Beyond rational choice theory: how collectivism and biased probability assessments help explain costly voting

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Abstract

In this paper, we explore behavioral mechanisms that may help explain why voter turnout is often in excess of that predicted by rational choice theory. In particular, we explore two channels that that cause such 'over-voting': (1) voters may act beyond narrow self-interest in that they consider their decision part of a cooperative, collective action with other voters, or (2) voters may overestimate the likelihood of influencing the outcome of the election due to subjective probability weighting. We find that both collectivist inclinations and probability weighting are positively associated with voting behavior. In experimental elections, we find that voters are more likely to incur a personal cost to vote if they score higher on a measure of psychological collectivism, i.e., the proclivity to internalize group goals as one's own, to care for the outcome of one's group, to trust other members of one's group, and to follow group norms. Similarly, we find that cooperativeness in incentivized prisoner dilemma games also correlates with higher levels of voter turnout. Furthermore, we find that participants who assign more psychological weight to small probabilities, as measured with incentivized gambles, are more likely to incur a cost to vote. In addition to explaining the source of some over-voting, our results imply that collectivist and probability weighting individuals are over-represented in voting pools.

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1 Introduction

Since the seminal work of (Downs, 1957), economists have puzzled over the paradox of voting. In their rational choice framework, potential voters choose to either pay a cost to vote for their preferred party, or to abstain. If the expected benefits of voting do not outweigh the cost of voting, then it is irrational for an individual to incur the cost to vote. The expected benefit of voting is equal to the benefits associated with their preferred option winning the election, relative to the alternative, weighted by probability that one's vote is pivotal (i.e., it changes the outcome of the election). Thus, due to the ever-decreasing likelihood of any one voter having a pivotal impact on the outcome of the election, voter turnout rates are predicted to decline as the size of the electorate grows (Downs, 1957; Palfrey and Rosenthal, 1985), and to be near zero in large elections. This is obviously contrary to what is observed in experience, both in terms of real-world elections and voting experiments (Duffy and Tavits, 2008; Morton and Tyran, 2012; Faravelli, Kalayci and Pimienta, 2019). Key questions relevant to the basic functionality of democracy remain: why do certain voters participate much more than this rational choice framework would predict, and are these voters representative of the larger population?

In this paper, we explore experimentally whether this paradox of voting can be explained by known behavioral biases that may lead to a distortion of this simple cost-benefit analysis. In a costly voting experiment, we measure individual behavioral traits relevant to the cost-benefit analysis behind the voting decision and document the hypothesized empirical relationship between those traits and voter participation. In the process, we show that the pool of active voters is selected from the larger population according to traits that plausibly also affect vote choice in real-world elections.

In particular, we consider whether voters (1) act beyond narrow self-interest when engaging in the collective action of voting, or (2) over-estimate the likelihood of influencing the outcome of the election.[2]

The first channel invokes a type of collective reasoning or preference which goes beyond individual self-interest. In our experimental setting, the election can be viewed as a competition between two teams to gather the most votes. Voters who identify with this team effort, or who are generally inclined to participate in collective efforts, may therefore obtain

[2] One could also consider perceptions of the benefits of voting (voters may have different opinions about how good each option will be for public welfare, for example) or risk preferences. The former we render moot in our experimental setting and the latter cannot explain over-voting on average (Agranov et al., 2018). We control for risk preferences and empirically verify that risk aversion anti-correlates with voting.

non-monetary utility from participation.

The second channel is implicated by prospect theory (Tversky and Kahneman, 1992; Kahneman and Tversky, 1979), or subjective probability weighting in particular. In essence, in the absence of significant learning opportunities, individuals tend to treat small probability events as more likely than they objectively are (van de Kuilen, 2009). Because the probability of being a pivotal voter is small, especially in larger elections, individuals may over-evaluate the expected benefits of voting if they exaggerate this likelihood. We therefore hypothesize that overevaluation of small probabilities increases individuals' willingness to vote.

We present an experiment testing for these effects in a private value election, based on the costly voting model of Palfrey and Rosenthal (1985) and the experimental framework of Faravelli, Kalayci and Pimienta (2019). Each experimental election involves two parties (A and B). Each voter is independently assigned to party A with probability γ and to party B with probability $1 - \gamma$. Each voter is given an independent choice to either incur a cost and vote for his or her own party or to abstain from voting. Each voter's individual voting cost is private information, as is their party affiliation, although the distribution of private costs, γ, and the size of the electorate are all common knowledge. The party that obtains the most votes wins the election, which entitles each of its members to a payout.

Because personal traits cannot be randomly assigned to voters, we instead randomly assign voters to treatments and measure these traits in a host of tasks independent from the experimental election. We verify that these traits are indeed stable regardless of whether they are measured before or after voting, supporting their interpretation as exogenous personal parameters.

