# Beliefs and Voting Decisions: A Test of the Pivotal Voter Model

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*We report results from a laboratory experiment testing the basic hypothesis embedded in various rational voter models that there is a direct correlation between the strength of an individual's belief that his or her vote will be pivotal and the likelihood that individual incurs the cost to vote. This belief is typically unobservable. In one of our experimental treatments we elicit these subjective beliefs using a proper scoring rule that induces truthful revelation of beliefs. This allows us to directly test the pivotal voter model. We find that a higher subjective probability of being pivotal increases the likelihood that an individual votes, but the probability thresholds used by subjects are not as crisp as the theory would predict. There is some evidence that individuals learn over time to adjust their beliefs to be more consistent with the historical frequency of pivotality. However, many subjects keep substantially overestimating their probability of being pivotal.*

Why do people vote? While many theories have been offered (for a survey see Dhillon and Peralta 2002), the simplest and most widely used framework is the pivotal voter model (Ledyard 1984; Palfrey and Rosenthal 1983, 1985; see also Downs 1957; Tullock 1967). This model asserts that voters have only instrumental concerns—their motivation is to affect the outcome of the election as opposed to noninstrumental motivations, such as warm-glow altruism—and that in making the decision to vote they are rational, self-interested expected payoff maximizers. In particular, people vote if the expected benefit of voting is greater than the cost.

This model has widespread appeal but it is simultaneously the most extensively debated theory in political science (Green and Shapiro 1994, 47–48). The problem is straightforward: the expected benefit calculation involves the voter's probability that he or she will be pivotal to the election outcome. As in large electorates, where this probability is very small, rational citizens should not vote. This, however, contradicts the evidence. It is this paradox that feeds the rational choice controversy (Friedman 1995). Indeed, the apparent anomaly has led to the search for an extra term—the "D-term" or "a sense of civic duty"—to make voting rational (Riker and Ordeshook 1968). However, this explanation remains theoretically unrewarding (Bendor, Diermeier, and Ting 2003).

Given this central controversy in the discipline, it is curious that empirical studies examining the assumptions and predictions of the pivotal voter model are scarce and indirect. Field data can usually provide only weak tests of the model as they pose challenge to measurement and provide little control over extraneous factors (see Levine and Palfrey 2007). Among the difficulties are the unobservability of voters' costs of voting, benefits from an election victory, and their beliefs as to whether they will be pivotal to the election outcome—all of which play a critical role in the theory (Green and Shapiro 1994, 47–71).

Undoubtedly, the greatest controversy surrounds the measurement and relevance of the probability of any voter being pivotal—the trademark of the rational choice theory of turnout (Aldrich 1993; Foster 1984; Green and Shapiro 1994, 47–71). Various proxies have been used to measure pivotality, such as the expected or perceived closeness of the election (Blais and Young 1999; Blais,

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Young, and Lapp 2000; Ferejohn and Fiorina 1975; Foster 1984; see also Matsusaka and Palda 1993 for a review) and the size of the electorate (Hansen, Palfrey, and Rosenthal 1987; see also Bendor, Diermeier, and Ting 2003, 274–75). However, these proxies have been criticized as being a "far cry" from the actual concept of pivotality (Aldrich 1993, 259; Cyr 1975, 25; Green and Shapiro 1994, 54–55; Shachar and Nalebuff 1999). Survey measures, such as whether the respondent has thought of the possibility that his or her vote might decide the election, or whether the respondent thinks the probability of such an event is higher than "absolute zero" or "almost zero" (Blais, Young, and Lapp 2000), or replacing decisiveness by political efficacy (Clarke et al. 2004) provide interesting insights into turnout decisions, but remain imprecise for measuring pivotality. Thus, the tests based on these proxies cannot be considered as tests of the pivotal voter model (Merlo 2006).[1]

An alternative to working with field data is to conduct laboratory experiments that enable one to control both the cost of voting and the payoff to the party that wins. Using neutral language and anonymous interaction experiments can minimize other factors that might affect voting decisions, such as the fulfillment of "civic duty" or the avoidance of peer sanctions for nonparticipation. Several prior experimental studies have tested various aspects of the pivotal voter model, including the implications of different voting rules (plurality vs. proportional) (Schram and Sonnemans 1996a), communication, group identity, and individual characteristics such as the student's university major (Schram and Sonnemans 1996b), various comparative static predictions including the effects of variations in electorate sizes (Großer, Kugler, and Schram 2005; Levine and Palfrey 2007), exogenously varying the pivot probabilities by designating active individuals whose vote determines the outcome (Feddersen, Gailmard, and Sandroni 2007), and asymmetric information (Battaglini, Morton, and Palfrey 2005). However, none of these prior studies has examined subjects' beliefs about being pivotal and assessed the extent to which subjects (1) form correct beliefs and (2) appropriately condition their behavior on those beliefs—questions that lie at the heart of the pivotal voter model.

