Voter Information Search and Ranked Choice Voting

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Abstract: Does ranked choice voting (RCV) change the information search behavior of voters? 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, and a follow-up survey with bonus incentives to add to the robustness of the results. 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. The results are particularly important in the context of the rapid expansion of RCV.

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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. 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 that 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(Burden et al., 2014; Berinsky, 2005).

The empirical evidence for RCV has been tentatively positive, with authors mostly confirming its positive effects on campaign civility (Donovan, Tolbert, and Gracey, 2016) 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. 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 set. In addition, given the importance of partisanship as a

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

heuristic, 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 and bipartisanship, given the increase in cross-partisan information and the aforementioned results regarding campaign civility.

I first test this theory using a survey experiment on a nationally diverse sample of 1488 respondents recruited using Lucid Theorem[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 spend more time reviewing information about the candidates, search beyond their party, and be 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 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 show clear patterns of age, education, and party effects. Assuming pathologic inattentiveness, these results would be much closer to random.

[2]The primary 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.

In order to increase confidence in the null findings, I run a follow-up survey on a sample of 1186 registered US voters on Amazon.com's Mechanical Turk (MTurk), which includes bonus incentives for responding correctly to the informational section, as well as an expanded slate of demographic and ideological questions. The null effects hold across all hypotheses, even after respondents are provided with specific financial incentives to learn about the candidates. The follow-up survey also shows a clear improvement in median response time and a very low rate of abstention. However, there are still some concerns; MTurk's sample is not nationally representative, so while variation in age and education still exists, it is too limited to improve on the conclusions of the initial survey experiment. Additionally, MTurk respondents do not improve on the correct response rate in the informational section, despite the incentive structure. While some issues do persist, the conclusion from the combination of the two surveys should be that, even after direct incentives, voters are particularly hesitant to change their information search behaviors in response to a shift to RCV.

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

[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.

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.

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

In theory, 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. 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

[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)

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, New York, and most recently Portland, OR, starting in 2024. 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 also conduct all elections using RCV starting in 2022. At the state level, Maine has been conducting most national elections using RCV since 2018, while Alaska has approved and implemented RCV for all general elections 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 less 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 a strong causal case, 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 benefits on candidate entry is mixed, with research focusing primarily on local elections.

While some research suggests that voter satisfaction is enhanced by the ordinal and pref-

erential features of RCV (Farrell and Mcallister, 2006), several empirical works 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)[7]. The same generational gap is consistently present in support for RCV reforms, even within groups with lower support for RCV overall, such as Republicans (McCarthy and Santucci, 2021). Another concern is 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 (D. 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. There are also more positive signs for RCV, especially in areas with long-standing implementation, where strong majorities surveyed in exit polls express their support for its continued usage [8].

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, the benefits of a broadening field of candidates would only affect voters if they 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". While I make no causal claim to this effect, if voters do not adapt their information search to incorporate additional candidates it is not unlikely that this will have an upstream effect on candidate entry decisions, meaning that benefits from a broadening field may not materialize to begin with. Similarly, the empirical evidence that voters tend to under-fill their ballots, leading to exhaustion (Burnett and Kogan, 2014), is highly suggestive that voters either lack an understanding of RCV or do not care to inform themselves of the alternative candidates; this same fact could also lead to a persistence of strategic voting, if voters treat the informational environment presented by RCV as simply the same as under the previous system. Therefore, the promise of RCV is locked behind an assumption about voters increasing, or at

[7]It should be noted here that ballot spoilage is not exclusive to RCV, and has broadly been shown to be affected by ballot design and differences in voting machine technology(D. C. Kimball and Kropf, 2005; Pachón, Carroll, and Barragán, 2017; Carman, Mitchell, and Johns, 2008). For RCV, this means that graphic design choices, the positioning of the candidate or abbreviation of instructions can likely affect spoilage rates, although this is not necessarily relevant to the experiment at hand, where all respondents are presented with the same ballot.

[8]Several of these surveys and exit polls are listed by FairVote, an election reform advocacy group, on their website at fairvote.org/resources/data-on-rcv/

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, 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, 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[9], the party heuristic can, at most, dictate the top and bottom candidates without needing more information on the independent candidates in between.

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

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[10][11], or expand their effort in information acquisition. Therefore, if voters are to provide incentives for wider candidate entry and better fit their interests to the whole field of candidates, 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 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.

[10]Under-voting occurs when voters fill in fewer ranks than are necessary to avoid ballot exhaustion.

[11]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.

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. 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). This leads to the following hypotheses:

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

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

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 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.

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[12] 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, acial characteristics, partisanship, and education, according to Lucid Theorem's audience targets[13]; 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.

After consent and attention checks, respondents answer a brief pre-survey questionnaire[14] 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

[12]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.

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

[14]The full survey is available in Appendix.

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

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[15]. 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.

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

The biographical summary focuses on prior work and political experience, but also gives racial 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; random allocation of respondents should still ensure that results are driven by the treatment, and not systematic reactions to candidate attributes. 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 particularly for the independents who are coded as libertarian and fiscally conservative moderate, were loosely 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. In this section respondents perform particularly poorly, with the correct response rate being only just as good as random[16]. 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. [17] Additionally, respondents seem to be fairly sincere when they did not view the candidate profile;

[16]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.

[17]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; while this does not indicate an improvement in terms of information search and retention, it should indicate that respondents are engaging with the survey in good faith, since they are not "guessing" but legitimately responding they are not sure of the correct response in case where they cannot know, since they did not click on the relevant profile to receive the information in the first place.

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, who may have simply checked top line partisanship and read quickly through one or two policy positions, 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 [18]. The vast majority (around 73 percent) either voted for their copartisan candidate or ranked their copartisan candidate as their first preference, and only 7 percent of partisans voted for the opposing party, which provides an indication that the results are not random, but inputted in good faith by the respondents[19]. Additionally, 69 percent of the respondents in the treatment group filled out a full ballot, with four ranked choices[20]. 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

[18]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.

[19]Based on the 2021 CCES (Schaffner et al. 2021) around 91 percent of voters voted for their party's Presidential candidate, and around 7 percent voted for their partisan opponent. While the first of these numbers is somewhat lower than the percentage in the survey, the second matches the CCES national sample. In addition, the level at which respondents are voting for their co-partisans should still indicate good faith survey completion, even if it does not indicate an exact match with the real world preferences of voters.

[20]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.

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 Rasch model I implement works by estimating the 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[21]. 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.

