regarding voting behavior, including that citizens have some sense for where candidates stand (or at least act as if they do). We find little evidence consistent with this assumption about voters' knowledge. In particular, we find that voters in House races failed to distinguish between relatively extreme and centrist candidates of the same party, and thus appeared unable to engage in the sort of ideologically nuanced voting that spatially-minded theorists and reformers envisioned. More to the point, we find that voters failed to shift towards ideologically proximate candidates on the top-two ballot. Consequently, the top-two primary reform appears so far to have failed to help centrist candidates in California and thus may not be the promised cure for polarization.

Because our study is experimental it has an advantage over previous studies, but it also has limitations. Open primaries may still moderate legislators' behavior even if voters fail to recognize or explicitly reward such centrism because politicians may mistakenly think they do. Indeed, one study finds that roll call voting in the California state assembly moderated somewhat following the reform (Grose 2014). Candidates and voters may also take time to learn about and adapt to new rules, as they did for earlier electoral reforms in California (Gaines and Cho 2002; Masket 2009). In particular, it may take time for moderate candidates to learn how to effectively inform voters about their centrist views. It may also take time for moderate politicians to react to the rule change and enter the field at greater rates. The top-two ballot's effect on candidate entry and positioning is beyond the scope of our study (though the similarity in the distributions of estimated primary candidate ideology in 2010 and 2012 is potentially inconsistent with an effect; more generally, see Rogowski [2013]).

These results may also not generalize to other states and electoral contexts. California is the most polarized state at the elite level, as measured by roll call votes in the state legislature (Shor and McCarty

2011), which may disadvantage centrists. It is also worth noting, however, that the 2012 House primaries in California featured several well-funded and experienced moderates, and yet voters still failed to recognize their ideological centrism and disproportionally vote for them when given the chance (e.g., Abel Maldonado in the 24th District and Anthony Adams in the 8th).

Open primaries may also succeed in higher salience races, such as gubernatorial or US senatorial contests, where voters have better access to information about candidate ideology. In fact, in contrast with our findings about voter ignorance in this article, a small fraction of voters appear to learn something about non-incumbent gubernatorial and senatorial candidates' positions in closed primaries and use this information to select proximate candidates (Hirano et al. 2014). Future work could investigate whether open primaries help moderate candidates in higher-salience races. It is worth keeping in mind, however, that research on the introduction of the direct primary in US Senate races found no sign that these primaries contributed to polarization (Hirano et al. 2010).

Even if voters fail to favor moderate candidates in the open primary, they may still do so in the general election, especially those where same-party candidates run against each other. Kousser et al. (2013) investigate this question for the eight such California congressional races in 2012 and conclude that the moderate candidate won in half of the cases but lost in the other half. More generally, they conclude that California elected slightly more extreme candidates in 2012 than in 2010, despite redistricting reform and the open primary, a conclusion consistent with this article's findings.

Our findings also have implications for research on voter competence. Scholars have noted that, by using heuristics, voters may act as if informed even though they are not (Lupia 1994). For example, one could use the gender or race of a candidate or her endorsements as a proxy for ideology. However, little research to this point has investigated the quality of heuristics in legislative primaries—low-information electoral contexts in which the predominant heuristic for ideological voting (candidate partisanship) may fail.

At least in this case, the experimental results cast doubt on the quality and availability of heuristics in legislative primaries. If voters could rely on heuristics, they should select more proximate candidates when provided the opportunity to do so in the open-ballot condition, but we failed to find this result. Of course, with time, campaigns and interest groups may learn to provide voters with the cues they need.

The intent of open primaries is to counter growing polarization among representatives and thus potentially to improve democratic accountability. Yet in the case examined here no such improvement occurred. For significant change to develop, reformers may have to go beyond the rules to find a way to substantially increase voters' information. In fact, by increasing voters' options without increasing information, our experiment suggests that the top-two ballot led to slightly worse voting decisions: voters chose candidates ideologically further from themselves under the top-two format than with the closed ballot. Since most voters are moderate, this means they chose more extreme candidates on average. Given the limitation of voters' information, reforms that make voters' decisions harder may lead to worse, not better, decisions, a result supported by some laboratory studies (Cunow 2013). More positively, we did find that the top-two primary created the potential for improved proximity voting. But realization of this outcome will ultimately hinge on the ability of motivated candidates and interest groups to improve voters' knowledge of the electoral landscape and, perhaps, to reduce the hold of party identification on voting decisions. Predicting the future is a fool's game, to be sure. But politicians respond to incentives in the long run, so the jury remains out on the consequences, intended and unintended, of this latest reform.

