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ORIGINAL ARTICLE

The concreteness of social knowledge and the quality of democratic choice

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Abstract
Democracy relies on citizens who are politically knowledgeable and engaged. However, when a voter gains political knowledge regarding important issues, through television, town halls, or social media, she also learns that there are many other politically knowledgeable voters, highlighting the importance of social knowledge in political participation. Will a voter with concrete—as opposed to hypothetical—knowledge about other voters' political knowledge have an increased incentive to participate? Or instead, will concrete social knowledge about other voters actually inhibit participation? In this article, we develop a novel experimental design that focuses on whether concrete knowledge about other voters' political knowledge influences political participation. Our main result shows that concrete social knowledge decreases individual voters' willingness to vote, and thereby reduces the probability democracy chooses the majority preferred alternative, i.e. the quality of democratic choice.

Keywords: Laboratory experiment; collective decision-making; hypothetical knowledge; voting games

1. Introduction

Perhaps the most important role of democratic institutions is to represent the interests of the majority, and ensuring this key function in modern democracies requires having an engaged and politically knowledgeable electorate. But democracy is undermined by at least two problems. First is the democratic dilemma (Lupia and McCubbins, 1998), where voters lack the expertise to make decisions that protect their interests.[1] Second, and more problematic, is when voters who are politically knowledgeable lack the willingness or desire to engage in democratic decision-making. These two issues have a similar impact on *the quality of democratic choice*: the probability that the majority preferred alternative is achieved through democratic decision-making (Tyson, 2016).

In response to recent declines in political engagement, scholars and policymakers have suggested that better achieving the democratic ideal of a knowledgeable and engaged electorate requires improving voters' overall interest and knowledge of politics. The context where political knowledge is acquired, for example, through town halls and most recently social media, typically bundles together a number of different characteristics, each of which influences the quality of democratic choice differently (Kenny, 1998). Social media and polls have been shown to significantly increase voters' interest in politics which can increase voter turnout (e.g., Bernhardt *et al.*, 2008), but this impact may be context-dependent (Großer and Schram, 2010).

[1]Druckman (2019) gives an overview of how the term "democratic dilemma" is used in the literature.

In the interest of improving political participation, scholars have focused on the importance of enriching community, with a particular emphasis on the political role of social connections (Putnam, 1966, 2000; Huckfeldt, 1979; Gerber *et al.*, 2008), and appeals based on group membership (Herrera and Martinelli, 2006; Schnakenberg, 2014). Giles and Dantico (1982) find that voters are more likely to participate in politics when surrounded by similar peers (see also Großer and Schram, 2006). Enriching social ties, however, entails a number of distinct mechanisms, each of whose all-else-equal influence is not well understood. In this study, we address one component entailed by stronger community ties, which is the extent to which increased social ties lead to a more concrete understanding about other voters' political knowledge. In particular, are voters with concrete social knowledge more (or less) motivated to participate in the democratic process? Are voters who know how many other voters are politically knowledgeable more engaged than those for whom such knowledge is hypothetical?

Consider, for example, a voting scenario where voters must choose between a policy that is tough on crime, using a large portion of the public budget on policing, and another that uses it for other things (perhaps mental health infrastructure). There may be partisan voters who prefer heavy police funding regardless of the current crime level, and partisan voters who want to defund the police in any state of the world. A majority of voters, however, are independent and want more police funding only when crime is high, and not when crime is low. Now suppose that independent voters learn about the level of crime through town halls (instead of, for instance, local news), where they also gain a concrete understanding of how many other independent voters are politically knowledgeable as well as the relative strength of partisan support. Does this kind of concrete social knowledge, compared to a situation where the presence of other politically knowledgeable voters is hypothetical, influence voters' propensity to participate in democratic decision-making?

To understand the importance of concretely knowing who is politically knowledgeable, we need to study the concreteness of social knowledge in isolation. To achieve this goal, we conduct experiments to isolate how the way political knowledge is obtained affects voter participation and the quality of democratic choice. Our experiments consist of an election where some voters are exogenously endowed with *political expertise*, telling them which alternative is best. Other voters, who do not receive expertise, do not know which alternative best serves the common good, and thus, cannot beneficially contribute to democratic decision-making.[2] Our main experimental treatment varies whether voters have concrete (as opposed to hypothetical) knowledge about the expertise of other voters. Specifically, in our *Treatment Group*, we reveal the number of expert voters among the electorate, whereas in our *Control Group*, this information remains hidden.

Our main findings address the quality of democratic choice, which is the probability that the majority preferred alternative is collectively chosen by voters. Our first main result is that concretely revealing the number of expert voters causes a 12 percentage point drop in the quality of democratic choice, suggesting that having concrete knowledge that there are other expert voters, perhaps because political knowledge is obtained in social settings, causes a decline in the quality of democratic choice.

We identify our main result using a novel experimental design that measures voters' desire to politically engage by directly eliciting each voter's *willingness to vote*, i.e. the largest cost a voter is willing to incur to ensure that her vote counts. We show that revealing the number of expert voters among the electorate causes a significant decline in the average willingness to vote by expert voters, establishing that the 12 percentage point decline in the quality of democratic choice is in fact because of reduced engagement by expert voters at the individual level, and not due to outliers, ecological differences between treatment groups, or incorrect 2nd-order beliefs about nonexpert voting.

[2]See Brennan (2009) or Saunders (2020) for normative arguments against uninformed voting. In our experiment, nonexpert voters lack instrumental incentives to vote.

From the perspective of policy-making, our results suggest that increased levels of political knowledge are not necessarily beneficial toward improving democratic outcomes. This is relevant to understanding the role of new media technologies, such as various social media platforms (e.g., Lupia, 2016) and how social media affects the interplay between independent voters and partisan voters (e.g., Druckman *et al.*, 2018). Our results imply that distributing political information in public settings will be less effective than providing that same information in a decentralized manner. However, decentralized dissemination of information can be quite costly, thus suggesting key tradeoffs between several factors—material, normative, and strategic—affecting collective decision-making and the design of democratic institutions.

Our main experimental treatment distinguishes between voters who have concrete social knowledge and voters for whom this knowledge is hypothetical. In a related article, Esponda and Vespa (2014) show that, even when political information is perfectly reliable (as in our experiment), voters fail to accurately make inferences from hypothetical events (see also Esponda and Vespa, 2016). Since the acquisition and circulation of political knowledge is a social activity, it is important to understand how various aspects of political knowledge acquisition influence political participation, and consequently the quality of democratic choice. Existing empirical studies have found mixed results (Huckfeldt, 1979; Huckfeldt and Sprague, 1987). Specifically, McClurg (2006) finds that the societal supply of political expertise can encourage higher levels of political participation, whereas Mutz (2002) finds that people in social interactions that cross lines of political difference are less likely to participate in politics. It is important to stress that we are not studying whether a better informed electorate is good for democracy (see, e.g., Schnakenberg, 2016; Prato, 2018; Schnakenberg and Turner, 2019). Additionally, our study is not about the tension between minority and majority interests, as we study a common value setting.