The hypotheses are tested using difference-in-means tests, and OLS regression. For the model of abstention, a linear probability model is preferred. While I include results from the

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

OLS models in my discussion, it should be noted that the difference-in-means test should suffice to convince readers of the experimental outcomes, under the assumption that balance has been achieved between groups (Mutz and Pemantle, 2015; Mutz, Pemantle, and Pham, 2019). A first set of control variables is supplied by Lucid, and includes age, education[22], 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. 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: 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

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 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 Figure 1, which depicts 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

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

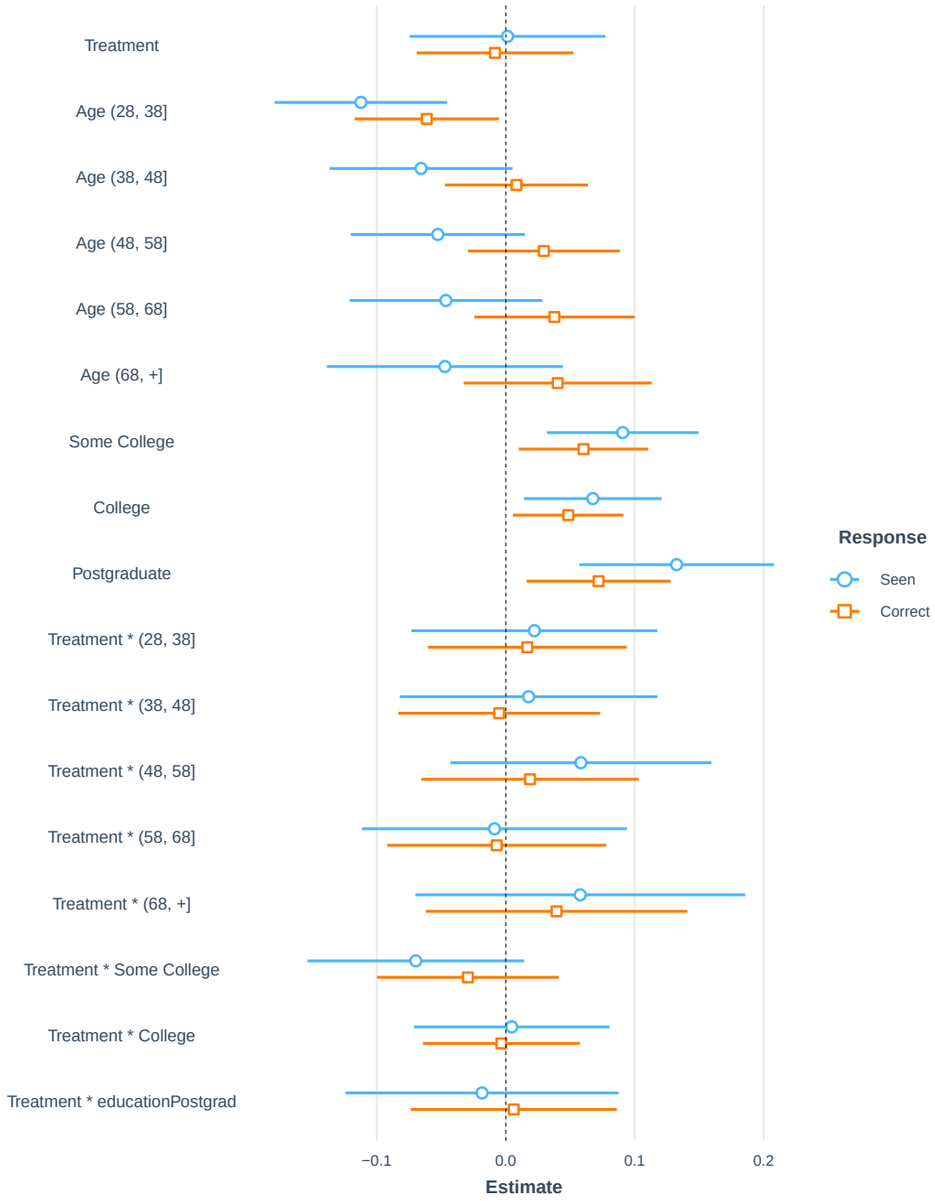


Figure 1: Test of H1: Treatment effect, education, and age.

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[23].

[23]I should note that there are some statistical power concerns with the age and education interaction effects, given that each age bin contains approximately 250 observations, while each education category approximately 375. While this is not optimal, the data should still allow for enough statistical power for a one-sided difference in proportions test to identify an approximately one correct response difference in the treated group. I present the distributional characteristics of the variables in Appendix. In addition, the same regressions with age as a continuous variable replicates the null effect; this is omitted for parsimony.

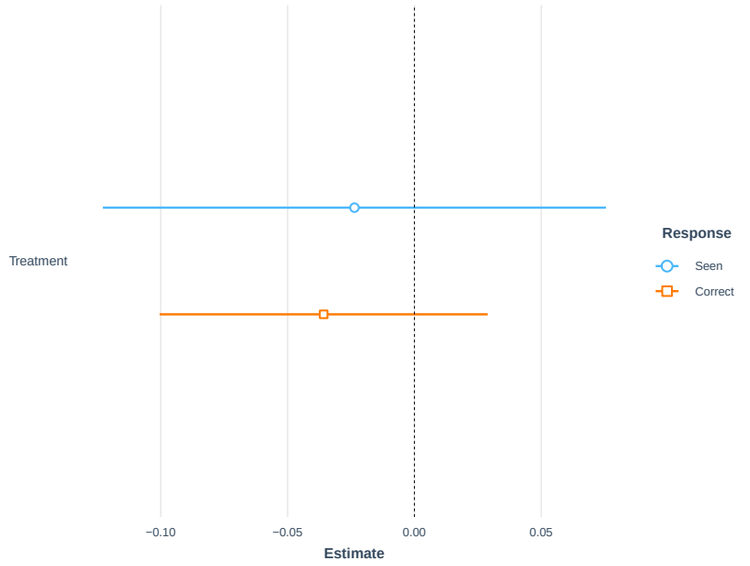


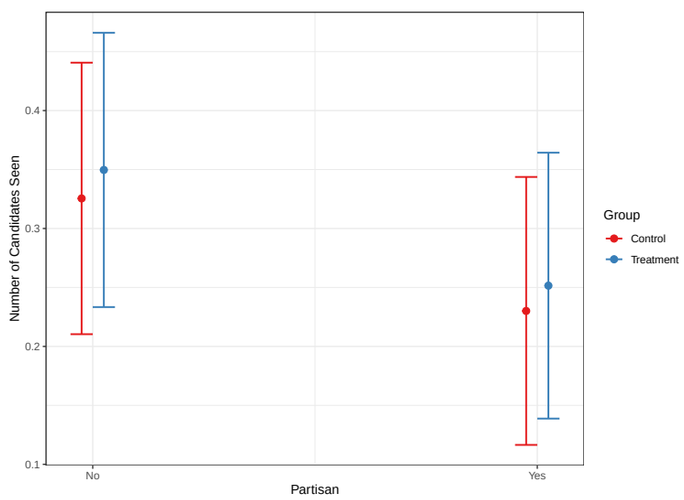
Figure 2: Test of H1a: Treatment effect for non-copartisan response.

When the response variable is restricted to non-copartisan candidates, education and age effects remain roughly similar, although there is still no difference between control and treatment means. 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 benefits of RCV that rely on an expanded candidate selection pool would most likely not materialize.