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Appendix

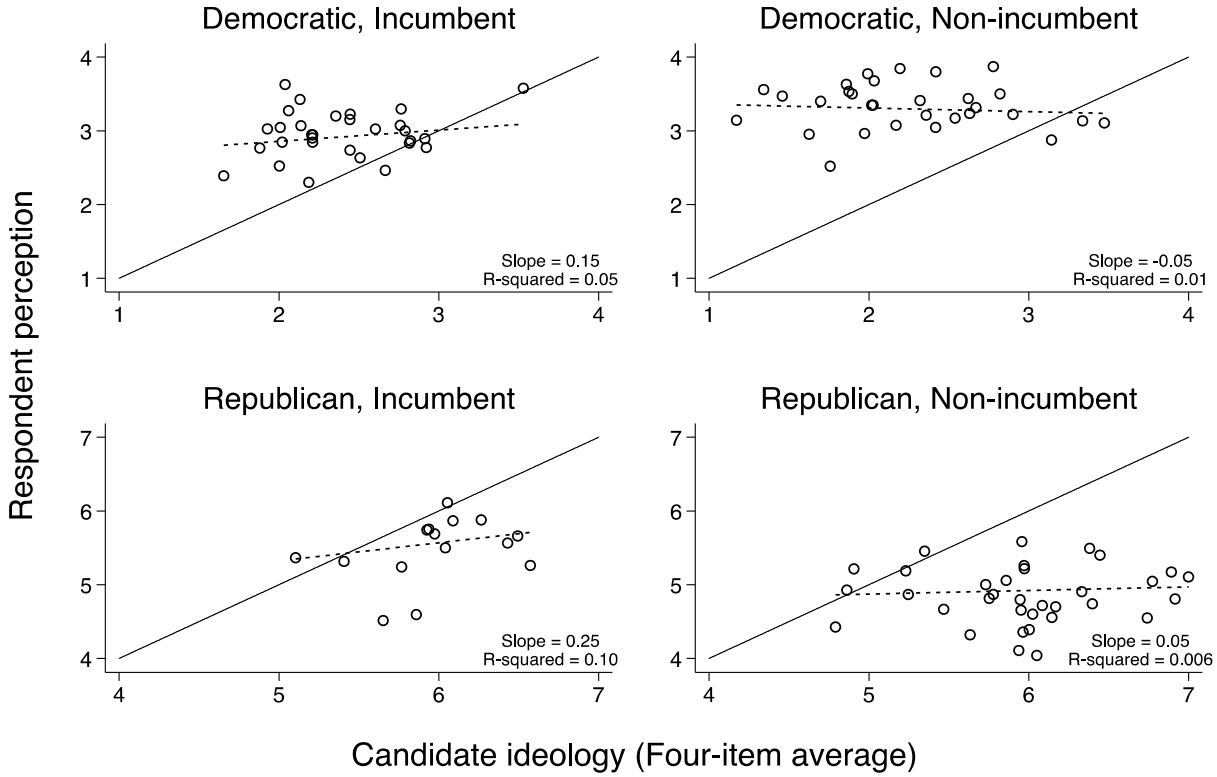
Given the complexities of the survey, accounting for the sample sizes in analyses is complicated. To make things clear, the table below shows how we get from the sample sizes of the full survey, to the actual samples used in the analyses.

As the table shows, we lose a large number of participants because they failed to report a vote intent on the ballot. Given the low salience of the primary and low turnout rate in the actual primary, this falloff is not unexpected. As we note in the article, however, we find a similar rate of "don't know" responses to the vote choice question in the treatment and control groups.

Total N for survey (completed first question): 4,599	*20 best-case districts (category 3)*	*All 34 districts (category 2 & 3)*
N for		
Districts included in ballot experiment (excludes faces exp. districts)	1,775	2,916
Excluding participants who said they would not vote	1,670	2,733
Excluding participants who did not report a vote intent	1,043	1,683
Excluding participants who voted for nonviable candidates	878	1,452
Excluding respond. who voted for candidates for whom we lack ratings	878 (Table 4,col. 1)	1,449 (Table 4,col. 2)
Excluding participants who failed to report their own ideology	835 (Table 1,col. 1)	1,365 (Table 1,col. 2)

Note: This table does not show the sample sizes for 19 category 1 districts, in which we interviewed (4,599-2,916 =) 1683 participants. In these districts, we conducted an unrelated experiment about candidates' faces, though we do use these participants in Figure 1.

