Accuracy Motivations, Predispositions, and Social Information in Political Discussion Networks *

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Abstract

This analysis studies how variation in individuals' motivation to form accurate judgments affects the process of political discussion. I use a small-group experiment in which participants compete to elect the simulated candidate who best represents their true preferences. I manipulate economic incentives to control participants' accuracy motivations. The results show that accuracy-motivated participants, compared to those with weaker accuracy goals, seek discussants with more expertise and a more diverse set of viewpoints, place greater emphasis on socially-provided messages, and reduced emphasis on political predispositions. As a result of these differences, however, accuracy-motivated participants rely more heavily on biased information. Hence, accuracy motivations do not produce more accurate judgments or better decisions. Although previous work on political discussion has largely ignored the role of motivations, these results suggest that accuracy motivations play an important, but nuanced role in this process. Strengthened accuracy motivations increase individuals' exposure to political expertise and ideological diversity, but also increase their potential to be misled.

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Political discussion serves a vital role in the process of democratic participation. Voters rely on social interaction to gain information (Marcus et al., 2000; Sniderman et al., 1991) and understand the implications of the information they receive (Huckfeldt et al., 1998; McClurg, 2006a,b). Recent studies suggest that the role of political discussion can—at times—be detrimental; voters often accept biased information uncritically (Ryan, 2011) and fail to weight it relative to its informational value (Huckfeldt and Sprague, 1995). A normatively appealing goal, then, is to identify the conditions that promote the efficacy of political discussion. This study seeks that end by drawing insight from psychology, which suggests that the ways that people seek and evaluate information are contingent on the set of motivations they bring to the deliberative task (Kunda, 1990; Lodge and Taber, 2000).

General psychological theories of information processing suggest that information effects depend on both the supply of information *and* people's motivation to acquire and carefully evaluate such information (Chaiken, 1980; Delli Carpini and Keeter, 1996). Yet studies of political discussion have not explored the effects of these motivations. While several motivations may affect information processing, this analysis focuses exclusively on motivations to form accurate perceptions. Dual process models of information processing such as Petty and Cacioppo's (1996) Elaboration Likelihood Model suggest that strong accuracy motivations often encourage deeper, more effortful systematic processing, rather than the more automatic, heuristic processing common when accuracy goals are weak. Indeed, accuracy motivations typically trigger a more comprehensive and balanced search for information, more careful evaluation of information, and more complex decision rules.

Yet accuracy motivations do not eliminate the use of heuristics, nor do they always improve decision making. In fact, Pelham and Neter (1995) show that accuracy goals *increase* the use of faulty heuristics for some complex decision tasks. Likewise, accuracy goals cause people to draw from a broader range of information when forming evaluations,

which in turn encourages people to rely on information of little diagnostic value at the expense of other available information that is more informative (Tetlock and Boettger, 1989). These results appear to extend to political reasoning, as voters often make worse decisions as they rely on broader sets of information (Lau and Redlawsk, 2006). As Kunda (1999) explains, "There are cases in which the harder we think, the more likely we are to resort to faulty reasoning strategies. In such situations accuracy goals can, ironically, increase error and enhance bias."

These disparate results suggest two divergent perspectives about the effect of accuracy motivations in the context of political discussion. In the first perspective, the more systematic and thorough search for information triggered by accuracy motivations will improve political decision making. In the second, accuracy motivations will increase citizens' exposure to heterogeneous viewpoints and thereby decrease their resistance to persuasion (Levitan and Visser, 2009) and increase their potential to be misled (Downs, 1957). While exposure to heterogeneous viewpoints will not be detrimental if people can effectively screen out bad information (e.g., Levitan and Visser, 2008), it will be if it causes voters to rely less heavily on more diagnostic information (e.g., Tetlock and Boettger, 1989).

To test these competing expectations, this study employs a small-group experiment in which participants acquire information and form judgments about computer-generated candidates. The experiment uses economic incentives to experimentally control the strength of participants' accuracy motivations. The results suggest that accuracy-motivated voters, compared to those with weaker accuracy motivations, seek a more diverse set of viewpoints, place greater emphasis on contemporaneous information, and less emphasis on predispositions. In the experiment, this open-minded process of information acquisition and evaluation does not, however, improve the quality of accuracy-motivated participants' decisions relative to the decisions of participants with weaker accuracy goals. Instead,

this process creates a tradeoff in which accuracy-motivated voters ignore informative base rates about candidate positions in favor of socially-communicated information that is less diagnostic, but more contemporaneous.

1 The Role of Motivation in Political Discussion

Political expertise and interest are in short supply (Converse, 1964; Delli Carpini and Keeter, 1996) and distributed unevenly across the population (Djupe and Sokhey, 2013). This uneven distribution creates variation in voters' costs of becoming informed about the candidates, parties, and the issues at stake in an election (Davis et al., 1970; Downs, 1957). High-cost voters can subsidize the costs of becoming informed by seeking information from low-cost associates. Yet delegating to better-informed discussants creates incentive for discussants to mislead in instances where an individual's preferences differ from those of the discussant. For this reason, Downs (1957, 232) suggests that individuals seek discussants who are 1) better informed than themselves and 2) share their preferences—a set of criteria I will refer to as the *Downsian ideal*.

A central concern of research on political discussion has been to understand the relative influence of these two criteria—expertise and shared preferences—in shaping voters' political discussion networks. The limited supply of political expertise in the population often forces voters to choose between these two criteria in their political discussions. The necessity of this tradeoff is further elevated because most social networks are not formed on the basis of political preferences. Rather, political communication networks tend to include the friends and family with whom people often discuss other important matters (Klofstad et al., 2009), thus further limiting the supply of available discussants. When choosing political discussion partners, voters generally place greater emphasis on expertise than shared preferences (Ahn et al., 2010; Huckfeldt, 2001). Consequently, much political discussion occurs between voters who disagree with one another politically and

therefore this discussion fails to achieve the Downsian ideal (Huckfeldt et al., 2004). People may discuss politics differently, however, depending on the motivations they bring to the discussion. Work on political discussion has largely ignored the role that motivation may play in shaping the nature and influence of discussion (for a notable exception, see Huckfeldt and Sprague, 1995, chapter 5). This inattention is problematic because the motivations that drive people to seek and process information affect the ways in which they seek and process this information.

Motivations are the wishes, desires, and preferences an individual associates with the outcome of a deliberative task (Kunda, 1990, 480). Motivations are typically classified in terms of the importance an individual attaches to a "correct" decision (accuracy motivations) and the desirability of reaching a particular conclusion relative to its alternatives (directional motivations) (Lodge and Taber, 2000). The guiding principle for work in motivated reasoning is that all reasoning is motivated and, for political decisions, accuracy motivations tend to be weak while directional motivations are the norm (Taber and Milton Lodge, 2012). Thus, previous work on political discussion has largely been conducted in the presence of directional goals, but we know little about how accuracy motivations may influence discussion.

Although weak accuracy motivations may be the default for most voters, stronger accuracy goals arise in many scenarios. Across a range of decision tasks, people adopt accuracy motivations most frequently when the importance of a correct decision increases (Beach and Mitchell, 1978; McAllister et al., 1979) and when the stakes of a decision are made clear (Kruglanski and Freund, 1983; Tetlock, 1985). Directional motivations also tend to give way to accuracy motivations when people receive sufficient information in conflict with their predispositions (Maheswaran and Chaiken, 1991; Redlawsk et al., 2010). In electoral politics, accuracy motivations should thus be most prevalent in the presence of vigourous voter-mobilization efforts, the emergence of high-stakes issues, and

in times of crisis (Atkeson and Maestas, 2012). People's emotions also govern their motivations, even the relatively weak emotions cued by political ads (Brader, 2006). Anxiety, in particular, triggers a more accuracy-driven search for information (Valentino et al., 2008), which is particulary consequential given the preponderance of attack ads in recent elections.

2 Competing Perspectives on the Effects of Accuracy Motivations

How might variation in these motivations shape the acquisition and evaluation of social information? One perspective suggests that accuracy motivations should improve citizens' political decision-making. A second suggests that accuracy motivations will decrease the quality of citizens' political decisions.