In this article, we present results from a series of laboratory experiments. We adopt the neutral language participation game design (Großer and Schram 2006; Schram and Sonnemans 1996a, 1996b)[2] and add to it a belief elicitation stage that precedes the voting stage. In the belief elicitation stage, we ask subjects to state a subjective probability as to whether their own decision to vote or not will be decisive for the election outcome. We incentivize truthful revelation of individual beliefs using a proper scoring rule, and subject earnings are determined in small part by the *ex post* accordance of their beliefs with election outcomes.[3] In addition, we are able to study whether subjects learn over time to form correct beliefs with regard to their pivotality in the finitely repeated election game. In sum, our study provides the first *direct* test of the pivotal voter model.

We find that average participation rates are consistent with the theoretical prediction, suggesting that the model works well on an aggregate level. However, our main interest is on the individual level. Here we provide evidence that subjects are more likely to vote the higher their subjective beliefs of being pivotal—as prescribed by the pivotal voter model. The predicted probability of participating is more then twice as high for those who are certain of being pivotal than for those who believe that their chance of being pivotal is zero. On the other hand, we find that subjects consistently overestimated the probability that their decision to vote or abstain would be pivotal, though this difference declined somewhat with experience. Furthermore, the fit between their beliefs about decisiveness and turnout was considerably worse than the theory predicted: many subjects whose perceived pivotality probability was higher than the cost of voting did not vote while many of those who stated a probability considerably lower than the cost of voting still decided to participate.[4] Overall, thus, the evidence with regard to the pivotal voter model is mixed. Yet, the study should not be interpreted as an attempt to "prove" or "disprove" the pivotal voter model or the rational choice theory in general. Rather, the purpose has been to uncover those aspects of the theory that are useful for understanding turnout decisions.

[1]Coate, Conlin, and Moro (2004) test the pivotal voter model by looking at turnout in local Texas elections and considering closeness as a measure of pivotality. However, as above, this does not provide a direct test of the model. See also Battaglini, Morton, and Palfrey (2005, 21) on how such tests are not nuanced enough as tests of the pivotal voter model.

[2]See Palfrey and Rosenthal (1983) and Schram and Sonnemans (1996a) for a justification why turnout decision can be represented as a participation game.

[3]Several other experimental studies have sought to elicit subjects' subjective beliefs in environments other than the voting game that we examine (Costa-Gomes and Weizsäcker 2005; Croson 2000; McKelvey and Page 1990; Nyarko and Schotter 2002; Offerman, Sonnemans, and Schram 1996; Rutström and Wilcox 2004). The evidence from these studies regarding the impact of belief elicitation procedures on subject behavior is mixed. For this reason, we report data from our own control treatment without belief elicitation for the purposes of comparison.

[4]As detailed below, we normalized the benefits from one's preferred candidate winning to one and set the cost of voting to 0.18.

Our findings are for small groups of 20 subjects. An obvious issue is whether our experimental findings "scale up" to larger electorate sizes, where the probability of being pivotal is likely to be closer to zero. We see no reason why our findings should not scale up, but acknowledge that this claim is difficult to test.[5] Conducting controlled laboratory experiments with much larger populations is not presently feasible; Internet experiments do not provide the same level of control, as one cannot rule out communication or collusion among subjects, and survey evidence is not directly comparable to laboratory findings.

On the other hand, the laboratory provides the pivotal voter theory with an *idealized* test environment—one where factors other than pivotality (such as civic duty or the sanction of others) have been carefully removed, and where subjects are given much more experience and information concerning election outcomes and pivotality than they might ordinarily encounter as voters in real elections. If the theory does not predict well in this idealized environment (with admittedly few participants), we might expect it to perform rather poorly in the less-controlled world of real elections with large numbers of participants.