In our study, expert voters essentially face a kind of threshold public good environment, potentially under population uncertainty (see, e.g., Myerson, 1998; Bouton and Castanheira, 2012). McBride (2006) studies a threshold public good game where the threshold is uncertain, and shows that increased threshold uncertainty increases equilibrium contributions, provided the value of the public good is sufficiently high. In an experimental study, McBride (2010) evaluates behavior in a context similar to a public goods game, where subject beliefs are elicited to measure uncertainty. McBride (2010) finds that contributions increase in subjects' subjective assessment of the likelihood their contribution will be pivotal in public good provision.

The quality of democratic choice, which is the probability the majority preferred alternative is adopted, is also typically used in studies (experimental or otherwise) on Condorcet's Jury Theorem (e.g., Austen-Smith and Banks, 1996; Feddersen and Pesendorfer, 1997, 1998; Guarnaschelli *et al.*, 2000; Gerling *et al.*, 2005). Specifically, studies that focus on information aggregation in elections measure the probability that a policy choice is matched with an unknown state of the world and refer to this quantity as "informational efficiency," see, in particular, Battaglini (2017). Each expert voter in our experiment is truthfully told the state of the world and the votes of partisan voters with certainty, thus the benefits of voting do not come from aggregating dispersed signals, but instead, come from an increased likelihood of defeating partisan voters.[3]

Finally, our study is connected to the literature on the swing voter's curse (Feddersen and Pesendorfer, 1996), which is the phenomenon whereby uninformed voters cast votes (often according to a mixed strategy) to offset a known partisan bias, thus increasing the likelihood that informed voters are pivotal (see also Feddersen and Pesendorfer, 1999; Fey and Kim, 2002). Battaglini *et al.* (2010) evaluate the swing voter's curse in a laboratory setting, where human subjects play the role of independent voters and partisan votes are played by the computer

[3]Such concerns are also different from tactical coordination in multi-candidate elections (Myatt and Fisher, 2002; Myatt, 2007).

(i.e., partisan bias is exogenously assigned). In our experiment, each level of the partisan bias is equally likely, making our setting similar to Battaglini *et al.* (2010)'s treatment where each state of the world is equally likely and the partisan bias equals zero.[4] Because Battaglini *et al.* (2010)'s study was designed to evaluate the swing voter's curse, both informed and uninformed voters are told the realization of the partisan bias.

There are three substantial differences between our study and those focusing on the swing voter's curse. First, in our experiment, subjects do not receive noisy signals but are told the state of the world truthfully.[5] Second, our outcome measure is subjects' willingness to vote, which is the maximum cost a subject is willing to pay to ensure that her vote is counted; there are no vote costs in Battaglini *et al.* (2010)'s experiment. Third, nonexpert voters in our experiment are *not* told the partisan bias. This is a critical difference between our experiment and Battaglini *et al.* (2010), since this implies that the swing voter's curse cannot arise in our experiment. It is important to stress that avoiding swing voter's curse style behavior in our experiment was intentional, as our experiment was designed to isolate the role of the concreteness of social knowledge.

2. The experiment

Our experiment corresponds to an election where the best alternative for every independent voter (or subject) is the same (i.e., our setup is one of common values). Each election consists of five subjects and two additional "partisan" votes that we refer to as the *partisan bias*, which in our experiment is randomly determined by the computer. Each value of the partisan bias, corresponding to either two votes for A, two votes for B, or one vote for each, A and B, is equally likely, and thus, each occurs with probability $\frac{1}{3}$. Voters are tasked with collectively choosing an alternative, A or B, and collective decisions are made by simple plurality, i.e. whichever alternative receives more votes is the decision that is applied to everyone. Ties are broken by a fair coin toss.

To determine the best alternative for voters, a state of the world, which is either A or B, is drawn from a uniform distribution, so $P(A) = P(B) = \frac{1}{2}$. Each voter receives a "high" payoff whenever the collective decision matches the state of the world and a "low" payoff whenever the collective decision differs from the state of the world. The partisan bias can represent the behavior of a set of voters whose preferences are independent of the state of the world, and thus, may (or may not) be in agreement with the interests of experimental subjects.

The alternative which best serves voters' interest is not known to every voter. Instead, some voters are *expert voters*, who are told the state of the world and are thus perfectly informed of the alternative that best serves the common interest. Both expert voters and nonexpert voters know the distribution of expertise, i.e. how expertise is exogenously assigned among voters, as well as the distribution of the partisan bias. However, only expert voters are told the realized state of the world and the realized partisan bias.

The partisan bias in our experiment varies expert voters' instrumental "importance" in the election outcome by altering the hurdle that expert voters must cross in order to achieve their most preferred alternative. There are three different scenarios. First, the partisan bias is *supportive* when two partisan votes favor independent voters' preferred alternative. Second, when one partisan votes for A and the other partisan votes for B, we say the partisan bias is *neutral*. Third, the

[4]In their study, subjects are randomly assigned to an electorate of seven voters and rematched throughout the session. Participants are asked to make discrete—and costless—vote choices under three different partisan biases (0, 2, and 4), and two states of the world. Participants receive noisy signals of the state of the world under two different prior distributions over the state of the world ($\frac{1}{2}$ and $\frac{5}{9}$).

[5]The swing voter's curse, as it is originally articulated in Feddersen and Pesendorfer (1996), does not rely on noisy information or information aggregation. Thus the experiments conducted by Battaglini *et al.* (2010) do not measure a pure swing voter's curse. Instead, a modification of our design where realizations of the partisan bias are told to nonexpert voters, and the state of the world is told to expert voters, would isolate the swing voter's curse experimentally.

partisan bias is *against* when the computer casts two votes for the alternative that does not serve independent voters' interests.

We do not inform nonexpert voters of the realized partisan bias in the experiment to avoid the strategic incentives such information might create (e.g., the swing voter's curse) which would pollute the treatment effect we consider. In our setting, expert voters know perfectly the state of the world and partisan bias and so there is no benefit from aggregating information through voting. This feature of our design distinguishes our study from those that focus on the Condorcet jury theorem where aggregating information is crucial.

2.1 Procedures and treatments

In each round (election), before voters make any decisions, it is *exogenously* determined which subjects are given expertise. An expert voter is told the state of the world, and the value of the partisan bias. In each round, to determine whether a subject privately receives expertise, the computer generates a random integer for each subject from a uniform distribution between 1 and 100. If an individual's number is below or equal to the exogenous (symmetric) cutoff of 70, then she is told the state of the world and the partisan bias. When a subject's number is higher than 70, she is not given this information.[6] This design implies that on average about 70 percent of subjects have expertise in each election. We provide expertise in this way because using a cutoff on a discrete uniform distribution is relatively simple for subjects to understand, and hence, manage calculations.