Accuracy motivations may improve people's ability to use political discussion to learn about politics by encouraging them to conduct more thorough searches for information and more open-minded evaluations of information. Lacking accuracy motivations, individuals typically rely on heuristics and a cursory evaluation of readily-available information. In the presence of accuracy goals, people conduct a more thorough search for relevant information and a more careful, less biased evaluation of the information they encounter. (Baumeister and Newman, 1994; Fiske and Taylor, 1991; Tetlock, 1985). Therefore, increases in accuracy motivations should increase the propensity for voters to seek expert discussants as they search for the most diagnostic information available.

When making inferences about an individual, people can draw from individuating information that relates only to the particular individual under scrutiny as well as category-level predispositions—base rates, stereotypes, and beliefs about the behavioral tendencies of members of a particular group. Accuracy motivations often cause people to disregard

category-level predispositions in favor of individuating and contemporaneous information. Interpersonal discussion provides one of the most common means that people receive up-to-date, contemporaneous political information (Katz, 1957; Walsh, 2004). Therefore, people should rely more heavily on social information when they hold strong accuracy goals. This process of rejecting predispositions in favor of individuating information tends to improve the quality of decision making in contexts where predispositions are unavailable or uninformative (Freund et al., 1985; Kruglanski and Freund, 1983). Finally, accuracy motivations encourage people to seek a broader, more balanced, spectrum of information (Lundgren and Prislin, 1998). Thus, accuracy motivated citizens should seek information from discussants with a broader set of political views.

For these reasons, Mercier and Sperber (2011) argue that, when sufficiently motivated to seek the truth, people can evaluate arguments in an unbiased manner. They therefore conclude that discussion can provide a means to improve the quality of judgments. Yet Ryan (2011) shows that it is precisely this unbiased acceptance of arguments that can lead people astray. The results from his experimental analysis suggest that political discussion tends to improve the quality of political decisions only for uninformed independents. Informed partisans, on the other hand, often make worse choices after discussion because they underweight their own knowledge and often ignore the bias of their discussants. These results are consistent with observational work suggesting that once social information is acquired, voters tend to weight information from experts similarly to information from less-knowledgable discussants. For example, Huckfeldt and Sprague (1995, chapter 9) show that the information individuals receive from discussants who they perceive to be politically expert is no more influential in shaping their vote preferences than is information from less expert discussants [175]. It therefore appears that people often readily accept the social information that they receive and thus the utility of political discussion hinges on the content of information conveyed in the discussion *as well as* the quality of

information the discussants already possess.

Thus, accuracy goals may reduce people's decision-making capacity by making them more receptive to social information while decreasing their ability to screen out biased information. If accuracy motivations cause people to seek discussion with people from a broader set of views, they will inevitably encounter more discussion with people of opposing viewpoints. While such discussion may be normatively appealing (Fishkin, 1991), it is at odds with the Downsian ideal because, In Downs' view, discussants with opposing views have greater incentive to mislead than do like-minded discussants. Moreover, people embedded in more politically-heterogeneous networks are less resistant to persuasion (Levitan and Visser, 2009)—at least when the discussant provides strong arguments (Levitan and Visser, 2008). Thus, accuracy motivations may increase the supply of misleading information while also increasing individuals' receptiveness to such information. Likewise, Neuberg and Fiske (1987) demonstrate that accuracy goals decrease reliance on predispositions in favor of individuating information. As Kahneman and Tversky (1973) demonstrate, however, reliance on individuating information over predispositions leads individuals astray in contexts where these predispositions are accurate and relevant. In U.S. elections, party identification and ideology are two predispositions on which voters commonly rely and each conveys meaningful information (Huckfeldt et al., 1999). Thus, accuracy goals may cause voters to ignore such diagnostic information to the detriment of their decision-making capacity.

Ultimately, accuracy motivations may improve the quality of political discussion in some ways while harming it in others. The next section details a research design that can clarify its impact on the various processes underpinning political discussion.

3 Experimental Design

An experimental design provides the best means to identify the effects of accuracy goals on social information processing because it can isolate all information that participants receive and therefore identify the effect of each piece of information. Moreover, by exogenously generating the pool of potential discussants, this experiment can separate instances of persuasion from patterns of homophily, in which individuals associate with others who share similar characteristics. Finally, the experiment provides a valid and unambiguous method to identify "correct" votes in order to objectively measure the quality of decision making in the presence and absence of accuracy goals.

Students from two lower-division political science classes at the University of California, Davis were invited to participate in the experiment in exchange for a small amount of course credit, five dollars, and the opportunity to earn more money, as described below. In the experiment, groups of nine participants use networked computers to play a series of election games in which they are paid when they elect the computer-generated candidate whose position is closest to their own.[1] Participants can purchase private information about the candidates and communicate with each other to learn about the candidates. Half of the groups are also paid for forming accurate judgments of the candidate positions (accuracy treatment) while the other half are not (control group).

Each experimental session lasted about one hour, in which each group played an average of six elections. Each election is a contest between two computer-generated candidates, candidate A and candidate B, who are represented by a general ideological position ranging from zero to six.[2] Participants are instructed that their goal is to elect

[1]The experiment is programmed in z-Tree (Fischbacher, 1999), a software designed specifically for computer-based small-group experiments. Of the 54 participants, 28 were male (52%) and 26 were female. 30 self-identified as non-Hispanic caucasian (56%) and 13 as Asian (24%), with the remaining participants identifying as Hispanic, African American, or "other." 29 participants identified as independents (54%), while 20 identified as Democratic (37%) and five as Republican (9%). Below, I discuss the implications of this Democratic-leaning group for the external validity of the study.

[2]This sample size provides 324 participant-elections (54 participants $\times$ six elections). With these observations, the sample has the statistical power to capture participant-election level Phi coefficients

the candidate whose position is closest to their own. Candidate positions are generated randomly each election, while participant positions are held constant for the entire experimental session. Candidate positions are equal to the total number of conservative stances they take on six substantive policy issues. Participants' positions are determined at the beginning of the experimental session, using a pretest that asks them to identify which binary alternative is closest to their own on the same six policy issues. Table I displays the wording of these six items. Like the candidates, participants' overall positions are equal to the total number of conservative stances taken on these six issues. For the duration of the experimental session, each participant's screen displays his or her overall position in the upper-left corner of the screen.[3]

Participants are never told the candidates' overall positions. Rather, they must seek information to help them estimate these positions. Three types of information are available to participants:

Public information (free). At the start of the experimental session, participants are informed, truthfully, that Candidate A tends to be liberal, with a .45 probability of taking a conservative stance on any given issue, while candidate B tends to be conservative, with a .56 probability of taking a conservative stance on any given issue.[4]

as small as .22 (with $\Pi = 0.8$). Likewise, since each election has two candidates, this sample provides 648 candidate evaluations (54 participants $\times$ six elections $\times$ two candidates per election) and thus the power to capture Phi coefficients at this level as small as .16.

[3]Most studies of voting in the experimental economics tradition randomly assign participants' policy positions on an abstract continuum. This design breaks from that tradition by using participants' real policy views to determine their positions in the study. Most political reasoning occurs with at least moderate directional goals (Lodge and Taber, 2000), but a purely abstract policy dimension is unlikely to provide such motivations. By using substantive policy issues, I am able to randomly assign accuracy motivations in an environment more like the real world where directional goals may be prevalent. I discuss the implications of this choice for the external validity of the study below.

[4] The probabilities are not symmetric in order to emphasize that each candidate's stances are independent of the other candidate's stances. Thus, both candidates may take the same stance on one or more issues in any given election. Given these probabilities, candidate A's expected overall position is 2.7 and candidate B's expected overall position is 3.4. In expectation, candidate A will be more conservative overall than candidate B roughly 25% of the time, candidate B will be more conservative than candidate A about 54% of the time, and the two candidates will be equally conservative the remaining 21% of the

Table I: Item wording for pretest and candidate positions.

