Information and Ranked Choice Voting

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Abstract: Does ranked choice voting (RCV) change the information search behavior of voters? The answer to this question is important in the context of the proposed representational benefits of RCV, which I argue are conditional on voters broadening their information search. I present a theory, consistent with past work on voter behavior and information, which shows that (1) RCV is a more complex informational environment than typical single preference voting, which then predicts that (2) voters should be motivated to search for and retain more information on the candidates, and particularly on non-copartisans. This change in behavior should correspond to an increase in the cost of voting. I design a survey experiment to test this theory on a nationally diverse sample of U.S. adults. Results indicate that voters do not adapt their information search and retention behaviors, nor do they spend more cognitive effort in the process of voting.

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Introduction

The U.S. electoral system is often criticized for producing disproportionate results, incurring high numbers of wasted votes, and being unable to provide effective representation due to two-party dominance reducing the viability of potentially preferred non-partisan options. Ranked Choice Voting (RCV) presents a possible improvement on these faults. Activists and supporters focus on its potential to increase representation, campaign civility, and candidate diversity at relatively minimal cost to the voters themselves[1] This promise has led to an increasing number of jurisdictions which employ some form of ranked ballot in their elections, most recently mayoral elections in New York City, and all federal and local elections in Alaska starting in 2022. However, no electoral reform comes without a cost, which often can be less than clear without a thorough examination of the exact consequences of the new policy (Berinsky, 2005; Burden et al., 2014).

The empirical evidence for RCV has been mixed, with authors mostly confirming its positive effects on campaign civility (Donovan, Tolbert, and Gracey, 2016; Neely and Cook, 2008) but expressing doubt about whether voters comprehend RCV enough to access its benefits (Donovan, Tolbert, and Gracey, 2019; Cerrone and McClintock, 2021; Burnett and Kogan, 2014). Crucially, research has not yet examined the effects of RCV on how voters inform themselves prior to elections. This is a linchpin issue for the promise of RCV to be realized. Increases in the diversity of the field of candidates is undermined in importance if voters are still exclusively informed about the major partisan options. Similarly, while voters could in theory pick the candidate most ideologically proximate to them without much regard to strategic voting or fears of "vote-splitting", such an assessment requires deeper knowledge of the field of candidates than would be necessary to just pick the candidate who shares their partisan affiliation.

In this paper I apply existing theory on voter information to generate a narrative of how voters may respond to RCV, consistent with them accessing the theorized representational benefits. Previous scholarship tends to agree that, when preparing for an election, voters make a trade-off between being informed enough to accurately express their preferences, and expending as little cognitive capacity as possible (Kunda, 1990; Lupia and McCubbins, 1998; Basinger and Lavine, 2005). Starting from the model of voter information search presented by Lau et al. (2006) it is fairly straightforward to show that RCV presents a more challenging informational environment than typical single preference voting; voters cannot as effectively rely on candidate elimination or strong partisan heuristics, but must venture further in order to present an accurate ranking of their preferences. This means that, assuming voters are willing to expand their investment of cognitive resources into their pre-election information search, they should spend more time seeking and retaining more information on the candidate

[1]For reference on activist support of RCV, FairVote provides a comprehensive list of arguments on their website.

set. In addition, given the importance of partisanship as a heuristic (Lau, 2013), it should be expected that partisan voters need to expand their information search beyond just their copartisan candidate, in order to break ties between independents or rank them relative to the candidate of the opposing party. This set of predictions, assuming they hold, paints a picture of RCV as a trade-off between increased cost of voting and an increase in information, representation, and bipartisanship, given the increase in cross-partisan information and the aforementioned results regarding campaign civility.

I test this theory using a survey experiment on a nationally diverse sample of 1488 respondents[2]. Participants are told they will be voting in a state-wide election using either RCV in the treatment group, or typical single preference voting in the control group. The exact RCV prompt is modeled after Congressional election information from the Maine Secretary of State's website. Voters are then presented with four candidates, and are given the option to view an information package for each candidate. This information package contains short biographical information, policy positions, and endorsements, which covers typical informational heuristics (Lau and Redlawsk, 2001). The participants are then presented with a set of questions designed to test their knowledge of each candidate, before being asked to vote[3]. Consistent with the theory, I expect to see respondents who expect to vote using RCV spending more time reviewing information about the candidates, searching beyond their party, and being better able to answer factual questions about the candidates' policies, background, and endorsements.

The results are null across almost all hypotheses. Voters seem very hesitant to change any aspect of their behavior when presented with RCV; they do not click on more candidate profiles, they do not spend more time receiving information, nor do they score higher in the knowledge test section. Null effects are also present when the response is restricted to non-copartisan candidates, meaning that voters do not conduct a broader search to ensure ideological proximity or to gauge the relative ranking of independents. Proving a well-specified null result is complex, and there is some doubt cast by the survey findings as to the success of the experiment. For example, respondents spend relatively little time on the survey as a whole, and appear to be slightly worse than random at responding to the knowledge questions. This is particularly concerning given that the sample comes from Lucid Theorem, which has in the past had issues with respondent attentiveness (Aronow et al., 2020). I present evidence that the survey has worked as intended and produced a valid null result that should address these concerns; specifically, respondents appear to vote based on their partisan affiliation, show low rates of abstention, interact with a median of two candidate profiles, are honest when responding that they are not sure of a piece of information, and

[2]The survey experiment presented in this paper was approved as exempt by the UC San Diego Institutional Review Board, and designated code 801419.

[3]The survey experiment analyzed in this paper was pre-registered with OSF, with EGAP registration ID: 20211128AA.

show clear patterns of age, education, and party effects. Assuming pathologic inattentiveness, these results would be much closer to random.

I expand on the existing literature in three ways. First, I apply past theory on voter information to the new environment presented by RCV, which expands our understanding of voter response to complexity. Second, I add to the growing literature on RCV by expanding its scope to information effects; almost no research has empirically tested the changes that rank ordering might entail for how voters seek out and retain information. Third, I add to the broader literature on the behavioral impact of electoral reform, which has extensively studied changes in American elections such as voter identification laws (Grimmer and Yoder, 2021), mail voting (Gerber, Huber, and Hill, 2013), early voting (Gronke, Galanes-Rosenbaum, and Miller, 2007), or a top-two primary system (Kousser, Phillips, and Shor, 2015)[4]. More specifically, this type of research advances our knowledge of the menu of potential options for improving the function of American democracy, of which RCV is a relatively understudied part.

The Promise of Ranked Choice Voting

Ranked choice voting can be employed with wide variation in ballot type, tabulation system, and requirements. As such, several electoral systems can be described as having elements of ranking as part of their process. For the purposes of this paper, I use the term "ranked choice voting" (RCV) to refer to any such electoral system with ranking elements, where voters are required to express an ordinal preference between candidates or parties. Some key elements of RCV include, but are not limited to:

- **Minimum/Maximum Ranks:** RCV systems may entail a minimum or maximum required number of ranked candidates. For example, a voter's ballot may only be considered valid if they selected at least four candidates in ordered preference. Alternatively, jurisdictions such as Maine in the U.S. do not require any number of rankings.
- **Tabulation System:** RCV tabulation varies substantially across jurisdictions, particularly across different countries that employ Westminster or Proportional electoral systems. For example, the alternative vote/instant run-off tabulation system sequentially eliminates candidates and re-allocates their voters to their subsequent most preferred candidate until the winner reaches a majority. Conversely, Slovenia and Kiribati employ a Borda count, which allocates points to each candidate based on the full ranked ballots of the voters.

[4]Kousser et a. (2015) similarly find and publish an informative null result of the reforms they examine on effective representation, which indicates the importance of *disproving*, as well as confirming the potential benefits of electoral reforms.

- **Partisanship:** Many implementations of RCV, particularly in the U.S., occur in non-partisan jurisdictions, such as local mayoral races.

In practice, RCV presents four main benefits. First, it allows for fewer wasted votes, since ballots will still play a role in deciding the winners even after the first preference is eliminated from contention. This means that, especially in single member district systems, voters have more influence over the eventual winner, as long as they input enough ranks in their ballot to avoid exhaustion[5]. Second, RCV should reduce negative campaigning, since candidates are now vying to be ranked on the ballots of opposing voters, and as such will not risk completely alienating the opposition. This has important impacts not only on voter satisfaction with campaigns, but also crucially on participation, since prior research has found that attack adds can have a demobilizing effect (Ansolabehere et al., 1994)[6]. Third, RCV should have an upstream influence on candidate entry, since potential candidates will not fear splitting the vote and allowing for the election of an ideological opponent. For example, a far left Democrat may now run as an independent in a district represented by a centrist of the same party, without necessarily fearing that this will split Democrats and lead to the election of a Republican, since voters of each candidate can just express the two as first and second preference respectively. This should increase representation, since voters can now rank candidates closer to their ideal point higher on their ballot. The effect on candidate entry has also been theorized to especially help historically under-represented groups run for office, given it allows for building political experience outside the party structure, induces less negative campaigning, and counteracts a spoiler effect that has been historically used as a cudgel especially against potential women candidates (Terrell, Lamendola, and Reilly, 2021). Fourth, RCV should, in theory, reduce incentives for strategic voting, since voters can now pick a candidate beyond the "lesser of two evils" presented by the two mainstream parties. It is also much more complex for voters to think strategically given the tabulation systems being employed, which are not clearly ex ante deconstructed into strategic options.

In the United States, ranked choice voting is mainly implemented in local elections, in cities such as Oakland, Minneapolis, San Francisco, and most recently New York City in 2020. Implementation at the municipal level is set to expand, since all cities in Virginia and Utah have been given the option to adopt RCV, and Amherst, Albany, Boulder, and Burlington

[5]Ballot exhaustion in an RCV context is when, after sequential rounds of candidate eliminations, no more ranked candidates remain on a voter's ballot. This means that they no longer influence the result of the subsequent rounds, and as such their ballot is "exhausted". Exhaustion is more likely when voters rank fewer candidates.

[6]It should be noted that this result is hotly contested. For more on this discussion in the literature, Freedman et al. (2004) note that advertising has a *positive* effect regardless of content, while other authors claim that the effects of advertisement are conditional on the emotion being conveyed (Marcus and Mackuen, 1993; Brader, 2005). Others yet claim that advertisement in general and negative adds in particular have no discernible effect on participation (Lau, Sigelman, and Rovner, 2007; Krasno and Green, 2008), although the measurement strategies used here have also been contested by Franz et al. (2008)

will also conduct all elections using RCV starting in 2022. At the state level, Maine has been conducting all elections using RCV since 2018, while Alaska has approved a state-wide implementation of RCV starting in 2022. Several Southern states such as Alabama, Mississippi, and Louisiana implement RCV for military and overseas voters in runoff elections, since re-distribution of ballots between general and runoff would be impossible.

Research on RCV has broadly focused on the first three of the aforementioned benefits, with little work being done on strategic voting. In terms of Campaign civility, Kropf (2021) uses text analysis of online activity and newspaper articles to show that local election candidates in an RCV setting are more likely to directly engage with each other, and less likely to use negative language than their counterparts in plurality election cities. Donovan et al. (2016) also presents evidence in favor of RCV's effects on civility, showing how voters in RCV districts appear to be significantly more satisfied with their local election campaigns in terms of candidate conduct. On the other hand, Clark (2020) finds no effect on perceived campaign civility in an online survey of Maine voters. However, apart from Clark (2020), these studies have not extended to Congressional or state-wide elections, nor do they present any strong causal methodology, relying more on matching or simple differences in means between curated selections of cities.

In terms of candidate entry, John et al. (2018) study local elections in California cities before and after RCV implementation, and find a nine percentage point increase in candidates from racial and ethnic minority backgrounds, although they find no impact in entry for women. Terrell et al. (2021) conduct a similar study of bay area cities and find a significant increase in entry and success rates of women candidates in jurisdictions after the implementation of RCV. Cerrone and McClintock (2021) predict and demonstrate modest gains in candidate entry for RCV districts in Maine when compared to other states, but posit that they are not significantly different than what could be expected given its electoral history. On the whole, evidence of candidate entry is mixed, and similarly to campaign civility focuses primarily on local elections, without providing a strong causal claim for the effects they identify.

While direct representational differences in terms of policy implementation have not been studied, the extent to which RCV is able to better represent voters can be approximated through voter satisfaction and participation. While some research suggests that voter satisfaction is enhanced by the ordinal and preferential features of RCV (Farrell and Mcallister, 2006), most empirical work shows that voters are confused by the novel electoral system. Older and less educated voters show consistently poor understanding of RCV (Donovan, Tolbert, and Gracey, 2019), which leads to enduring and widespread over-voting, which is when a voter ranks more candidates than is legally permitted, thus spoiling their ballot (Neely and Cook, 2008; Clark, 2020). Representational benefits are minimized due to the prevalence of ballot exhaustion, which happens when all ordered candidates from a ballot are eliminated (Burnett and Kogan, 2014). It is not surprising, therefore, that voters often

either appear dissatisfied with the implementation of RCV (Cerrone and McClintock, 2021), or very hesitant to endorse its expansion (Kimball and Anthony, 2021). This result also extends to local election officials; in a survey of local administrators conducted by Anthony et al. (2021), the majority of respondents and the overwhelming majority of Republican respondents were in favor of abolishing RCV in the state, citing its relative complexity and doubting its benefits for representation.