After the level of expertise is exogenously determined, each subject faces a voting decision, and our goal is to elicit each subject's *desire to engage*. Specifically, a subject's decision in our experiment consists of two parts. First, a subject can either vote for *A*, vote for *B*, or *Abstain*. If a subject abstains, her willingness to vote is recorded as 0. Second, each subject who did not abstain is asked how much she is willing to pay (in experimental points) to ensure that her vote is counted. To elicit a subject's *willingness to vote*, we use a modification of the Becker-Degroot-Marshak (BDM) mechanism (Becker *et al.*, 1964). That is, if a subject wants to vote, either for *A* or *B*, she must enter her willingness to vote, which is an integer in the interval [1, 100]. At this point, subjects do not know their own voting cost, nor do they know the voting costs of other subjects (although they know the distribution from which voting costs are drawn). Once a subject has reported her willingness to vote, the computer independently draws a vote cost from a uniform distribution between 1 and 100 experimental points, and implements the subject's decision rule. Namely, if a subject's vote cost is higher than her reported willingness to vote, then she does not pay her vote cost and her recorded vote is not counted toward the collective decision. Instead, if a subject's randomly generated vote cost is equal to or lower than her reported willingness to vote, then her vote is counted and she pays her voting cost.

When the democratically chosen alternative matches the state of the world, all subjects (expert and nonexpert) receive 110 experimental points. If the collective decision does not match the state of the world, then all subjects receive 10 experimental points. Each subject is given an endowment of 100 experimental points from which her vote cost is deducted should her vote be counted. This endowment ensures that every voter's experimental points are always positive. In every session, the exogenously assigned cutoff determining whether a participant receives expertise is common knowledge. The state of the world, the level of the partisan bias, and the realized number of expert voters are *ex ante* unknown. To eliminate possible cross-election influences like session effects (Fréchette, 2012), subjects do not know who they are paired with in an electorate, and between election rounds subjects receive *no* feedback regarding the outcome of previous elections, previous realized voting costs, or the voting decisions of other subjects.

To investigate the role of the concreteness of social knowledge, we compare the *Treatment Group*, where voters are told the number of expert voters, with the *Control Group*, where voters

[6]It is important to stress that subjects do not incur a cost for expertise.

are not given this information. If the number of expert voters is not revealed, subjects must infer how many voters have expertise (which is stochastic) from the distribution determining expertise among the population (described above). In both groups, the level of expertise (measured by the *ex ante* probability an individual voter is an expert) is the same, and hence, our results are not driven by differences in the level of political expertise.

2.2 Subjects

Our experiment was conducted at the XS/FS Experimental Social Science Laboratory at Florida State University where subjects were registered university students. No one participated in more than one session. The experiment was programmed using z-Tree (Fischbacher, 2007) and conducted via computerized network. Subjects were seated at individual computer terminals and not allowed to see other subjects' choices. A total of 120 subjects participated in our study, where 60 were randomly assigned to the Control Group and 60 to the Treatment Group. We ran a total of eight sessions. In each session, subjects were randomly assigned to groups of five participants, who interact throughout a session, i.e. there was not rematching between participants regarding their group assignment. Critically, subjects did not know the identity of members of their group, and we did not provide subjects with feedback regarding the outcome or distribution of votes in any previous election. We conducted four sessions with 12 independent electorates for each treatment group. They participated in a total of 30 elections in each session.

In order to help subjects understand the procedure of the experiment, we used numerical examples in the instructions as well as a comprehension quiz at the end of the instructions to illustrate the setup and process of the experiment. Subjects were given time to read the instructions at their own pace, provided an explanation of any incorrect answers, and allowed to ask questions about the quiz. A whole session lasted for about 50 minutes and one of the 30 elections was randomly selected by the computer as the election to be paid. The average earning for subjects was about \$22 in which a \$7 show-up payment was included. Figure 1 summarizes the progression of activities within the experiment and the instructions received by subjects are reported in online Supplementary Appendix E.

3. Experimental results

In this section, we present our main experimental findings regarding the quality of democratic choice and the willingness to vote among expert and nonexpert voters. We use electorate averages as the unit of observation, leaving us with 12 independent electorates for each treatment group, and perform non-parametric tests on electorate averages. All statistical tests reported in this section are two-sided and non-parametric. We use both Mann–Whitney tests (whose p-values are denoted by p_{MW}), and the less conservative Fisher–Pitman permutation tests (whose p-values are denoted by p_{FP}). Recalling that subjects receive *no* feedback throughout our experiment, the statistical analysis reported can be viewed as an exceedingly conservative approach, since we take the same approach to our statistical analysis as other experiments where participants are provided with feedback and information of previous election periods.

3.1 Expert voters

We are interested in how the concreteness of social knowledge affects the extent to which democratic decision-making represents the interests of the majority (absent other frictions of collective decision-making). First, we explore our main treatment effect regarding how knowing the number of voters with political expertise affects the *quality of democratic choice*—which is the probability the majority preferred alternative is selected through democratic decision-making. To see that the decline in the quality of democratic choice results from decreased engagement of expert

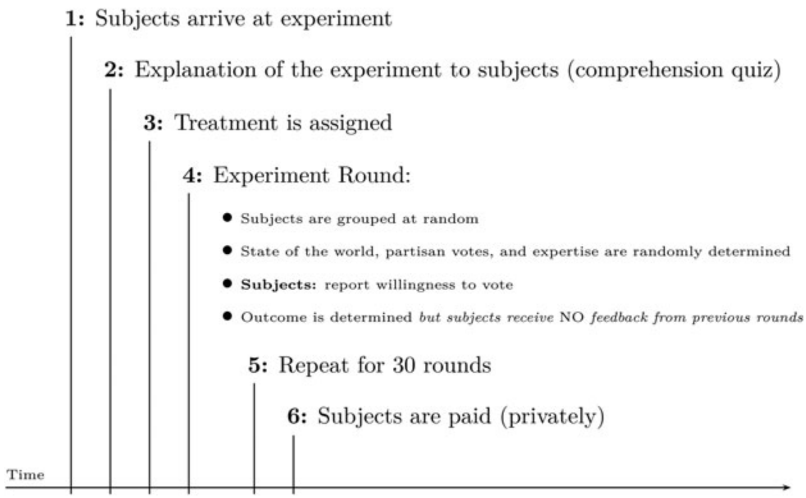

Fig. 1. Structure of experiment.

voters, we look directly at the propensity of expert voters to vote, by comparing the average willingness to vote, when voters are told the number of expert voters, to the willingness to vote when this information remains hidden.

In the Control Group, the quality of democratic choice is 76 percent, whereas in the Treatment Group the quality of democratic choice is 64 percent. Comparing the Control Group with the Treatment Group shows that concretely knowing the number of expert voters causes the quality of democratic choice to decline by 12 percentage points, a statistically significant decline of about 16 percent ($p_{MW} = 0.021$, $p_{FP} = 0.022$).

Having established that knowing how many voters have expertise reduces the quality of democratic choice, we need to show that this decline is indeed the result of declining engagement among expert voters. In our experiment, expert voters' level of engagement is measured by computing the average willingness to vote among expert voters in the treatment and control groups. Figure 2 reports the average willingness to vote in the control and treatment groups along with the associated confidence intervals for each treatment, averaging over realizations of the number of expert voters, states of the world, and levels of the partisan bias. In the Control Group, the average willingness to vote among expert voters is 40.3 (measured in experimental points), and in the Treatment Group, the average willingness to vote among expert voters is 24.9. Thus, concretely informing voters of the number of expert voters reduces their willingness to vote by 15.4 experimental points, which is a statistically significant decline of about 38 percent ($p_{MW} < 0.001$, $p_{FP} < 0.001$).