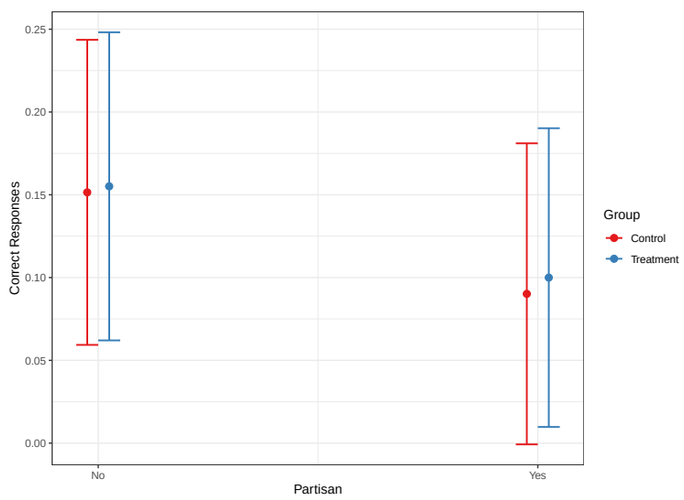
The same theoretical mechanism is further tested in H1b. A variable indicating partisan status is added to the regression and interacted with the treatment. The marginal effects plots in Figure 3 reveal no evidence of a heterogeneous treatment effect of partisan status. However, they do reveal that independent respondents both seek and retain more information than their Democrat or Republican counterparts[24]. 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.

[24]Full regression tables 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

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 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 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[25].

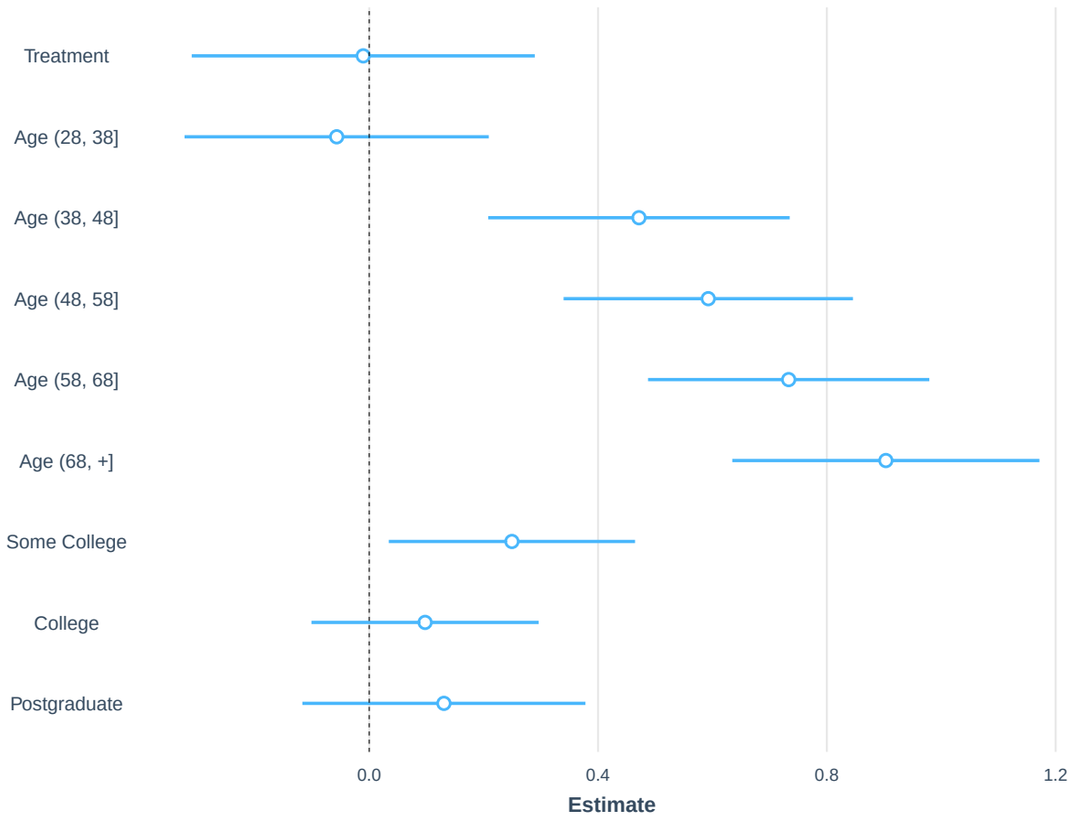


Figure 4: Test of H2: Time spent searching.

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 however does not hold up after the inclusion of interaction effects, which is depicted in Figure 5. The negative effect here does admittedly run somewhat counter

[25]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.

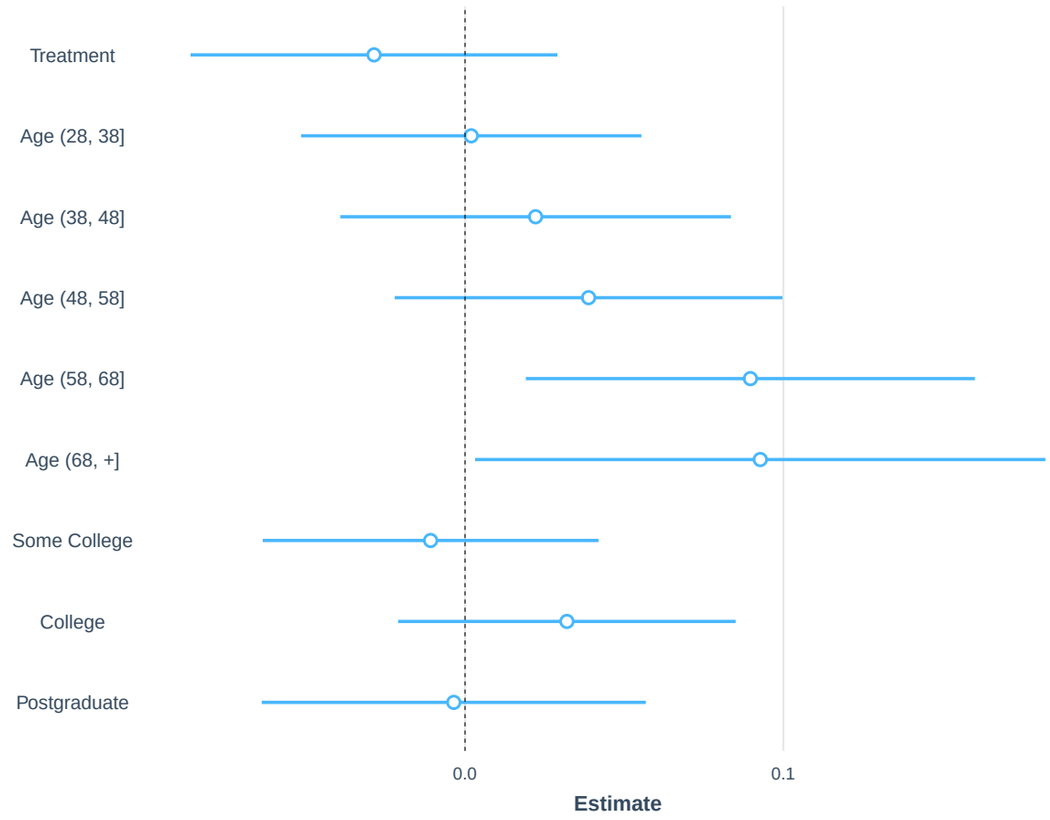


Figure 5: Test of H2a: Probability of abstention.