We use two methods to measure individual variation in collectivism, drawing from the psychological and economic literature:

Psychological collectivism as a sociological and psychological construct describes a number of cultural preferences (Triandis, 1995; Jackson et al., 2006). Highly collectivist individuals view themselves as parts of a wider group (e.g., a family, tribe, or nation), they emphasize their connectedness to others, they tend to subordinate themselves to the goals of the social collective, and they are highly motivated by group norms (Jackson et al., 2006; Triandis, 1995). In contrast, individualistic types are motivated by personal rather than group goals, they value independence and self-reliance and focus on their own individual needs and rights rather than those of the wider collective (Triandis, 1995). We hypothesize that individuals with collectivist inclinations will be more likely vote, and therefore employ an adapted version of Jackson et al.'s (2006) psychological collectivism survey measure.

While economic games are less suitable for eliciting nuanced aspects of personality, they can indirectly measure these traits by capturing their behavioral implications in relevant simple scenarios. They also have the advantage of incentive compatibility. To this end we supplement our psychological measure of collectivism with a series of prisoner's dilemma games that gauge each voter's willingness to set aside their own material interests for a common good. Together, we capture both incentivized actions and the deeper psychological factors that drive those actions.

Subjective probability weighting is measured using an incentivized pairwise lottery-choice task, similar to those employed by Holt and Laury (2002) and Wu and Gonzalez (1996). We also measure risk aversion and several other traits that provide useful controls, described in more detail in Section 2.2 below.

Our results can be summarized as follows. First and foremost, we find strong evidence that individuals with collectivist inclinations are more likely to incur a cost to vote. We find a one standard deviation increase in the score on Jackson et al.'s (2006) collectivism measure leads to a statistically significant 4.4% increase in the probability of voting. Similarly, we find evidence for a positive association between cooperativeness in strategic interactions and voting behavior: a one standard deviation increase in cooperativeness in our prisoner's dilemma games increases the probability of voting by 7%.

Secondly, we find that individuals who give more psychological weight to small probabilities are more likely to incur a cost to vote. A one standard deviation increase in the score on a small probability weighting measure leads to a statistically significant increase in the probability of voting by 5.5%.

Our study is motivated first of all by the theoretical and empirical literature on costly voting when voters act collectively. Several models have proposed that preferences that can be broadly described as collectivist or cooperative can overcome the Downsian paradox, going back to Riker and Ordeshook's (1968) seminal calculus of voting that posits that a key role for the benefit a citizen experiences from the act of voting itself, whether that benefit derives from pride in performing a their civic duty or expressing preferences or other factors (Fiorina, 1976; Tullock, 1971; Brennan and Buchanan, 1984; Brennan and Lomasky, 1993; Schuessler, 2000; Jankowski, 2002; Edlin, Gelman and Kaplan, 2007). Harsanyi (1980), Feddersen and Sandroni (2006), and Coate and Conlin (2004) make a more specific assumption about the civic duty that voters try to fulfill: they suppose that individual voters are rule-utilitarians, meaning that they prefer to follow the strategy that maximizes group welfare if followed by everyone. Another strand of literature studies how groups working as units can solve the paradox of voting, via collectivist preferences, social pressure, or other mechanisms Morton (1987); Uhlaner (1989); Morton (1991); Schram and van Winden (1991); Grossman and Helpman (2001); Palfrey and Pogorelskiy (2019); Levine and Mattozzi (2020).

The basic premise that individuals vote because they have collectivist (broadly defined) or expressive preferences leads to comparative static predictions about electoral results when aspects of an election, such as size or vote mechanism, are changed (Huck and Konrad, 2005; Edlin, Gelman and Kaplan, 2007; Feddersen, Gailmard and Sandroni, 2009; Morgan and Várdy, 2012; Kaplow and Kominers, 2020). Several papers have experimentally tested these predictions, with somewhat encouraging results overall (Carter and Guerette, 1992; Fischer, 1996; Tyran, 2004; Feddersen, Gailmard and Sandroni, 2009; Shayo and Harel, 2012). Another strand of literature focuses on the social pressure to vote, or to vote altruistically, rather than the innate preferences of voters (Gerber, Green and Larimer, 2008; Dellavigna et al., 2017; Morton and Ou, 2019; Khalil, Mookerjee and Tierney, 2019; Palfrey and Pogorelskiy, 2019, e.g.). We take a more direct approach of measuring whether individuals who are relatively more collectivist are also relatively more likely to vote. One other study using a similar approach is Fowler (2006), who measures individual voters' altruism and finds that partisan altruists, at least, are more likely to vote. We focus, instead, on collectivist or cooperative preferences that may drive voters to want to "do their part" to help their side win an election regardless of altruistic concerns, along with other cognitive biases that are predicted to be associated with voter turnout.[3]