A common thread missing from the above studies is to what extent voters adapt their information gathering and retention behavior based on the switch from single preference voting to RCV. The importance of this question can be quickly ascertained when examining the promised benefits of ranked ballots. For example, assuming the empirical evidence existed, benefits of increased representation through a broadening of the field of candidates would only help if voters could adequately inform themselves of these alternatives, *and* of the basic premise that they can freely rank based on their preferences with no fear of "vote-splitting". Similarly, the empirical evidence that voters tend to under-fill their ballots, leading to exhaustion (Burnett and Kogan, 2014), is highly suggestive of the fact that voters do not reap the representational benefits. Strategic voting could also persist if voters are unaware of the potential alternatives presented by independent candidates. Therefore, the promise of RCV is locked behind an assumption about voters increasing, or at least adapting, their informational behavior to compensate for the new environment. This is a critical gap in understanding the trade-offs that RCV entails. Currently, RCV implementation is outpacing research on its behavioral consequences, and research is immediately necessary in order to assess whether it will function as intended (Berinsky, 2005). In this paper, I attempt to bridge this gap, adding to our knowledge on the downstream effects of RCV and its capacity to fulfill its promise.

Information and Heuristics

Initial theories on voter information centered either around rational choice accounts that required voters to act as "*ambulatory encyclopedias*" (Lau, Redlawsk, et al., 2006), dispassionately updating their beliefs based on new information (Downs, 1957), or as simply socialized party members (Campbell et al., 1980) who care little about active updates to their deep-rooted partisan identity. These views are augmented by Popkin (1991) who draws on previous theory to argue that voters exhibit "low-information rationality", by drawing on information and cues from their social circle and the media in order to keep a running tally of assessments on candidates and parties. Lupia advances the idea of low-information voters compensating for their lack of knowledge by using elite queues (Lupia, 1992), and later on empirically demonstrates that voters who rely on polls, endorsements, and shared information from their community act almost as if they were fully informed in a set of non-partisan

California ballot measures (Lupia, 1994). Voters, in this model of information, are able to make a reasoned choice by reacting to fairly simple informational cues from trusted sources (Lupia and McCubbins, 1998).

This view of heuristics leading to an adequately informed vote is undermined by Bartels (1996), who shows an approximate deviation of ten percentage points between informed voters and uninformed voters with otherwise similar demographic characteristics. Kuklinski and Quirk (2000) expand on this view, claiming that political scientists are often overly optimistic about voter cognition and information. Citizens are called upon to make overwhelmingly complex decisions, and while heuristics are a cognitive strategy that is typically employed, voters will be overconfident in their assessments of these cues, biased as information receivers, and resistant to disconfirmatory evidence (Kuklinski and Quirk, 2000). Lau and Redlawsk (2001) employ a dynamic process tracing strategy to simulate an electoral campaign, testing how voters seek out heuristics such as party, endorsements, polling, ideology, and candidate appearance. They conclude that, while heuristics do tend to improve correct voting for individuals with extensive political knowledge and experience, they actively damage accuracy for political novices. This result, however, does not exclude the presence of a signaling mechanism (Lupia, 1992), and does not take into account how heuristic-based voting may influence the equilibrium actions of politicians (Ashworth and Mesquita, 2014).

Regardless of whether heuristic use leads to accurate voting, it is overwhelmingly clear that voters do rely on such cues to form their preferences over the list of candidates. Another consistent result is the critical importance of party as an informational cue. For Snyder and Ting (2002), party acts as a "brand" for politicians that provides voters with useful summary information about the approximate policy positions of candidates. When this "brand" is not present, voters will rely on heuristics that might convey partisan identification, such as endorsements from copartisan elites (Lupia, 1994), and will be substantially less likely to accurately reflect their preferences (Lau, 2013). From another perspective, partisan identification supersedes a simple heuristic and becomes a social identity (Campbell et al., 1980), which causes voters to display directional motivated reasoning (Kunda, 1990). This means that voters will be more likely to seek out and trust evidence that supports their own party (Adida et al., 2020), and thus hold beliefs that increasingly diverge from those of their non-copartisans (Little, Schnakenberg, and Turner, 2021).

Beyond reliance on party identification, Lau et al. (2006) present a broad set of strategies that voters employ in order to inform themselves prior to an election. Voters apply decision heuristics as a means of *decomposition*, breaking down a complex political choice into a set of specific variables such as party, endorsements, or ideology. Voters also *edit* the presented choice set; in elections, this means that they will immediately disqualify irrelevant alternatives, such as independent candidates that have no chance of winning. Voters proceed to apply the selected decision metrics on the limited choice set, either sequentially searching

across all candidates based on their most important metric, or exhaustively searching each candidate across the selected variables (Lau, Redlawsk, et al., 2006). These strategies are used in the context of a maximization problem that voters face when trying to inform themselves. This is summarized by Basinger and Lavine (2005) in the form of two axioms: voters want to be *sufficiently* informed in order to accurately present their preferences, but want to do this by expending the *least effort* possible. Put differently, voters use decomposition and editing strategies because they are "cognitive misers" (Fiske and Taylor, 1991).

RCV as a Complex Informational Environment

RCV presents voters with a substantially more complex choice, because it diminishes the effectiveness of typical information acquisition strategies in contrast to single preference voting. Starting with decomposition, consider a partisan voter trying to pick their single most preferred candidate; it is quite likely that the party heuristic is in and of itself sufficient for making that choice. However, assuming a two-party system, RCV, and the presence of independent candidates[7], the party heuristic can, at most, dictate the top and bottom candidates without needing more information on the independent candidates in between. This heuristic is *exhausted*, since it cannot provide more information on the voter's relative preferences between the candidates. Therefore, decomposition as a strategy is harder under RCV, since some heuristics, particularly discrete heuristics such as party, do not contain enough information to produce a full ordering of candidates. Similarly, editing also becomes more complex under RCV. Assuming one of the goals of RCV is to allow voters to rank according to preference and better represent their interests, voters cannot immediately eliminate candidates based on viability. Additionally, viability itself becomes substantially harder to calculate from simple polling, since voters are often unfamiliar or confused by the exact system of choosing a winner in RCV (Anthony et al., 2021). Similarly, it is also harder to eliminate based on partisanship, since independents can often be more radical than mainstream party candidates, meaning that voters need to examine all of them to produce a full relative ordering.

Consequently, RCV voters either limit their definition of sufficiency to an under-vote[8], or expand their effort in information acquisition. Therefore, if voters are to reap representational benefits and provide incentives for wider candidate entry, they must become more informed than under single preference voting; if they are to pick the candidate that best represents them, they must also exhibit this increase in information on heuristics that have some substantive benefit (policy position, ideology, experience, representational characteristics), and

[7]This setup is typical of RCV elections in Maine, or partisan mayoral elections across the US.

[8]This appears to be the case in several municipal elections. Burnett and Kogan (2014) examine ballot exhaustion across four California municipalities, and find that 9.6 to 27.1 percent of ballots do not survive to the final round.

not simply by candidate appearance or ballot position(Cunow et al., 2021).

H1: RCV voters, compared to single preference voters, will seek and retain more information.

In addition, given the effect of RCV on partisan editing strategies, it is reasonable to expect that voters spend more time researching non-copartisan candidates, since they cannot simply arrive at their final decision by picking based on party. Given partisans are, by definition, more likely to vote based on party, it should also be expected that RCV will have more of an effect on their behavior.

H1a: RCV voters, compared to single preference voters, will increase the amount of information they seek and retain on non-copartisans.

H1b: Partisan RCV voters, compared to partisan single preference voters, will increase the amount of information they seek and retain, relative to independents.

Lastly, given differential rates of comprehension of RCV, voters that have a hard time understanding the ranking process should also exhibit a more limited response to the shifting informational environment.

H1c: Younger, more educated RCV voters will exhibit a larger increase in information search and retention, when compared to older, less educated RCV voters.

The Consequences of Complexity

The flip-side of the above optimistic case is that since strategies and heuristics are weaker, voters will need to spend more cognitive effort to reach a level of minimum comfort with expressing their preferences at the ballot box. They will not only need to rely on strictly more information, but often on more complex heuristics, since breaking ties involving independent candidates may often require an ideological assessment that partisans are not used to making for nonpartisan candidates; for example, it may be hard for a Democrat voter to assess the difference between a Republican and a libertarian based on endorsements alone, which may lead them to consider specific policy positions. This leads to the following hypothesis:

H2: RCV voters, compared to single preference voters, will expend more cognitive resources.

The consequences of this increased cost are likely to be reflected in terms of participation, with abstention increasing for individuals faced with the prospect of voting using RCV. This is consistent with results on how changing the cost of voting affects participation (Downs, 1957; Burden et al., 2014; Berinsky, 2005). Additionally, this increased complexity, along with the complexity of RCV as a system, should also lead to ballot spoilage[9] particularly

[9]Broadly speaking, spoilage occurs when a voter incorrectly follows instructions on how to fill out and submit their ballot. In the RCV context this may include ranking too many or too few candidates, or ranking

for older and less educated voters, consistent with previous results (Anthony et al., 2021; Cerrone and McClintock, 2021).

H2a: RCV voters, compared to single preference voters, will be more likely to abstain.

H2b: Older, less educated RCV voters will be more likely to spoil their ballots.

Before proceeding, it should be noted that the exact process by which RCV affects voters may be reversed from the ordering of the hypotheses as presented above. Voters are first brought up against the complexity of the new system, and as such can choose to abstain *prior* to any information acquisition. This means that, conceivably, the above effects stand only for voters that make the initial choice to participate despite the increased complexity, and as such those who abstain should show no effect from RCV. The models presented below include *all* voters, gauging an average response across a nationally diverse sample; at worst, these models include some noise from voters who pick not to participate *because* of RCV, rather than those who abstain due to any issues with the candidate pool or their own voting habits, who should be randomly distributed between treatment and control. In Appendix D I split the sample based on abstention, and find the exact same results for both groups.

In this section, I focus on the optimistic case as a direct test of the benefits of RCV and the issues that it might entail with regards to increases in the cognitive costs of voting. Of course, as will become abundantly clear in the results section of this paper, the case presented here may not necessarily be fulfilled. There are several alternative mechanisms that might be taking effect. For instance, Cunow et al. (2021) argue that increasing the complexity for voters can lead to "choice overload" (Iyengar and Lepper, 2000), which is connected with decreased intake of information per candidate, less time spent learning about candidate policy, higher abstention rates, and higher spoilage rates. If voters faced with RCV are exhibiting choice overload, similar results to those hypothesized above should be present in H2 and potentially H1a, while all other hypotheses should be null or even *negative*, given that voters should be expected to learn less about policy. Another alternative is that voters are simply unwilling to adapt their informational behavior at all; their cognitive budget is strict and their common strategies for information acquisition are deeply ingrained enough that a shift in electoral method does not cause them to change their practices. This would mean RCV increases the complexity of voting, but does not do so enough to induce choice overload. In this case, all hypotheses should be null apart from H2b, assuming voters stick to their trained behaviors. In both these alternative cases, voters using RCV would be faced with higher complexity but less information, meaning the benefits of RCV to representation would be substantially undermined.

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the same candidate in multiple positions. In non-RCV contexts, this looks like failing to sign a mail ballot or voting for more than one candidate.

Experimental Design and Data

Survey Design

In order to test the above hypotheses, a nationally diverse sample of 1,488 respondents was recruited between December 13 and 20, 2021 through Lucid Theorem, all U.S. citizens[10] aged 18 and older. The participants were asked to complete a survey, which they completed in a median time of four minutes. The sample closely reflected national demographics on age, ethnoracial characteristics, partisanship, and education, according to Lucid Theorem's audience targets[11]; however, given the size of the sample, the results should not be assumed to generalize equally within all demographic groups. Another issue relevant to Lucid Theorem is the existence of a concerning number of low-quality respondents or bots (Aronow et al., 2020). In order to circumvent this issue, attention checks were used to screen respondents, leading to the elimination of around a thousand potential subjects. While this is certain to not have fully corrected the issue, the survey experiment design detailed here is robust to some noise from low quality respondents, assuming equal distribution between control and treatment groups.

Table 1: Balance table for treatment versus control groups. Mean Difference is standardized.

Variables	Type	Mean Difference	t-Test P-Value
Republican	Binary	-0.0004	0.984
Democrat	Binary	-0.049	0.256
Independent/Other	Binary	0.049	0.239
Registered	Binary	0.018	0.545
RCV Experience	Binary	0.0004	0.996
Age	Contin.	0.0002	0.987
Gender	Binary	-0.013	0.757
Asian	Binary	0.008	0.704
Black	Binary	-0.022	0.421
Indigenous	Binary	0.001	0.824
Other	Binary	0.026	0.341
Pacific Islander	Binary	-0.001	0.790
White	Binary	-0.012	0.774
Hispanic	Binary	0.004	0.907
No Response (Hisp)	Binary	0.006	0.663
Non-Hispanic	Binary	-0.009	0.768
Education: High School	Binary	0.012	0.748
Education: Some College	Binary	-0.008	0.827
Education: College	Binary	0.012	0.786
Education: Postgraduate	Binary	-0.016	0.587

[10]Note here that the respondents are not registered voters. They are asked about registration status in order to ensure balance between treatment and control. Registration status is used as a control variable in all regressions, but does not achieve statistical significance. Lucid Theorem does not permit exclusively sampling registered voters.