While expert voters respond differently to the concreteness of social knowledge, this feature would be concerning if expert voters responded qualitatively differently to treatment at different levels of the partisan bias. To show that our main results are not because of averaging across different levels of the partisan bias, we break down our analysis of the willingness to vote by different levels of the partisan bias. When the partisan bias is against, the average willingness to vote is 51.5 in the Control Group and 32.4 in the Treatment Group, which is significantly different ($p_{MW} < 0.001$, $p_{FP} < 0.001$) and corresponds to a decline in political engagement of about 37 percent due to revealing the number of expert voters. When the partisan bias is neutral (i.e. equal to zero), the average willingness to vote is 46.4 in the Control Group and 28.5 in the Treatment Group, which is statistically significant ($p_{MW} < 0.001$, $p_{FP} < 0.001$) and implies about a 39 percent decline in the willingness to vote of expert voters. When the partisan bias is supportive, the average willingness

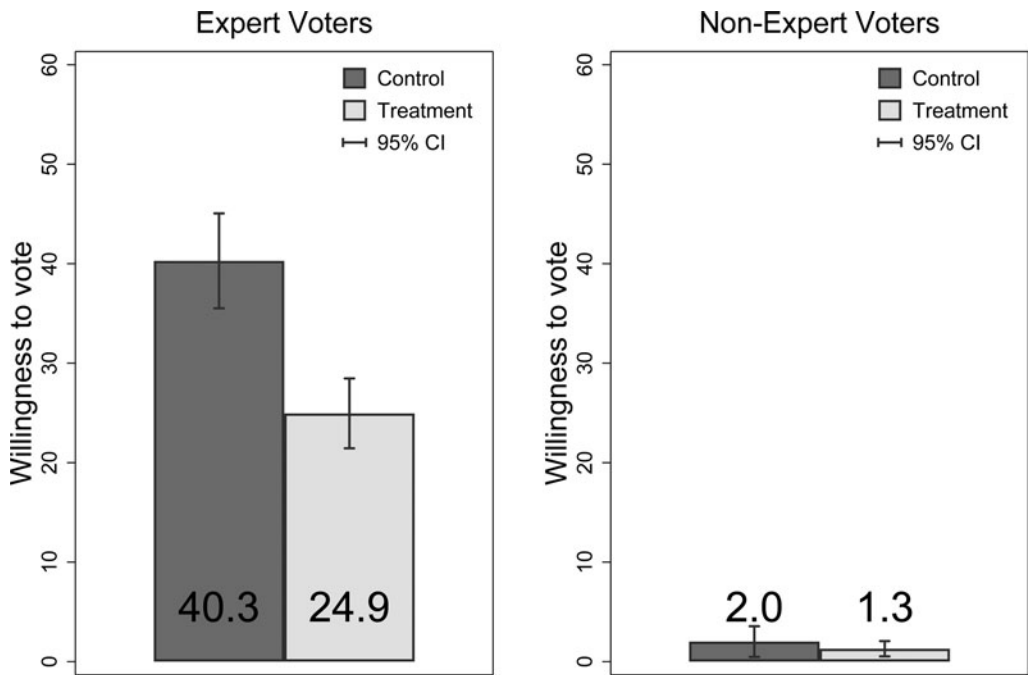

Fig. 2. Average willingness to vote by treatment.
Note: We average (non)expert voters' willingness to vote and use electorate level averages as the unit of independent observation in the statistical analysis.

to vote in the Control Group is 21.3 and it is 13.6 in the Treatment Group, which is also marginally significant ($p_{MW} = 0.095$, $p_{FP} = 0.026$) and implies about a 36 percent drop in the willingness to vote among expert voters. These results show that when broken down by the partisan bias, although the size of our treatment effect changes, our main finding remains: revealing the number of expert voters consistently reduces political participation by expert voters.

3.1.1 Realization of expert voters

We now break down the analysis by investigating individual voters' willingness to vote as a function of the realized number of expert voters. We note that if expert voters were fully rational and purely instrumental, then because expert voters know the number of realized expert voters only in the Treatment Group, we would expect a flat average willingness to vote in the Control Group, but in the Treatment Group, expert voters should change their willingness to vote with the realized number of expert voters.

Figure 3 shows the relationship between willingness to vote and the realized number of expert voters. These results suggest that the concreteness of political knowledge changes voters' desire to engage, but voters' willingness to vote is not (completely) driven by strategic considerations. Indeed, in a regression using willingness to vote as the dependent variable, and the number of expert voters as the independent variable, we find little evidence that individuals' willingness to vote changes significantly over the realized number of expert voters, whether we pool the data together or separate the analysis by partisan bias. We analyze the behavior of expert voters in the Treatment Group with the same method and find that the average willingness to vote does not change with the realized number of expert voters.

When the partisan bias is against or neutral, the potential role of nonexpert voters is confounded with other potential incentives for expert voters. However, we can explore the potential strategic influence of nonexpert voting by focusing on the case of a supportive partisan bias. If

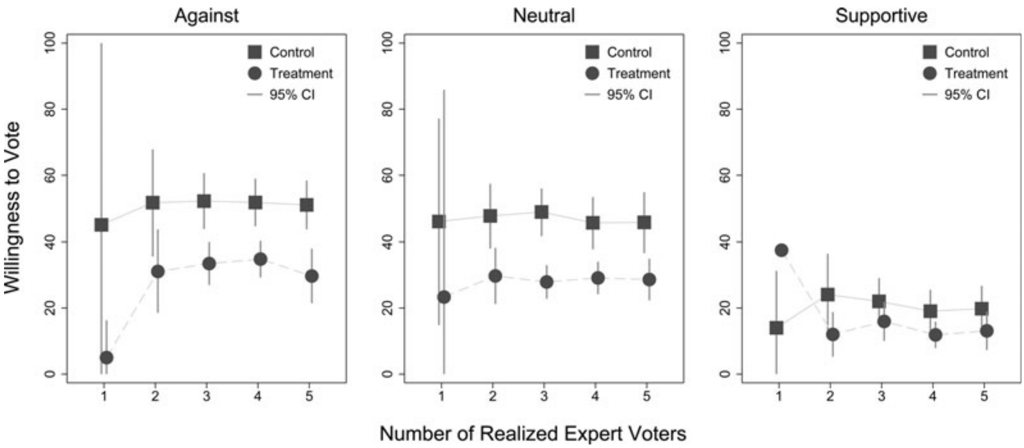

Fig. 3. Willingness to vote by the realized number of expert voters.
Note: In both the Control Group and the Treatment Group, on average about 70% of subjects have expertise in each election. There are few observations in which there are only one expert voter. For the cases in which there are only one expert voter, the lower bound of 95% CI is restrict at 0 and the upper bound of 95% CI is restrict at 100.

expert voters anticipate the votes of nonexpert voters, and respond accordingly, then when the partisan bias is supportive, expert voters' willingness to vote should depend on the realized number of expert voters only through the corresponding difference in the number of nonexpert voters. By examining the average willingness to vote by the number of expert voters when the partisan bias is supportive in the Treatment Group, we find no evidence that expert voters' average willingness to vote changes across different realizations of the number of expert voters. This finding suggests that expert voters do not strategically change their willingness to vote to defeat potential nonexpert voting (which does not generally occur, see Section 3.2).