While the hypothesized non-material utility of voting calls on social preferences, the importance of the likelihood of pivotality to the calculus of voting calls on risk preferences. The basic premise, that a higher likelihood of pivotality leads to greater voting, has already been established experimentally by eliciting beliefs about this likelihood (Duffy and Tavits, 2008) or by confirming other key predictions of the pivotal voter model (Battaglini, Morton and Palfrey, 2009; Levine and Palfrey, 2007; Faravelli, Kalayci and Pimienta, 2019).[4] Risk aversion itself (and/or loss aversion) depresses the motivation to vote and has been rejected as an explanation for the paradox of voting (Agranov et al., 2018), but subjective probability weighting has not been explored to our knowledge. Relatedly, however, Herrmann,

[3]Touching on similar themes, (Schram and Sonnemans, 1996) find some suggestive evidence that selfish versus cooperative goals, as rated by participants, are associated with behavior in a participation game.

[4]Indirect evidence using field data is less conclusive: Gerber et al. (2020) find that while polls indicating close elections cause people to update their beliefs accordingly, these beliefs do not affect voting behavior in the gubernatorial elections they study, but Khalil, Mookerjee and Tierney (2019) find that overall turnout levels in earlier election phases in India depress later turnout, consistent with voters who are concerned with being pivotal.

Jong-A-Pin and Schoonbeek (2019) present a yet-to-be tested model which incorporates probability weighting alongside several other tenets from prospect theory. While we do not comprehensively test Herrmann, Jong-A-Pin and Schoonbeek's (2019) model in this paper, we do provide supportive experimental evidence showing that individuals who assign more psychological weight to small probabilities are more likely to vote.

The remainder of the paper is structured as follows: Section 2 describes the experimental design, measures, procedures, and hypotheses. Section 3 reports our findings and Section 4 concludes.

2 Experimental Design and Hypotheses

2.1 Experimental Design

Our experimental design is based on Palfrey and Rosenthal's (1985) costly voting model and the experimental design of the incomplete information voting game of Faravelli, Kalayci and Pimienta (2019). The model assumes that there are N potential voters split between two parties (A and B). The probability of a voter belonging to A (B) is γ $(1-\gamma)$. Both N and γ are common knowledge. Each voter is given an independent choice to either vote for his or her own party or to abstain from voting altogether. Voting incurs a cost which differs from voter to voter, drawn from a commonly-known uniform distribution. Each voter's individual voting cost is private information, as is their party affiliation. There is no cost associated with abstaining. The party that obtains the most votes wins the election, which affords each one its affiliates a payoff equal to π. Affiliates of the losing party do not receive anything. In the case of tie, everyone receives $\frac{\pi}{2}$.

Following Faravelli, Kalayci and Pimienta (2019), we utilize a 2×2 between-subjects experimental design, with N and γ as the treatment variables (see Figure 1): In small elections, $N = 20$, and in large elections, $N = 200$. In lopsided elections, $\gamma = 0.35$, and in close elections, $\gamma = 0.49$.[5]

These treatments allow for the demonstration (replicating Faravelli, Kalayci and Pimienta (2019)) of three known effects inherent to the costly voting model: (1) smaller electorates will lead to higher turnout rates (the size effect), (2) closer elections will result in higher turnout rates (the competition effect), and (3) higher voting costs will decrease the likelihood of

[5]In line with Faravelli, Kalayci and Pimienta (2019) we opt for 0.49 instead of 0.5 for the close election treatment condition to avoid perfect symmetry. This is because subjects may interpret 0.5 as being an equal split between parties A and B.

Figure 1: Treatment groups

	$N = 20$	$N = 200$
$\gamma = 0.49$	*Small close*	*Large close*
$\gamma = 0.35$	*Small lopsided*	*Large lopsided*

voting (the cost effect). These effects are additionally predicted to interact with behavioral traits, however, which is our focus in this study. When the pivotal probably is relatively high, in small and/or close elections, even non-collectivist voters may be rationally inclined to vote. As the likelihood of affecting the outcome drops, we may observe a stronger selection effect in which turnout is dominated by highly collectivist voters. Similarly, overweighting of small probabilities may not be relevant when the pivotal probability is relatively high, while large and/or skewed elections could lower the pivotal probability into the range in which voters who overweight small probabilities are significantly more likely to vote.