Issue	Position	Statement
Abortion	Liberal	I support abortion rights
	Conservative	I oppose abortion rights
Same-sex marriage	Liberal	I support same-sex marriage
	Conservative	I oppose same-sex marriage
Health care reform	Liberal	We should not repeal the Obama health care reform
	Conservative	We should repeal the Obama health care reform
Immigration	Liberal	We should provide a path to citizenship for employed illegal immigrants
	Conservative	We should not provide a path to citizenship for employed illegal immigrants
Environmental regulations	Liberal	We should increase environmental regulations
	Conservative	We should decrease environmental regulations
Gun control	Liberal	We should ban the sale of assault rifles
	Conservative	We should allow the sale of assault rifles

Private information (costly). At the start of each election, participants can purchase up to three pieces of private information. Each piece of private information gives the stance of both candidates on one of the six policy issues, using the same wording listed in Table I.[5]

Social information (free). After purchasing private information, participants make three separate requests for information (at no cost) from any of the other eight participants. This information consists of an integer signal about each of the candidates' overall positions.

Every time participants receive a piece of private or social information, they are asked to estimate the overall position of each candidate. Then, all participants cast their vote for one of the two candidates and the winner of the election is determined by majority rule. Participants are awarded 50 Experimental Currency Units (ECUs), if the candidate whose overall position is closest to their own wins the election and lose 50 ECUs if that candidate loses the election. Participants equally close to both candidates neither gain nor lose ECUs due to the outcome of the election.

Two treatments are experimentally assigned. First, the cost of private information is randomly assigned at the start of each session and held constant across all elections. In any given session, four participants pay 25 ECUs for each piece of private information, three participants pay five, and two participants pay zero. Second, all participants in half of the sessions are informed they will receive an additional payment in each election for forming accurate impressions of the candidate positions. This extra accuracy motivation

time.

[5]For example, a participant may receive the following message, which constitutes one piece of private information:

> Topic of piece of information: Health care. Candidate A believes we should not repeal the Obama health care reform. Candidate B believes we should repeal the Obama health care reform."

Thus, each piece of information provides a signal about both candidates. Participants are never told which stances are conservative and which are liberal.

persists through all elections in the experimental session, paying 15 ECUs for correctly identifying the overall position of either candidate and 30 ECUs for correctly identifying the position of both candidates. The groups in the accuracy treatment receive this incentive in addition to the payouts associated with the winner of each election, while the control groups receive only the electoral payouts. At the conclusion of the experimental session, participants are paid a five dollar show-up fee as well as one dollar for each 100 ECUs they earned over the duration of the experiment. See Appendix A for the text of all instructions given to participants and Appendix B for item wording.

While accuracy goals may typically be less prevalent than directional goals in political reasoning, they are never absent entirely (Lodge and Taber, 2000, 186-187). The incentives in the experiment therefore provide all participants with one accuracy motivation: to elect the candidate closest to his or her position. This motivation is relatively weak, however, because each participant casts only one of nine votes in an election and hence the electoral payoff hinges on the behavior of others. The accuracy treatment contains an additional, though small, accuracy motivation to estimate each candidate's true position. In contrast to the electoral payout, this payout is conditional only on the participant's own estimates.

Thus, each election proceeds as follows:

1. Participants receive 100 ECUs and the two candidates' positions are drawn using their respective probability distributions.

2. Participants decide how many pieces of private information to purchase at their assigned cost

3. Participants receive their private information. Each piece tells each candidate's binary stance on one of the six issues. After each piece of information, participants enter an estimate of each candidate's overall position on the seven-point scale.

4. Participants must request information from one other participant at no cost to the sender or receiver. To facilitate this choice, participants are informed of the other participants' positions on the seven-point scale, how much private information they purchased, and the policy issues on which they received private information about the candidates.

5. Those participants from whom other participants requested information decide whether to accept or reject each request. These discussants are told that there is no penalty for providing information nor for denying information requests.[6] If discussants decide to provide information, they enter an estimate of each candidate's overall position on the seven-point scale. Discussants do not need to provide the same estimates to all requesters.

6. Participants receive the social information provided by their discussant and then are asked to update their estimates of the candidates' overall positions. They are also reminded of their previous estimate for each candidate's overall position, but are provided no other summary of the information they have received.

7. Participants repeat steps 4-6 two more times. Thus, all participants make three requests for social information. A participant may request information from the same discussant on multiple occasions in the same election.

8. Participants cast their vote for one of the two candidates.

9. The winning candidate is revealed to all participants, as is the overall position of each candidate. Participants are told their earnings in ECUs for the election, as well as their total accumulated earnings thus far in the experimental session.

10. A new election begins at step one.

[6]Most participants provided information on request; only 3% of requests were denied.

4 Results

The experimental manipulation may affect participants' selection of discussants, the content of the messages they send, the way they process information, and the quality of their judgments and vote choices. The analysis will explore each potential consequence, beginning with the selection of discussants.

4.1 Discussant Selection

While accuracy goals tend to increase information seeking in many settings, people in the accuracy treatment do not purchase significantly more private information than those in the control (1.92 pieces of information vs. 1.88 pieces; $t_{(df=304)} = 4.4; p = 0.23$). Previous analysis of related experiments suggests that the marginal utility of private information declines with each subsequent piece of new information (Ahn et al., 2010, 771). Therefore, the decision not to purchase additional information may be a profit-maximizing choice. Instead, participants in the accuracy treatment appear to be more strategic and open-minded in their search for free social information. Table II, shows the influence of various criteria on participants' selection of discussion partners.

In this dyadic analysis, each observation represents a pair of participants in a single request for information. The dependent variable equals one if the participant requesting information (ego) requested information from the potential discussant (alter) and zero if the ego chose a different discussant. The multilevel logistic regression allows the intercept to vary across elections (six elections per ego) and requests (three requests per election) to account for clustering at each level. In the table, the model is run separately for participants in the control and accuracy treatments. The final column of the table displays the Wald test p-value testing the null hypothesis that the coefficients in the two treatments are equal ($\hat{\beta}_{Control} = \hat{\beta}_{Accuracy}$).[7]

[7]An alternative specification is to run a single model in which each explanatory variable is inter-

Table II: Multilevel logistic regression of subjects' selection of discussants, by treatment.

	Control Estimate (Std. Error)	Accuracy Estimate (Std. Error)	Wald test p-value
Alter's expertise	**0.64**	**0.94**	0.00
(Amount of private info purchased by alter)	(0.075)	(0.074)	
Ideological distance between ego and alter	**-0.08**	0.04	0.04
(|Ego's position − alter's position|)	(0.050)	(0.032)	
Ego previously requested info from alter	**-3.40**	**-3.75**	0.57
(0 = No; 1 = Yes)	(0.458)	(0.422)	
Number of new topics	-0.02	-0.07	0.60
(Policy topics on which alter received private info, but ego did not)	(0.073)	(0.068)	
Intercept	**-3.09**	**-4.11**	0.00
	(0.189)	(0.240)	
Standard deviation of intercept			
Across elections	0	0	
Across requests	0.18	0.28	
N			
Observations	3672	3672	
Subjects	27	27	
Elections	6	6	
Requests	3	3	
AIC	2490	2290	
Deviance	2476	2276	

Notes: DV equals one if ego chose alter as discussant, zero otherwise. Wald test measures difference of coefficients across treatments. Bolded coefficents are statistically significant at $p < .05$ (one-tail).

Before each request, participants were told the amount of information their potential discussants purchased, their overall position on the seven-point scale, and the issue area of each piece of information they received. The table shows that most participants used the alter's expertise as the primary criteria for selection, where expertise is operationalized as the amount of private information the alter purchased. The strength of this relationship is significantly stronger for participants in the accuracy treatment than for those in the control, as evidenced by the statistically significant Wald test p-value. Thus, accuracy-motivated participants place a higher premium on the expertise of potential discussants than do control participants. These logistic regression coefficients translate to odds ratios of 1.9 in the control and 2.6 in the accuracy treatment. After controlling for other factors, control participants were almost twice as likely to request information from a potential discussant for each additional piece of information the alter purchased. Accuracy treatment participants were about two-and-a-half times more likely to request information from a potential discussant for each additional piece of information the alter purchased.