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.

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.

Follow-up Survey

A weakness of the above survey is that it creates a respondent financial incentive structure that is focused on attentive completion of the overall task, but relies on respondents buying into the task of informing themselves on the candidates exclusively through prompts emphasizing the importance of an informed vote. This could potentially have been the reason behind the relatively low correct response rate exhibited by respondents. In addition, the low median response time of approximately four minutes raises some concerns about attentiveness, even after respondents pass attention checks. Lastly, Lucid Theorem does not explicitly allow for sampling only registered or active voters, who are arguably the key demographic for detecting an effect of RCV. In order to address these concerns, I conduct a follow up survey of 1186 registered U.S. voters on Amazon.com's Mechanical Turk (MTurk), recruited in December 2022. Contrary to the Lucid Theorem sample, the MTurk sample is *not* nationally diverse, and skews quite heavily towards white, educated, young Democrats. Given the skew and the sample size limitations, I do not use the follow up sample to further test for education and age heterogeneous treatment effects[26].

The survey design is very similar to the original, with a few changes. First, and most importantly, respondents are paid $0.75 for survey completion, with an available bonus of $0.10 per correct response in a selected set of questions, reaching a total bonus of $1.40 and total possible payout of $2.15. This change shifts respondents' monetary incentives from adequate survey completion to attentive participation in the information acquisition and test section. Second, I shift from a randomly selected set of questions to the same set of twelve for each respondent, with three questions per candidate; I also explicitly identify the two independents as Green and Libertarian party candidates, with respective shifts in platform and prior experience. Third, I add a set of policy and ideology questions on free market ideals, climate change, and conservative versus liberal identification in order to further secure sample balance; I also add an incentivized question on RCV in order to motivate respondents to pay attention to the treatment prompt. Fourth, I add measures to ensure the quality of the sample, including a Captcha test and further attention checks. These measures led to filtering of respondents from an initial pool of 1478 to the final 1186[27].

The median response time for the follow up survey was eight minutes, substantially higher than the original Lucid Theorem sample; respondents were also much less likely to abstain, with only 14 abstentions recorded. In addition, trust in the free market and hesitancy to combat climate change positively correlate with respondent vote choice in favor of either

[26]The full regression tables are available in the Appendix; the interaction terms of the treatment with age and education are still not statistically significant.

[27]I have also lightly reworked language in the treatment and control prompts, as well as in the candidate profiles, candidate questions, and consent form in order to work with the new survey platform. These changes are all minor, and should not substantially alter the results, particularly given randomization.

the Libertarian or Republican candidate as their preferred option or first choice rank; the same is true for voters who reported being more strongly conservative and having a stronger Republican identification[28]. Respondents report participating in the 2020 and 2022 national elections at a very high rate of approximately 97%, which is reasonable given their registered voter status. Surprisingly, and despite direct financial incentives that respondents are repeatedly reminded of, the average number of profiles interacted with by the respondents drops to 1.5, with a median of one profile seen; similarly, the correct response rate is also lower, ranging between 11% and 28.5% with an average of 18.5%, after accounting for guesses on unseen profiles. Respondents are also clearly better at responding to questions on the Republican and Democrat candidates, with no average response rate being worse than random, apart from one question on the Democrat.

Additionally, and in contrast with the Lucid Theorem sample, while there is an overall correlation between voting for the copartisan candidate, and party or ideology, the vote share data is somewhat concerning. Only 40% of self-declared Democrats voted for the Democrat candidate as their first option, and 22% of Democrats voted for the Republican; for Republicans, the numbers are 61% and 5.5% respectively. The age and education effects present in the Lucid Theorem sample also do not replicate to MTurk, although this may be related to the lack of variation within those variables; for instance, around 65% of respondents are college educated and in the first two age groups. Overall, the MTurk sample does show correlational structure that indicates enough attentiveness to draw valuable conclusions, and should add to the overall confidence in the null effects of the experiment, especially given their robustness to changes in respondent incentive structure, the increased median response time, and the direct sampling of registered voters.

The treatment and control groups are effectively balanced on all included covariates, as is evidenced in Table 7 in the Appendix. As is clear from Table 3, the null results from the first survey persist to the second sample. Respondents in the treatment group are not more likely to interact with more profiles or retain more information, either on all candidates or on non-copartisans, which serves as evidence against H1 and H1a; there is also no significant interaction effect with partisanship for H1b. Respondents additionally do not spend more time reviewing candidate profiles in the treatment group, which gives further evidence against H2. The follow-up survey unfortunately cannot add information with regards to abstention rates, given the extremely low rate of abstainers (around 1%). Regression tables for these hypotheses are placed in the Appendix for the sake of parsimony.

[28]The same result does not replicate for support of third party candidates in isolation. Free market support is not correlated with support for the libertarian candidate as a first choice option versus all others, and willingness to combat climate change does not correlate with similar support for the Green party.

Table 3: Difference in means tests for the MTurk sample.

Hypotheses	Control Mean	Treatment Mean	Difference	t-Test
H1				
Profiles Seen	1.516	1.519	0.003	0.961
Correct Responses	2.203	2.143	−0.060	0.703
H1a				
Non-Copartisan Seen	0.837	0.834	−0.004	0.947
Non-Copartisan Correct	1.316	1.276	−0.041	0.733
H2				
Logged Time	3.636	3.633	−0.003	0.964

Discussion

The results 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 information search habits unless they are institutionally forced to do so, as they are in non-partisan elections (Lupia, 1994), or when 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 partisanship, 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 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.

The hesitant change explanation also indicates a limitation of this study. While I am confident in the result that voters do not adapt their behavior based on the RCV treatment present here, this does not account for repetition of RCV elections, nor does it discount the probability that an intense, short term informational campaign from elections officials could make voters change their behaviors. The direct evidence from the surveys on this point is mixed; while the Lucid Theorem sample shows a consistent negative effect of prior RCV ex-

perience on information search and retention (see Appendix A), the MTurk sample shows the exact opposite: a strong, positive effect of prior experience on the examined outcomes (see Appendix B). In addition, if the view of voters as cognitive misers, even to the point of their understanding of voting procedures, is to be believed, the speed at which such acclimation may occur is likely to be quite slow; if voters do not expend effort to inform themselves on the candidates, why would they rapidly inform themselves about RCV? The current survey experiments cannot fully address the question of gradual behavioral shift; more research is necessary to elucidate this point. However, the evidence I give does create an imperative to inform voters about RCV itself in a way that is at least substantially stronger than the treatment presented in the survey experiments, if they are to shift their information search behavior.

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 may want to finish the survey as fast as possible, and not necessarily provide a strong response. There are some further 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 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.