## Pivotal Voter Model

We consider the complete information participation game approach to modeling voting pursued by Palfrey and Rosenthal (1983). Specifically, there are two teams of players of size $M$ and $N$, and all team members have a choice between two actions, vote (participation) or do not vote (abstention/nonparticipation). The cost of voting $c \in (0, 1)$ is assumed to be the same for all agents; abstention is costless. Each member of the winning team receives a payoff benefit $B > 0$, while each member of the losing team earns a payoff of zero. The utility function is assumed to be linear, as is standard in the literature. Specifically, letting $p$ denote the probability of casting a pivotal vote, the *net return* to voting, $R = pB - c$. Note that we abstract away from any fixed benefits to voting, such as the utility one gets from a "civic duty" to vote or from the avoidance of sanctions from not voting; our neutral language experimental design makes such concerns unimportant. Normalizing $B = 1$, it follows that players will rationally choose to vote whenever $p > c$, and will rationally choose to abstain if $p < c$.

[5]As Börgers (2004, 57) observes, "This paradox [of voting] suggests that a conventional game-theoretic analysis of costly voting is out of place if large electorates are considered. By contrast, for small electorates there seems to be no reason why observed voting behavior should not be rational."

The rule used to determine the outcome of voting is simple plurality. As for ties, we flipped a coin *in advance* of each election to determine which team would win in the event of a tie; the pre-announcement of the winner in the event of a tie aids in assessments of pivotality (as described later). Given the pre-announcement of the tie-breaking rule, the setting corresponds to the "status quo" rule where there is a default winner in the event of a tie.

For our setting with $M = N > 0$ and the status quo rule, it follows from Palfrey and Rosenthal (1983) that there are no pure strategy equilibria. There may exist quasi-symmetric, totally mixed strategy equilibria where each member of the group that does not win a tie chooses to vote with probability $q$, defined implicitly by

$$\left(\frac{M+N-1}{N}\right) q^{N}(1-q)^{M-1} = c, \tag{1}$$

and members of the group that wins a tie vote with probability 1–$q$. As Palfrey and Rosenthal (1983) show, there exist values of $c$ for which equation (1) yields either 0, 1 or 2 solutions for $q$.

We chose parameters for the experiment, $M = N = 10$ and $c = 0.18$, that are very close to the case where there is a unique, quasi-symmetric totally mixed strategy equilibrium. Our aim was to try to reduce the set of equilibria that subjects might coordinate on so as to have a more reasonable chance of predicting turnout.[6] In the unique mixed strategy equilibrium with $M = N = 10$, we have $q = N/(N + M - 1) = 0.53$ and $1 - q = 0.47$.[7] It follows that turnout in this equilibrium involves $(2M - 1)N/(N + M - 1) = 10$ participants out of an electorate of size 20, or a turnout rate of 50%. While turnout is of interest to us, the primary focus of this article is on the consistency of subjects' beliefs with their action choices. We now turn to a description of our experimental design and main hypotheses.

## Experimental Design and Hypotheses

The computerized experiment was run at the Experimental Economics Laboratory of the University of Pittsburgh.

[6]There may also exist *asymmetric* equilibria, where some agents play pure strategies while others play mixed strategies, but for simplicity, we follow Levine and Palfrey (2005) and Battaglini, Morton, and Palfrey (2006) and focus on symmetric equilibria only.

[7]The value of $c$ needed to implement the unique mixed strategy equilibrium is 0.17697. Given that the smallest increment of monetary payment is 0.01, we chose to set $c = 0.18$. Technically speaking, for $c = 0.18$, there are two totally mixed strategy equilibria, $q_1 = 0.514883$ and $q_2 = 0.53773$, but we prefer to consider $q = 0.53$ as the relevant benchmark.

Subjects were recruited from the university's student population using newspaper advertisements and email. Each subject participated in only one session and had no prior experience with our experimental setup or knowledge of our research agenda. The only demographic data we collected was on gender; 53.6% of our subjects were female and the fraction of females in each session ranged from 45% to 65%.

Our experimental design involved two treatments. In the "beliefs" treatment we elicit subjects' beliefs as to whether their voting decision will be pivotal to the election outcome prior to their voting decision. In the "control" treatment, we do not elicit beliefs. Thus the control treatment enables us to determine whether eliciting subjective beliefs with regard to pivotality affected behavior, for example, made subjects more likely to carefully weigh the expected benefit from voting against the cost.[8] We conducted three sessions of the control treatment and four sessions of the beliefs treatment.

## Control Treatment

In the control treatment, subjects were randomly assigned to one of two groups labeled X and Y at the start of the experimental session. We were careful to use neutral language in both treatments and avoid any context with regard to "voting" or "elections" as we did not want to cue subjects' beliefs with regard to social norms or sanction surrounding voting decisions. Subjects were told that in each "round" of the experiment (20 rounds total), they were to decide whether to purchase a "token" or not (equivalent to casting a vote or abstaining). Purchasing a token cost them $0.18, i.e., we set the cost of voting to $c = 0.18$. The payoff to each member of the winning group is $1, while the payoff to each member of the losing group is $0.