Accuracy-treatment participants are also typically more willing than those in the control to seek information from discussants with divergent preferences, as shown by the coefficients associated with the ideological distance between ego and alter. Participants in the control tended to weakly prefer like-minded discussants, while accuracy-motivated participants had no such tendency. The Wald test suggests this difference between treatments is statistically significant. The associated odds ratio in the control treatment is .9, corresponding to a 10% decrease in the likelihood of selecting a potential discussant for each unit further the potential discussant's preferences were from the participant's. This weak desire to seek out shared preferences in the control, absent in the accuracy treatment, resulted in a relatively large across-treatment difference in the average distance

acted with a treatment indicator. The conclusions of substantive and statistical significance with this specification are identical to those drawn from Table II.

between participants and their discussants. The average distance on the seven-point scale between participants and their selected discussants was 1.3 units in the control and 2.4 units in the accuracy treatment. Overall, then, accuracy-motivated participants tended to expose themselves to more diverse viewpoints than did participants in the control, who acted consistently with the Downsian ideal in their preference for like-minded discussants.

The models include a control for whether a participant had previously requested information from a potential discussant in the same election. In both models, the coefficient is negative and statistically significant with similar magnitude. This result shows that participants in both treatments rarely returned to the same discussant multiple times in a single election (only about 1% of all requests returned to the same discussant).

One potentially useful strategy participants could employ is to search for discussants whose judgments of the candidate were based on new bits of information: issue topics on which the discussant received private information, but the participant did not. Neither experimental group systematically applied this strategy, however, as participants' propensity to choose a discussant had little association with the number of new topics the potential discussant could provide. There was no significant difference between the experimental groups in this regard. If given more opportunity for discussion, this selection criterion may have had more influence; in this constrained environment, however, it appears discussants' expertise dominated the selection process, particularly in the accuracy treatment.

In summary, compared to control participants, those in the accuracy treatment pursued discussants with more expertise and encountered greater ideological diversity.[8]

[8]As a manipulation check, Appendix C provides a ROC plot of the model predictions from Table II. The plot shows the accuracy payment was effective in encouraging systematic processing and a more careful evaluation of potential discussants.

4.2 Message Bias

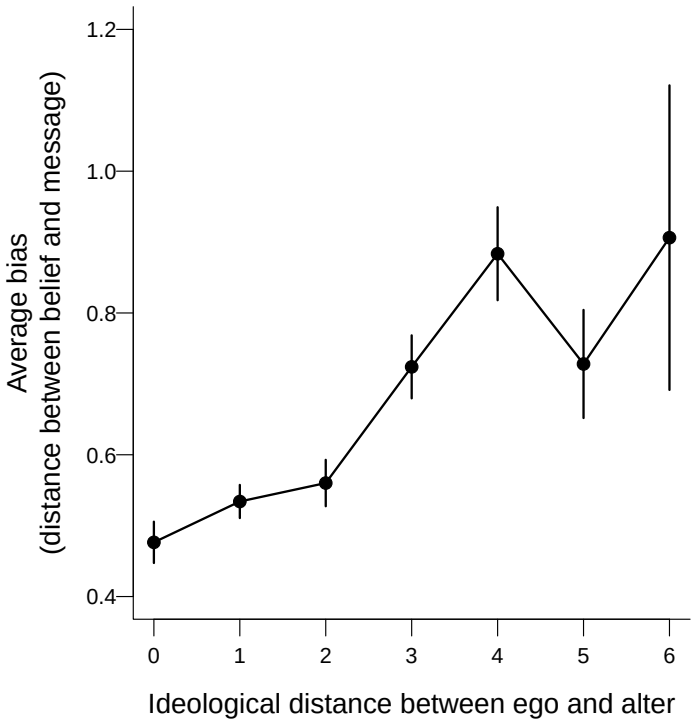
While participants in the accuracy treatment may have improved the quality of information they received by seeking more expert discussants, Downs (1957) suggests they also may have increased their exposure to misinformation by seeking discussants with more divergent viewpoints. Indeed, Figure 1 shows that social information was more biased when coming from discussants with opposing preferences. The Y axis shows the average bias of messages: the average difference between A) a discussants's true belief about the position of a candidate (as measured by their most recent updated judgment) and B) the message they sent about the position of the candidate. The X axis shows the ideological distance between the discussant and the participant requesting information. The figure depicts a weak, positive relationship between the bias of social information and the distance between sender and receiver. As this distance increases, the sender tends to skew social messages away from their recent judgments ($\hat{\beta} = 0.07; t(1774) = 5.2; p < 0.001$).[9] Taking Table II & Figure 1 together, it is apparent that participants in the accuracy treatment exposed themselves to more biased information by seeking information from a more diverse set of viewpoints.

4.3 Evaluating Information

Once participants receive social information, they must evaluate that information and integrate it into their judgments of the candidates. The process of integrating this information may vary by treatment, as accuracy-motivated voters should be more adherent to contemporaneous information and less reliant on the base rates provided by the public information. Table III regresses participants' final judgments of each candidate's position on their prior beliefs about the candidate, the mean social message they received, and an

[9] In analysis not shown here, there is no significant differences in the magnitude of this relationship between the accuracy and control groups.

Figure 1: Bias of messages increases with ideological distance between sender and receiver.



indicator of which candidate's position the respondent is estimating. Each participant has two observations per election: The first observation is the participant's final judgment of Candidate A; The second, Candidate B. To account for this clustering, the models allow the intercept to vary across participants and elections.

Each form of information available to participants—private, social, and public—are included as explanatory variables in this model. The effect of private information can be seen by the coefficients associated with the prior because participants' priors are defined as their judgments about a candidate's position immediately after receiving their last piece of private information and immediately preceding their first piece of social information. The effect of social information is captured by the mean social message a participant received about the candidate in this election. The Candidate B indicator variable captures public information because positive coefficients would indicate that participants tend to judge Candidate B as more conservative than Candidate A, after controlling for the private and social information they received in this election (Recall that participants are told that Candidate B will tend to be more conservative than Candidate A). Huckfeldt et al. (2014) show that political experts tend to rely more heavily on privately acquired information and less heavily on social information than do people with less expertise. The model therefore also includes interactions between each source of information and the amount of private information the participant purchased.

Figure 2 uses the model estimates to compare the relative influence of each information type across treatments. Part A of the figure shows that accuracy-treatment participants and those in the control rely on private information in similar manners. Participants who invested more heavily in private information place greater weight on their priors, though Table III shows this interaction is only statistically significant for participants in the accuracy treatment. Nonetheless, the figure shows that there are no significant between-group differences in the weight that participants place on their priors. For par-

Table III: Multilevel linear regression of participants' final judgments of candidate positions on information received, by treatment.

	Control Estimate (Std. Error)	Accuracy Estimate (Std. Error)	Wald test p-value
Expertise	0.03	**0.32**	.29
(Amount of private info purchased)	(0.222)	(0.158)	
Prior	**0.17**	0.07	.33
(Judgment of candidate position prior to social info)	(0.092)	(0.060)	
Mean social message	**0.71**	**1.12**	.02
(Mean of all social messages received in election)	(0.141)	(0.100)	
Candidate B	**0.45**	-0.22	.03
(0 = No; 1 = Yes)	(0.254)	(0.169)	
Expertise × Prior	0.07	0.10	.52
	(0.042)	(0.027)	
Expertise × Mean social message	-0.08	**-0.21**	.11
	(0.070)	(0.045)	
Expertise × Candidate B	0.04	**0.15**	.44
	(0.121)	(0.077)	
Intercept	0.21	**-0.58**	.19
	(0.481)	(0.361)	
Standard deviation of intercept			
Across subjects	0.18	0.00	
Across elections	0.13	0.03	
Residual	0.79	0.68	
N			
Observations	306	306	
Subjects	27	27	
Elections	6	6	
AIC	783	684	
Deviance	731	626	

Notes: DV is each participant's final estimate of a candidate's position. Wald test measures difference of coefficients across treatments. Bolded coefficients are statistically significant at $p < .05$ (one-tail).