[11]https://lucidtheorem.com/faq

After consent and attention checks, respondents answer a brief pre-survey questionnaire[12] and are divided into treatment and control groups through random allocation, which successfully led to balance on observables between the groups as is evident from Table 1. Both groups are informed that they will vote on a set of four simulated candidates, who they should assume are running for statewide office in their state. They are also informed that they will be allowed to abstain, if they chose to do so. The control group is told they will vote using single preference voting, which is briefly explained as the standard voting system applied in most U.S. jurisdictions. The treatment group is given a prompt explaining RCV, which closely mimics the information publicly available on the Maine Secretary of state's website[13]. This means that the exact system they are briefed on does not require a specific amount of rankings for the ballot to be valid, displays the partisanship of the candidates on the ballot, and uses an alternative vote measure to determine a winner. This is chosen as the model of RCV given it is the only currently implemented statewide RCV method, which reinforces the external validity of the study. Additionally, it provides a good test of the theory since it is minimally restrictive: partisanship is present and there are no minimum and maximum ranking requirements.

Both groups are then directed to a page that displays the names and partisan affiliations of the four candidates (two independents, one Democrat, one Republican). They are instructed to select as many or as few candidates as they want from the list to learn more about. If they select no candidates, they are led to the next section of the survey. Less than five percent of the respondents selected to see no further information, while the average number of candidates selected was 1.93. For the selected candidates, the respondents are presented with a short biographical summary, a set of policy positions, and a small number of endorsements. The biographical summary focuses on prior work and political experience, but also gives ethnoracial and gender information about the candidate. For example, the Republican candidate is presented using he/him pronouns and is said to be of white Irish descent. Exactly four policy positions are given per candidate, in a neutral journalistic tone; as an example, for one of the independent candidates, the prompt reads "Ruiz is pro-life, citing religious reasons for his position." The candidate profiles do not vary across respondents, and are the same across groups. The time the respondents spend in the candidate information section is recorded.

It should be noted that the task of creating a set of simulated candidates that are equally plausible across all districts in the United states is quite daunting. While some effort is made to present a relatively broad spectrum of politicians, this does not mean that the exact set is perceived to be equally plausible for a voter in Orange County as it might be for a voter in Connecticut. The approximate policy positions of the candidates, and in particular the independents who are coded as libertarian and fiscally conservative moderate, was loosely

[12]The full survey is available in Appendix A.

[13]https://www.maine.gov/sos/cec/elec/upcoming/rankedchoicefaq.html

inspired by candidates for U.S. House in Maine RCV elections, in order to make sure the set is plausible in an RCV context. The experimental design should, again, be robust to different perceptions of plausibility, assuming group balance; however this may complicate both external validity and make it harder to detect a treatment effect.

After the end of the candidate section, voters are given a set of eight questions to assess their acquired knowledge of the candidates. These are selected randomly by sampling two questions from a pool of four, for each candidate. They address endorsements, prior experience, and policy positions. Responses on candidates that the voters did not select to learn about are automatically coded as being incorrect. The questions are set up to assess knowledge of more complex heuristics, based on the increased representation promise of RCV, and the difficulty of decomposition when the party heuristic is absent. In this section respondents perform particularly poorly, with the correct response rate being only just as good as random[14]. While this result is not optimistic for the validity of the survey design, it should be noted that individual questions exhibit a very wide range of correct responses, from around 35 percent to 14 percent, well above and below what would be expected if respondents were uniformly guessing, respectively. The correct response rate also rises if "Not Sure" responses are coded as *correct* in the cases where the respondent has not seen the profile. Additionally, respondents seem to be fairly sincere when they did not view the candidate profile; around 70 percent of responses to questions they could not have possibly known are "Not Sure", as compared with a "Not Sure" response rate of 30 percent when they plausibly could have known the answer. These results suggest that, at worst, the questions may have been too difficult for respondents, leading to an adverse environment for detecting an effect. They do not suggest an overwhelmingly high rate of random responses and low attention.

The respondents are then asked to vote, with the control group asked to select their single preferred candidate or abstain, and the treatment group given a grid RCV ballot [15]. As previously stated, it is possible for RCV voters to spoil their ballots if, for example, they input more than one candidate as their second option, or if they skip a rank. Approximately thirteen percent of RCV respondents spoiled their ballots, slightly above the average for RCV districts in Minneapolis and San Francisco which is around ten percent (Neely and Cook, 2008). It should also be noted that the vast majority (around 73 percent) either voted for their copartisan candidate or ranked their copartisan candidate as their first preference,

[14]This is a surprising result, which was not predicted by average performance in soft-launches of the survey. More specifically, the first pilot had an average correct response rate of around 35 percent, with individual questions ranging between 15-50 percent. The pilot sample was also fairly representative based on Lucid demographic requirements, although the much smaller sample size did lead to an intensified issue with representation of relatively small subgroups.

[15]Voting occurs after the question step in order to make sure that the process of voting does not act as a confounding treatment on the retained information; voters could conceivably be better at remembering facts if they first were tasked with a complex ranking procedure, rather than a single choice. In this experiment, the knowledge test matters more than the ballots.

which provides a strong indication that the results are not random, but inputted in good faith by the respondents. Additionally, 69 percent of the respondents in the treatment group filled out a full ballot, with four ranked choices[16]. This is a good indicator that respondents are engaging with the ranking system and not randomly clicking through, as it would be very easy to spoil a fully ranked ballot.

While the artificial nature of this survey should create questions as to the generalizability of the results, I stress the following four points. First, the treatment briefs, ballot structure, and RCV system was raised directly from real Maine elections, meaning that voters should have a very similar experience to when they are actually asked to vote in an American RCV election. The addition of an abstention option is also not typical of simulated campaign surveys, but further adds a degree of similarity between the survey and real voting procedure. Second, despite some concerns, the candidates themselves are heavily influenced by candidates from Maine RCV elections, but adapted to better reflect national demographics and issues, meaning that they should provide at least a glimpse at voter response to a realistic RCV choice set. Third, there are fail-safes built into the survey; attention checks remove a large amount of poor quality respondents, random guessing is weeded out through profile viewing data, and voters are uniformly given the option to express ignorance, rather than being forced to respond. Fourth, even if there is some sacrificed external validity, such survey experiments present a unique option to study the behavioral effects of RCV on a national level without just relying on the states that have already implemented it. Exclusively studying Maine, Alaska, and a set of mayoral elections should present at least equally strong generalizability concerns, and waiting until wide RCV implementation has occurred is closing the barn door after the horse has bolted.

Operationalization and Controls

The first hypothesis splits into two parts: seeking and retaining more information. Information seeking is measured as the proportion of profiles that each respondent selected to learn more about. Information retention is measured first as the proportion of correct responses. A potential issue with this operationalization comes from varying item difficulty, as is evident from the differential correct response rates from the respondents. In order to show robustness of the model to this issue, I use item response theory (IRT) to implement a Bayesian Rasch model (Bürkner, 2020; Nguyen et al., 2014). IRT models the relationship of individual items to an unidentified latent trait, commonly measured by all the questions on the test. More specifically, the 1PL or Rasch model I implement works by estimating the

[16]One concern here is that this result does not appear to fully reflect the under-voting rate present in Maine, where only 48 percent of voters ranked every single candidate in the 2020 July Congressional primary, and only around a quarter fully filled in their ballot in 2018. Respondents appear more comfortable fully expanding on their choices in the simulated survey environment, which makes the null results in information more concerning.

probability that respondent i correctly answers question j as the logistic function of the sum $\theta_i + \beta_j$, where β_j represents the difficulty of the question and θ_i the latent ability of each individual. As such, this model solves the issue of varying question difficulty by controlling for it while estimating individual scores. Some specifics on the assumptions and implementation of the model can be found in the Appendix, and the resulting model for H1 that uses the latent trait estimated values is indistinguishable from the simple proportion of correct responses model.

For H1a, which predicts that RCV respondents increase the amount of information they seek and retain on non-copartisans, the response variables of H1 are restricted to exclusively non-copartisan candidates; for independents, non-copartisan is taken to mean Republican or Democrat candidates[17]. H1b and H1c, which correspond to heterogeneous treatment effects of partisanship, and age and education respectively, are assessed using interaction effects between the treatment and the variables of interest. Cognitive effort, as referred to in H2, is measured as the logged time respondents spent learning about the candidates. The log is taken because it better fits distributional assumptions for the response in OLS, since there is a substantial skew to the right in terms of the distribution of time spent gaining information. Ballots are coded as spoiled according to Maine election standards: a ballot is valid only if it includes ranks starting from first and proceeding step-wise to the final rank, and there is exclusively one rank given to each candidate.

The hypotheses are tested using difference-in-means tests, and OLS regression. For the models of spoilage and abstention, a linear probability model is preferred. A first set of control variables is supplied by Lucid, and includes age, education[18], race, and Hispanic identification. Another set is asked in pre-survey questions. Respondents are asked for their party identification, whether they are registered to vote, and whether they have previous RCV experience. These are asked primarily to ensure balance between groups; more politically active respondents or respondents with previous RCV experience could potentially skew information search and retention. They are also used in the OLS models to reduce standard errors and control for heterogeneous treatment effects beyond those that were hypothesized.

Results

Treatment Group and Information Effects

Table 2 presents the results from a set of t-tests between control and treatment group for the full and non-copartisan specifications of the response variable. The difference in means

[17] An alternate specification, with independents having no copartisans, is presented in Appendix B.

[18] In all regressions, the reference category for these variables is the 18-28 age group and individuals with no college education, respectively.

Table 2: Difference in means tests.

Hypotheses	Control Mean	Treatment Mean	Difference	t-Test
H1				
Profiles Seen	1.931	1.948	0.017	0.760
Correct Responses	1.873	1.851	-0.022	0.805
H1a				
Non-Copartisans Seen	0.919	0.923	0.003	0.943
Non-Copartisasn Correct	0.935	0.925	-0.009	0.881
H2				
Logged Time	4.124	4.127	0.004	0.944
H2a				
Abstention	0.065	0.042	-0.023	0.018

between the proportion of seen profiles and correct responses is not statistically significant, which provides a strong first indication against H1.

Results for H1 are further presented in Table 3 and Figure 1, which graphically presents point estimates and 95% confidence intervals for selected covariates. Respondents do not change the amount of information they seek or retain based on the treatment, relative to the single preference baseline. The effects of education on the sample as a whole confirm expectations, as individuals with at least some college education appear to be seeking and retaining more information across treatment and control groups. Individuals aged 28 to 48 appear to be seeking less information than the baseline group, although this effect is not present for older ages. There are no consistent effects of age on the number of correct responses individuals submit either; the 28-38 age group appears worse than the baseline at correctly answering questions, although all other age groups are statistically indistinguishable. The education effect outlined here provides some evidence for the validity of the experiment, but there is no evidence in favor of H1. It is also clear that there are no heterogeneous treatment effects based on age or education, which is evidence against H1c.

When the response variable is restricted to non-copartisan candidates, education and age effects remain roughly similar, although there is still no treatment effect present, as is clear from Figure 2. This is evidence against H1a, meaning that respondents in the treatment group appear quite hesitant to even reallocate their time and effort to different candidates, as the partisanship mechanisms in the theory would predict. The evidence implies that voters faced with RCV will continue to seek out independents and non-copartisans at approximately the current rate, which means that the representational benefits of RCV that rely from an expanded candidate selection pool would most likely not materialize.

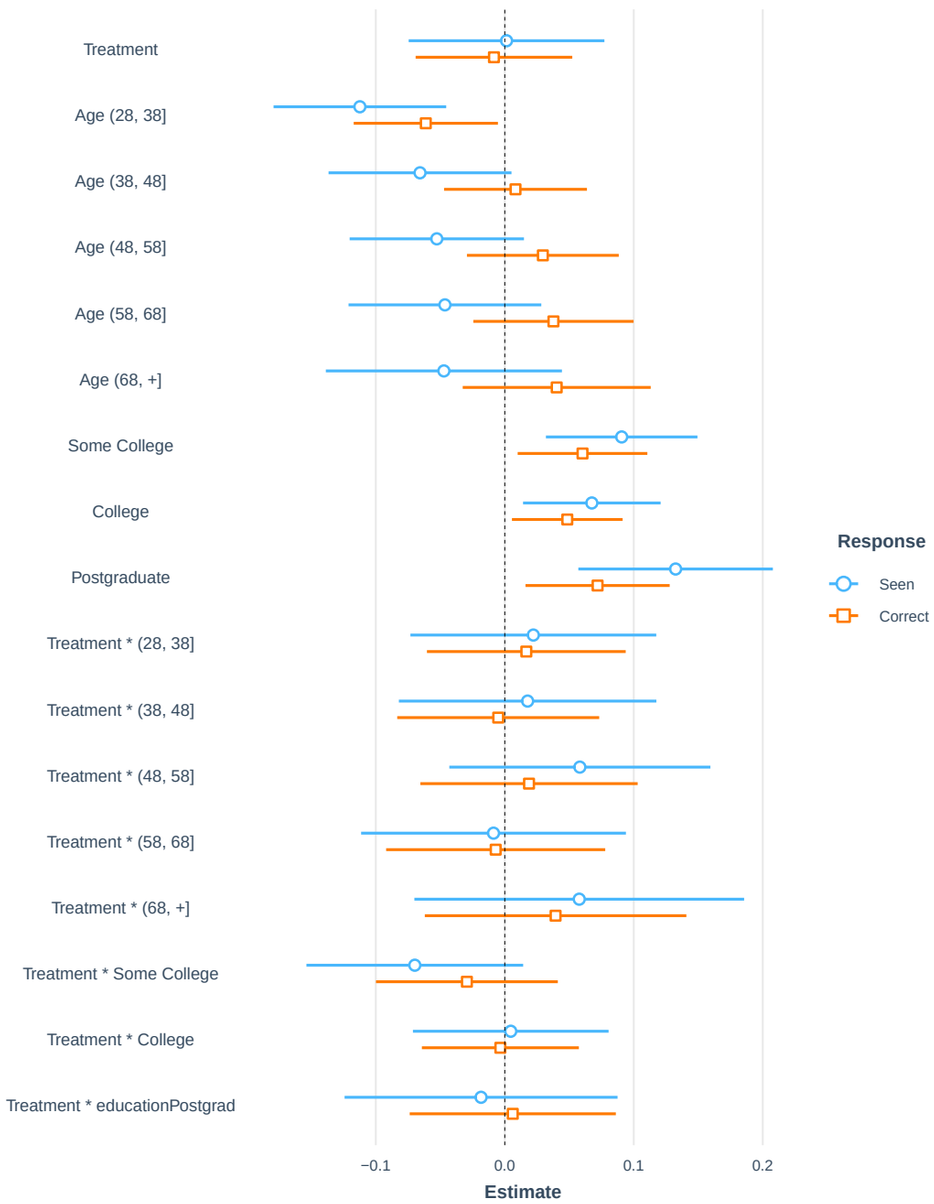


Figure 1: Test of H1: Treatment effect, education, and age.