The importance of the findings in the two survey designs is, of course, conditional to some degree on external validity: how closely does the task the respondents are asked to perform map to a real world environment? Admittedly, the stakes for participation in a survey are necessarily lower than the stakes for participating in a real world election. I argue that the design and incentive structure of the above survey adequately captures the behavioral impact

of RCV and, at worst, should bias *in favor* of finding an effect. Beyond the aforementioned discussion of the design of survey materials, the task itself presents voters with a reward for general participation, and a reward for attentiveness in information gathering; these map onto theorized rewards for civic participation in an election, plus individual rewards for attentive participation which come from the selected candidate, either from some form of identity expression or policy match. Beyond just being rewarded for participation, respondents are told that inattentiveness may jeopardize their payoff by having their response rejected, and additionally are encouraged through verbal motivation in the Lucid Theorem sample and financial incentives in the MTurk sample. The fact that the reward for attentiveness varies between samples, but the effects of the treatment do not, should serve as evidence towards the robustness of the findings. Similar to real environments, voters also face a cost to being informed; this cost is of almost the exact same nature, and comes in the form of time and effort to seek and retain information.

Another concern beyond the incentive structure of the survey is the form and timing of information. The nature of the information itself is, admittedly, not an exact match for the real world, as voters are also informed through partisan content, social media, and repeated exposure to news sources, rather than a simple summary as provided here. However, the existence of other types of sources should not discount the results of the experiment, unless there is a substantive reason why voters should respond to RCV by *exclusively* seeking out information of a source non-compatible with the given vignettes; journalistic summaries and quick policy position statements exist in real campaigns, and are likely one of the sources voters consult. Another argument could be that the survey does not mimic the type of repeated, almost intuitive exposure to information (Popkin, 1991) that occurs in real environments. While true, this again should not bias the results; if voters are collecting enough information exclusively through repeated exposure, then there is no reason why a treatment effect of RCV would not appear in a well-powered single iteration of such an information search. Repetition should shift the amount of information, or the strength of recollection, but would endanger the null effects in this study only if it also shifts the *nature* of the search. In the same vein, it could be argued that the way information is presented in this survey is actually too direct and accessible to voters, especially for non-copartisan candidates, for which voters are not as informed. This is, again, correct, but if it has any effect should bias in favor of observing a treatment effect of RCV, particularly for the hypotheses on non-copartisan candidates; the survey makes it very easy for respondents to adapt their search behaviors, and yet they still do not.

In addition, it could be argued that the use of simulated candidates makes voters less likely to buy into the required task, and that it may provide them with a differing perception of candidate viability than what would occur in real settings. For the simulated candidates, I contest that this is the only viable alternative for a national survey; even if the candidates were real individuals, there is no option other than the Presidential and Vice-Presidential

election in which all US registered voters would vote on the same set of candidates from different parties, meaning I would have to "transfer" the candidates between states, leading to the same fabrication. Presidential candidates are also not a viable option; voters are likely already significantly more informed about their policy positions, and therefore I would not be testing their information search behavior, but their retention from the previous election. While it would be possible to present voters with, for instance, the candidates in their local U.S. House race, this would incur the same issues with the Presidential race but also add extreme variation in number of candidates, candidate viability, candidate agenda, and required design effort to create tailored questions across all states. An alternative could be a localized design in only a small set of districts, which I leave to future study. Furthermore, while perceptions of viability should differ, such differences would be more likely to bias the results in favor of the increased information hypothesis, rather than against it. If the argument is that respondents may view candidates as more viable in a setting with lower stakes, therefore increasing their attention towards independents and non-copartisans, that would make it more likely that they are more informed, particularly given the fact that the information is more easily available in the survey than it likely is in reality, at least for the independents. This has not materialized in increased information about non-copartisan candidates, meaning that differences in perceptions of viability have likely not biased the results towards null effects.

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 cognitive effort (Basinger and Lavine, 2005), respondents either do not recognize the opportunity offered by RCV or feel that their previous information search behaviors suffice. If the latter is true they are likely incorrect, as is explained by the theory section of this paper. 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 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 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 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 unequivocally 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, policy or ideological representation. 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 the benefits of RCV 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 (Berin-

sky, 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: Lucid Theorem Sample

Table 4: 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 5: Regression table for H2 with all covariates.

	Dependent variable:	
	log(Time)	Abstained
Treatment	−0.010	−0.029
	(0.153)	(0.029)
Age [28, 38)	−0.057	0.002
	(0.136)	(0.027)
Age [38, 48)	0.472***	0.022
	(0.134)	(0.031)
Age [48, 58)	0.593***	0.039
	(0.129)	(0.031)
Age [58, 68)	0.733***	0.090**
	(0.125)	(0.036)
Age [68, +)	0.903***	0.093**
	(0.137)	(0.046)
Some College	0.249**	−0.011
	(0.110)	(0.027)
College	0.098	0.032
	(0.101)	(0.027)
Postgraduate	0.131	−0.004
	(0.126)	(0.031)
Previous RCV Experience	−0.447***	−0.035***
	(0.078)	(0.014)
Registered Voter	0.193**	−0.061**
	(0.092)	(0.025)
Democrat	−0.014	0.029**
	(0.068)	(0.014)
Independent/Other	0.216***	0.059***
	(0.070)	(0.017)
No Hispanic Answer	−0.178	−0.024
	(0.177)	(0.058)
Non-Hispanic	0.184*	−0.030
	(0.110)	(0.025)
Black	−0.243	−0.003
	(0.149)	(0.028)
Indigenous	−0.264	0.092
	(0.387)	(0.123)
Other Ethnicity	0.040	0.046
	(0.166)	(0.036)
Pacific Islander	−0.150	0.126
	(0.549)	(0.193)
White	−0.120	0.012
	(0.127)	(0.024)
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**
	(0.202)	(0.040)
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 6: 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 Tables: MTurk Sample

Table 7: Balance table for treatment versus control groups in MTurk sample. Mean Difference is standardized.

Variable	Type	Standardized Mean Difference	T-Test P-Value
Age	Contin.	−0.075	0.447
Black	Binary	−0.032	0.142
Hispanic	Binary	0.002	0.904
Asian	Binary	−0.002	0.862
Pacific Islander	Binary	0.002	0.766
White	Binary	0.026	0.400
Indigenous	Binary	0.005	0.763
Education: High School	Binary	0.008	0.562
Education: Some College	Binary	−0.002	0.912
Education: College	Binary	−0.018	0.698
Education: Postgraduate	Binary	0.012	0.790
Democrat	Binary	−0.010	0.821
Republican	Binary	0.018	0.699
Independent	Binary	−0.004	0.881
Party: Other	Binary	−0.002	0.813
Party: Not Sure	Binary	−0.002	0.777
Prior RCV Experience	Binary	0.003	0.949
Knowledge	Contin.	0.038	0.788
Voted: 2020	Binary	0.003	0.795
Voted: 2022	Binary	0.005	0.722

Table 8: Regression table for H1 and H2 (MTurk sample)