Formally, we test four hypotheses, corresponding to the effects of psychological collectivism or cooperativeness, or to the subjective over-weighting of small probabilities, on voter turnout, and the expected interaction between these traits and the probability of being a pivotal voter:

Hypothesis 1. *Participants who more highly identify with a collectivist orientation, or who are more inclined to behave cooperatively, will be more likely to incur a cost to vote.*

Hypothesis 2. *Participants who assign more subjective weight to small probabilities will be more likely to incur a cost to vote.*

Hypothesis 3. *The association between collectivism and voting propensity is stronger in large and/or skewed elections.*

Hypothesis 4. *The association between overweighting of small probabilities and voting propensity is stronger in large and/or skewed elections.*

2.2 Measures of individual traits

To measure for collectivism, we utilized an adapted version of the psychological collectivism survey shown by Jackson et al.'s (2006) to be a reliable, internally consistent, and valid measure of individual differences in collectivist inclinations within group settings. The survey

Measure Item
1. I felt comfortable counting on other group members.
2. I was not bothered by the need to rely on group members.
3. I felt comfortable trusting other group members.
4. I care about the wellbeing of the group.
5. I was concerned for the needs of the group.
6. I followed the norms of the group.
7. I cared about the goals of the group more than my own goals.
8. I emphasized the goals of the group more than my own individual goals.
9. Group goals were more important to me than individual goals.

Table 1: Adapted Jackson et al. (2006) psychological collectivism survey. The instructions participants read were as follows: "Think about the groups to which you currently belong, and have belonged to in the past, such as work groups or social groups. The following questions below ask about your relationship with, and thoughts about, these groups. Respond to the following questions, as honestly as possible, using the response scales provided (1= Strongly Disagree to 5 = Strongly Agree)."

consists of fifteen five-point Likert-scale questions. Six of the original survey's questions, pertaining to preferences about working in groups versus alone and other irrelevant features of the setting, were not included due to their lack of applicability. The remaining nine questions were adopted and are shown in Table 2.2. These questions measure the proclivity to internalize group goals as one's own, to care for the outcome of one's group, to trust other members of one's group, and to follow group norms. The sum of the responses for all nine questions, out of 45, is taken as our measure. Answers were not incentivized, but we included a tenth question to check that respondents were paying attention. Respondents who failed this check were omitted from any analysis making use of the collectivism measure (our results do not change if we include them). [6]

To supplement this psychological measure, we also utilized prisoner dilemma games to measure proclivity to cooperate in real decisions. Subjects were paired together to play a series of seven prisoner dilemma games simultaneously against each other. One interaction from each series was randomly selected to count towards the overall payoff for the partici-

[6] The vetting question utilized a five-point Likert scale in an identical fashion to the other questions in the survey, and it read as follows: 'To show you're paying attention, please select option 'Agree' below'.

pants. The relative payoffs for cooperation increased with each successive game in the series. We recorded participants' propensity for cooperation by examining their switching points between defaulting and cooperating across the seven games (a unique switching point was enforced); the total number of cooperative choices made is taken as our measure, out of seven possible. Additionally, we vetted for understanding and concentration on this task with the first of the seven games, in which mutual "defection" Pareto dominates mutual cooperation. Anyone who nonetheless chose to cooperate in this game was omitted from any analysis that makes use of this measure (however our results do not change if we include them). The complete list of prisoner's dilemma games and associated instructions are provided in the appendices.

Subjective probability weighting was measured using an incentivized pairwise lottery-choice task, similar to those employed by Holt and Laury (2002) and Wu and Gonzalez (1996). In each decision, participants had to choose between a guaranteed \$0.25 and a gamble involving a probability weighting level. We then recorded how participants responded to small ($\rho = 0.01$), medium ($\rho = 0.5$) and large ($\rho = 0.99$) probabilities by examining their switching points across a series of six different lottery-choice decisions for each of these three weightings. The total number of risky choices, out of six possible, is taken as our measure of probability weighting at each probability level. To control for the potentially confounding effects of risk aversion, we also adopted Holt and Laury's (2002) risk aversion measure. This again utilizes incentivized lottery-choice decisions, but changes the variance of the potential payoffs, keeping probability weighting fixed. The total number of safe choices, out of ten possible, serves as our measure of risk aversion. In addition, we also vetted for concentration and understanding in both the risk aversion and probability weighting tasks by including one lottery decision in each series with a clearly dominant option. Anyone choosing the non-dominant option in these choices was omitted from any analyses making use of these measures (however our results do not change if we include them).

We additionally attempted to use an incentivized elicitation device to measure misunderstanding of the law of large numbers (Benjamin, Rabin and Raymond, 2016) in order to test an additional hypothesis about the relevance of this bias to voting. Because responses were almost uniformly extremely inaccurate, we are not able to draw conclusions based on this data and will omit related discussion for the sake of brevity.

In addition to these measures, we also included an updated, less-used version of the cognitive reflective task (Thomson and Oppenheimer, 2016), a 5-item Raven's intelligence test (Bilker et al., 2012), and key demographic questions in the survey for use as control variables. For further detail, the complete instructions are provided in the Online Appendix.