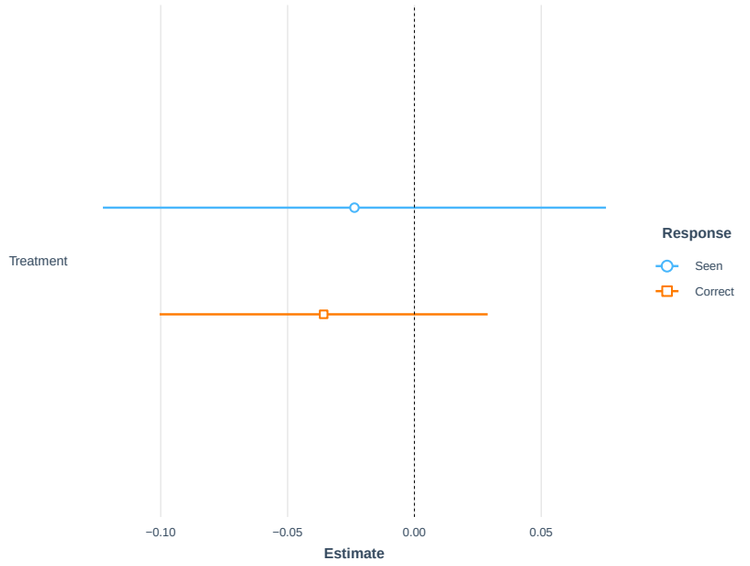


Figure 2: Test of H1a: Treatment effect for non-copartisan response.

The same theoretical mechanism is further tested in H1b. A variable indicating partisan status is added to the regression and interacted with the treatment. The marginal effects plots in Figure 3 reveal no evidence of a heterogeneous treatment effect of partisan status. However, they do reveal that independent respondents both seek and retain more information than their Democrat or Republican counterparts[19]. This is consistent with the theory on the importance of partisan attachment as a heuristic. Independents have to conduct a somewhat broader search to decide on their preferred candidate, given the lack of an automatic option, which leads them to seeking more profiles and as such retaining more information.

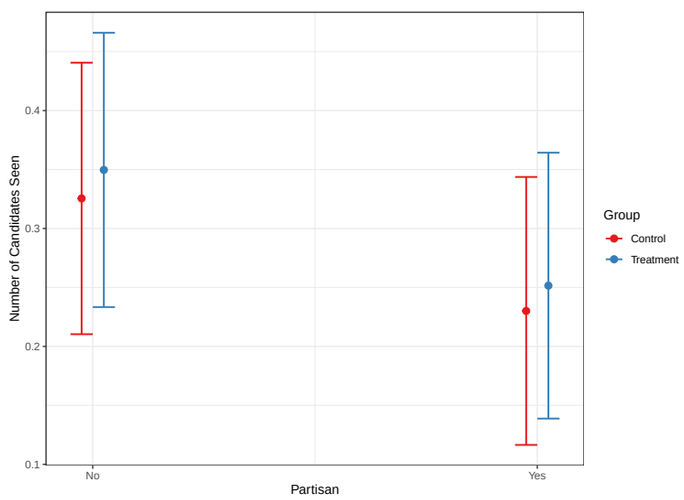
The overall conclusion on H1 is that respondents do not change any aspect of their information search and retention patterns, at least when it comes to the partisanship of candidates or knowledge on their endorsements and policy positions. The effects of partisanship and education provide some corroboration for the validity of the survey design; they are clearly non-random, and show an effect consistent to expectation.

Complexity

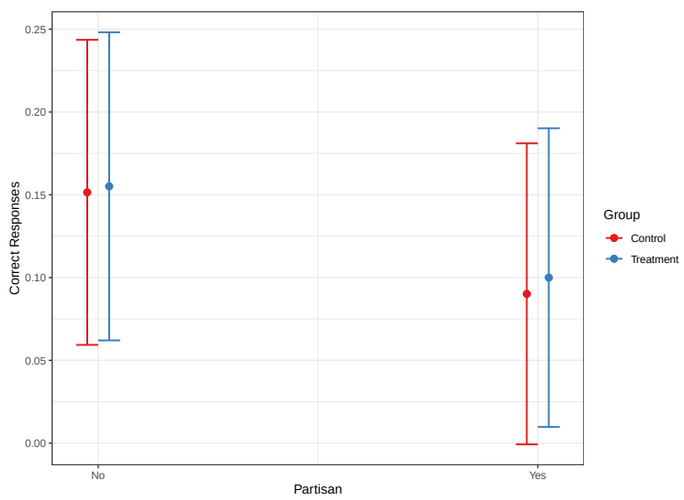
The results relating to H1 suggest a different picture from the theory. Respondents do not shift the amount of profiles they interact with, nor are they better at retaining information; therefore, it should be expected that the treatment has *not* led them to expend any additional effort. This is corroborated by the results in this section.

Respondents, on average, spend the same time on the candidate section between control and treatment groups, as is shown from the results of the t-test and the regression presented in Table 4 and Figure 4. Similarly to the previous section, there also appear to be no heterogeneous treatment effects of age and education, although older voters appear to be

[19]Full regression table can be found in the Appendix, where this effect is also clearly present.



(a) Candidates seen



(b) Correct responses

Figure 3: Test of H1b: Treatment effect interacted with partisanship

Table 3: Hypothesis H1, OLS regressions with heteroskedasticity-robust standard errors.

	Dependent variable:					
	Profiles Seen	Interactions	Non-copartisans	Correct Responses	Interactions	Non-copartisans
Treatment	0.006	0.001	−0.024	−0.007	−0.008	−0.036
	(0.016)	(0.039)	(0.051)	(0.012)	(0.031)	(0.033)
Age [28, 38)	−0.104***	−0.112***	−0.152***	−0.054***	−0.061**	−0.083***
	(0.026)	(0.034)	(0.045)	(0.021)	(0.028)	(0.031)
Age [38, 48)	−0.061**	−0.066*	−0.093**	0.004	0.008	−0.043
	(0.027)	(0.036)	(0.046)	(0.021)	(0.028)	(0.031)
Age [48, 58)	−0.027	−0.053	−0.089**	0.038*	0.030	−0.023
	(0.027)	(0.034)	(0.045)	(0.022)	(0.030)	(0.032)
Age [58, 68)	−0.052*	−0.046	−0.056	0.033	0.038	0.010
	(0.028)	(0.038)	(0.049)	(0.022)	(0.032)	(0.036)
Age [68, +)	−0.020	−0.047	−0.033	0.059**	0.040	0.010
	(0.033)	(0.047)	(0.058)	(0.026)	(0.037)	(0.043)
Some College	0.056**	0.091***	0.083**	0.045**	0.060**	0.051*
	(0.022)	(0.030)	(0.039)	(0.018)	(0.026)	(0.028)
College	0.069***	0.067**	0.077**	0.046***	0.048**	0.044*
	(0.020)	(0.027)	(0.034)	(0.016)	(0.022)	(0.024)
Postgraduate	0.123***	0.132***	0.169***	0.075***	0.072**	0.071**
	(0.027)	(0.038)	(0.048)	(0.022)	(0.028)	(0.032)
Treatment * Age [28, 38)		0.022	0.034		0.017	0.051
		(0.049)	(0.064)		(0.039)	(0.042)
Treatment * Age [38, 48)		0.018	0.028		−0.005	0.033
		(0.051)	(0.065)		(0.040)	(0.043)
Treatment * Age [48, 58)		0.058	0.126*		0.019	0.088*
		(0.052)	(0.067)		(0.043)	(0.045)
Treatment * Age [58, 68)		−0.009	0.008		−0.007	−0.003
		(0.052)	(0.067)		(0.043)	(0.047)
Treatment * Age [68, +)		0.058	0.091		0.039	0.079
		(0.065)	(0.081)		(0.052)	(0.058)
Treatment * Some College		−0.070	−0.064		−0.029	−0.024
		(0.043)	(0.055)		(0.036)	(0.039)
Treatment * College		0.005	0.014		−0.003	−0.004
		(0.039)	(0.050)		(0.031)	(0.034)
Treatment * Postgraduate		−0.018	−0.038		0.006	0.001
		(0.054)	(0.068)		(0.041)	(0.045)
Constant	0.317***	0.320***	0.147**	0.138***	0.139***	0.074
	(0.052)	(0.051)	(0.066)	(0.042)	(0.045)	(0.046)
Models include controls for ethnoracial characteristics, voter registration, previous RCV experience and party.						
Observations	1,488	1,488	1,488	1,488	1,488	1,488
Adjusted R^2	0.095	0.081	0.079	0.099	0.084	0.070
Note:						*p<0.1; **p<0.05; ***p<0.01

spending significantly more time, on average, viewing candidate profiles. It should be noted here that there were some data-related issues with measuring the time variable; there were major outliers in terms of survey completion time, and some missing values for the time spent on the candidate section. The latter observations are not included in the regression, but they appear random enough so as to not harm balance between control and treatment. The outliers in terms of survey completion, some reaching up to twenty five *hours* in total, are included in the regression, since their time spent on the candidate section appears to be about average; the best guess is that they left the survey open on an electronic device on a different page, and returned to it the next day.

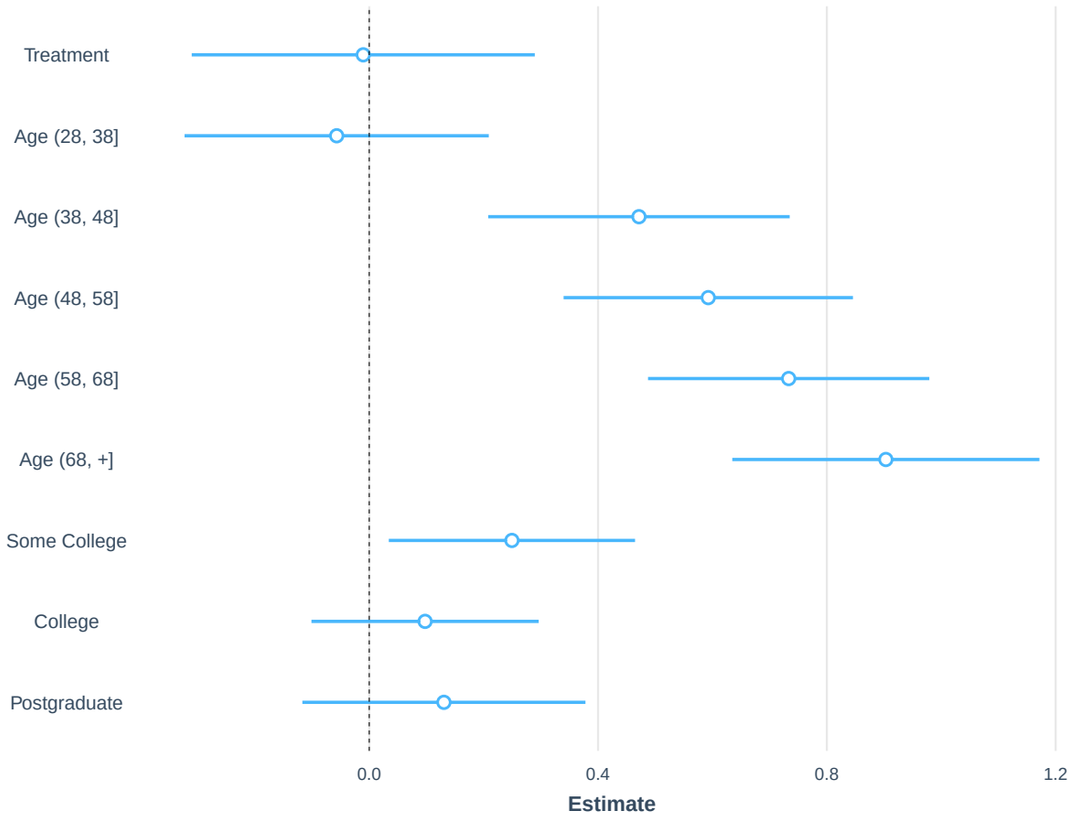


Figure 4: Test of H2: Time spent searching.