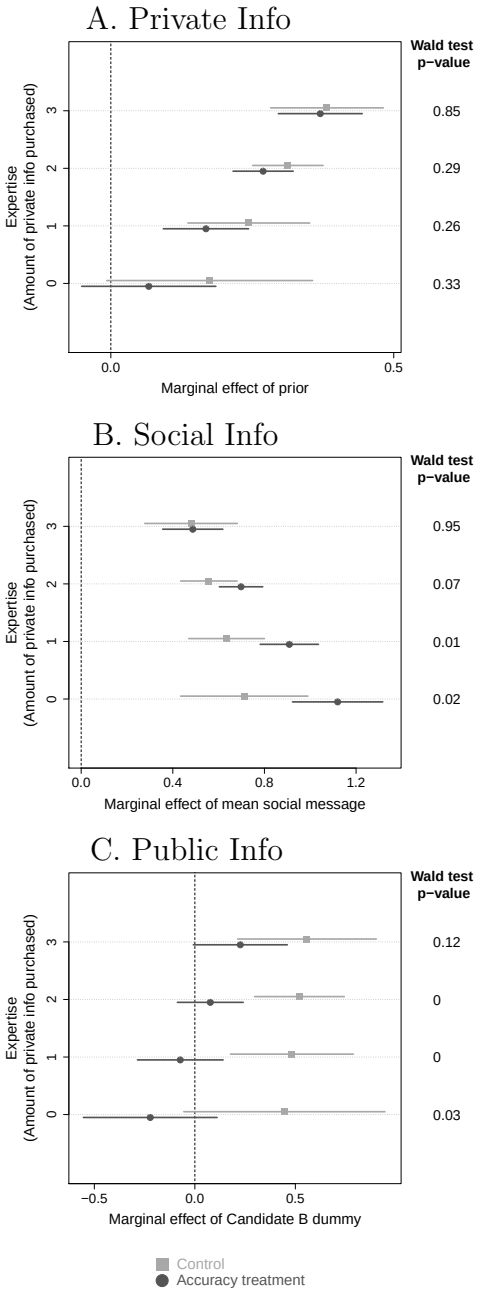
ticipants who purchased no private information, priors had no systematic influence on their final judgements. Among participants who purchased the maximum three pieces of information, a participant's final judgment of a candidate was about a half-unit more conservative for each one-unit increase in the conservativeness of his or her prior.

Larger differences emerge between the treatments when looking at weights placed on social and public information. Part B of the figure shows that accuracy-motivated participants with little private information placed greater emphasis on social information than did similar participants in the control.[10] Accuracy-treatment participants who purchased no information translated their social messages directly to their final judgments; The participant placed the candidates about one unit more conservative for each one-unit increase in the conservativeness of the average social message. The effect in the control was only .7 units more conservative for a unit increase in the conservativeness of the social message. Political expertise, as measured by private information investment, weakened the relationship between social messages and updated judgments. Experts place relatively little weight on social information and there is no statistically significant between-treatment difference in this effect.

Part C of the figure shows that accuracy treatment participants placed significantly less weight on the public information. The public information has no statistically significant effect on the judgments of accuracy-motivated participants, but exerts a noticeable impact on control participants' judgments. The between-treatment difference in these effects is statistically significant for participants purchasing less than the maximum three pieces of private information. Control participants tended to place Candidate B a half-unit more conservative than Candidate A, which is close to the expected .7 unit difference implied by the public information.

[10]These difference are statistically significant for participants who purchased fewer than two pieces of information. Note that overlapping confidence intervals do not indicate a lack of statistical significance, because the magnitude of the *difference* between estimates is often significant in the presence of overlap (Schenker and Gentleman, 2001), as shown by the Wald tests here.

Figure 2: Marginal effect of information on updated judgment.



Note: Lines indicate 95% confidence intervals. Wald test measures difference of point estimates across treatments. Figure based on estimates from Table III.

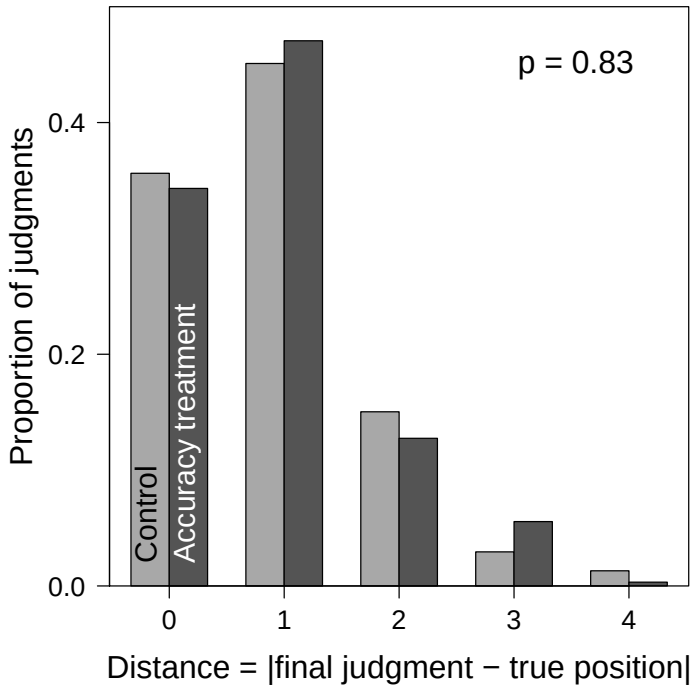
In sum, participants in the control rely heavily on their predispositions, assuming Candidate A was more liberal than Candidate B, while participants in the accuracy treatment tend to ignore this information. In contrast, accuracy-treatment participants tend to rely more heavily on socially communicated, contemporaneous information. While consistent with expectations grounded in social psychology, the results suggest that accuracy motivations may lead voters astray, increasing their reliance on (often biased) social information, at the expense of reliance on informative base rates.

4.4 Judgment Accuracy

Thus far, the results suggest that individuals with greater accuracy motivations discuss politics with more expert individuals from a more diverse range of perspectives. They subsequently weight the social information they acquire more heavily in their judgments than do those with weak accuracy motivations. The benefit of greater expertise in their discussion networks, however, may be offset by the increase in their potential to be misled through discussion with people from opposing viewpoints. Thus the net effect of accuracy goals on political behavior is unclear. Do these patterns of information acquisition and processing lead accuracy-treatment participants to better decisions? In the context of this experiment, the answer is, "no." Figure 3 shows the average distance of participants final judgments of candidate positions from the candidates' true overall positions. The accuracy of judgments in the control treatment was roughly equal to the accuracy of judgments in the accuracy treatment ($\hat{\mu}_{Control} = .89; \hat{\mu}_{Accuracy} = .91$; Wilcoxon rank-sum test$= 46,390; p = 0.83$).

Likewise, participants in the accuracy treatment were no more likely to vote correctly than were participants in the control treatment, as shown in Figure 4. The figure shows that about 77% of votes were cast for the "correct" candidate, where the correct candidate is defined as the candidate whose overall position is most proximate to the par-

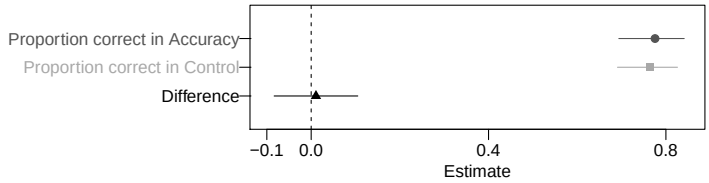
Figure 3: Accuracy of final judgments by treatment.



Note: Figure pools participants' final judgments of each candidates' overall position. The p-value indicates there is no statistically significant difference in judgment accuracy between the control and accuracy treatment (calculated with a Wilcoxon rank-sum test).

ticipant's overall position. Again, the incentive for accurate judgments did not increase (or decrease) significantly the propensity for participants to vote correctly (76.4% voted correctly in the control and 77.5% voted correctly in the accuracy treatment. A difference of proportions test produces a p-value of 0.85). These results suggest that accuracy goals do not necessarily improve the quality of the conclusions participants reach.

Figure 4: Correct voting by treatment.



Note: Correct votes are those cast for the candidate whose overall position is closest to the participant's. Lines indicate 90% confidence intervals. Figure omits participants equally close to both candidates.