	Dependent variable:				
	Profiles Seen	Non-Copartisan	Correct Answers	Non-Copartisan	Log(Time)
Treatment	0.115	0.191	−0.039	0.009	0.661
	(0.158)	(0.203)	(0.147)	(0.163)	(0.512)
Age (28,38]	0.010	0.078**	0.026	0.053**	0.113
	(0.028)	(0.036)	(0.025)	(0.026)	(0.153)
Age (38,48]	0.117***	0.194***	0.074**	0.098***	0.270
	(0.035)	(0.045)	(0.032)	(0.034)	(0.179)
Age (48,58]	0.068	0.096	0.081*	0.076	0.657***
	(0.048)	(0.061)	(0.046)	(0.048)	(0.224)
Age (58,68]	0.055	0.045	0.094	0.077	0.622**
	(0.060)	(0.080)	(0.067)	(0.071)	(0.261)
Age (68+]	0.152	0.129	0.194	0.109	0.606
	(0.114)	(0.174)	(0.130)	(0.160)	(0.538)
Some College	−0.027	0.025	−0.064	−0.015	−0.273
	(0.150)	(0.197)	(0.147)	(0.166)	(0.470)
College	−0.151	−0.124	−0.151	−0.116	0.304
	(0.133)	(0.178)	(0.130)	(0.147)	(0.401)
Postgraduate	−0.187	−0.153	−0.198	−0.170	0.061
	(0.134)	(0.180)	(0.130)	(0.148)	(0.408)
No Previous RCV	0.106***	0.103***	0.179***	0.167***	0.855***
	(0.021)	(0.026)	(0.021)	(0.022)	(0.092)
Republican	−0.114***	−0.189***	−0.078***	−0.107***	−0.253***
	(0.017)	(0.021)	(0.014)	(0.015)	(0.088)
Independent	−0.114***	−0.010	−0.107***	−0.078***	−0.205
	(0.027)	(0.035)	(0.021)	(0.024)	(0.141)
Hispanic	−0.088	−0.098	−0.004	0.025	0.407
	(0.068)	(0.086)	(0.057)	(0.058)	(0.454)
Asian	0.017	0.035	0.092	0.032	0.710*
	(0.104)	(0.129)	(0.086)	(0.093)	(0.403)
Pacific Islander	−0.098	−0.053	−0.069	−0.107	−0.247
	(0.229)	(0.188)	(0.132)	(0.076)	(1.023)
White	−0.041	−0.057	0.026	0.018	0.403**
	(0.034)	(0.044)	(0.027)	(0.028)	(0.167)
Indigenous	−0.136***	−0.301***	−0.101**	−0.106**	−0.061
	(0.044)	(0.060)	(0.046)	(0.047)	(0.287)
Treatment:Age (28,38]	−0.028	−0.059	−0.042	−0.048	−0.329
	(0.039)	(0.049)	(0.034)	(0.035)	(0.209)
Treatment:Age (38,48]	−0.125***	−0.173***	−0.105**	−0.119***	−0.428*
	(0.048)	(0.060)	(0.042)	(0.045)	(0.242)
Treatment:Age (48,58]	−0.020	−0.019	−0.058	−0.050	−0.457
	(0.065)	(0.083)	(0.060)	(0.063)	(0.306)
Treatment:Age (58,68]	0.038	0.071	−0.029	−0.003	−0.413
	(0.086)	(0.113)	(0.093)	(0.099)	(0.401)
Treatment:Age (68+]	−0.319***	−0.470***	−0.393***	−0.275*	−1.227**
	(0.117)	(0.177)	(0.132)	(0.161)	(0.560)
Treatment:Some College	0.002	−0.053	0.166	0.117	0.212
	(0.187)	(0.233)	(0.174)	(0.193)	(0.601)
Treatment:College	−0.090	−0.146	0.056	0.012	−0.494
	(0.157)	(0.201)	(0.145)	(0.161)	(0.492)
Treatment:Postgraduate	−0.025	−0.064	0.135	0.100	−0.113
	(0.158)	(0.203)	(0.146)	(0.162)	(0.505)
Constant	0.555***	0.423**	0.274**	0.210	2.717***
	(0.139)	(0.185)	(0.134)	(0.151)	(0.451)
Observations	1,186	1,186	1,186	1,186	1,073
Adjusted R^2	0.098	0.117	0.146	0.133	0.098
Note:					*p<0.1; **p<0.05; ***p<0.01

Table 9: Regression table for H1b (MTurk)

	Dependent variable:	
	Profiles Seen	Correct Responses
Treatment	0.570	−0.035
	(0.561)	(1.470)
Age (28,38]	−0.033	0.148
	(0.128)	(0.336)
Age (38,48]	0.441***	0.831**
	(0.146)	(0.382)
Age (48,58]	0.291	0.992*
	(0.195)	(0.510)
Age (58,68]	0.288	1.256**
	(0.234)	(0.613)
Age (68,+]	0.603	2.295
	(0.661)	(1.731)
Some College	−0.035	−0.623
	(0.475)	(1.246)
College	−0.525	−1.668
	(0.408)	(1.069)
Postgraduate	−0.720*	−2.337**
	(0.417)	(1.092)
No Previous RCV	0.467***	2.237***
	(0.076)	(0.198)
Partisan	0.313*	1.125***
	(0.163)	(0.426)
Hispanic	−0.420	−0.175
	(0.295)	(0.772)
Asian	−0.021	0.933
	(0.347)	(0.910)
Pacific Islander	−0.389	−0.842
	(0.680)	(1.783)
White	−0.182	0.281
	(0.143)	(0.375)
Indigenous	−0.451*	−1.014
	(0.254)	(0.666)
Treatment:Age (28,38]	−0.111	−0.480
	(0.175)	(0.459)
Treatment:Age (38,48]	−0.497**	−1.246**
	(0.204)	(0.534)
Treatment:Age (48,58]	−0.105	−0.735
	(0.267)	(0.701)
Treatment:Age (58,68]	0.111	−0.413
	(0.339)	(0.889)
Treatment:Age (68,+]	−1.046	−4.204
	(1.314)	(3.444)
Treatment:Some College	−0.103	1.769
	(0.629)	(1.647)
Treatment:College	−0.397	0.638
	(0.521)	(1.366)
Treatment:Postgraduate	−0.163	1.530
	(0.533)	(1.397)
Treatment:Partisan	−0.075	−0.433
	(0.233)	(0.611)
Constant	1.700***	1.777
	(0.461)	(1.207)
Observations	1,186	1,186
Adjusted R^2	0.066	0.128
Note:		*p<0.1; **p<0.05; ***p<0.01

C Alternate Independents Specification for H1a.