3.1.2 The potential influence of outliers

To show that our main results are not driven by outliers, but instead, reflect a systematic change in expert voters' willingness to vote, we present the distributions of expert voters' willingness to vote across treatments. Figure 4 reports cumulative density distributions of the willingness to vote by treatment. Generally, the distribution of subjects' willingness to vote is shifted across treatments, suggesting that the average treatment effect is consistent and systematic. Putting everything together shows that our results hold at each level of the partisan bias and are not unduly influenced by outliers.

In sum, our results provide strong evidence that a particular kind of social knowledge—being told how many expert voters are present at election time—decreases the quality of democratic choice. Moreover, our results also provide strong evidence that the decrease in the quality of democratic choice is the direct result of decreased engagement by expert voters. Taken together, these results suggest that when political knowledge is obtained in a social context that also concretely conveys that other people are also gaining political knowledge, the quality of democratic choice decreases.

3.2 Nonexpert voters

Our experiment is explicitly designed to focus on expert voters, and the influence of concrete social knowledge on their political participation. Since a nonexpert voter in our experiment has not been told the state of the world or the realized partisan bias, she does not know which alternative best serves her interests. If a nonexpert voter were to cast a vote, she may

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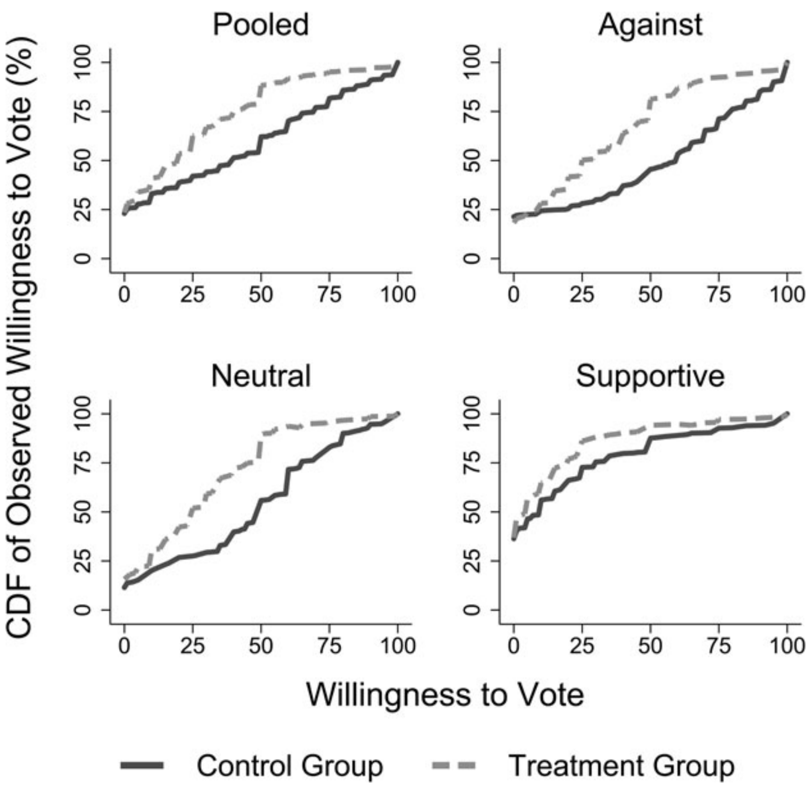

Fig. 4. Cumulative density distribution of expert voters' willingness to vote.

undermine the efforts of expert voters, and as a consequence, we expect that nonexpert voters should not be willing to incur much cost to ensure that an uninformed vote counts. In this section, we explore two things. First, do nonexpert voters vote in our experiment? Second, are expert voters' choices influenced by the hypothetical possibility that nonexpert voters vote differently based on their presence in the Treatment Group versus the Control Group?

In our experiment, nonexpert voters abstain about 93 percent of the time, meaning they do not express a vote choice or a positive willingness to vote. While a small minority of nonexpert voters express a vote choice, we find that both in the control and treatment groups, nonexpert voters' average willingness to vote is extremely small and significantly lower than that of expert voters. In the Control Group, the average willingness to vote of nonexpert voters is about 2.0 (out of 100), while in the Treatment Group, the average willingness to vote of nonexpert voters is about 1.3 (out of 100); which are not statistically distinguishable ($p_{MW} = 0.641$, $p_{FP} = 0.456$). Since nonexpert voters' willingness to vote is quite small, in our experiment, their realized voting cost is often prohibitively high, leading non-expert voters' choices to *not* count more than 98 percent of the time.

To further examine the treatment effect on willingness to vote, we compare the effect of becoming experts for each individual relative to being non-expert voters across the treatment and control groups, and perform a kind of difference-in-differences analysis. Expert voters' willingness to vote is significantly higher than nonexpert voters, whether in the Control Group (40.3 versus 2.0, $p_{MW} < 0.001$, $p_{FP} < 0.001$) or Treatment Group (24.9 versus 1.3, $p_{MW} < 0.001$, $p_{FP} < 0.001$). Figure 5 reports the difference of willingness to vote between expert and nonexpert voters in the control and treatment groups along with the associated confidence intervals for each treatment. The difference is 38.3 in the Control Group and 23.7 in the Treatment Group, which is

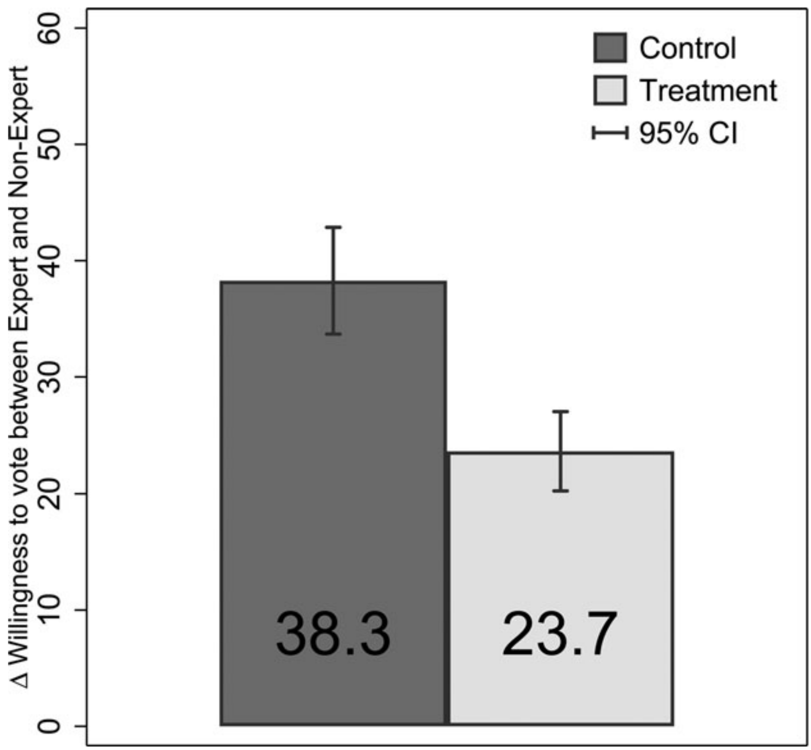

Fig. 5. Marginal effect between expert and non-expert voting.
Note: We average (non)expert voters' willingness to vote and use electorate averages as the unit of independent observation in the statistical analysis.

statistically significant at the 0.001 level. These results suggest that individuals' voting choices respond to whether they are expert voters.