In Table 2, there appears to be a statistically significant difference in the mean abstention rate of the two groups, with individuals in the treatment group being *less* likely to abstain, counter to expectations. This result is partially corroborated by the first regression on Table 4, but does not hold up after the inclusion of interaction effects, which is also clear in Figure 5. The negative effect here is therefore quite weak, but does admittedly run somewhat counter to expectations from the theory. One possible explanation here could be a novelty effect making first-time RCV participants more likely to engage with the ballot, which would be consistent with the fact that participants who reported prior experience with RCV were also more likely to abstain in general, as can be seen from the full regression tables in Appendix A.

Lastly, ballot spoilage in the treatment group is examined in Figure 6. There appears to be no consistent effect of age or education on ballot spoilage, which goes counter to

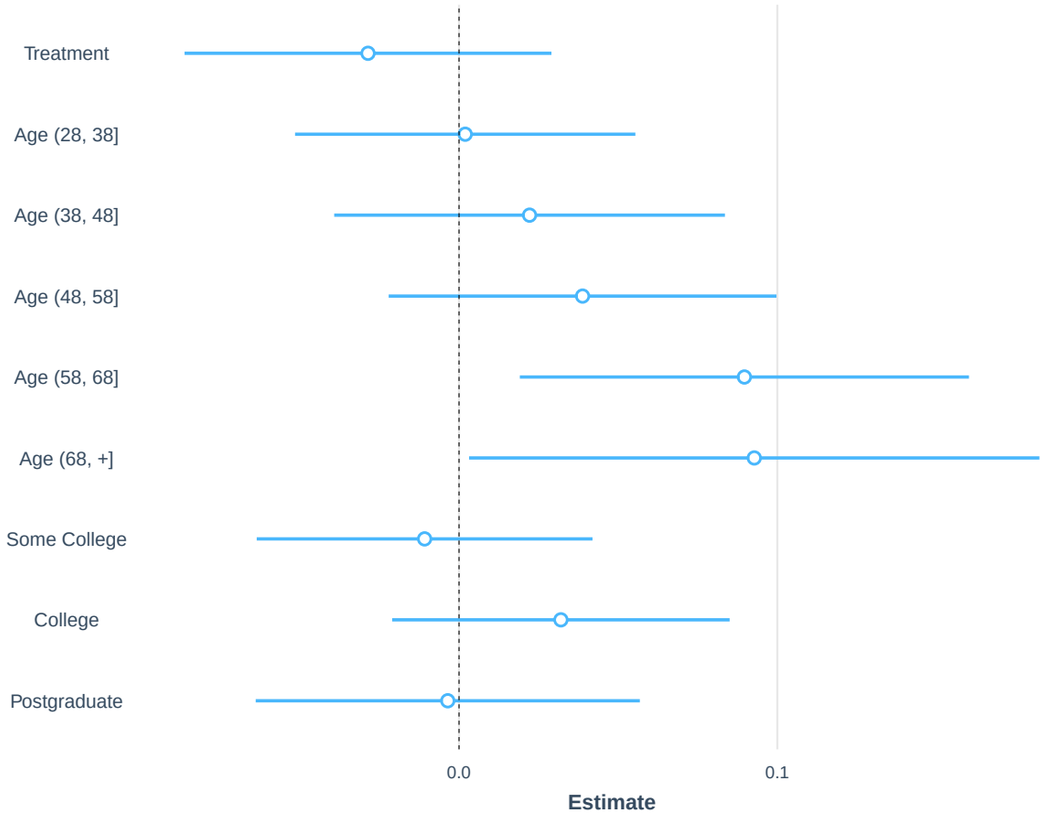


Figure 5: Test of H2a: Probability of abstention.

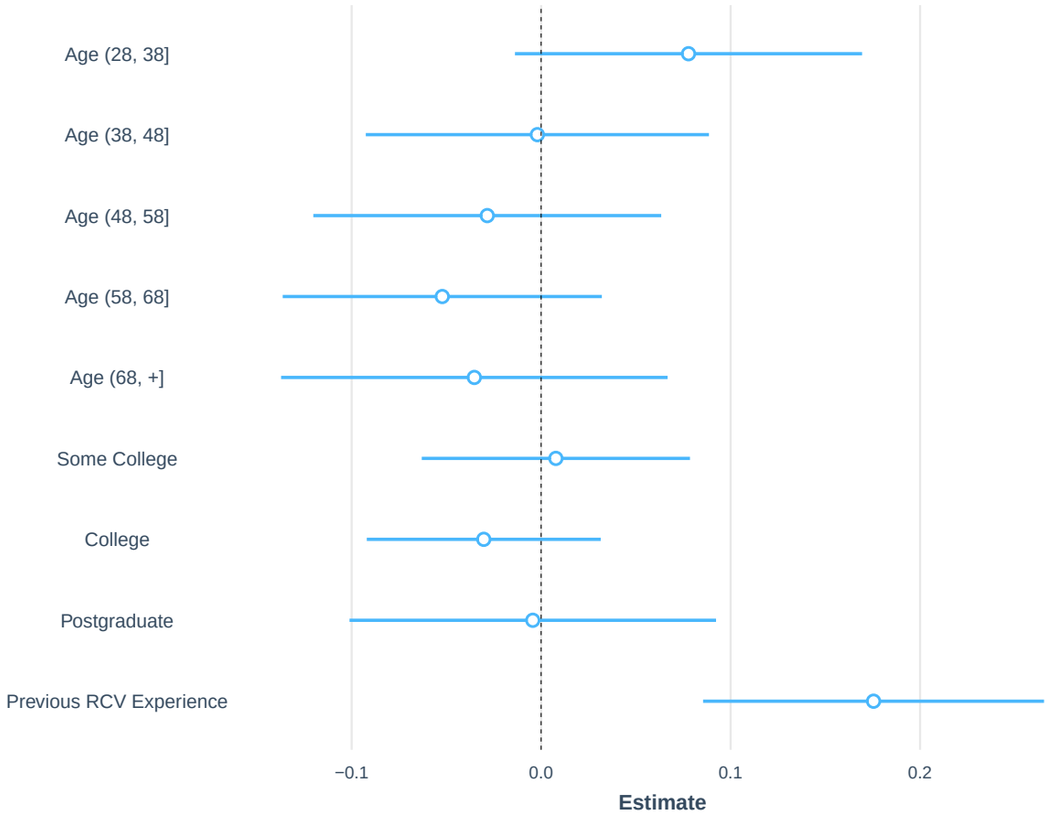


Figure 6: Test of H2b: Probability of spoilage.

Table 4: Hypothesis H2, OLS regressions with heteroskedasticity-robust standard errors.

	Dependent variable:				
	log(Time)	Interactions	Abstained	Interactions	Spoiled (Treat)
Treatment	−0.010	−0.010	−0.029**	−0.029	
	(0.053)	(0.153)	(0.013)	(0.029)	
Age [28, 38)	−0.057	−0.057	0.002	0.002	0.078*
	(0.099)	(0.136)	(0.020)	(0.027)	(0.047)
Age [38, 48)	0.472***	0.472***	0.022	0.022	−0.002
	(0.095)	(0.134)	(0.020)	(0.031)	(0.046)
Age [48, 58)	0.593***	0.593***	0.039*	0.039	−0.028
	(0.099)	(0.129)	(0.021)	(0.031)	(0.047)
Age [58, 68)	0.733***	0.733***	0.090***	0.090**	−0.052
	(0.095)	(0.125)	(0.023)	(0.036)	(0.043)
Age [68, +)	0.903***	0.903***	0.093***	0.093**	−0.035
	(0.104)	(0.137)	(0.029)	(0.046)	(0.052)
Some College	0.249***	0.249**	−0.011	−0.011	0.008
	(0.081)	(0.110)	(0.018)	(0.027)	(0.036)
College	0.098	0.098	0.032*	0.032	−0.030
	(0.073)	(0.101)	(0.017)	(0.027)	(0.031)
Postgraduate	0.131	0.131	−0.004	−0.004	−0.004
	(0.093)	(0.126)	(0.021)	(0.031)	(0.049)
Treatment * Age [28, 38)		0.127		0.060	
		(0.192)		(0.037)	
Treatment * Age [38, 48)		−0.082		−0.001	
		(0.190)		(0.037)	
Treatment * Age [48, 58)		0.183		−0.025	
		(0.193)		(0.036)	
Treatment * Age [58, 68)		0.099		−0.042	
		(0.181)		(0.042)	
Treatment * Age [68, +)		0.067		−0.047	
		(0.195)		(0.054)	
Treatment * Some College		−0.164		0.014	
		(0.161)		(0.036)	
Treatment * College		−0.014		−0.052	
		(0.144)		(0.033)	
Treatment * Postgraduate		−0.128		0.002	
		(0.179)		(0.038)	
Constant	3.429***	3.429***	0.085**	0.085**	0.141
	(0.185)	(0.202)	(0.039)	(0.040)	(0.087)
Models include controls for ethnoracial characteristics, voter registration, previous RCV experience and party.					
Observations	1,419	1,419	1,488	1,488	737
Adjusted R^2	0.175	0.173	0.034	0.035	0.068
Note:					*p<0.1; **p<0.05; ***p<0.01

empirical results on RCV in the observational setting. While this casts some doubt about the external validity of the survey, two factors should be mentioned. First, there is no randomization mechanism for the treatment group, meaning that internal validity of the test on unobservable characteristics cannot be guaranteed; the test is simply not set up to accurately detect such effects, and should only be taken as an indication. Second, even if a result *should* be detected, it is likely much smaller in the experimental setting, since there is no situational pressure that would exist at the ballot box; individuals voting may have waited in line, be aware that others are waiting to vote, and may not have the ability to easily correct mistakes on their ballot. All these factors are not present in the experimental setting, and therefore the test of H2b may be under-powered to detect a result, rather than it indicating a lack of validity for the survey as a whole.

Consistent with the previous section, respondents appear to be entirely unaffected by the treatment both in terms of information and on cognitive effort. Returning to the Basinger and Lavine (2005) trade-off, respondents presented with the RCV condition are not altering their cognitive budget, which in turn leads them to seek and retain the same amount of information, while broadly maintaining the same pattern of cross-partisan search. Respondents appear comfortable expressing their ranked order preferences with the information they already have at hand. While it is conceivable that they are correct in their assessment, contrary to the theory of RCV as informationally complex, it is quite interesting that the two groups reach a different electoral outcome; in the control group, the Democrat wins with 37.7 percent of the vote, while in the treatment group the Democrat and Republican survive to the last round, where the Republican wins with 51.4 percent of the aggregated preference votes. Two groups of individuals, randomized and balanced on observable characteristics, reach different electoral results while using the exact same information set. While it is conceivable that this is an accurate reflection of true ranked preferences, it is more likely somewhat concerning that the results of an election are switched when the electoral system becomes more complex, without voters adapting to that complexity.

Discussion

The results here are clearly inconsistent with the hypothesized view of RCV, as voters do not adapt their information search and retention strategies as measured by the survey instrument. One potential alternative explanation for these results comes from the study of ballot length and choice overload (Iyengar, Wells, and Schwartz, 2006; Cunow et al., 2021). Previous research has argued that as the choice set of candidates available increases, voters exhibit "choice overload" (Iyengar, Wells, and Schwartz, 2006; Iyengar and Lepper, 2000), which leads to confusion, incorrect voting, and abstention (Cunow, 2014; Cunow et al., 2021). Specifically, Cunow et al. (2021) argue that despite the purported benefits to representation

from a wider choice set, voters tend to not alter or even reduce the amount of policy information they seek for each candidate. This is confirmed through a similar survey experiment with simulated candidates in Brazil. Cunow (2014) also argues that voters presented with a larger ballot are likely to be "first-issue hunters", who quickly scan between candidates for a single, separating issue. Cohen (2018) empirically shows that higher levels of ballot length in South American democracies is predictive of higher abstention and spoilage rates, as voters are confused or turned off by complex ballots.

Could voters in this study be facing a similar "choice overload" effect, which leads them to rely on their previously trained party heuristics and a quick, single-issue scan of independent candidates? The survey results do not indicate that this is the case. RCV voters do not show *any* increase in information retention or search, which should be present even if they are simply learning from an additional single-issue scan. Nor are they expanding their information search to other candidates, even superficially, since there is no effect on non-copartisan information. Further still, abstention shows a weak *negative* effect for the treatment group, which is the exact opposite of what would be predicted by a "choice overload" phenomenon. Coupled with the null effect of the treatment on cognitive effort, this means that respondents who vote using RCV in this study appear less confused, and more simply impassive about RCV, not recognizing the complexities that it incurs. However, a form of "choice overload" should not be rejected as a potential outcome of RCV. Shifts in ballot length examined in studies of Latin America range the number of candidates from three to fifteen in a non-partisan setting, while the current RCV survey experiment only includes four candidates, two parties, and forces no minimum number of rankings. This means that perhaps RCV could have an actively detrimental "choice overload" effect in local election settings, which are often non-partisan and include a much larger range of candidates. RCV could still cause choice overload, but *conditionally* based on the choice set of candidates, the existence of the partisanship heuristic, or even increased rates of understanding of what RCV actually entails and how it functions. Further research is required to examine how the complexity of RCV might affect informational behavior in different electoral settings across the United States.