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

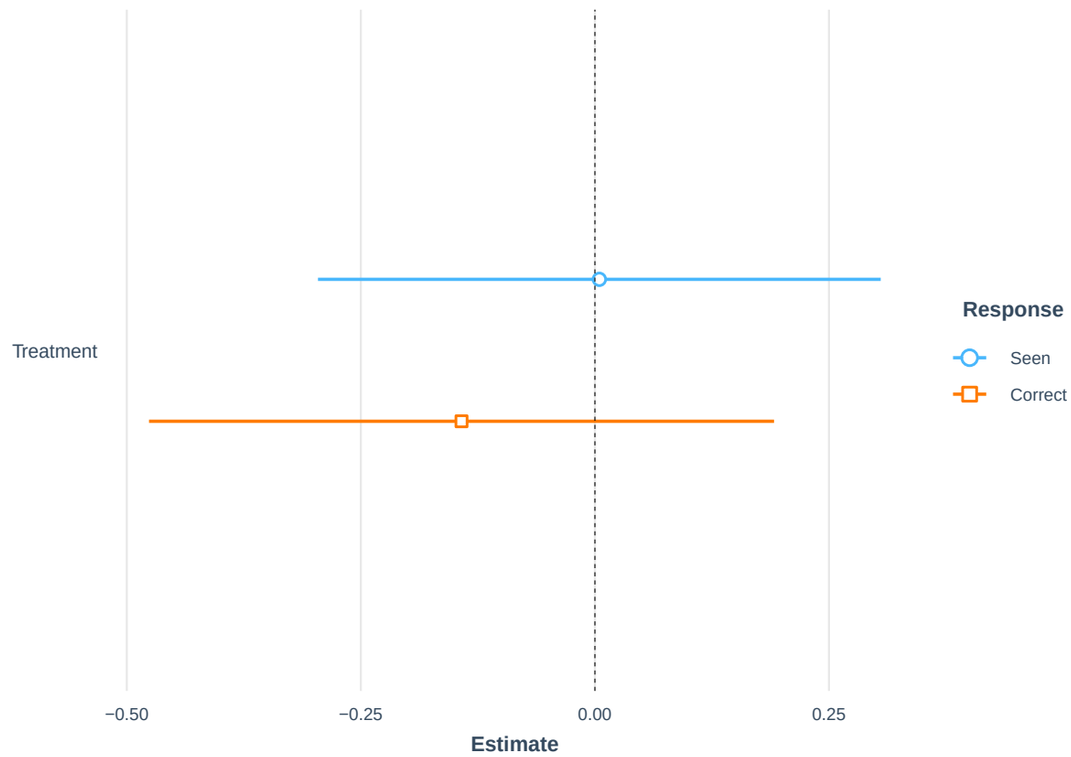


Figure 6: Alternate specification of H1a.

Table 10: 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

D 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 affect 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 Tables 11 and 12 exhibit similar null findings for H1 using the Lucid and MTurk samples; the t-test p-values between scores for Treatment and Control groups were 0.75 and 0.70 respectively.

Table 11: Comparison of IRT score model with correct response model for the first survey.

	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 acial characteristics, voter registration, previous RCV experience and party.		
Note:	*p<0.1; **p<0.05; ***p<0.01	

Table 12: Comparison of IRT score model with correct response model for the second survey.

	Dependent variable:	
	Proportion Correct	IRT Score
Treatment	−0.586	−0.158
	(1.388)	(0.499)
Age (28,38]	0.275	0.027
	(0.333)	(0.120)
Age (38,48]	0.866**	0.264*
	(0.378)	(0.136)
Age (48,58]	0.954*	0.313*
	(0.503)	(0.181)
Age (58,68]	1.107*	0.302
	(0.606)	(0.218)
Age (68+]	2.304	0.838
	(1.712)	(0.615)
Education2	−0.768	−0.188
	(1.233)	(0.443)
Education3	−1.843*	−0.558
	(1.057)	(0.380)
Education4	−2.380**	−0.710*
	(1.080)	(0.388)
Treatment:Age (28,38]	−0.477	−0.171
	(0.452)	(0.163)
Treatment:Age (38,48]	−1.234**	−0.408**
	(0.528)	(0.190)
Treatment:Age (48,58]	−0.682	−0.116
	(0.692)	(0.249)
Treatment:Age (58,68]	−0.334	−0.028
	(0.878)	(0.316)
Treatment:Age (68+]	−4.694	−2.118*
	(3.406)	(1.224)
Treatment:Education2	2.116	0.681
	(1.631)	(0.586)
Treatment:Education3	0.788	0.216
	(1.350)	(0.485)
Treatment:Education4	1.713	0.517
	(1.381)	(0.497)
Constant	3.334***	0.449
	(1.162)	(0.418)
Observations	1,186	1,186
Adjusted R^2	0.147	0.140
Models include controls for acial characteristics, voter registration, previous RCV experience and party.		
Note:	*p<0.1; **p<0.05; ***p<0.01	

E Results by Abstention Status for the Lucid Theorem Survey

Table 13: Results for base models with abstained population excluded (Lucid Sample).

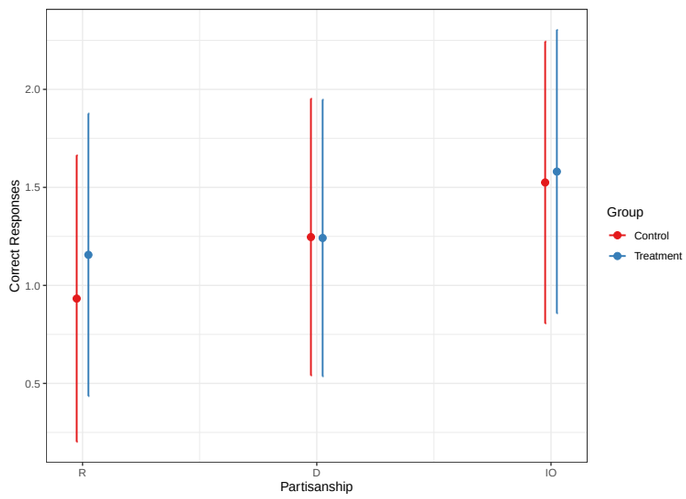
	Dependent variable:		
	Profiles Seen	Correct Responses	Logged Time
Treatment	−0.002	−0.015	−0.016
	(0.040)	(0.032)	(0.157)
Age [28, 38)	−0.088**	−0.051*	−0.038
	(0.035)	(0.030)	(0.139)
Age [38, 48)	−0.050	0.010	0.465***
	(0.037)	(0.029)	(0.139)
Age [48, 58)	−0.032	0.044	0.658***
	(0.035)	(0.031)	(0.133)
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 acial characteristics, voter registration, previous RCV experience and party.			
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 14: Results only for abstainers for the Lucid Theorem Survey.