3.3 Pivotal voting models

To what extent can a simple pivotal voting model, where a voter's incentive to vote is motivated purely by the likelihood that her vote is pivotal, explain our experimental results? In Supplementary Appendix G we present two pivotal voting models, one resembling our Control Group and the other resembling our Treatment Group, and characterize the vector of vote cost cutoffs that are symmetric Bayesian Nash equilibria of these pivotal voting models.[7] To assess the ability of such a pivotal voting model to provide insights into the behavior in our experiment, we consider what cutoff willingness to vote would be "predicted" by such a model, which are presented in Table 1.[8]

Taking the predicted values from Table I, we find the average willingness to vote is computed to be 17.2 in the Control Group and 7.8 in the Treatment Group, which suggests that the average willingness to vote should be higher in the Control Group than in the Treatment Group. Our experimental observation is that the average willingness to vote is 40.3 in the Control Group and 24.9 in the Treatment Group. Thus, the results derived from pivotal voting models are

[7]In Online Appendix H, we present a symmetric quantal response equilibrium, attributing deviations from the pivotal voting model to logistic decision errors.

[8]Relative to the model in the appendix we set the solidarity and duty terms to 0, and using the parameters of our experiment.

qualitatively consistent with our experimental results. So, on the surface, a pivotal voting model seems to capture our main treatment effect well.

Does the qualitative congruence of our treatment effect with what is produced by comparing pivotal voting models suggest that such models can give insights into our observed treatment effect? When breaking down the analysis by treatment and partisan bias, we find little to no evidence that voters' willingness to vote is primarily motivated by reasoning consistent with pivotality concerns. In Table I, we present the observed average willingness to vote for each scenario of our experiment with the Bayesian Nash equilibrium vote cost cutoff from a pivotal voting model in brackets. Aside from the results that are inconsistent with pivotality concerns presented in previous sections, Table I provides a number more "predictions" that are not borne out in our data. First, when the partisan bias is neutral and there is only one expert voter, the voter's willingness to vote should be the highest, yet, in our experiment, we find several scenarios where the average willingness to vote is higher. Second, a pivotal voting model suggests that expert voters' willingness to vote should decrease as the number of realized expert voters increases in the Treatment Group. Looking at the left column of Figure I and Table I in which the partisan bias is against, voters in our experiment consistently report a higher willingness to vote than would be warranted purely from pivotality concerns. A natural explanation for this "over-voting" might be the presence of a duty term, a la Riker and Ordeshook (1968), which could easily be incorporated into a pivotal voting model.[9] However, when we move to the middle column in the Treatment Group where the partisan bias is neutral, the pattern reverses for some values of the realized number of expert voters, namely, voters in our experiment are "under-voting" relative to the Bayesian Nash equilibrium of a pivotal voting model. This cannot be easily reconciled by simply appealing to a duty term. These patterns suggest that a pivotal voting model does not capture salient concerns motivating voters' willingness to vote at the level where decisions take place, despite the relatively good match at the aggregate level, where the factors that cause variation in voters' decisions are averaged out.

4. Normative implications

In this section, we investigate the normative implications of concretizing social knowledge using two different consequentialist welfare criteria. First, we consider a maximum engagement criterion where all expert voters engage in voting and examine the quality of democratic choice. Second, we consider a utilitarian criterion, where full engagement is not endorsed as it is too costly, and explore the extent to which voters fall short of economic efficiency.

4.1 Maximum engagement

We are interested in how the concreteness of social knowledge affects the ability of democracy to represent the interests of the majority, measured by the quality of democratic choice, which is the probability that the alternative preferred by the majority of voters is the alternative that is collectively chosen through democratic decision-making.[10] In our experiment, the majority preferred option always corresponds to the alternative favored by subjects in our experiment. The partisan bias may, however, complicate voters' ability to achieve their most preferred alternative by acting as a hurdle voters must defeat.

In our experiment, a nonexpert voter is not told the state of the world, and consequently, does not know whether a vote for A or a vote for B best serves her interest. Additionally, were a nonexpert voter to caste a vote, she may undermine the efforts of expert voters, who know which

[9]Indeed, in Supplementary Appendix G, we include such a term.

[10]It is important to stress that lower participation can improve welfare when alternatives are endogenous (Prato and Wolton, 2018).

Table 1. Symmetric Bayesian Nash equilibrium predictions

		Partisan bias			
		Against	Neutral	Supportive	Average
Control group	Obs. [Pred.]	51.5 [0 or 27.6]	46.4 [24.0]	21.3 [0]	40.3 [17.2]
Treatment group	Aggregate	32.4 [0 or 1.6]	28.5 [21.8]	13.6 [0]	24.9 [7.8]
	$M = 1$	5 [0]	23.3 [50.0]	37.5 [0]	
	$M = 2$	31.1 [0]	29.7 [33.3]	11.8 [0]	
	$M = 3$	31.2 [0]	29.4 [26.8]	15.3 [0]	
	$M = 4$	35.0 [0 or 33.3]	27.8 [22.9]	12.0 [0]	
	$M = 5$	27.6 [0 or 34.7]	28.4 [20.2]	13.8 [0]	

Note: We report the experimental observations (Bayesian Nash equilibria computed based on pivotal voting models) outside (inside) of brackets. In the Control Group, given voters do not know concretely about how many expert voters are there, only average willingness to vote following the distribution of expertise in our experiment is estimated. In the Treatment Group, besides reporting the average willingness to vote of all realizations of the number of expert voters, since voters know how many expert voters are there in each election period, the results are also reported by $M = x$ that represents the number of realized expert voters. Note that when the partisan bias is against, there are multiple equilibria and zero is always an equilibrium willingness to vote, and when the partisan bias is supportive, the predicted willingness to vote is always zero.

alternative best serves her interests. Thus, an uninformed vote inhibits the ability of democratic choice to represent the interests of the majority.

To represent the quality of democratic choice, let the parameter $\rho \in [0, 1]$ be the *level of political engagement by expert voters*, which is the probability that an expert vote is cast. In the context of our experiment, and assuming that nonexpert voters do not vote, we can represent the quality of democratic choice as $Q(\rho) =$:

$$\underbrace{\frac{1}{2}\left(\frac{1}{3}\left[1-\frac{1}{2}\binom{5}{0}\rho^0(1-\rho)^5\right]+\frac{1}{3}\left[1-\binom{5}{0}\rho^0(1-\rho)^5-\binom{5}{1}\rho^1(1-\rho)^4-\frac{1}{2}\binom{5}{2}\rho^2(1-\rho)^3\right]\right)}_{\text{the state of the world is A and the partisan bias favors B}}$$

$$+\underbrace{\frac{1}{2}\left(\frac{1}{3}\left[1-\binom{5}{0}\rho^0(1-\rho)^5-\binom{5}{1}\rho^1(1-\rho)^4-\frac{1}{2}\binom{5}{2}\rho^2(1-\rho)^3\right]+\frac{1}{3}\left[1-\frac{1}{2}\binom{5}{0}\rho^0(1-\rho)^5\right]\right)}_{\text{the state of the world is B and the partisan bias favors A}}$$

$$+\underbrace{\frac{1}{2}\left(\frac{1}{3}+\frac{1}{3}\right)}_{\text{the partisan bias favors voters' preferred alternative}}.$$

The first line represents the probability voters achieve their most preferred alternative when the political engagement of expert voters is ρ, the state of the world is A, and the partisan bias favors B. The second line represents a similar probability but when the state of the world is B and the partisan bias favors A. The last line is the probability that the partisan bias favors voters' preferred alternative.