Figure 1: Participants' Perceptions of 7-Point Candidate Ideology (Incumbents and Viable Challengers Only)



Note: The figures show participant perceptions of 109 viable, major-party candidates for 52 of California's 53 districts against our measurement of their actual positions using the four-item average (author ratings, MTurk ratings, Project Vote Smart ratings, and CF scores). As a reference, each plot also shows the 45° line. The weak or absent relationship between perception and reality also holds separately for each of the ideology measures (not just the four-item average) and they hold when we only examine the 20 best-case districts. They also hold when we examine only same-party perceptions (e.g., Republican perceptions of Republican candidates). After we exclude participants who say they will definitely not vote, 4,520 participants rated at least one candidate. We weight the least-square estimates by the number of participants rating each candidate, which averages 38 for incumbents and 25 for challengers. We do not show the plot for the 11 no party preference (NPP) candidates for whom we have ratings (we lack data on three candidates), but the findings are similar with a slope of 0.06 and an R^2 of 0.04.). In a larger version in the online supporting information (section 6), we label each data point with the candidate's last name and district

Figure 2a: Possible Proximity Voting in Top-Two and Closed Ballot Conditions

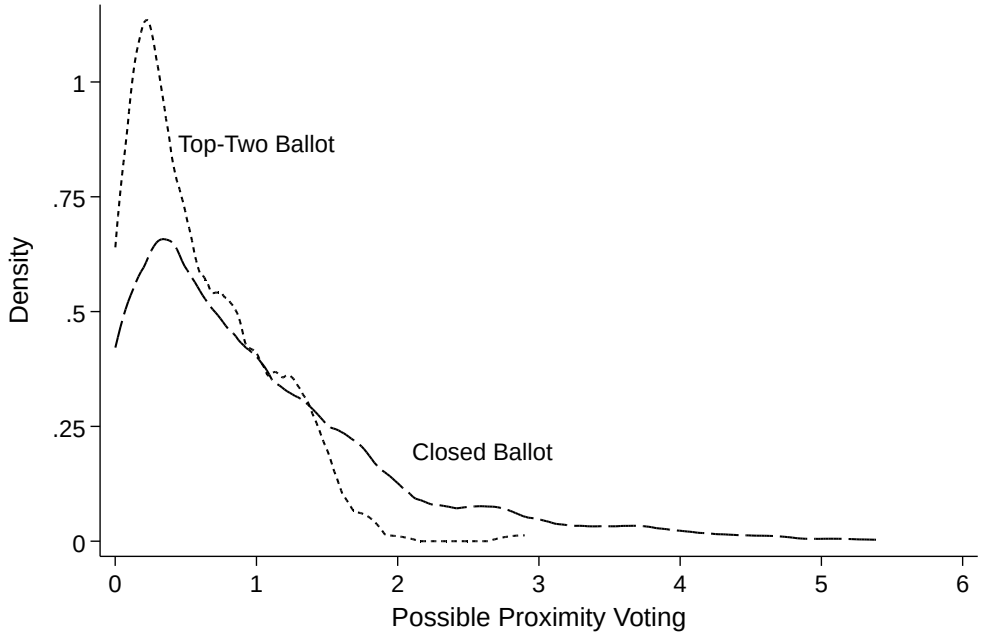
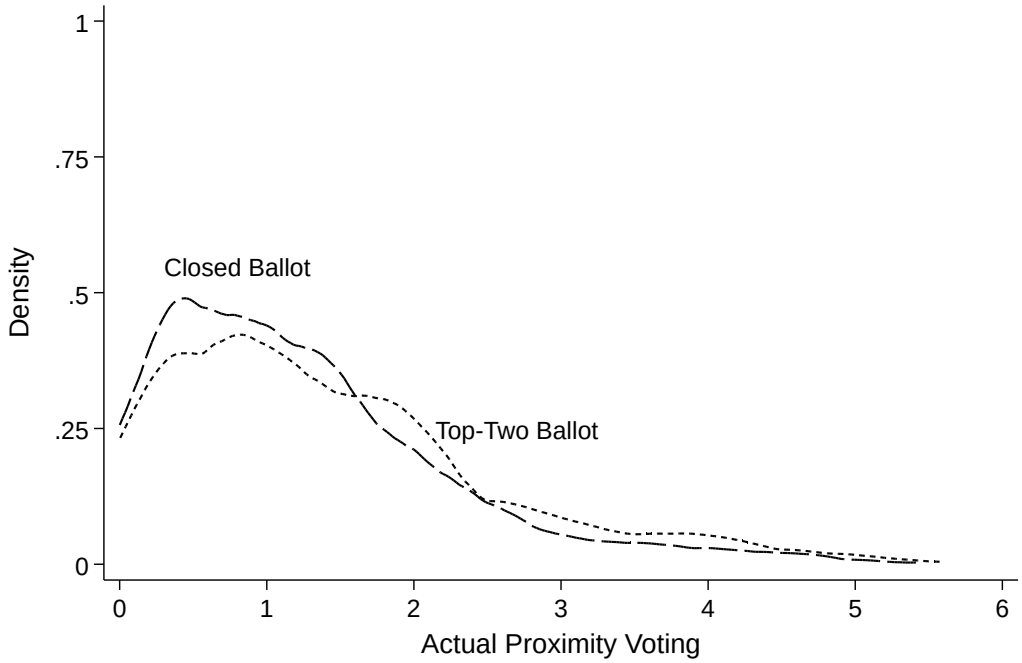
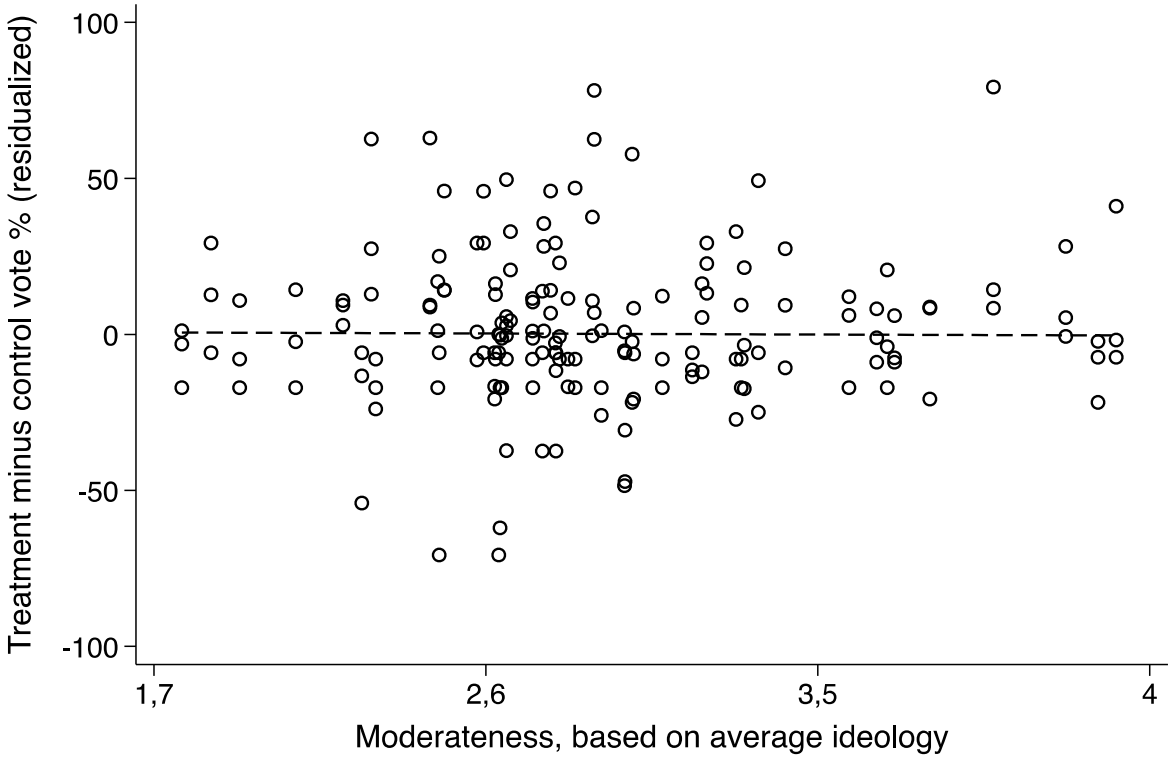