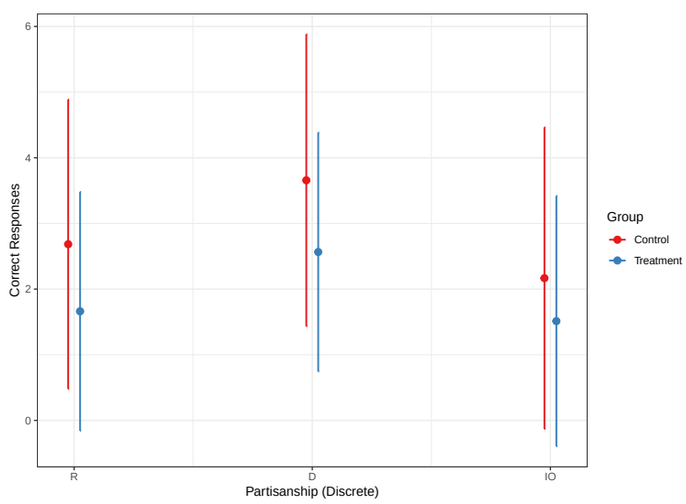
	Dependent variable:		
	Profiles Seen	Correct Responses	Logged Time
Treatment	−0.123	0.005	−0.034
	(0.155)	(0.158)	(0.646)
Age [28, 38)	−0.393***	−0.181*	−0.331
	(0.124)	(0.104)	(0.318)
Age [38, 48)	−0.246	−0.009	0.321
	(0.156)	(0.127)	(0.523)
Age [48, 58)	−0.238*	−0.066	−0.190
	(0.131)	(0.128)	(0.384)
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 acial characteristics, voter registration, previous RCV experience and party.			
Note:			*p<0.1; **p<0.05; ***p<0.01

F Party Effects

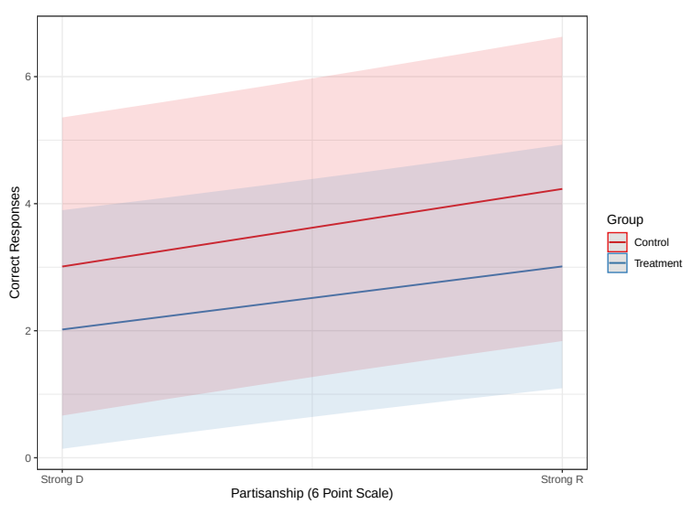
Prior research in Ranked Choice Voting has emphasized partisan differences across a variety of subjects, such as the response of election officials (Anthony et al., 2021) or voter satisfaction (Cerrone and McClintock, 2021; D. Kimball and Anthony, 2021), and is included as an important covariate in almost all relevant survey designs. In this paper, I do not make a claim as to the partisan differences between how Republicans and Democrats change information search behaviors, but rather that partisanship itself is an important factor in affecting information search changes due to RCV. While exploring the effects of party identification or the intensity of party identification were not a pre-registered goal of the current project, in this section I test for potential interaction effects. I run three additional regressions of the correct response rate on all covariates, adding interaction effects with party identification for both survey samples, and intensity of party identification on a six point scale for the MTurk sample in particular. The results are presented in Figure 7. There is not a significant difference in treatment effects between or within party identification. While this is an indication that the hesitancy to alter information search for an RCV environment is consistent between parties, further research is necessary in order to establish potential differences in long-term effects of implementation, especially in the case of a supply-side shift in information.



(a) Effect by party (Lucid)



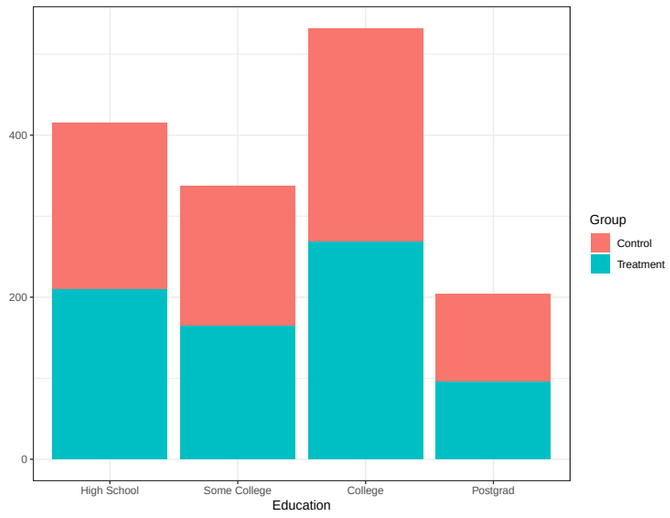
(b) Effect by party (MTurk)



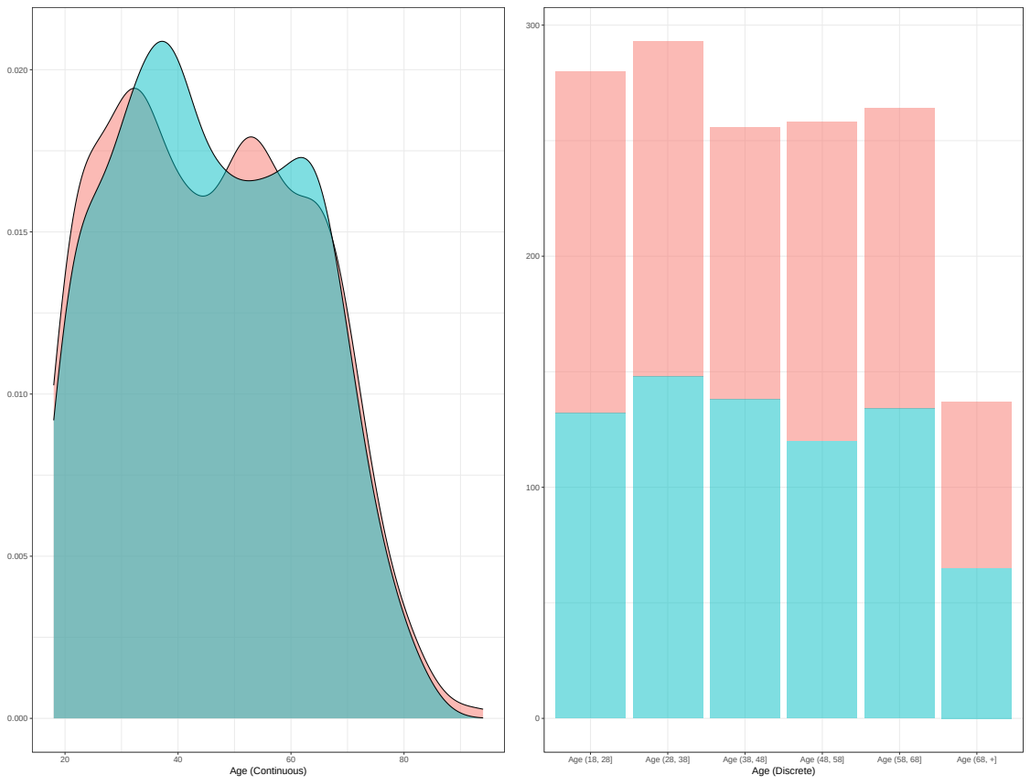
(c) Effect by partisan identification (Lucid)

Figure 7: Party interaction effects with treatment assignment

G Age and Education Distribution



(a) Distribution of the education variable



(b) Distribution of the age variable

Figure 8: Age and Education distributions for the Lucid Theorem sample

H Survey Instruments

The following are the survey instruments applied for the first and second survey.