2.3 Procedures

A total of 800 subjects took part in the experiment. Subjects were recruited through Amazon's Mechanical Turk and access was restricted to users located in the USA. The experiment was conducted via Qualtrics, an online survey platform. Participants were randomly selected to play the election game either before or after the completion of the collectivism survey, pairwise lottery-choice decisions, prisoner dilemma games, and other elicitation tasks; all participants answered a set of questions specifically about voting behavior and opinions on the very last page. Participants received a base payment of \$0.75, while a number of additional payoffs were available conditional on the outcomes of elicitation tasks and the voting game itself. Below, we describe the procedure of the voting game in detail.

In order to increase the external validity of the the experiment, the language used throughout the experiment referred to the voting game as an election. Prior to commencing the voting game, each participant was given the following information: There are N people, him/herself included, who are taking part in the election. There are two groups, A and B, to which he or she will be assigned. The computer will determine the group the subject belongs to by generating a random number between 1 and 100. All numbers are equally likely to be drawn. If the number falls between 1 and $100 \times \gamma$, the subject will belong to group A, while if the number falls between $100 \times \gamma + 1$ and 100, then the subject will belong to group B. Both γ and $1 - \gamma$ are then depicted as percentages in a pie chart. The subject is informed that the same procedure applies to all other participants. The computer then generates the number and the subject is assigned a group. The subject is then informed of how the payoffs are determined. The subject is informed that they will receive an automatic payoff of \$1. If they abstain from voting, they will keep all of the automatic payoff. However, if they choose to vote, they will incur a cost, which is deducted from their automatic payoff. The subject is then informed of their voting cost, which is a random number of cents drawn uniformly from $[0, 100]$. This distribution is common knowledge but individual costs are privately known. Following this, the subject is then told that the outcome of the election will determine whether or not they receive an additional payoff. If more people from their group pay the cost to vote than do others from the opposing group, they will receive a \$2 winning payoff. They are informed that they will receive \$1 if the election is a tie, and if they lose, they will receive nothing. The subject is then given the option to vote and incur the cost, or to abstain.

Participants were randomly assigned to the four treatment groups, with 200 subjects recruited to each treatment. After omitting responses that failed one of the included attention checks, provided nonsensical free-text responses to questions, or that came from duplicate IP addresses, our final sample size was 770. The experiment began on 19 October 2019 and lasted approximately 24 h. In total, we had 20 small elections (10 close and 10 lopsided) and 2 large elections (1 close and 1 lopsided). On average, participants took 17.8 min to complete the experiment. Subjects were paid within three days of completion. The average payment to participants was $4.06, which included the base payment of $0.75. This equates to approximately $13.68 per hour, which at the time of this study was nearly twice that of the US federal minimum wage ($7.25).

3 Results

To check for order effects, participants were randomly selected to play the election game either before or after the completion of the collectivism survey, prisoner dilemma games, pairwise lottery decisions, and other elicitation tasks. Our results confirm that all of these measures are independent of prior experience with the voting game, lending confidence to our interpretation of them as exogenous psychological factors. On the other hand, we observe significant order effects in the voting game itself, and therefore present results only for the cohort who played the voting game prior to completing the other tasks ($N = 391$) in order to avoid cross-contamination.[7] Table 3 shows descriptive statistics for this primary sample. In all four treatment groups, we find no statistically significant differences in terms of the demographic variables—income, education, age, and ethnicity (Mann–Whitney tests, p-values > 0.05).

As expected, consistent with prior findings (Levine and Palfrey, 2007; Morton and Tyran, 2012; Faravelli, Kalayci and Pimienta, 2019), we observe voter turnout rates that are significantly larger than theoretical predictions, consistently across all four treatment conditions. Table 3 shows observed voting rates along with theoretical predictions based on symmetric Bayesian Nash equilibria for each voting game. While voters do respond to the lower likelihood of being pivotal in large and/or skewed elections by voting less often in those cases, voting rates are nonetheless higher than expected in all elections, and especially in large and/or skewed elections.

We proceed with a probit regression framework, with the binary choice to vote in the

[7] Our main conclusions, however, continue to hold in the full sample.

	Full Sample	Small Close	Large Close	Small Skewed	Large Skewed
Annual Income	\$48,989	\$50,809	\$47,608	\$48,406	\$48,883
Education	4 yr college	4 yr college	4 yr college	4 yr college	4 yr college
Age	35	35	35	36	35
Male	57.29 %	52.38 %	60.87 %	61.54 %	55.33 %
African American	9.46 %	9.52 %	10.87 %	7.69 %	9.71 %
Observations	391	105	92	91	103

Table 2: Descriptive statistics of the primary sample. Mean annual income is calculated by converting a categorical variable to a continuous scale. The categorical variable takes k values from 1–10, with 1 indicating an annual gross income below \$20,000 and 10 indicating an annual income of \$100,000 or above. Education is a categorical variable taking on 7 possible states, ranging from 1 (completion of high school education) to 7 (completed doctoral or professional (JD,MD) degree). Age is median age.