Because of the symmetry in our experiment (between states of the world and levels of the partisan bias), whether the state of the world is A or B is immaterial for the quality of democratic choice, and we can simplify to

$$Q(\rho)=\frac{1}{3}\left(2-\frac{3}{2}\binom{5}{0}\rho^0(1-\rho)^5-\binom{5}{1}\rho^1(1-\rho)^4-\frac{1}{2}\binom{5}{2}\rho^2(1-\rho)^3\right)+\frac{1}{3}. \quad (1)$$

This expression gives the connection between the level of political engagement of expert voters, ρ, and the quality of democratic choice, which is the probability that voters achieve their most preferred alternative through democratic decision-making. If expert voters are fully engaged, the quality of democratic choice is determined solely by the level of political expertise, which in our experiment is 70 percent. Thus, by substituting $\rho = 0.7$ in expression (1), a straightforward calculation establishes that the *maximum* probability voters collectively choose the alternative that is best for all of them, when expert voters are fully engaged, is $Q(0.7) = 0.97$. This corresponds to the majority-preferred alternative being adopted as often as possible.

To interpret our experimental results with respect to this maximum engagement benchmark, in Figure 6 we compare the quality of democratic choice achieved in our experiment with the normative benchmark in which all expert voters engage in voting. We find that relative to when expert voters are maximally engaged, the actual quality of democratic choice is significantly smaller in both the Control Group ($p_{MW} = 0.002$, $p_{FP} < 0.001$) and Treatment Group ($p_{MW} = 0.002$, $p_{FP} < 0.001$), representing the (significant) drop resulting from reduced engagement by expert voters.

4.2 Utilitarian planner

In this section, we consider utilitarian welfare, under the implicit assumption that the only aspect relevant to voter welfare is their net payments in experimental points. To facilitate the analysis we focus on the problem of a utilitarian planner, who prescribes a symmetric cost cutoff for expert voters, which in principle, can depend on the partisan bias, denoted by β, and in the Treatment Group, the number of expert voters, $M \leq N$ (where N is the total number of voters). Suppose that an individual subject i's vote cost is c_i and that she votes if her vote cost is below a threshold $\bar{c}$.

In our analysis, we assume that the utilitarian planner, although benevolent, is not omniscient, and does not know individual voters' realized vote costs, realizations of the partisan bias, or the realized number of expert voters. It is important to emphasize that our utilitarian calculations are focused only on our experimental subjects, i.e. we do not consider partisan votes as being cast by people (which indeed they are not in our experiment). This is a reasonable way to proceed when considering the ethical implications of our particular experiment, but when applying our results to a context where partisan voters are people, some modifications may be necessary.

Focusing first on the Treatment Group, denote the utilitarian planner's symmetric cutoff prescription by $\bar{c}^*(M; \beta)$. For a supportive partisan bias, i.e. when $\beta = -2$, since no expert voter is pivotal, any level of voting is economically inefficient, and hence $\bar{c}^*(M; \beta = -2) = 0$. When the partisan bias is not supportive, the utilitarian planner must consider the (average) cost of endorsing voting among expert voters. Specifically, the total expected cost is

$$\sum_{j=1}^{M} \binom{M}{j} \bar{c}^j (1-\bar{c})^{M-j} j \cdot \frac{1}{\bar{c}} \int_0^{\bar{c}} c dc,$$

and, since $\int_0^{\bar{c}} c dc = \frac{1}{2}\bar{c}^2$, we can write the total expected cost as

$$\frac{1}{2}\bar{c} \sum_{j=1}^{M} \binom{M}{j} \bar{c}^j (1-\bar{c})^{M-j} j = \frac{M}{2}\bar{c}^2.$$

Thus, for a fixed partisan bias, $\beta \in \{0, 2\}$, and a fixed number of expert voters, M, the utilitarian planner's problem in the Treatment Group is

$$\max_{\bar{c} \in [0,1]} N \left(\sum_{k=\beta+1}^{M} \binom{M}{k} \bar{c}^k (1-\bar{c})^{M-k} + \frac{1}{2} \binom{M}{\beta} \bar{c}^\beta (1-\bar{c})^{M-\beta} \right) - \frac{M}{2}\bar{c}^2.$$

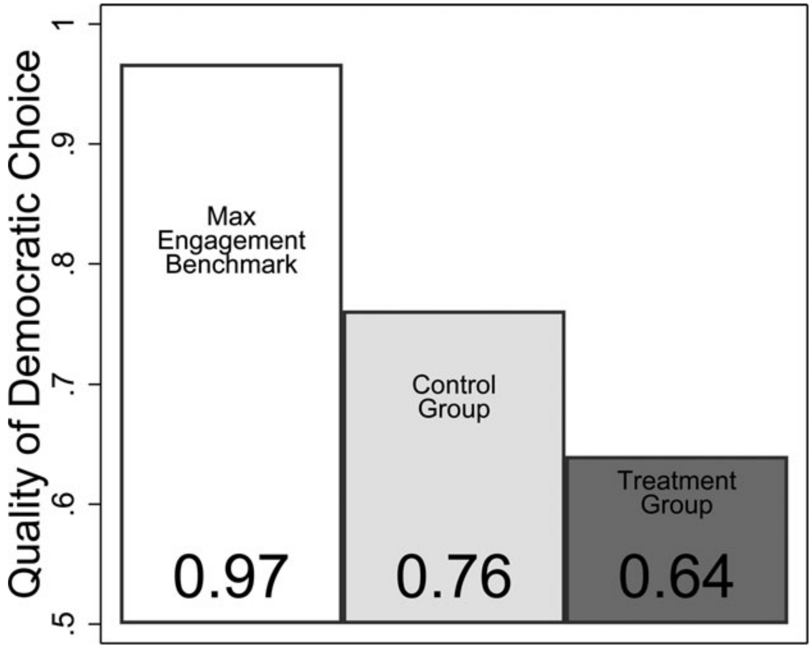

Fig. 6. The quality of democratic choice by treatment.
Note: In both the control and treatment groups, the estimates are based on actual votes of expert voters whose willingness to vote is greater or equal to voting cost. We use electorate as the unit of independent observation. We use electorate level averages in the analysis and perform Mann-Whitney and exact Fisher-Pitman permutation tests. The achieved quality of democratic choice in each group is smaller than the max. eng. benchmark (p<0.003).