While there was no substantive or statistically significant difference between groups in terms of correct voting, the evidence is not so strong to rule out the possibility of a substantively-important effect in similar contexts. Rainey (2014) suggests scholars use 90% confidence intervals to determine if all estimated effects within this range can truly be considered "negligible." As Figure 4 shows, the confidence interval of the between-group difference in correct voting ranges from -.08 to .11. Estimates falling near the extremes of this range would be substantively meaningful, relative to common predictors of correct voting. In a different experimental setting, Lau and Redlawsk (2006) for example, show that political sophisticates are about seven percentage points more likely to vote correctly than political novices (207). The present design thus lacks the statistical power to reject the possibility of substantively important effects. While the present study found no evidence of an effect, future work is therefore needed to test the robustness of this null finding.

More generally, all of the results presented in this study arise in a highly abstract

environment and these processes may play out differently in other settings. Perhaps larger payoffs for accurate assessments would have resulted in larger differences between the groups in this study.[11] Still, this small accuracy payment produced statistically significant and substantively important differences between the groups in terms of the selection of discussants and the relative weight participants placed on various information sources in their candidate evaluations. The null result may also occur due to ceiling effects, as members of the treatment voted correctly at an already high rate—76% of the time. Yet this number is close to the 77% of the electorate who vote correctly, on average, in real-world two-candidate U.S. Presidential elections (Lau and Redlawsk, 2006, 85). Thus, any ceiling effects here may also arise in real-world elections.

This study also simplifies the set of motivations that voters possess to isolate the effects of accuracy motivations. In the experiment, participants within each group share a single set of motivations, but in the real world, people possess a more heterogeneous set of motivations—varying in type and intensity. When a broader mix of motivations compete, the strongest motivations may win out. For instance, Redlawsk (2002) shows that accuracy motivations can override the biased processing that arises from directional goals and thus improve the quality of political decisions. Future work can help us better understand this interplay between competing motivations by simultaneously varying individual's level accuracy and directional incentives.

The results must also be interpreted in the context of the student sample, coupled with the endogenous assignment of preferences. The student sample produced groups of discussants that were more liberal than an average network of political discussants. Still, this sample provides a good initial test of the question because real-world discussion net-

[11] Although a larger incentive for accuracy may have improved the accuracy treatment's performance relative to the control, this larger incentive may have instead caused these participants to perform *worse*. Judgments in the accuracy treatment would be less accurate if this stronger incentive further increased the tendency to seek information from discussants with divergent views. As these results show along with previous work in this vein (e.g., Boudreau, 2009; Lupia and McCubbins, 1998), without an incentive to be truthful, discussants with opposing views will often provide misleading information.

works feature only small amounts of partisan disagreement between discussants (Erison and Erison, 2008). By one estimate, the average difference between survey respondents and their discussants was just over one unit on a seven-point party identification scale (see Sinclair, 2012, Table 4.4, pg. 98). This difference is similar to the 1.3 unit ideological difference between participants and their chosen discussants in the control and less than the 2.4 unit difference in the accuracy treatment (refer back to the Discussant Selection section). Still, future work can expand our understanding of accuracy goals in contexts with greater variation in political preferences.

5 Conclusion

As far back as The American Voter (Campbell et al., 1960), scholars have recognized that voters occasionally forgo their long-term partisan predispositions to focus on more up-to-date information about the particular candidates and issues involved in an election. This work helps explain the mechanisms underpinning that process. Accuracy motivations appear to affect the rules that voters use in their search for social information as well as the processes that govern their evaluation of that information. When citizens seek to reach a sound political decision, the results here suggest that they will pursue more informed discussants with a more diverse set of opinions. They will place greater emphasis on this contemporaneous, socially communicated information at the expense of the weight they attach to political predispositions. Yet the conclusions they reach may be no better than the ones they would reach when accuracy is less prioritized if the social information they receive is more biased.

This work helps clarify the role of accuracy goals in shaping political judgments. In their pioneering work on the role of motivation in the formation political judgments, Lodge, Taber, and colleagues argue that people are inherently biased in their reasoning. When thinking about politics, directional goals are triggered automatically, sub-

consciously, and even when voters become aware of these biases, they are often unable to correct for them (Lodge and Taber, 2005, 2013; Taber and Lodge, 2006). Druckman (2012) argues that Lodge et al.'s results—which focus largely on situations with weak accuracy goals—may not generalize to the real world where accuracy goals may be more prevalent. While more research in different scenarios is warranted, the results presented in this paper suggest that accuracy goals are not always a boon for political judgments, at least in the context of political discussion.

Rather than uniformly promote or degrade the efficacy of political discussion, the experiment suggests accuracy goals alter discussion patterns for both better and worse. People with greater accuracy goals appear to behave more Downsian as they place elevated weight on potential discussants' expertise, but at the same time, they are less Downsian in their propensity to seek discussants of opposing viewpoints (Table II). Of course, from a Downsian perspective, failing to seek information from like-minded discussants negates the benefit of talking to experts; When people discuss politics with others holding opposing views, they increase the potential to receive biased information. This potential is realized in the experiment as participants tend to send increasingly biased messages as their preferences diverge from the receiver (Figure 1). Moreover, because accuracy-motivated participants with relatively little private information place elevated weight on this socially communicated information (Figure 2B), this stream of biased information exerts greater influence on these participants than it would on similar participants in the control. Unfortunately for the participants, this elevated weight on social information comes with a reduced weight on informative base rates (Figure 2C).

These results may illuminate the mechanisms driving information-seeking in the wake of national crises. People are likely to adopt accuracy motivations following natural disasters, foreign conflict, and terrorist attacks (Atkeson and Maestas, 2012). This process may cause people to act against their own preferences as they seek and accept informa-

tion from biased experts. The anxiety following the September 11^{th} attacks may have prompted information seeking, but the dominant frame amongst elites was biased, attributing at least partial responsibility of the attacks to Iraq (Bennett et al., 2008). Since political discussion is often driven by elite frames (Walsh, 2004), accuracy motivations may have encouraged people to both seek this incorrect information and also readily accept this information, which could help explain Democrats' initial, atypical acquiesence toward the Iraq war.[12]

More optimistically, these results imply that accuracy goals may improve the quality of decisions in situations where predispositions and base rates are misleading or inapplicable. In the experiment, the base rates are informative because candidate positions are drawn from these distributions. This fact mimics the polarized nature of modern U.S. politics, but base rates in politics are not always informative. Chris Christie is not a typical Republican; Joe Donnelly is not a typical Democrat. Their policy positions often diverge from those of their party leaders. For such cases, base rates will be misleading and may prevent some voters from choosing the candidate who best represents their interests. Still, to the extent that accuracy goals cause political discussion with opposing viewpoints, they will increase the potential to be misled. Therefore, these results lend credence to the work of Boudreau (2009) and Lupia and McCubbins (1998), who argue that information from a discussant with whom you disagree is not useful unless the discussant is required to tell the truth.

This work also helps understand when cross-cutting political discussion—discussion between people with divergent views—is likely to occur. Much scholarly attention has been spent addressing the effects of such discussion, finding that it promotes political tolerance and the understanding of opposing viewpoints, but also increases ambivalence (Huckfeldt et al., 2004; Mutz, 2002, 2006), and may discourage participation (Mcclurg, 2003; Mutz, 2002; Mutz, 2006; but see also Huckfeldt et al., 2004; McClurg, 2006b; Nir,

[12]I thank Cherie Maestas for noting this implication.

2005; Klofstad et al., 2013). These results suggest that such discussion is likely to occur in the presence of accuracy goals, which are triggered when the stakes of a decision task are increased or made more apparent. In the times when politics intercedes with people's daily lives, such as a government shutdown or in the presence of a vigorous voter mobilization effort, people should be more likely to adopt accuracy goals, thereby encouraging this cross-cutting discussion. Previous observational work also suggests that such discussion increases the likelihood that people will vote against their partisan predispositions (Huckfeldt and Sprague, 1988; Sokhey and McClurg, 2012). The results presented here suggest that this relationship may be partially spurious; accuracy motivations increase cross-cutting political discussion while decreasing reliance on political predispositions.