Figure 2b: Actual Proximity Voting in Top-Two and Closed Ballot Conditions



Note: The figures show proximity calculated using the city-block method for best-case districts (category 3). In section 11 of the online supporting information, we show that the pattern is robust: it holds using the Euclidean method (see 11.1), in all 34 districts where we conducted the experiment (11.2), separately for registered Democrats, Republicans, and independents (see 11.2), with average participant perceptions (see 11.3), and with individual participant perceptions (see 11.4).

Figure 3: Did the Top-Two Primary Help Moderate Partisan Candidates?



Note: Each point represents a candidate's between-conditions vote differential among a particular registration group (Democrats, Republicans, or independents). In a larger version in the online supporting information (section 9), we label each data point with the candidate's last name, district, and the party registration of voters used to calculate treatment minus control vote percent (R, D, or I). The figure shows only best-case districts (category 3), but the same result holds in the 34 districts where we conducted the experiment (see supporting information section 9.1). We also drop one outlier: Lowenthal47I. Table 4 presents the regression model for these data, (Table 1 shows an individual-level model, and Tables 2 and 3 show that the individual-level finding is robust to coding and measurement decisions.)

Table 1: Did the Top-Two Primary Improve Proximity Voting? No

	(1) Best-case districts (category 3)	(2) All 34 districts (category 2 & 3)
	DV: Absolute distance of vote choice from self-placement	
Top-two ballot condition (treatment)	0.15**	0.13**
	(0.07)	(0.06)
Constant	1.22***	1.23***
	(0.05)	(0.04)
Observations	835	1,365
R-squared	0.02	0.04

Note: This table shows that the top-two ballot condition slightly increases absolute distance between participants' vote choices and their own ideologies (that is, reduces proximity). Standard errors in parentheses. We do not cluster the standard errors at the district level because, with only 20 (or even 34) clusters, clustering is unreliable (Angrist and Pischke 2009, ch. 8). We therefore likely underestimate standard errors, but showing that the true standard errors are actually larger, which they no doubt are, would not substantially alter our conclusions. District fixed effects included but not shown in table. Analysis restricted to viable candidates, but Tables 2 and 3 show that this pattern is robust to this and numerous other coding decisions. See the Appendix for an accounting of the number of observations. *** p<0.01, ** p<0.05, * p<0.1

Table 2: The Top-Two Ballot Fails to Improve Proximity Voting—Additional Robustness Checks

	Effect of treatment on ideological distance (SE)	*N candidate*	*N participant*
Full sample	0.154(0.070)	58	835
Respondent and Contest Characteristics			
Unaware of open ballot change	0.168(0.109)	55	409
Aware of open ballot change	0.144(0.094)	53	426
Said they would "Definitely vote"	0.070(0.082)	57	613
Knowledgeable voters (3/4 > of knowledge Qs)	0.052(0.076)	58	548
Districts with no No Party Preference (NPP) candidates	0.355(0.129)	18	227
Strategic Voting: Hedging			
Republicans in dist. with more registered Reps. than Dems.	-0.079(0.171)	12	143
Republicans in dist. with more registered Dems. than Reps.	0.145(0.137)	29	165
Democrats in dist. with more registered Reps. than Dems.	0.546(0.295)	13	75
Democrats in dist. with more registered Dems. than Reps.	0.172(0.110)	31	341
Districts with an incumbent	0.189(0.083)	34	601
Districts with open seats	0.060(0.134)	24	234
Robustness to Component Ratings			
Author ratings	0.142(0.078)	58	835
Mechanical Turk ratings	0.108(0.076)	56	823
CFscores	0.114(0.066)	54	788
Project Vote Smart	0.168(0.093)	41	746
Factor score from the four ratings (no listwise deletion)	0.132(0.121)	34	640
Other Robustness Checks			
Proximity calculated with Euclidean distance	0.553(0.291)	58	835
Proximity calculated with ave. respondent placement of candidates	0.025(0.059)	58	835
Proximity calculated with respondent placement	-0.080(0.084)	57	688
Weighted	0.179(0.074)	58	835

Note: This table shows that the top-two ballot condition generally increases the absolute distance between participants' vote choices and their own ideologies (reduces proximity) and that this tendency is robust to various alternative explanations, measurement decisions, and coding decisions. It reports the key coefficient from the model in Table 1 (col. 1), which is from the regression of city block proximity of vote choice on treatment assignment for best-case districts (category 3). District fixed effects included. For more robustness checks, see Table 3.