Using standard results (e.g., Hartley and Fitch, 1951), and the Leibniz integral rule, the first-order condition can be reduced to (details in the Appendix):

$$\frac{\bar{c}^{\beta}(1-\bar{c})^{M-\beta-1}}{\int_0^1 t^{\beta}(1-t)^{M-\beta-1}dt}+\frac{1}{2}\binom{M}{\beta}\bar{c}^{\beta-1}(1-\bar{c})^{M-\beta-1}[\beta-M\bar{c}]=\frac{M}{N}\bar{c}.$$

Then, since $N = 5$, and using standard properties of the β function, an interior $\bar{c}^*(M;\beta)$ solves:

$$5(M-1)(M-2)\bar{c}^2(1-\bar{c})^{M-3}+5\binom{M}{2}\bar{c}(1-\bar{c})^{M-3}[2-M\bar{c}]=\bar{c}.$$

Moving on to the Control Group, the utilitarian planner chooses a symmetric vote cost cutoff depending only on the partisan bias, β, which we denote by $\bar{c}^*(\beta)$.[11] As above, for a supportive partisan bias, $\bar{c}^*(\beta=-2)=0$. For a partisan bias of $\beta \in \{0, 2\}$, and a probability of having political expertise of γ, the utilitarian planner's problem in the control group is

$$\max_{\bar{c}\in[0,1]} N\left[\sum_{k=\beta+1}^{N}\binom{N}{k}(\gamma\bar{c})^k(1-\gamma\bar{c})^{N-k}+\frac{1}{2}\binom{N}{\beta}(\gamma\bar{c})^{\beta}(1-\gamma\bar{c})^{N-\beta}\right]-\frac{N}{2}\gamma\bar{c}^2.$$

Using that $\gamma = 0.7$ and $N = 5$ in our experiment, similar techniques as above allow us to reduce the

[11]If the planner's prescriptions depended on the realized number of expert voters, then this information would be revealed to voters through the planner's prescription, and the Control Group would be the same as the Treatment Group.

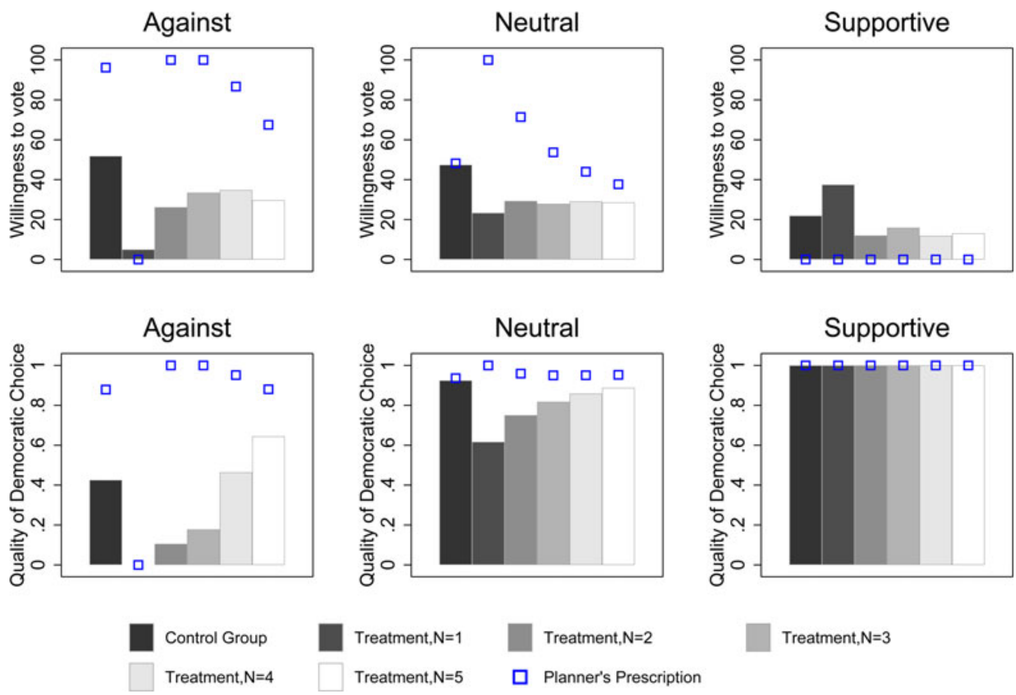

Fig. 7. Utilitarian optimal and experimental results by partisan bias.
Note: The vertical axis of figures in the first row shows the willingness to vote. The vertical axis of figures in the second row shows the quality of democratic choice.

planner's first-order condition in the Control Group to

$$10.5(0.7\bar{c})^2(1-0.7\bar{c})^2+0.35\binom{5}{2}\bar{c}(1-\bar{c})^2[2-3.5\bar{c}]=3.5\bar{c}.$$

We report the numerical values of the utilitarian planner's choice problems, both in terms of willingness to vote and quality of democratic choice, in Figure 7 and compare them to our experimental results.[12] From these results, it is straightforward to see that subjects in our experiment dramatically under-participate when the partisan bias is against (relative to what is economically efficient). When the partisan bias is against, the observed willingness to vote is always significantly different from the planner's prescription at the one percent level except in the Treatment Group when there is one expert voter. We see similar under-participation when the partisan bias is neutral by members of the Treatment Group. When the partisan bias is neutral, the willingness to vote in the experiment is always significantly different from the planner's prescription at the one percent level except when there is only one expert voter. This type of under-participation relative to what is economically efficient is consistent with free-ridership, and thus, it is not surprising that we see similar behavior in our experiment. Interestingly, in the Control Group, and when the partisan bias is neutral, the average willingness to vote is statistically identical to the utilitarian planner's prescription. And perhaps more surprisingly, in contrast to the other cases, when the partisan bias is supportive, voters *over-participate* relative to what is economically efficient, in both the Treatment and Control groups (and the difference is always statistically significant at one percent level). Ultimately, our experiment is not designed to adjudicate

[12]Detailed information is reported in online Appendix I.3.

what causes voters to under-participate or over-participate relative to what is economically efficient. Yet, the results of our experiment suggest that even in a laboratory environment, voters are motivated by non-material concerns, suggesting also that voter behavior may not be fully captured by pivotality concerns.

5. Conclusion

We show that even if the general distribution of political expertise is the same, when voters know concretely that there are other voters with expertise, they are less willing to politically engage. We report evidence from a laboratory experiment, where our main treatment effect isolates the relationship between voters' (possibly degenerate) beliefs regarding the number of expert voters and expert voters' willingness to vote, and consequently, the quality of democratic choice, which is the probability that the majority preferred alternative is reached through voting.

From a methodological perspective, our study departs from canonical designs where voters are only asked to make discrete vote choices. Instead, we directly elicit expert voters' willingness to vote, meaning the largest cost they are willing to incur to ensure that their vote is counted, which has at least two advantages over canonical designs. By directly eliciting a voter's desire to participate, our design contains more identifying variation than canonical designs. Second, canonical designs are forced to "back out" voters' incentives, often positing some structural model, which typically undermines causal interpretations of the experimental evidence. We are thus able to precisely investigate the causal effect of the concreteness of social knowledge, and moreover, show that our main results are the consequence of decreased engagement at the individual level.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/psrm.2021.65.

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