Many organizations such as the Deliberative Democracy Consortium—which represents over 50 foundations and non-profits—work to promote cross-cutting deliberation in order to foster trust and understanding between citizens and better-informed public opinion. Indeed, work on deliberative polling identifies a number of benefits resulting from these efforts (e.g., Farrar et al., 2010; Luskin et al., 2002). The present study suggests a means to foster real-world discussion amongst people with heterogeneous views, but the emerging picture of this discussion appears at odds with the goals of these organizations. Participants in the experiment did not selflessly help each other maximize the public good, instead biasing the information they sent in order to promote their own interests. Yet Levitan and Visser (2008) provide hope that the consequences of seeking diverse discussion partners may be less dire in the real world. They show that only strong arguments are more persuasive in diverse networks, while the effect of weak arguments is not enhanced. A great deal of research supports the view that people are more resistant to counter-attitudinal arguments as the argument strength decreases (For a review of this work, see Petty and Cacioppo, 1986). In the present study, however, social messages consisted of only numeric signals and, therefore, the argumentative strength did not

vary across messages. Thus, while accuracy goals in the real world may promote greater openness to social information, people may be better able to filter out valueless information than they were here. Yet as recent observational survey work suggests, political discussion often fails to enhance individuals' ability to vote consistently with their own interests (Sokhey and McClurg, 2012), instead promoting the interests of experts in their network (Ryan, 2010). Thus, these results help clarify important mechanisms in contexts where people seek discussion as a means of persuasion rather than enlightenment, or cases where people are unable to open-mindedly evaluate their discussants' views (e.g., Taber and Lodge, 2006; Redlawsk, 2002; Strickland et al., 2011).

A Instructions to participants

At the start of each session, a research assistant read aloud the following instructions to all participants:

Thank you for participating in today's experiment. I will be reading from a script to ensure that every session of this experiment receives the same instructions. These instructions explain the nature of today's experiment as well as how to navigate the computer interface you will be working with. We ask that you please refrain from talking or looking at the monitors of other participants during the experiment. If you have a question or problem, please raise your hand and one of us will come to you. Today's experiment consists of up to 15 time periods in which two candidates compete in an election. [**Control:** Each period awards you money if you elect the candidate whose position is closest to your own.] [**Treatment:**: Each period provides you with two ways to earn money. First, you earn money if you form accurate estimates of the candidates' positions. Second, you earn money if you elect the candidate whose position is closest to your own.]

Your position is based on a short survey you will take before the experiment begins. You will be asked to identify the statement that most closely matches your own stance on six policy issues. Your position in the experiment will be a number between zero and six, which equals the number of conservative stances you took on the issues. Your position will be the same for all periods of the experiment. In this practice round, your position is set at five, corresponding to five conservative positions and one liberal position. The candidates' positions will change each period and will be selected randomly. Each candidate will take either a liberal or a conservative stance on the six policy issues. Each

candidate's position for the period is equal to the number of issues on which it took a conservative stance. Candidate A has a .55 probability of taking a liberal stance on any given issue and Candidate B has a .56 probability of taking a conservative stance on any given issue. That means, on average, Candidate B's position will be slightly more conservative than Candidate A's position, but in any given period, either candidate may be more conservative than the other. You do not need to choose the candidate who shares your stance on any issue, but rather, you must choose the candidate closest to the same total number of liberal and conservative stances as you.

This experiment allows you the opportunity to earn Experiment Currency Units or ECUs. At the end of the experiment, your earnings in ECUs will be translated into dollars at the rate of one ECU equals one cent. So, if you end with a balance of 1,000 ECUs, you would be paid ten dollars plus the five dollar show-up fee for a total of 15 dollars. We will pay you in cash at the end of the experiment.

The experiment proceeds as follows: At the beginning of each period, you will be given an endowment of 100 ECUs. [**Control:** Your goal is to elect the candidate whose position is closer to your own. To accomplish this goal, you will both buy private information about the candidates' positions and ask other participants for information. At the end of each period, ECUs will be awarded based on the outcome of the election. If the candidate closer to you wins the election, you will earn 50 ECUs. If the candidate closer to you loses the election, you will lose 50 ECUs. If both candidates were equally close to you, you are awarded no ECUs and you have no ECUs deducted from your total.] [**Treatment:**: Your goals are to form an accurate estimate of the candidates' positions and to elect the candidate whose position is closer to your own. To accomplish these goals, you will both buy private information about the candidates' positions and ask other participants for information. At the end of each period, ECUs will be awarded based on the outcome of the election. If your final estimate of Candidate A's position is correct, you will earn 15 ECUs and if your final estimate of Candidate B's position is correct, you will earn 15 ECUs. Then, if the candidate closer to you wins the election, you will earn 50 ECUs. If the candidate closer to you loses the election, you will lose 50 ECUs. If both candidates were equally close to you, you are awarded no ECUs and you have no ECUs deducted from your total.]

Please turn to your computer screens. We have prepared several demonstration screens to help you get familiar with the actual screens you will see during the experiment.

[**SCREEN ONE**] This is the first screen you will see in each period. The top of each screen will display the period, your participant number, your position, and the time remaining in each period. We suggest that you make your decisions for a screen within the time limit. You should be careful in making your choices, but it is in your own best interest to make your choices as quickly as you are able.

This screen tells you the price of private information and asks you how many pieces you would like to buy. Each piece of private information will inform you of each candidate's

stance on one of the six policy issues. The price of private information is randomly assigned to each participant and will cost 25 ECUs, five ECUs, or the information will be free. If information costs you 25 ECUs, you can buy up to two pieces of private information. Otherwise, you can obtain up to three pieces of private information.

Enter the amount of information that you wish to buy and click OK.

[**SCREEN TWO**] On the next set of screens, you will view the information you purchased and enter an estimate of each candidate's position after each new piece of information. Each piece of information shows each candidate's stance on one of the six policy issues. Not all participants will see information about the same issues. For example, one participant may get information about health care policy, while another participant may get information about immigration reform. If you did not purchase any information, none will be displayed, but you will still enter an estimate of the candidate's positions.

On each of these screens, enter estimates for the two candidates and click OK.

[**SCREEN THREE**] After receiving private information, you have the opportunity to seek social information. On this screen, you are given the chance to request information from one of the other eight participants. The screen shows you the position of each participant, the amount of information that they purchased, and the topic of each piece of information that they received. You can use this information to decide from whom to request information. Keep in mind your participant number displayed in the upper left corner of the screen, as you cannot request information from yourself.

Enter the participant number from whom you want information and then click OK.

[**SCREEN FOUR**] This screen displays the subjects who requested information from you. If no participants ask you for information in the experiment, you must still click OK to move on.

At this point, you have the opportunity to accept or reject the request for information. You are not penalized or rewarded for accepting requests and you are not penalized nor rewarded for rejecting requests. To reject a request from a participant, simply leave both boxes blank for that participant. If you accept the request, you must provide an estimate of both candidates' positions. You do not need to provide identical information to each of the participants who requested information from you. The top of the screen displays your most recent estimate of the candidates' positions.

After you have done this for all of the participants who requested information, click OK.

[**SCREEN FIVE**] This screen tells you whether the other participant accepted your request for information. If so, the screen also tells you the estimates of each candidate's position provided by the other participant. The screen then asks you to update your

estimate of each candidate's position. In the experiment, this screen will be followed by two more rounds of social information like the one you just saw, but we will skip those screens for this demonstration. At the end of the third round of social information, you will be asked for one last estimate of the candidates' positions on a screen like this. [**Treatment:** At that point, you will earn 15 ECUs for estimating correctly either candidate's position and 30 ECUs for estimating correctly both candidates' positions.]

Enter estimates for the two candidates and click OK.

[**SCREEN SIX**] On this screen, the election will take place. You are reminded of your position and your most recent estimate of the candidates' positions. Based on this information and the knowledge that Candidate A has a .55 probability of taking a liberal stance on any given issue and Candidate B has a .56 probability of taking a conservative stance on any given issue, your goal is to figure out the candidates' positions and vote for the candidate closer to your position. There are examples on the handout. If your position is five and you believe candidate A's position is six and candidate B's position is three, then you should vote for candidate A because five is closer to six than three. If your position is five and you believe candidate A's position is one and candidate B's position is three, you should vote for candidate B because five is closer to three than one.