Table 3: Did the Top-Two Primary Improve Proximity Among Subgroups? No

	Effect of top-two ballot on ideological distance (SE)	*N candidate*	*N participant*
Full sample	0.13(0.06)	58	835
Ideologically moderate (4s on 7-point scale)	-0.09(0.06)	49	182
Ideologically moderate (3-5s on 7-point scale)	0.07(0.08)	54	340
Independent voters (3-5 on 7-point scale)	0.06(0.16)	50	178
Democratic voters (1-2 on 7-point scale)	0.27(0.11)	41	384
Republican voters (6-7 on 7-point scale)	0.05(0.11)	39	271
Centrist districts (Dem-Rep registration within 10%)	0.14(0.09)	31	492
Districts with high independent registered voter proportion	0.12(0.10)	32	443
Districts with 4+ candidates	0.22(0.10)	36	399
Districts with 4+ viable candidates	0.20(0.18)	20	151
Districts with 3 candidates or less	0.09(0.13)	15	262
Districts with 2 viable candidates	0.21(0.11)	14	314
Non-viable candidates included	0.18(0.07)	94	990

Note: This table shows that the top-two ballot condition generally increases absolute distance between participants' vote choices and their own ideologies across (reduces proximity) and that this tendency is robust across subgroups. It reports the key coefficient from the model in Table 1 (col. 1), which is from the regression of city block proximity of vote choice on treatment assignment for best-case districts (category 3). District fixed effects included.

Table 4: Did the Top-Two Primary Help Moderate Candidates? No

	(1) Best-case districts (category 3)	(2) All 34 districts (category 2 & 3)
	DV: Treatment – control vote share	
Candidate ideological moderateness	-0.02	0.04
	(0.08)	(0.06)
Observations	174	270
Candidates	58	90
Voters	878	1,449
Districts	20	34
R-squared	0.32	0.30

Note: This table shows that the top-two ballot (treatment) did not increase the vote share of moderate candidates compared to the closed ballot (control). Standard errors in parentheses clustered at the candidate-level. For these estimates, we rescale candidate ideological moderateness to 0-1. Indicator variables for voter party registration interacted with candidate party registration (fixed effects) included but not shown. We weight observations (candidate-voter party registration type) by the total number of votes cast for the candidate by voters of the party across both conditions. We do not cluster the standard errors at the district level because, with only 20 clusters, clustering is unreliable (Angrist and Pischke 2009, ch. 8). We therefore likely underestimate standard errors, but showing that the true standard errors are actually larger, which they no doubt are, would not substantially alter our conclusions. Table 1 shows an individual level model and Tables 2 and 3 show that the main individual-mobile finding is robust to coding and measurement decisions. See the Appendix for an accounting of the number of observations. *** p<0.01, ** p<0.05, * p<0.1

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[1] California adopted another primary reform, the blanket primary, for its 1998 primary elections, but the Supreme Court declared it unconstitutional. Under the blanket primary, voters could vote for any candidate and the top vote-getter *from each party* moved on to the general election.

[2] More precisely, Hall and Snyder (2013) report that their estimates are too imprecise to reach strong conclusions.

[3] We present these results and numerous other findings in an online supporting information available at https://www.ocf.berkeley.edu/~glenz/openprimary/SI.pdf. See sections 1 and 2 for evidence on the survey vote results closely mirroring actual election results.

[4] We originally planned to conduct the experiment in 36 districts with 3308 participants. Due to technical errors in assigning 201 participants to their districts, and human errors in creating ballots for District 10 and District 37, we were left with 2916 participants across 34 districts.

[5] See online supporting information section 4.

[6] Cronbach's α = 0.98 for all candidates; α = 0.40 Democratic and α = 0.50 Republican

[7] Cronbach's α = 0.98 for all candidates; α = 0.68 Democratic and α = 0.41 Republican. See supporting information section 4 for additional details on these measures (and for plots of candidate ideology by district).

[8] This was not an isolated incident. We could list other egregious examples of voters' apparent ignorance of candidate ideology, and do so in supporting information section 6.