The experimental instructions, available at http://www.pitt.edu/~jduffy/pivotalvoter, made the payoffs to the winning team and the cost of buying a token public knowledge to all subjects. In addition, the instructions explained the plurality rule used to determine the winning group and the pre-announced tie-breaking rule which was to pick one team randomly each round to be the winning team in the event of a tie. Prior to the start of the experiment, subjects had to answer several quiz questions designed to test their comprehension of the rules and payoffs for the experiment. Subjects played 20 rounds of this game, remaining in the same team over all rounds.[9] They were paid their net earnings from all 20 rounds played.

The timing of moves within a round was as follows. First, the random determination as to which team will win a tie was made and announced. Second, subjects were asked to decide whether or not to purchase a token. Finally, the results of the round were revealed to all subjects. Specifically, at the end of each round, subjects were informed of the number of members of their group of 10 who purchased a token, the number of members of the other group of 10 who purchased a token, and which group had won for that round. In the event of a tie, the pre-announced tie-breaking rule determined the winning group. All members of the winning group earned $1 less the cost of purchasing a token, if they purchased a token. Similarly, all members of the losing group earned $0 less the cost of purchasing a token, if they purchased a token.

Notice that in each round of the control treatment, subjects' *net* earnings consist of one of four possible payoffs: $1, $0.82, $0, or −$0.18; the latter negative payoff occurs when a subject buys a token and his or her team loses. To rule out the possibility that subjects finish the experiment with a net loss, we provided subjects with a $6 show-up fee. As we only played 20 rounds of the voting game, the maximum loss possible was $20 \times (-0.18) = \$3.60$ and subjects were informed that such losses would come out of their show-up fee. In practice, all subject payments (including the show-up payment) were greater than $6 for both treatments. The average total payoff earned by subjects in the three control sessions was $14.55 for a 90-minute experiment.

## Belief Elicitation Treatment

The belief elicitation treatment differed from the control treatment in only one respect. Prior to deciding whether or not to buy a token, subjects were asked to report their subjective belief as to whether their decision to buy a token would be decisive (pivotal) or not.[10] To aid subjects

[8]There is conflicting evidence on the obtrusiveness of belief elicitation procedures (see, for example, Offerman, Sonnemans, and Schram 1996; Rutström and Wilcox 2004).

[9]We considered random rematching of subjects into the two teams each period so as to avoid "repeated game" effects, but we decided that such a design might adversely affect subject learning, especially with regard to the probability that any individual subject is pivotal. A second consideration is that the natural field settings in which our results would be most applicable are ones that likely involve repeated interactions among the *same* individuals, e.g., members of a political party. For these reasons, we chose to have subjects remain as members of the same team in all 20 rounds played.

[10]For current purposes, we consider the terms "decisive" and "pivotal" as synonyms. In the instructions we used the term "decisive" in order to make the concept easier to understand for the subjects. As explained below, subjects were given a precise working definition of "decisive."

in formulating this belief, the conditions under which their decision to buy or not buy a token would be decisive were carefully explained in the experimental instructions. The decisiveness conditions made use of the fact that one group was randomly selected at the start of each round to be the winning group in the event of a tie.

The timing of moves within a round was as follows. First, the random determination as to which team would win a tie was announced. Second, subjects stated their subjective belief as to whether their decision to purchase a token would be decisive. Third, subjects were asked to decide whether or not to purchase a token. Finally, the results of the round were revealed. The information revealed at the end of each round included the same information that was revealed at the end of a control session, and additionally, subjects were reminded of their stated belief and whether their token purchase decision was decisive or not for the outcome of the round. The latter information was intended to provide subjects with the feedback necessary to better align their decisiveness beliefs with actual outcomes.

It is perhaps useful to quote the instructions with regard to the conditions under which an individual subject's token purchase decision is decisive:

You are decisive under any of the following conditions.

Suppose that group X wins a tie.

1. If there is a tie then everyone in group X who *bought* a token is decisive.
2. If there is a tie then everyone in group Y who *did not buy* a token is decisive.
3. If group X loses by one token, then everyone in group X who *did not buy* a token is decisive.
4. If group Y wins by one token, then everyone in group Y who *bought* a token is decisive.

Suppose instead that Y wins a tie.