Please vote for one of the candidate's and then click OK.

[**SCREEN SEVEN**] This is the final screen. The two candidate's positions are revealed, as is the outcome of the election and whether your estimates of the candidates' positions were correct. You will also learn for which candidate you should have voted, as well as the number of ECUs you earned in this period. That is calculated by starting with your 100 ECU endowment. From that the cost of the information you purchased is subtracted. [**Treatment:** 15 ECUs are added for each candidate's position you correctly estimated.] If the candidate closer to your position won the election, you earn an additional 50 ECUs. If the candidate closer to your position lost, you lose 50 ECUs. If both candidates were equally close to your own position, you earn no ECUs but do not lose any either.

The most ECUs you can earn in a single period is [**Control:** 150] [**Treatment:** 180] ECUs; the fewest ECUs you can earn in a single period is zero. Beneath your earnings for this period, you will see the total number of ECUs earned up to this point in the experiment. The experiment will consist of up to 15 periods just like this one. At the end of the experiment, you will be asked for some demographic information and then a couple of questions about your general political leanings.

This concludes the demonstration screens. We are now ready to begin the actual experiment. You will first answer a series of questions about your political opinions. The information you provide on these screens will be used to estimate your position throughout the experiment. We ask that you follow the rules of the experiment. Anyone who violates the rules may be asked to leave the experiment with only the five dollar show-up fee. Are there any questions before we start?

All participants in the accuracy treatment were given this handout:

GENERAL INFORMATION

We ask that you please refrain from talking or looking at the monitors of other participants during the experiment. If you have a question or problem please raise your hand and one of us will come to you.

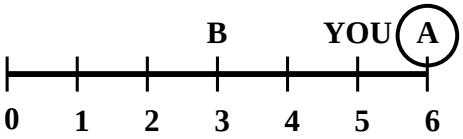
At the end of the experiment, your earnings in ECUs will be translated into dollars at the rate of one ECU equals one cent.

Candidate A has a .55 probability of taking a *liberal* stance on any given issue.
Candidate B has a .56 probability of taking a *conservative* stance on any given issue.

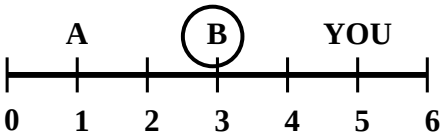
A candidate's position in each period is equal to her total number of conservative stances.

VOTING FOR CLOSEST CANDIDATE

EXAMPLE ONE



EXAMPLE TWO



PAYMENT FOR EACH ROUND

Profit= Endowment – Cost of Information + Candidate Estimates + Outcome of Election

Candidate Estimates

If neither of your final estimates is correct, you don't gain any ECUs
If one final estimate is correct, you gain 15 ECUs
If both final estimates are correct, you gain 30 ECUs

Outcome of Election:

If candidate whose position is closer to you wins, you earn 50 ECUs.
If candidate closer to you loses, you lose 50 ECUs.
If both candidates are equally appealing to you, you don't earn or lose ECUs.

We ask that you follow the rules of the experiment. Anyone who violates the rules may be asked to leave the experiment with only the $5 show up fee.

All participants in the control group were given this handout:

GENERAL INFORMATION

We ask that you please refrain from talking or looking at the monitors of other participants during the experiment. If you have a question or problem please raise your hand and one of us will come to you.

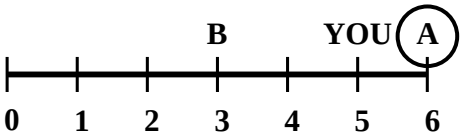
At the end of the experiment, your earnings in ECUs will be translated into dollars at the rate of one ECU equals one cent.

Candidate A has a .55 probability of taking a *liberal* stance on any given issue.
Candidate B has a .56 probability of taking a *conservative* stance on any given issue.

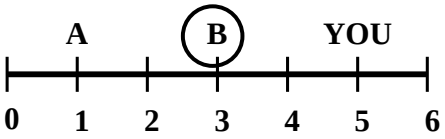
A candidate's position in each period is equal to her total number of conservative stances.

VOTING FOR CLOSEST CANDIDATE

EXAMPLE ONE



EXAMPLE TWO



PAYMENT FOR EACH ROUND

Profit= Endowment – Cost of Information + Outcome of Election

Outcome of Election:
If candidate whose position is closer to you wins, you earn 50 ECUs.
If candidate closer to you loses, you lose 50 ECUs.
If both candidates are equally appealing to you, you don't earn or lose ECUs.

We ask that you follow the rules of the experiment. Anyone who violates the rules may be asked to leave the experiment with only the $5 show up fee.

B Item Wording

This appendix provides the wording of each input item in an election:

[PURCHASING PRIVATE INFORMATION]
You can spend a total of 50 ECUs on private information now. You can buy a maximum of **[2 or 3]** pieces of private information. Each piece of information provides Candidate A's position and Candidate B's position on one of the six issues. Each piece of information will cost you **[0, 5, or 25]** ECUs. How many would you like to buy?

[ESTIMATING CANDIDATE POSITIONS]
Given this information, what do you think the candidates' positions are?
Candidate A: ___
Candidate B: ___

[REQUESTING SOCIAL INFORMATION]
You may request information on the candidates' position from one of the other six participants. There is no cost for this social information. Please enter the participant number of the player from whom you would like to request information.

[SENDING SOCIAL INFORMATION]
You previously thought the candidates' positions were:
Candidate A: [most recent Candidate A estimate]
Candidate B: [most recent Candidate B estimate]

You may now provide information to other participants. What do you want to tell these participants? (You do NOT need to provide identical information.)

[VOTING]
It is now time to cast your vote.

I vote for:

- CANDIDATE A
- CANDIDATE B

[candidate selected using radio button]

C ROC Plot for Table II

Figure 5 provides a ROC plot of the model predictions from Table II. The figure shows the rate of correctly predicted social information requests (i.e., true positive rate) relative to the false positive rate for participants in the accuracy treatment (dotted line) and for those in the control group (solid line). While both models provide a significant improvement over random guessing (as indicated by the straight line) across all thresholds, the predictions for accuracy-treatment participants outperform predictions for those in the control. This finding suggests that the selection criteria that participants had at their disposal, which serve as the explanatory variables in the model, are better predictors in the accuracy treatment than in the control. Thus, the search for information was more stochastic in the control than in the treatment. Equivalently, as the literature on motivated reasoning would suggest, people in the accuracy treatment were more systematic in their search for information than those in the control. Therefore, it appears that the accuracy payment was effective in encouraging systematic processing and a more careful evaluation of potential discussants.

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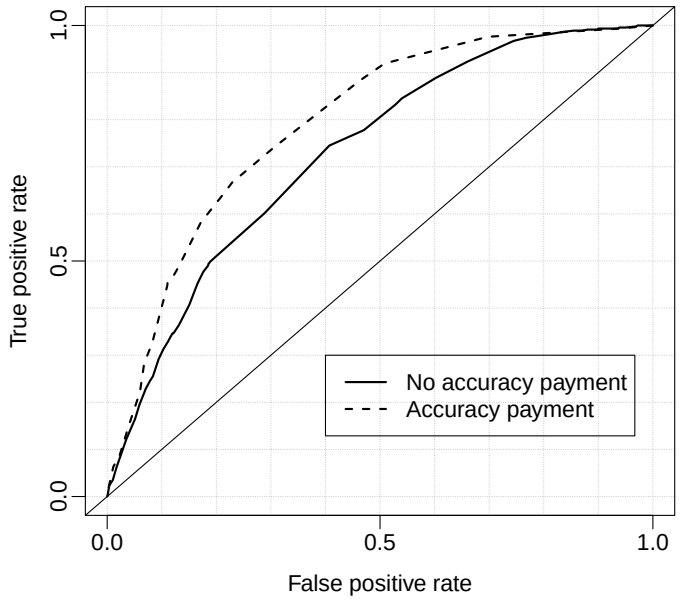
Figure 5: ROC plot of model predictions for social information requests, by treatment.



Note: Figure based on estimates from Table II.

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