[9] 116 viable, major party candidates competed, so we are missing data on seven candidates (missing ideological ratings for one candidate, participant ratings for five candidates, and both for one candidate). We do not show the plot for the 11 no party preference (NPP) candidates for whom we have ratings (we lack data on three candidates), but the findings are similar with a slope of 0.06 and an R^2 of 0.04. For additional results, see supporting information section 6.

[10] This held for Democratic candidates in 29 of 48 districts and held for Republican candidates in all 48 districts where we interviewed respondents. For more details, see supporting information section 5. A potential problem with these findings is that voters may respond to the ideology scale relative to their own districts rather than in an absolute sense. Although our survey cannot address this criticism, Kousser, Phillips, and Shor (2013) measures mean constituent ideology by scaling responses to policy preferences and reaches a similar conclusion. See supporting information section 5 for plots and more on our analyses of self-reported ideology.

[11] Supporting information section 7 presents an example.

[12] This procedure does produce slight differences between the treatment and control groups, but allows us to speak to actual effects of the reform in California. See supporting information section 8 for more detail.

[13] Evidence suggests that the top-two ballot reform failed to alter the distribution of candidate positions compared to the previous primary (at least according to CFscores, see supporting information section 4.5) or of general election candidates (Kousser et al. 2013). So our counterfactual may not be far off the ideal counterfactual. (We discuss this point further in the Alternative Interpretations section below.)

[14] We present these results in section 10 of the supporting information. We should note that one potential problem with the individual-level analysis is that we measured participants' ideology post-treatment, but we reassuringly fail to find evidence of post-treatment bias, that is, an effect of the treatment on participant ideology (see supporting information section 8). Moreover, the candidate-level analysis below (see Figure 3 and Table 4) yields the same finding but does not rely on self-reported ideology.

[15] Supporting information section 11.2 presents the within-district results graphically.

[16] Among all survey respondents, just 41.3% reported being aware of the switch to the top-two ballot. This increased to 42.4% in best case (category 3) districts, and increases further to 50.5% when restricting the sample to participants who indicated a preferred candidate for U.S. House of Representatives.

[17] See supporting information section 1 for the correspondence between actual election results and the results of our top-two condition. See supporting information section 13 for the knowledge battery and the distribution of political knowledge in the sample.

[18] Supporting information section 4 presents more detail on these analyses: Section 4.2 plots the distribution of candidate ideology, while section 4.5 compares the distributions of candidate CF scores in 2012 and 2010.

[19] We conducted several additional analyses that yield evidence inconsistent with hedging driving the main results. Supporting information section 14 presents these, while section 15 presents a density plot comparing possible to actual proximity of choice among crossover voters. Supporting information section 16 presents the evidence against raiding visually.

[20] The first-dimension of the principal component factor analysis has an eigenvalue of 3.70 and accounts for 92.4% of the common variation (the second factor has an eigenvalue of 0.15 and accounts for 3.7%).

[21] Supporting information section 11 graphically presents the raw data for these findings.

[22] Since Democratic and no party preference (NPP) candidates tend to be more moderate, a tendency to vote for them in the open ballot condition for any reason—i.e., dislike of the Republican Party—will make the open ballot appear to favor moderate candidates. Failing to account for this tendency in the candidate-level analysis thus has the potential to create the false appearance of a moderating effect. By calculating the treatment effect separately by voter party registration, we eliminate this potential bias (see supporting information section 9 for more details).

[23] To convey a sense for these calculations, consider the case of Abel Maldonado. He would have a positive treatment effect (more vote share) among registered Democrats if he received any votes at all from them in the treatment condition, since he obviously received no votes from them in the control condition. He is unlikely to receive a positive treatment effect for registered Republicans, but he may avoid a negative treatment effect (less vote share) if he prevented defections. Since the mean treatment effect will generally be negative for in-party candidates and positive for out-party candidates, and since NPP candidates tend to be more moderate and never appear on a closed ballot, we remove mean differences using dummy variables for the three candidate parties interacted with the three voter party registration types.

[24] Supporting information section 9.2 presents tables showing the regression results after rounding candidate ideology. Supporting information section 9.3 presents the analysis investigating the top-two's effect on advancement rather than vote share.

[25] We discuss these results more fully and present plots in supporting information section 17.