Another alternative, which would require further inquiry, is that voters are very hesitant to change their cognition on information unless they are institutionally forced to do so, like they are in non-partisan elections (Lupia, 1994), or the signal that change is necessary is particularly strong, as would be the case with a five fold increase in the number of candidates. Note that RCV is more complex, but typically does not *force* this complexity on the voters; they are still able to input a small number of rankings, they can still learn based on their past practices and heuristics, and they can still garner the satisfaction that comes from participation despite their ballot being exhausted, which is something they would have to consciously calculate ex post. Why would voters not shift their behaviors? Perhaps this is consistent with the general depiction of voters as "cognitive misers"(Fiske and Taylor, 1991) who exhibit motivated reasoning based on an ingrained, intuitive combination of partisan-

ship, heuristics, normative assessments and policy stereotypes (Kuklinski and Quirk, 2000). From this theoretical basis, it could follow that voters are misers when they are called to adapt their information search on a macro level, and do not consider the fact that full representation under RCV requires more information than before. Perhaps an effect may be clearer when RCV systems require a minimum number of ranks for a ballot to be counted as valid, thus setting a hard institutional mandate on the exact extent of participation. In addition, voters in observational settings may adapt their practices over time, as they realize the potential of RCV. This effect would be very hard to test for in a survey experiment setting, but should be further examined.

A last alternative explanation for the reported results may be that the methodology is flawed. This would likely be either the fault of the survey or of the incentive structure for Lucid respondents. Respondents in online surveys typically get compensated for completion, and as such would want to finish the survey as fast as possible, and not necessarily provide a strong response. There are some reasons for concern. Respondents performed particularly poorly on average in the information test, and spent a relatively low median time of four minutes on the survey. I have tried to address these concerns by pointing at some patterns that indicate respondents are participating in good faith: partisans vote for copartisans, respondents submit extensive rankings and abstain rarely, they tend to genuinely express their ignorance when it is impossible they would know the answer to a survey question, and there are clear patterns of education and partisanship effects that appear consistent with the theory. In addition, respondents who see more profiles and submit more correct responses spend a larger amount of time on the survey as a whole; if their behavior was an outcome of random chance, then there should be no relationship between survey completion and how well they performed. I also control for randomness by dropping implausible correct responses, and perform standard attention checks. On the whole, while I cannot rule out that several respondents were simply mechanistically completing the survey, the survey results appear consistent with expectations and should be trusted. Low accuracy on the information questions is also an issue, but is somewhat assuaged by the variance in average correct response rates per question. This was also a necessity of the survey set-up; the point of the representational benefits of RCV rest on respondents being able to expand their knowledge of *useful* endorsement and policy information, and as such the theory mandated some complexity from the knowledge test.

Conclusion

Subjects in this survey experiment do not show any evidence of altering their information search and retention behavior in response to the complexities presented by Ranked Choice Voting. In the context of the trade-off between effective expression of preferences and cogni-

tive effort (Basinger and Lavine, 2005), respondents either do not recognize the representational opportunity offered by RCV or feel that their previous information search behaviors suffice. If the latter is true they are likely incorrect, or at least unable to sufficiently access the representational benefits that RCV promises. This hesitancy is clearly documented by the consistently null results in all hypotheses. Across age and education groups, which could be said to proxy RCV comprehension based on past literature, voters do not spend more time on researching candidates, nor are they more effective at responding to questions regarding policy and endorsements. Voters also examine approximately the same number of candidates, and do not seem to allocate their time more broadly when faced with the prospect of ranking.

This study contributes to our understanding of voter behavior and RCV is a number of ways. First, the theory in this paper demonstrates that RCV represents a more complex informational environment than typical single preference voting. By connecting RCV with canonical works on voter information, I show that voters need to adapt their information search behavior if they are to access the representational benefits that ranked preference can provide. I also show the importance of sequential heuristic exhaustion which should, but crucially *does not*, lead voters to broaden their information search if they are to accurately express their ranked candidate ordering.

Second, I give strong evidence that voters do not adapt to this complex informational environment, and seem to stay committed to their previous patterns of information search with regards to policy, endorsement, and the number of candidates they examine. This presents an empirical contribution to the literature of election reform, since it shows that giving voters more options, choice, or expanding their opportunities for representation does not imply that they will capitalize on that opportunity, consistent with the final conclusion, but not the mechanism, of research on ballot length (Cunow et al., 2021; Cohen, 2018). The findings in this paper suggest that voters may be unable to access the representational benefits that are theorized to follow RCV implementation. If voters do not adapt their information search they can't identify and reward ideologically proximate candidates, which means there is little incentive for expanded candidate entry, which in turn leads to exactly the same "lesser of two evils" choice, just with a more complex and unwieldy ballot. This result could also compound issues of fair representation, considering that those who report lack of comprehension of RCV, and as such should be more likely to abstain or spoil their ballots as a sacrifice to an uncertain benefit, are older and less educated voters (Donovan, Tolbert, and Gracey, 2019). Therefore, a perverse consequence (Berinsky, 2005) of RCV could be shifting the composition of the electorate.

A last implication is that more research is necessary before further expansions of RCV in the United states. While the goal of expanded choice and civility is admirable, we simply do not know enough about how voters will respond to RCV in order to fully sanction its

implementation. A natural first step is to examine if the null results produced in this study actually lead to worse, or at least unimproved, representation. It is highly unlikely, but perhaps voters do get enough information in the status quo to accurately express their preferences under RCV. A similar design to Cunow et al. (2021) or Lau et al. (2006) could help discern if voters are ranking "correctly" based on their preferences. These studies could be augmented with observational data from Maine and Alaska, in order to examine how voters are adapting their informational search in real world conditions. Another avenue of research could examine a supply-side explanation of increased information, as suggested by Neely and Cook (2008). They posit, but do not directly test, that campaigns in RCV elections could have incentives to more broadly share information, even reaching the point of cooperating on public outreach. This could be conceptualized as a form of informational subsidy, which does not require that voters expend more cognitive effort but leads to information itself becoming less costly. Given such a subsidy, it would be possible for voters to access representational benefits without increasing their cognitive budget, therefore circumventing the problem illustrated in this survey design.

Hasty expansion of promising electoral reforms can lead to unintended consequences (Berinsky, 2005; Burden et al., 2014). The results presented in this paper should urge caution about RCV as an experiment for the improvement of American elections. While further research into supply-side mechanisms or representational outcomes may prove this concern unwarranted, these studies should be conducted prior to a wider implementation of a mostly untested electoral reform. It is therefore critical and time sensitive to conduct more observational and experimental studies on how ranking affects voter behavior, before it is fully institutionalized and its consequences are in wide effect.

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A Regression Tables

Table 5: Regression table for H1 with all covariates.

	Dependent variable:			
	Profiles Seen	Non-Copartisan	Correct Answers	Non-Copartisan
Treatment	0.001	−0.024	−0.008	−0.036
	(0.039)	(0.051)	(0.031)	(0.033)
Age [28, 38)	−0.112***	−0.152***	−0.061**	−0.083***
	(0.034)	(0.045)	(0.028)	(0.031)
Age [38, 48)	−0.066*	−0.093**	0.008	−0.043
	(0.036)	(0.046)	(0.028)	(0.031)
Age [48, 58)	−0.053	−0.089**	0.030	−0.023
	(0.034)	(0.045)	(0.030)	(0.032)
Age [58, 68)	−0.046	−0.056	0.038	0.010
	(0.038)	(0.049)	(0.032)	(0.036)
Age [68, +)	−0.047	−0.033	0.040	0.010
	(0.047)	(0.058)	(0.037)	(0.043)
Some College	0.091***	0.083**	0.060**	0.051*
	(0.030)	(0.039)	(0.026)	(0.028)
College	0.067**	0.077**	0.048**	0.044*
	(0.027)	(0.034)	(0.022)	(0.024)
Postgraduate	0.132***	0.169***	0.072**	0.071**
	(0.038)	(0.048)	(0.028)	(0.032)
Previous RCV Experience	−0.093***	−0.063**	−0.104***	−0.083***
	(0.021)	(0.027)	(0.015)	(0.016)
Registered Voter	0.102***	0.106***	0.071***	0.070***
	(0.023)	(0.029)	(0.018)	(0.020)
Democrat	0.035*	0.057**	0.023	0.042***
	(0.019)	(0.024)	(0.015)	(0.016)
Independent/Other	0.117***	0.167***	0.071***	0.102***
	(0.020)	(0.025)	(0.016)	(0.018)
No Hispanic Answer	−0.107**	−0.096	−0.089**	−0.087**
	(0.051)	(0.067)	(0.039)	(0.038)
Non-Hispanic	0.063**	0.096***	0.014	0.038*
	(0.028)	(0.035)	(0.022)	(0.022)
Black	−0.116***	−0.157***	−0.088**	−0.098**
	(0.041)	(0.052)	(0.037)	(0.039)
Indigenous	−0.023	−0.058	−0.090	−0.152**
	(0.122)	(0.161)	(0.060)	(0.070)
Other Ethnicity	−0.038	−0.029	−0.041	−0.025
	(0.048)	(0.060)	(0.040)	(0.042)
Pacific Islander	−0.278***	−0.326***	−0.169***	−0.160**
	(0.065)	(0.100)	(0.061)	(0.081)
White	−0.013	−0.014	−0.027	−0.022
	(0.038)	(0.047)	(0.033)	(0.036)
Treatment * Age [28, 38)	0.022	0.034	0.017	0.051
	(0.049)	(0.064)	(0.039)	(0.042)
Treatment * Age [38, 48)	0.018	0.028	−0.005	0.033
	(0.051)	(0.065)	(0.040)	(0.043)
Treatment * Age [48, 58)	0.058	0.126*	0.019	0.088*
	(0.052)	(0.067)	(0.043)	(0.045)
Treatment * Age [58, 68)	−0.009	0.008	−0.007	−0.003
	(0.052)	(0.067)	(0.043)	(0.047)
Treatment * Age [68, +)	0.058	0.091	0.039	0.079
	(0.065)	(0.081)	(0.052)	(0.058)
Treatment * Some College	−0.070	−0.064	−0.029	−0.024
	(0.043)	(0.055)	(0.036)	(0.039)
Treatment * College	0.005	0.014	−0.003	−0.004
	(0.039)	(0.050)	(0.031)	(0.034)
Treatment * Postgraduate	−0.018	−0.038	0.006	0.001
	(0.054)	(0.068)	(0.041)	(0.045)
Constant	0.320***	0.147**	0.139***	0.074
	(0.051)	(0.066)	(0.045)	(0.046)
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 6: Regression table for H2 with all covariates.

	Dependent variable:		
	log(Time)	Abstained	Spoiled (Treat)
Treatment	−0.010	−0.029	
	(0.153)	(0.029)	
Age [28, 38)	−0.057	0.002	0.078*
	(0.136)	(0.027)	(0.047)
Age [38, 48)	0.472***	0.022	−0.002
	(0.134)	(0.031)	(0.046)
Age [48, 58)	0.593***	0.039	−0.028
	(0.129)	(0.031)	(0.047)
Age [58, 68)	0.733***	0.090**	−0.052
	(0.125)	(0.036)	(0.043)
Age [68, +)	0.903***	0.093**	−0.035
	(0.137)	(0.046)	(0.052)
Some College	0.249**	−0.011	0.008
	(0.110)	(0.027)	(0.036)
College	0.098	0.032	−0.030
	(0.101)	(0.027)	(0.031)
Postgraduate	0.131	−0.004	−0.004
	(0.126)	(0.031)	(0.049)
Previous RCV Experience	−0.447***	−0.035***	0.175***
	(0.078)	(0.014)	(0.046)
Registered Voter	0.193**	−0.061**	0.009
	(0.092)	(0.025)	(0.042)
Democrat	−0.014	0.029**	−0.017
	(0.068)	(0.014)	(0.032)
Independent/Other	0.216***	0.059***	0.018
	(0.070)	(0.017)	(0.031)
No Hispanic Answer	−0.178	−0.024	0.074
	(0.177)	(0.058)	(0.115)
Non-Hispanic	0.184*	−0.030	−0.056
	(0.110)	(0.025)	(0.061)
Black	−0.243	−0.003	0.104
	(0.149)	(0.028)	(0.065)
Indigenous	−0.264	0.092	0.154
	(0.387)	(0.123)	(0.199)
Other Ethnicity	0.040	0.046	0.053
	(0.166)	(0.036)	(0.077)
Pacific Islander	−0.150	0.126	0.419
	(0.549)	(0.193)	(0.338)
White	−0.120	0.012	0.003
	(0.127)	(0.024)	(0.049)
Treatment * Age [28, 38)	0.127	0.060	
	(0.192)	(0.037)	
Treatment * Age [38, 48)	−0.082	−0.001	
	(0.190)	(0.037)	
Treatment * Age [48, 58)	0.183	−0.025	
	(0.193)	(0.036)	
Treatment * Age [58, 68)	0.099	−0.042	
	(0.181)	(0.042)	
Treatment * Age [68, +)	0.067	−0.047	
	(0.195)	(0.054)	
Treatment * Some College	−0.164	0.014	
	(0.161)	(0.036)	
Treatment * College	−0.014	−0.052	
	(0.144)	(0.033)	
Treatment * Postgraduate	−0.128	0.002	
	(0.179)	(0.038)	
Constant	3.429***	0.085**	0.141
	(0.202)	(0.040)	(0.087)
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 7: Regression table for H1b