	Small ($N = 20$)	Large ($N = 200$)
Close ($\gamma = 0.49$)	**73.33%** (58.16%)	**65.22%** (26.98%)
Skewed ($\gamma = 0.35$)	**60.44%** (50.23%)	**55.34%** (16.79%)

Table 3: Voter turnout rates: observed vs theoretical predictions. Bold values represent the observed voting rates and bracketed values represent the predicted equilibrium voting rates for a rational voter under each treatment.

experimental election as the dependent variable. Logit and probit models were both considered, however, there were no statistically significant differences in terms of log-likelihood criteria between them. Further, the marginal effects of the coefficients across both model types were almost identical. We therefore adopt the probit specification out of convenience. Further, all of the models presented have been tested for probit-specific heteroskedasticity and have been found to be homoskedastic. All control variables (sex, income, education, ethnicity, religious beliefs, political orientation, marital status, children, and scores from both the cognitive reflection task and the Raven's intelligence test), and independent variables other than the treatment indicator variables, have been standardized to mean 0 and variance 1.

Tables 4, 5 and 7 all show that our two treatment dimensions, election size and skewness, affect voting in the predicted directions (relative to "Small Close" elections, which are predicted to have the highest likelihood of pivotality and highest voting rates) and these effects often, but not for all treatment indicators in all specifications, reach statistical significance. The individual cost of voting reliably dissuades voting.

Turning to our first hypothesis, we find that voters who are more collectivist, as measured either with the psychological collectivism survey or the incentivized prisoner dilemma games, are overall indeed more likely to vote across all treatments. Columns 1 and 2 of Table 4 show that the relationship between voting and cooperativeness in prisoner's dilemma games is large, statistically significant, and robust to the inclusion of demographic controls. A one standard deviation increase in cooperativeness is associated with a 7% increase in the likelihood of voting (p-value 2.2%). Columns 1 and 2 of Table 5 show the same for the relationship between voting and psychological collectivism: a one standard deviation increase in collectivism increases the likelihood of voting by 4.4% (p-value 3.6%)

Based on this analysis, we conclude in support of Hypothesis 1:

Result 1. *Those who more highly identify with a collectivist orientation, or who are more inclined to behave cooperatively, are more likely to incur a cost to vote.*

These results imply substantial selection of collectivist voters into the active electorate. In our analyzed sample, voters are on average nearly one quarter of a standard deviation (23%) more cooperative than non-voters. Because this represents real willingness to forego personal profit in order to cooperate with another anonymous individual, this may imply that outcomes of low-turnout elections are biased in favor of these preferences.

Our second hypothesis regards subjective probability weighting: *ceteris paribus*, participants who give more psychological weight to small probabilities, will be more likely to incur

	(1)	(2)	(3)
Cost to vote	-0.483	-0.515	-0.527
	(0.081)***	(0.085)***	(0.087)***
	[-0.159]	[-0.162]	[-0.164]
Small Skewed	-0.275	-0.331	-0.330
	(0.230)	(0.244)	(0.272)
	[-0.091]	[-0.104]	[-0.103]
Large Close	-0.203	-0.263	-0.027
	(0.223)	(0.235)	(0.282)
	[-0.067]	[-0.083]	[-0.008]
Large Skewed	-0.392	-0.468	-0.422
	(0.220)*	(0.233)**	(0.257)
	[-0.129]	[-0.147]	[-0.131]
Cooperativeness	0.221	0.221	0.105
	(0.095)**	(0.099)**	(0.193)
	[0.073]	[0.069]	[0.033]
Cooperativeness × Small Skewed			-0.005
			(0.282)
			[-0.002]
Cooperativeness × Large Close			0.428
			(0.291)
			[0.133]
Cooperativeness × Large Skewed			0.092
			(0.274)
			[0.029]
N	287	287	287
Controls?	N	Y	Y

Table 4: Cooperativeness and the probability of voting. The dependent variable indicates voting in the experimental election. All columns are estimated using probit regressions using the sub-sample who passed the attention check embedded in the cooperativeness elicitation. Vote cost, cooperativeness, and all controls have been normalized to have mean 0 and variance 1. Standard errors reported in parentheses and marginal effects are shown in square brackets. Statistical significance is indicated at .99 (***), .95 (**) and .90 (*) confidence levels.

