Information and Ranked

Introduction

The U.S. electoral system is often criticized for producing disproportionate results, incurring high numbers of wasted votes,

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

I test this theory using a survey experiment on a nationally diverse sample of 1488 respondents . 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 St 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 . Consistent with the theory,

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

Research on RCV has broadly focused on the first three of the aforementioned benefits, with little work being done on strategic voting. In terms of Campaign civility, Kropf (2021) uses text analysis of online activity and newspaper articles to show that local election candidates in an RCV setting are more likely to directly engage with each other, and less likely to use negative language than their counterparts in plurality election cities. Donovan et al.

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 other measures of campaign quality, such as the presence of any strnBT-2(ro)-9(m)-130(C)3(l)-10(ar)-9(k)-127(()4(2)-12(01 g0 G[( )] TJETQq0.00000912 0 612 1008 reW

across all candidates based on their most important metric, or exhaustively searching each candidate across the selected variables (Lau, Redlawsk,

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

After the end of the candidate section, voters are given a set of eight questions to assess their acquired knowledge of the candidates. These are selected randomly by sampling two questions from a pool of four, for each candidate. They address endorsements, prior experience, and policy positions. Responses on candidates that the voters did not select to learn about are automatically coded as being incorrect. The questions are set up to assess knowledge of more complex heuristics, based on the increased representation promise of RCV, and the difficulty of decomposition when the party heuristic is absent. In this section respondents perform particularly poorly, with the correct response rate being only just as good as random . While this result is not optimistic for the validity of the survey design, it should be 12 m691 37 7a2 1008 reW* 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.

are coded as *correct* in the cases where the respondent has not seen[( )] TJETsp53f1300001 0 0 1 523.42 649

which provides a strong indication that the results are not random, but inputted

Response

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

	Dependent variable:					
	Profiles Seen	Interactions	Non-copartisans	Correct Responses	Interactions	Non-copartisans
Treatment	0.006	0.001	−0.024	−0.007		

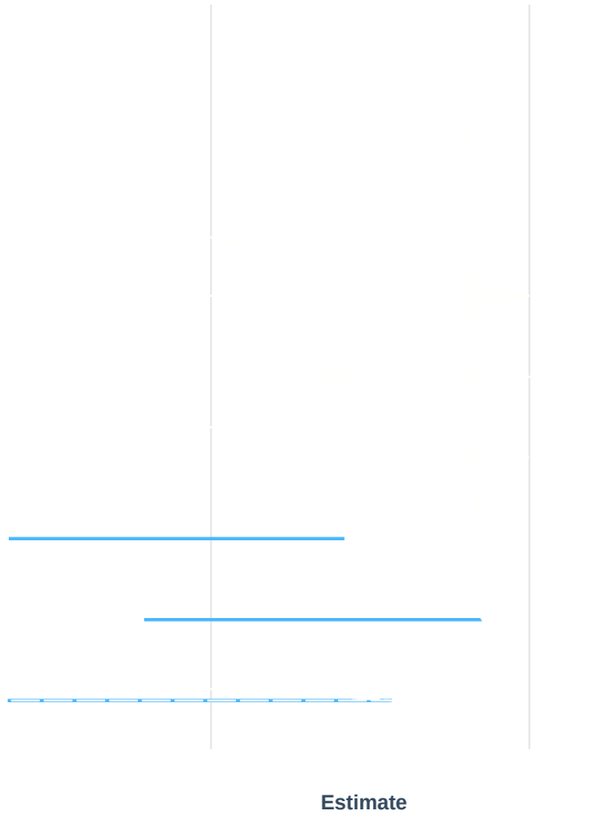

Figure 5: Test of H2a: Probability of abstention.

Figure 6: Test of H2b: Probability of spoilage.

Table 4:

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

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 -issue 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.

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

A last alternative explanation for the reported results may be that the methodology is flawed. This would likely be either the fault of the survey or of the incentive

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

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

Second, I give strong evidence that voters do not adapt to this complex informational environment, and seem to stay committed to their previous patterns of information search with regards to policy, endorsement, and the number of candidates they examine. This presents an empirical contribution to the literature of election reform, since it shows that giving voters more options, choice, or expanding their opportunities for representation does not imply that they will capitalize on that opportunity, consistent with the final conclusion, but not the mechanism, of research on ballot length (Cunow et al., 2021; Cohen, 2018). The findings in this paper suggest that voters may be unable to access the representational benefits that are theorized to follow RCV implementation. If voters do not adapt their information search

incentive for expanded candidate entry, which in turn leads to exactly the same of

This result could also compound issues of fair representation, considering that those who report

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implementation. A natural first step is to examine if the null results produced in this study actually lead to worse, or at least unimproved, representation. It is highly unlikely, but perhaps voters do get enough information in the status quo to accurately express their pref-

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Table 7: Regression table for H1b

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

B Alternate Independents Specification for H1a.

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

Figure 7: Alternate Specification

Table 8: Alternate specification of H1a:

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

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