	Dependent variable:	
Non-copartisan:	Profiles Seen	Correct Responses
Treatment	0.003	−0.012
	(0.045)	(0.035)
Age [28, 38)	−0.112***	−0.061**
	(0.034)	(0.029)
Age [38, 48)	−0.065*	0.009
	(0.036)	(0.028)
Age [48, 58)	−0.052	0.030
	(0.035)	(0.030)
Age [58, 68)	−0.048	0.037
	(0.038)	(0.032)
Age [68, +)	−0.049	0.039
	(0.047)	(0.037)
Some College	0.091***	0.061**
	(0.030)	(0.026)
College	0.069**	0.049**
	(0.027)	(0.022)
Postgraduate	0.136***	0.074***
	(0.038)	(0.028)
Previous RCV Experience	−0.092***	−0.102***
	(0.021)	(0.015)
Registered	0.104***	0.072***
	(0.023)	(0.018)
Partisan	−0.095***	
	(0.025)	
N/A	−0.110**	−0.090**
	(0.052)	(0.039)
Non-Hispanic	0.061**	0.013
	(0.028)	(0.022)
Black	−0.111***	−0.084**
	(0.041)	(0.037)
Indigenous	−0.029	−0.094
	(0.125)	(0.061)
Other Ethnicity	−0.038	−0.041
	(0.048)	(0.040)
Pacific Islander	−0.276***	−0.167***
	(0.066)	(0.060)
White	−0.016	−0.028
	(0.037)	(0.033)
Treatment * [28, 38)	0.021	0.015
	(0.049)	(0.039)
Treatment * [38, 48)	0.015	−0.007
	(0.051)	(0.040)
Treatment * [48, 58)	0.056	0.016
	(0.052)	(0.043)
Treatment * [58, 68)	−0.008	−0.007
	(0.052)	(0.044)
Treatment * [68, +)	0.057	0.039
	(0.065)	(0.052)
Treatment * Some College	−0.071*	−0.030
	(0.043)	(0.036)
Treatment * College	0.005	−0.003
	(0.039)	(0.031)
Treatment * Postgraduate	−0.014	0.008
	(0.054)	(0.041)
Control * Partisan		−0.061***
		(0.020)
Treatment * Partisan	−0.003	−0.055***
	(0.034)	(0.019)
Constant	0.438***	0.212***
	(0.053)	(0.045)
Note:		*p<0.1; **p<0.05; ***p<0.01

B Alternate Independents Specification for H1a.

In this section, independent respondents are modeled as having no copartisans for the purpose of testing H1b, rather than modeled as being copartisan with the two available independent candidates.

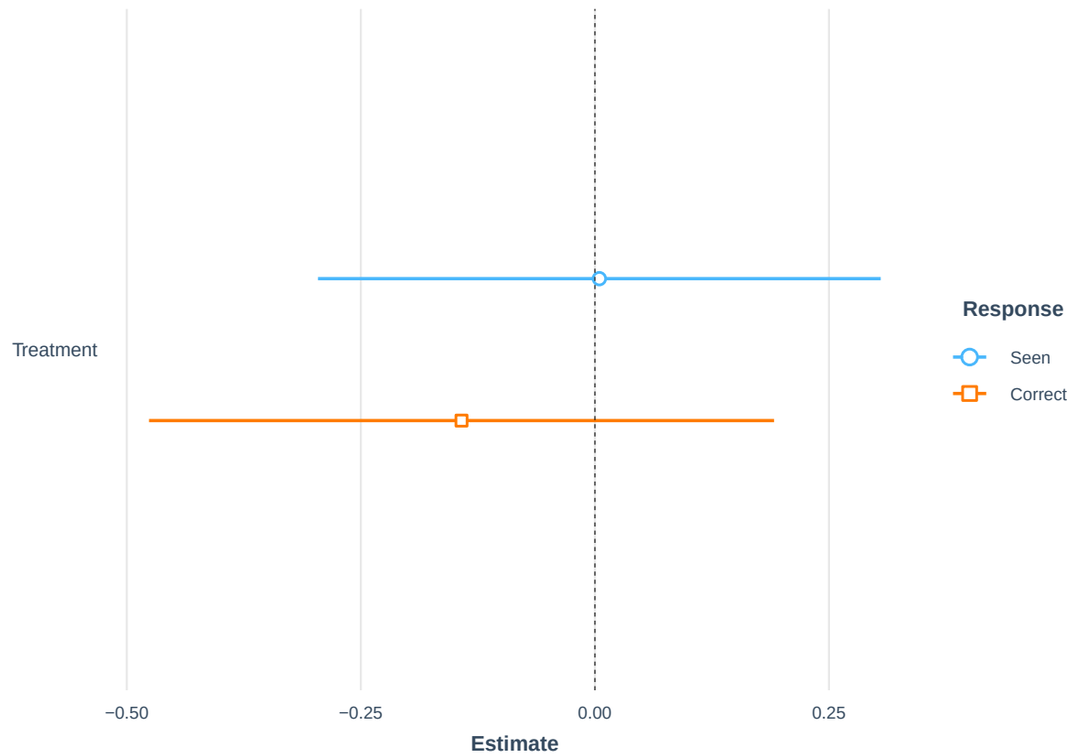

Figure 7: Alternate specification of H1a.

Table 8: Alternate specification of H1a: regression table.

	Dependent variable:	
	Profiles Seen	Correct Responses
Treatment	0.005	−0.142
	(0.153)	(0.170)
Age [28, 38)	−0.423***	−0.438***
	(0.136)	(0.161)
Age [38, 48)	−0.244*	−0.236
	(0.142)	(0.168)
Age [48, 58)	−0.209	−0.137
	(0.133)	(0.173)
Age [58, 68)	−0.209	−0.028
	(0.149)	(0.183)
Age [68, +)	−0.148	−0.038
	(0.184)	(0.221)
Some College	0.342***	0.324**
	(0.118)	(0.149)
College	0.271**	0.229*
	(0.106)	(0.123)
Postgraduate	0.537***	0.392**
	(0.149)	(0.165)
Previous RCV Experience	−0.273***	−0.478***
	(0.083)	(0.089)
Registered	0.397***	0.323***
	(0.089)	(0.093)
Democrat	0.165**	0.243**
	(0.073)	(0.095)
Independent	1.361***	0.134
	(0.078)	(0.087)
N/A	−0.362*	−0.419**
	(0.201)	(0.180)
Non-Hispanic	0.269**	0.205*
	(0.108)	(0.124)
Black	−0.458***	−0.555***
	(0.159)	(0.212)
Indigenous	−0.133	−0.776*
	(0.485)	(0.404)
Other Ethnicity	−0.140	−0.155
	(0.186)	(0.224)
Pacific Islander	−0.935***	−0.796**
	(0.307)	(0.349)
White	−0.028	−0.146
	(0.144)	(0.193)
Treatment * Age [28, 38)	0.031	0.232
	(0.194)	(0.217)
Treatment * Age [38, 48)	0.035	0.175
	(0.199)	(0.226)
Treatment * Age [48, 58)	0.198	0.454*
	(0.200)	(0.242)
Treatment * Age [58, 68)	−0.029	0.068
	(0.204)	(0.248)
Treatment * Age [68, +)	0.130	0.471
	(0.256)	(0.300)
Treatment * Some College	−0.235	−0.235
	(0.168)	(0.205)
Treatment * College	0.064	−0.060
	(0.151)	(0.176)
Treatment * Postgraduate	−0.068	−0.014
	(0.208)	(0.238)
Constant	0.347*	0.568**
	(0.200)	(0.254)
Note:		*p<0.1; **p<0.05; ***p<0.01

C IRT Implementation and Results for H1

IRT, commonly used in political science to measure responses such as ideological positioning (DeCrescenzo, n.d.; Clinton, Jackman, and Rivers, 2004), is implemented here as a robustness check for varying question difficulty. The 1PL model described briefly in the text of the paper is implemented using the **brms** package in R, which uses a Bayesian estimation procedure for latent ability scores and question difficulty. In this case I use Normal priors for all parameters, with partial pooling for both, which assumes some covariance between different questions answered by the same respondent (Bürkner, 2020). The model is then fit using Hamiltonian Monte Carlo. Goodness of fit is assessed from effective sample size and chain convergence parameters, which are all well inside typical value ranges.

However, it should be noted that the data in this case do not comply particularly well with the assumptions behind IRT (Nguyen et al., 2014). For example, IRT assumes that the questions are locally independent conditional on the latent trait. While pooling question difficulty somewhat deals with this issue, it does not account for the obvious covariation that comes from the questions referring, in groups, to specific candidates. The covariance structure, therefore, should show that answering correctly to one Republican question, should lead to a higher probability of answering the other question on the Republican correctly as well. It is also unclear whether the test is well calibrated to exhibit item invariance, which assumes that the estimated parameters are constant across subgroups. This may be violated, for instance, if different respondents exhibit different interpretations of the same question. In this case, it cannot be ruled out that factors such as partisanship color the interpretation of questions or facts; a phenomenon that is well documented in studies of voter information (Bartels, 2002; Bullock et al., 2013; Taber and Lodge, 2006). For this reason, I use IRT here as a robustness check rather than the main analytical tool of the paper. The results that follow in Table 9 exhibit similar null findings for H1; the t-test p-value between scores for Treatment and Control groups was 0.75.

Table 9: Comparison of IRT score model with correct response model.

	Dependent variable:	
	Proportion Correct	IRT Score
Treatment	−0.008	0.002
	(0.032)	(0.092)
Age [28, 38)	−0.061**	−0.184**
	(0.029)	(0.081)
Age [38, 48)	0.008	0.077
	(0.030)	(0.086)
Age [48, 58)	0.030	0.107
	(0.029)	(0.083)
Age [58, 68)	0.038	0.143*
	(0.030)	(0.087)
Age [68, +)	0.040	0.152
	(0.036)	(0.102)
Some College	0.060**	0.174**
	(0.025)	(0.072)
College	0.048**	0.151**
	(0.023)	(0.066)
Postgraduate	0.072**	0.233***
	(0.030)	(0.084)
Treatment * Age [28, 38)	0.017	0.056
	(0.041)	(0.116)
Treatment * Age [38, 48)	−0.005	−0.062
	(0.042)	(0.120)
Treatment * Age [48, 58)	0.019	0.037
	(0.042)	(0.120)
Treatment * Age [58, 68)	−0.007	−0.048
	(0.042)	(0.120)
Treatment * Age [68, +)	0.039	0.083
	(0.050)	(0.143)
Treatment * Some College	−0.029	−0.090
	(0.036)	(0.102)
Treatment * College	−0.003	−0.026
	(0.032)	(0.091)
Treatment * Postgraduate	0.006	−0.029
	(0.042)	(0.119)
Constant	0.139***	−0.305**
	(0.045)	(0.127)
Observations	1,488	1,488
Adjusted R^2	0.084	0.091
Models include controls for ethnoracial characteristics, voter registration, previous RCV experience and party.		
Note:	*p<0.1; **p<0.05; ***p<0.01	

D Results by Abstention Status

Table 10: Results for base models with abstained population excluded.