	(1)	(2)	(3)
Cost to vote	-0.449	-0.483	-0.484
	(0.071)***	(0.074)***	(0.075)***
	[-0.149]	[-0.151]	[-0.151]
Small Skewed	-0.394	-0.428	-0.424
	(0.196)**	(0.204)**	(0.206)**
	[-0.131]	[-0.134]	[-0.132]
Large Close	-0.261	-0.275	-0.287
	(0.198)	(0.208)	(0.209)
	[-0.087]	[-0.086]	[-0.090]
Large Skewed	-0.568	-0.590	-0.589
	(0.189)***	(0.196)***	(0.197)***
	[-0.189]	[-0.184]	[-0.184]
Collectivism	0.149	0.142	0.147
	(0.064)**	(0.069)**	(0.130)
	[0.050]	[0.044]	[0.046]
Collectivism × Small Skewed			0.037
			(0.188)
			[0.011]
Collectivism × Large Close			-0.079
			(0.190)
			[-0.025]
Collectivism × Large Skewed			0.010
			(0.182)
			[0.003]
N	390	390	390
Controls?	N	Y	Y

Table 5: Psychological collectivism and the probability of voting. The dependent variable indicates voting in the experimental election. All columns are estimated using probit regression using the subsample that passed the attention check embedded in the psychological collectivism survey. Vote costs, collectivism, and all controls have been normalized to have mean 0 and variance 1. Standard errors reported in parentheses and marginal effects are shown in square brackets. Statistical significance is indicated at .99 (***), .95 (**) and .90 (*) confidence levels.

	Small ($N = 20$)	Large ($N = 200$)
Close ($\gamma = 0.49$)	**20.5%** (23.2%)	**6.48%** (10.8%)
Skewed ($\gamma = 0.35$)	**9.48%** (20.1%)	**0.0000000641%** (6.7%)

Table 6: Probability of casting a pivotal vote. Probabilities implied by empirical voting rates are shown in bold, while theoretical predictions based on rational voter equilibrium are indicated in parentheses. Casting a pivotal vote requires that an individual's choice to vote either makes or breaks an even tie between the two parties.

a cost to vote because the chance of being pivotal in most elections (including these) is small. Table 3 presents the probabilities that any one participant will cast a pivotal vote under each treatment condition of our election game; as can be seen, the chances of being a pivotal voter are small under all conditions and decrease significantly in larger and skewed elections. We therefore focus our analysis on the role of overweighting of small probabilities, which we measured by observing how participants treated gambles with 1% chances of gains. Our other two measures of probability weighting, using 50% and 99% probabilities, serve as placebos.

Columns 1 and 2 of table 7 show that overweighting of small probabilities is indeed associated with higher voting rates. Because acceptance of small probability gambles can indicate either overweighting of small probabilities or risk lovingness, we control for risk aversion as well, which is, as expected, negatively related to voting. A one standard deviation increase in our 1% probability weighting measure is associated with a 5.5% increase in the likelihood of voting (p-value 3.0%). Column 3 shows our placebo tests: as expected, overweighting of intermediate and high probabilities are not related to voting behavior in this setting in which the probability of being a pivotal voter is low.

In light of this analysis, we conclude in support of Hypothesis 2:

Result 2. *Participants who assign more subjective weight to small probabilities, are more likely to incur a cost to vote.*

These results again imply substantial selection effects in the active electorate. In our analyzed sample, voters score on average more than one quarter of a standard deviation (27%) higher on our measures of probability weighting than non-voters. Because this measure reflects real willlingness to accept low-probability gambles, this indicates that electoral outcomes may be biased in favor of these preferences.

Turning our attention to interaction effects with our exogenous manipulation of pivotal

	(1)	(2)	(3)	(4)
Cost to vote	-0.537	-0.632	-0.633	-0.637
	(0.082)***	(0.094)***	(0.094)***	(0.095)***
	[-0.167]	[-0.178]	[-0.178]	[-0.177]
Small Skewed	-0.382	-0.565	-0.563	-0.500
	(0.235)	(0.257)**	(0.257)**	(0.274)*
	[-0.119]	[-0.159]	[-0.158]	[-0.139]
Large Close	-0.371	-0.474	-0.475	-0.464
	(0.227)	(0.244)*	(0.244)*	(0.244)*
	[-0.115]	[-0.133]	[-0.133]	[-0.129]
Large Skewed	-0.621	-0.781	-0.779	-0.818
	(0.222)***	(0.244)***	(0.244)***	(0.246)***
	[-0.193]	[-0.220]	[-0.219]	[-0.227]
Risk Aversion	-0.260	-0.265	-0.267	-0.267
	(0.087)***	(0.094)***	(0.097)***	(0.096)***
	[-0.081]	[-0.074]	[-0.075]	[-0.074]
Probability Weighting 1%	0.141	0.195	0.192	0.201
	(0.084)*	(0.091)**	(0.094)**	(0.193)
	[0.044]	[0.055]	[0.054]	[0.056]
Probability Weighting 50%			0.006	
			(0.099)	
			[0.002]	
Probability Weighting 99%			-0.017	
			(0.091)	
			[-0.005]	
Probability Weighting 1% × Small Skewed				0.159
				(0.303)
				[0.044]
Probability Weighting 1% × Large Close				-0.259
				(0.254)
				[-0.072]
Probability Weighting 1% × Large Skewed				0.138
				(0.247)
				[0.038]
N	305	305	305	305
Controls?	N	Y	Y	Y

Table 7: Subjective probability weighting and the probability of voting. The dependent variable indicates voting in the experimental election. All columns are estimated using probit regression using the subsample that passed the attention checks embedded in the probability weighting and risk aversion elicitations. Vote cost, risk aversion, probability weighting, and all controls have been normalized to have mean 0 and variance 1. Standard errors reported in parentheses and marginal effects are shown in square brackets. Statistical significance is indicated at .99 (***), .95 (**) and .90 (*) confidence levels.

probabilities via our two treatment dimensions, our results do not support Hypotheses 3 or 4. Column 3 in Table 4 shows that adding interaction effects between cooperativeness and the treatment indicator variables does not improve the model; a likelihood ratio test comparing the fully interacted model to the simpler version in column 2 fails to establish statistical significance of the set of interactions (p-value 0.36). Similarly, a likelihood ratio comparing the interacted model in column 3 of Table 5 to the baseline effect in column 2 fails to support the former (p-value 0.88). And again, the joint effect of the interactions shown in column 4 of Table 7 is insignificant in a likelihood ratio comparison to column 2 (p-value 0.30). If, instead of using the treatment indicator variables and their interactions as independent variables, we directly use the implied probabilities of being pivotal resulting from the observed turnout rates in each election type, we draw the same conclusion (this is analysis is omitted for brevity). While voters do respond to the probability of being pivotal overall, the interactions between this and the individual traits of interest are insignificant. We have the following conclusions:

Result 3. *The association between collectivism or cooperativeness and voting propensity is not strongly related to the probability of being pivotal as induced by election size and skewness.*

Result 4. *The association between subjective probability weighting and voting propensity is not strongly related to the probability of being pivotal as induced by election size and skewness.*

Both results could simply be because the interaction effect is too subtle to be observed in this experiment, but there are also other possible explanations. Result 4 may simply be due to the fact that the probability of being pivotal in any of the election types is fairly low and perhaps subjective probability weighting applies, on average, similarly across this range of probabilities.

More likely, the ability to detect such interaction effects, or their existence at all, may be undermined by the inaccuracy of voters' beliefs about their chances of being pivotal. It is well-established that, in general, individuals do not intuitively understand the law of large numbers (Benjamin, Moore and Rabin, 2017, e.g.), which implies in this setting that voters do not fully understand how pivotal probabilities change when electorates grow. In the context of elections specifically, previous experiments have established that voters do not have accurate beliefs about pivotal probabilities. Duffy and Tavits (2008) find that, consistent with our results, voters do respond by voting more on average when the probability of being pivotal is higher, but they persistently overestimate this probability even after adjusting downwards with experience and learning. Gerber et al. (2020) similarly find that even though voters do update their beliefs about election outcomes based on poll information, they consistently overestimate the likelihood of very close elections. Conclusively establishing whether selection of collectivist and/or probability weighting voters is stronger when elections are large and skewed may require much more intensive individual measurement of beliefs and risk preferences than is readily feasible. Nonetheless, we can certainly conclude that these effects, if they exist, are relatively minor.

4 Conclusion

We find evidence that suggests that costly 'over-voting' is associated with the individual traits of either (1) acting in such a way that considers the collective of other voters, or (2) over-evaluating the expected benefits of voting by way of subjective probability weighting bias. Whether collectivism is measured with a psychological survey or via incentivized cooperation tasks, concern for the group strongly predicts voter behavior, consistent with theoretical approaches that emphasize rule-utilitarianism and/or expressive voting. With the exception of Herrmann, Jong-A-Pin and Schoonbeek (2019), subjective probability weighting has received less attention in the voting literature, but we find that it plays an important empirical role.

These two mechanisms may help explain why voter turnout is often much higher than predicted by rational choice theory in both experiments and real-world elections. The importance of collectivism suggests that individuals perceive additional benefits of voting than their own personal outcomes. The importance of probability weighting, on the other hand, suggests that individuals over-weight the likelihood that they may actually influence the outcome of the election.

These results raise critical questions about how selection effects may influence election outcomes. In our experimental setting, participants who select into the voter pool are significantly more collectivist, and significantly more inclined to take low-probability gambles, than non-voters. In real-world elections this may translate to election outcomes that over-weight social interests and low-probability opportunities relative to the true distribution of preferences in the population. Future work is needed to assess the extent of this phenomenon in real elections.

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