	Dependent variable:		
	Profiles Seen	Correct Responses	Logged Time
Treatment	−0.002	−0.015	−0.016
	(0.040)	(0.032)	(0.157)
Age [28, 38)	−0.088**	−0.051*	−0.038
	(0.035)	(0.030)	(0.139)
Age [38, 48)	−0.050	0.010	0.465***
	(0.037)	(0.029)	(0.139)
Age [48, 58)	−0.032	0.044	0.658***
	(0.035)	(0.031)	(0.133)
Age [58, 68)	−0.022	0.049	0.744***
	(0.040)	(0.033)	(0.131)
Age [68, +)	−0.042	0.059	0.941***
	(0.049)	(0.039)	(0.142)
Some College	0.089***	0.060**	0.246**
	(0.031)	(0.027)	(0.114)
College	0.051*	0.033	0.051
	(0.028)	(0.023)	(0.105)
Postgraduate	0.107***	0.065**	0.089
	(0.039)	(0.030)	(0.132)
Treatment * Age [28, 38)	0.001	0.002	0.073
	(0.049)	(0.040)	(0.194)
Treatment * Age [38, 48)	0.007	−0.005	−0.079
	(0.051)	(0.041)	(0.194)
Treatment * Age [48, 58)	0.043	0.006	0.112
	(0.052)	(0.044)	(0.198)
Treatment * Age [58, 68)	−0.017	−0.007	0.095
	(0.054)	(0.044)	(0.186)
Treatment * Age [68, +)	0.066	0.030	0.033
	(0.067)	(0.054)	(0.200)
Treatment * Some College	−0.062	−0.023	−0.129
	(0.044)	(0.037)	(0.166)
Treatment * College	0.016	0.013	0.036
	(0.039)	(0.032)	(0.147)
Treatment * Postgraduate	0.005	0.014	−0.090
	(0.055)	(0.041)	(0.182)
Constant	0.348***	0.162***	3.488***
	(0.052)	(0.046)	(0.207)
Observations	1,391	1,391	1,345
Adjusted R^2	0.087	0.091	0.176
Models include controls for ethnoracial characteristics, voter registration, previous RCV experience and party.			
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 11: Results only for abstainers

	Dependent variable:		
	Profiles Seen	Correct Responses	Logged Time
Treatment	−0.123	0.005	−0.034
	(0.155)	(0.158)	(0.646)
Age [28, 38)	−0.393***	−0.181*	−0.331
	(0.124)	(0.104)	(0.318)
Age [38, 48)	−0.246	−0.009	0.321
	(0.156)	(0.127)	(0.523)
Age [48, 58)	−0.238*	−0.066	−0.190
	(0.131)	(0.128)	(0.384)
Age [58, 68)	−0.200	0.021	0.490
	(0.136)	(0.132)	(0.390)
Age [68, +)	−0.071	0.009	0.440
	(0.150)	(0.137)	(0.427)
Some College	0.122	0.031	0.254
	(0.100)	(0.067)	(0.350)
College	0.247***	0.206***	0.806***
	(0.074)	(0.067)	(0.293)
Postgraduate	0.427***	0.158*	1.408***
	(0.121)	(0.085)	(0.285)
Treatment * Age [28, 38)	0.664***	0.432*	2.244**
	(0.247)	(0.217)	(1.038)
Treatment * Age [38, 48)	0.191	0.026	0.775
	(0.237)	(0.182)	(0.946)
Treatment * Age [48, 58)	0.418*	0.237	2.104**
	(0.215)	(0.235)	(0.805)
Treatment * Age [58, 68)	0.122	−0.040	0.580
	(0.208)	(0.198)	(0.887)
Treatment * Age [68, +)	0.229	0.126	1.823**
	(0.329)	(0.229)	(0.866)
Treatment * Some College	−0.338*	−0.195	−1.671**
	(0.196)	(0.154)	(0.699)
Treatment * College	−0.179	−0.181	−1.218**
	(0.143)	(0.112)	(0.594)
Treatment * Postgraduate	−0.616**	−0.320	−3.033***
	(0.256)	(0.232)	(1.104)
Constant	0.165	0.036	2.839***
	(0.337)	(0.212)	(0.886)
Observations	97	97	74
Adjusted R^2	0.072	−0.027	0.224
Models include controls for ethnoracial characteristics, voter registration, previous RCV experience and party.			

Note: *p<0.1; **p<0.05; ***p<0.01

E Survey Instrument

UC San Diego
Research Affairs

Consent

Graduate student Theodore Ntounias, under the supervision of Professor Thad Kousser, is conducting a research study about Ranked Choice Voting in the United States. If you agree to participate, you will read some information and answer a set of survey questions. Research records will be confidential to the extent allowed by law. Your information will be protected by Lucid and by principal investigator Theodore Ntounias. Participation is entirely voluntary. You may refuse to participate or withdraw at any time without penalty or loss of benefits to which you are entitled. If you want additional information or have research questions about this study, you may reach Theodore Ntounias at 971-380-7112.

Do you consent to take this survey?

- ○ Yes, I consent.
- ○ No, I do not consent.

Attention Checks

People these days are very busy and may not follow what goes on in election campaigns. **We are testing how voters receive information.** To show that you have read this prompt, please answer **both** "Strongly interested" **and** "Not interested".

- [ ] Extremely interested.
- [ ] Strongly Interested
- [ ] Mildly interested.
- [ ] Not interested.
- [ ] Not sure.

Pre-survey Questions

Generally, do you consider yourself as a Republican, Democrat, independent, or what other party?

- ○ Republican
- ○ Democrat
- ○ Independent
- ○ Other: _____

Are you registered to vote?

- Yes
- No
- Not sure

Have you ever participated in an election where you were required to rank candidates, rather than pick a single prefered option?

- Yes
- No
- Not sure

Control prompt

In the following section you will be presented with a set of simulated candidates. Please assume that they are competing for statewide office in your state. Later on in this process, you will be asked to vote. Therefore, take as much time as you need to feel comfortable that your decision best reflects your interests and views.

You will vote using **single preference voting**. This means that you will be asked to pick your single most preferred candidate. After the election is over, the candidate with the most votes wins. This is the standard system employed in the vast majority of U.S. State and Federal elections.

Treatment prompt

In the following section you will be presented with a set of simulated candidates. Please assume that they are competing for statewide office in your state. Later on in this process, you will be asked to vote. Therefore, take as much time as you need to feel comfortable that your decision best reflects your interests and views.

You will vote using **ranked choice voting**. This means that you will be asked to choose your candidates in order of preference, by marking candidates as your first, second, third, and subsequent choices. You may rank as many or as few candidates as you see fit.

The votes are tabulated in rounds, with the lowest-ranked candidates eliminated in each round until there are only two candidates left. The one who is determined to have received the majority of the votes (more than 50%) in the final round is declared the winner. It is different from single preference voting (the standard in most US elections), in which voters choose only one candidate for each office and the winner is the candidate who receives the most votes.

Candidate Information

The candidates running for statewide office are, in alphabetical order:

Harwell, John; *Republican*.
Marsh, Susanne; *Independent*.
Ruiz, Michael; *Independent*.
Staley, Linda; *Democrat*.

You may select any number of candidates to learn more about their background, policy positions, and endorsements. Alternatively, if you wish to proceed to the

next section without learning more about any of the candidates, please select none of the options below.

Which candidates would you like to learn more about? (Select any number from the list below)

- [ ] Harwell, John; Republican
- [ ] Marsh, Susanne; Independent
- [ ] Ruiz, Michael; Independent
- [ ] Staley, Linda; Democrat

John Harwell, *businessman*

Party: Republican

Biography: Jon Harwell is of white Irish descent. After obtaining his law degree, Harwell founded a successful venture capital firm that has grown into one of the largest in the state. Harwell has previously sought election to the U.S. House of Representatives, but was unsuccessful in securing the Republican party's nomination.

Key Positions:

- Harwell supports lowering taxes on all businesses to encourage investment.

- Harwell supports an increase in funding for law enforcement, combined with stricter punishments for criminals.
- In the past, Harwell had publicly stated that he was pro-choice, but has since ammended his position, stating that he supports efforts to institute a six week abortion ban in the state.
- Harwell is in favor of increasing infrastructure spending in order to motivate economic growth.

Key Endorsements: Harwell has been endorsed by the National Rifle Association, the U.S. Chamber of Commerce, and former President Donald J. Trump.

Susanne Marsh, *activist*

Party: Independent

Biography: Marsh is a retired insurance agent. Since retirement, Marsh has engaged with multiple activist groups that support limiting the size of government and expanding personal freedoms. Marsh often cites her Eastern European roots as motivating her advocacy,

claiming her ancestors suffered the extremes of state overreach.

Key Positions:

- Marsh is strongly against any increase in taxation or regulation.
- Marsh is a strong believer in private healthcare over public funding for health coverage.
- Marsh has been ambivalent about abortion rights for most of her campaign. When pressed, she offered tentative support for allowing individuals to make their own moral judgements.
- Marsh has previously advocated for decriminalizing possession and use of all substances. She has amended that statement in her current campaign, stating that she only supports decriminalizing marijuana.

Key Endorsements: Marsh has been endorsed by ten state representatives, the Marijuana Policy Project, and former candidate for U.S. President. Jo Jorgensen.

Michael Ruiz, *state Treasurer*

Party: Independent

Biography: Michael Ruiz is the son of first-generation American immigrants from El Salvador. Ruiz attended the U.S. Air Force Academy, and is a veteran of the air force. Ruiz worked as an accountant before successfully running for state Treasurer as an independent candidate.

Key Positions:

- Ruiz is only accepting donations under $200.
- Ruiz has emphasized the need for fiscal stability. He has criticized both parties for engaging in meaningless spending that hurts the state's ability to respond to crises, when those arise.
- Ruiz is pro-life, citing religious reasons for his position.
- Ruiz has proposed a plan to expand integration and job security for veterans.

Key Endorsements: Ruiz is endorsed by a major local newspaper, several state legislators, and two former state Treasurers.

Linda Staley, *state Senator*

Party: Democrat

Biography: After graduating college, Staley served as the first African American mayor of her home town for a term and was then elected to the State Senate. She has served as chair of the Ways and Means committee.

Key Positions:

- Staley supports the creation of a public option for healthcare, available to all. She does not support Medicare for All.
- Staley has pointed to a wide range of bipartisan bills she has cosigned as evidence of her willingness to compromise.
- Staley has put forward a plan to expand housing access, which includes a tax cut on new development projects to expand housing options.
- Staley is pro-choice, and has pledged to strongly oppose any limitation on abortion rights.

Key Endorsements: Staley has been endorsed by EMILY's List, Planned Parenthood, and current President Joe Biden.

Pre-test information

In the following section you will be asked a set of questions about the candidates. Please answer to the best of your recollection. If you do not remember the answer, please respond "Not sure".

Candidate Questions: Republican

Has John Harwell, Republican, previously run for office?

- Yes, and failed to get the Republican nomination for the U.S. House of Representatives
- Yes, and was elected Governor
- Yes, and was the first Democrat mayor of his hometown
- No
- Not sure

Does John Harwell, Republican, support increases in government spending?

- No
- Yes, but exclusively for promoting housing development
- Yes, in most cases
- Yes, but only for law enforcement and infrastructure expansion
- Not sure

On which of the following issues has John Harwell, Republican, changed his position?

- Support of former President Barack Obama
- Abortion
- Restricting immigration
- Medicare for All
- Not sure

Which of the following policy positions has John Harwell, Republican, expressed support for?

- Stricter voter ID requirements
- Stricter punishment for criminals
- Expanding veterans' benefits
- A public healthcare option
- Not sure

Candidate Questions: Independent 1

Which of the following statements most accurately describes the prior experience of Susanne Marsh, Independent?

- Marsh is a former member of the Republican Party
- Marsh is a former state legislator
- Marsh is a retired insurance agent who has become an activist
- Marsh is the current lieutenant Governor
- Not sure

Which of the following best reflects the position of Susane Marsh, Independent, on the issue of abortion?

- Strongly pro-choice
- Tentatively pro-choice
- Tentatively pro-life
- Strongly pro-life
- Not sure

Which of the following policies does the campaign of Susanne Marsh, Independent, support?

- Dissolving the Drug Enforcement Administration (DEA)

- Decriminalizing all drugs
- Higher sentences for drug-related offenses
- Legalizing the use and possession of marijuana
- Not sure

Which of the following has endorsed Susanne Marsh, Independent?

- Former candidate for U.S. President Jo Jorgensen
- Senator Rand Paul
- Former U.S. President Bill Clinton
- The National Rifle Association (NRA)
- Not sure

Candidate Questions: Independent 2

Which of the following statements best describes the political experience of Michael Ruiz, Independent?

- Current State Treasurer
- Former Democrat candidate for Governor
- Current State Attorney General
- No prior political experience
- Not sure

Which of the following statements is true about Michael Ruiz, Independent?

- Ruiz is in favor of stricter border security
- Ruiz is a former Republican
- Ruiz has been endorsed by former President Donald J. Trump
- Ruiz has been endorsed by former statewide elected officials
- Not sure

Which of the following is a policy position expressed by the campaign of Michael Ruiz, Independent?

- Cutting taxes for businesses
- Providing expanded access to jobs for veterans
- Legalizing marijuana
- Instituting a national gas tax
- Not sure

Which of the following best reflects the positions of Michael Ruiz, Independent, in terms of managing the budget?

- The government should focus funds exclusively on security, at the expense of welfare

- Fiscal stability is a necessity that allows the state to respond to crises more effectively
- The government should spend as little as possible
- High spending is necessary to improve economic output
- Not sure

Candidate Questions: Democrat

As a State Senator, Linda Staley, Democrat, has been part of which committee?

- Education
- Agriculture
- Ways and Means
- Armed Services
- Not sure

To the best of your knowledge, does Linda Staley, Democrat, support tax cuts?

- Yes, as a tool to expand construction of affordable housing
- Yes, but only if the government can afford them
- Yes, but only for high income earners
- No
- Not sure

Which of the following does Linda Staley, Democrat, fully support?

- A public healthcare option
- A Green New Deal
- Stricter punishment for criminals
- Tougher border security
- Not sure

Which of the following has endorsed Linda Staley, Democrat?

- House Majority Leader Nancy Pelosi
- The American Conservative Union (ACU)
- The Green Party
- Planned Parenthood
- Not sure

Rank

Please rank as many or as few candidates as you want in order of preference, with "First Preference" being your most

preferred candidate. If you wish to abstain, you may proceed to the next section.

	First Preference	Second Preference	Third Preference	Fourth Preference
Harwell, John; Republican	☐	☐	☐	☐
Marsh, Susanne; Independent	☐	☐	☐	☐
Ruiz, Michael; Independent	☐	☐	☐	☐
Staley, Linda; Democrat	☐	☐	☐	☐

Vote

Please vote for your preferred candidate. If you wish to abstain, you may proceed to the next section.

- ○ Harwell, John; Republican
- ○ Marsh, Susanne; Independent
- ○ Ruiz, Michael; Independent
- ○ Staley, Linda; Democrat