1. If there is a tie then everyone in group Y who *bought* a token is decisive.
2. If there is a tie then everyone in group X who *did not buy* a token is decisive.
3. If group Y loses by one token, then everyone in group Y who *did not buy* a token is decisive.
4. If group X wins by one token, then everyone in group X who *bought* a token is decisive.

These explanations provide a complete definition of being pivotal. However, as a referee suggested, they are somewhat complicated and, given the long list of pivot possibilities, subjects may overestimate their probability of being pivotal. Because of this concern, we conducted an additional experimental session replicating all aspects of the belief elicitation treatment described here, but providing a shorter and simpler definition of decisiveness. The revised definition reads as follows.

Your decision to buy or not buy a token is decisive if:

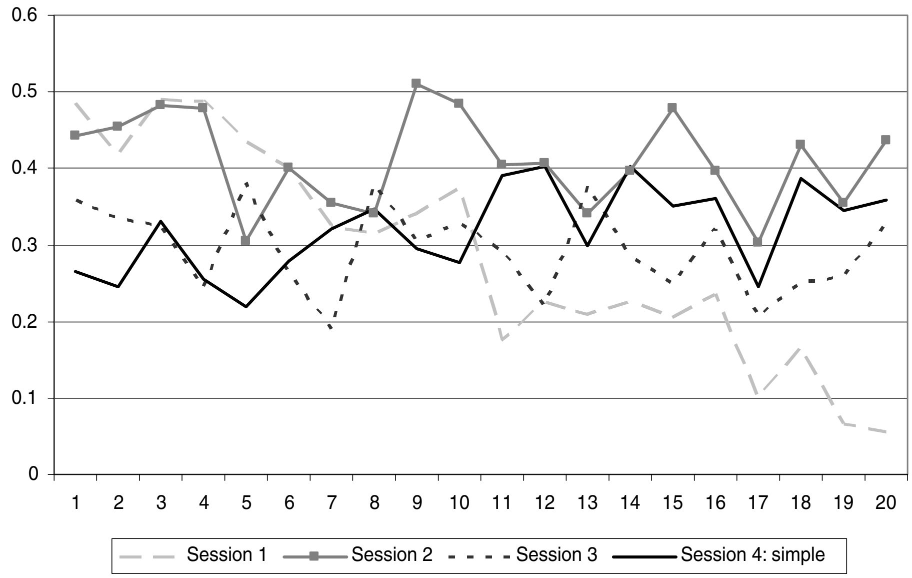
1. You are a member of the group that wins a tie and the number of tokens purchased by the other members of your group is one less than the number of tokens purchased by the other group.
2. You are a member of the group that loses a tie and the number of tokens purchased by the other members of your group is equal to the number of tokens purchased by the other group.

Unlike the longer definition above, these revised instructions focus on the decisions of *other* players in both groups. Thus, an additional benefit of these revised instructions is that they may help subjects realize that their belief of being decisive should be independent of their own decision to participate or abstain.

To make it incentive compatible for subjects to report their true beliefs regarding decisiveness, we used a proper scoring rule and gave subjects a small payment according to the accuracy of their stated beliefs. Specifically, we used the quadratic scoring rule originally developed by Brier (1950) for weather forecasting but more recently adopted by many experimentalists (McKelvey and Page 1990; Nyarko and Schotter 2002; Offerman, Sonnemans, and Schram 1996, among others). Suppose a subject reports the subjective probability $p$ that he or she will be decisive. *Ex post*, when the election results are determined, he or she is either decisive or not. Let $I_d$ be an indicator function that takes on the value "1" if the subject is decisive and "0" otherwise. The payoff we give to subjects for their stated belief each round is $\pi(p) = .010[1 - p - I_d)]^2$. That is, the maximum subjects can earn for a correct guess is \$0.10, and this amount diminishes quadratically as the guess deviates from the actual outcome, down to \$0.00. Theoretically, the quadratic scoring rule induces a risk-neutral agent to report his or her true, subjective belief with regard to the binary event, in our case, being decisive in the participation game (Camerer 1995, 592–93; Winkler and Murphy 1968). In setting the payoff for the decisiveness prediction, we followed Nyarko and Schotter (2002) in making this payoff small with respect to the payoff of winning an election (which was \$1). By keeping the payment for belief accuracy small, we sought to minimize strategic behavior in reporting of beliefs (e.g., as insurance against election outcomes).

Aside from elicitation of beliefs before voting decisions, there were no differences between the two treatments. Subjects in the belief elicitation treatment answered several additional quiz questions that tested their

**FIGURE 1 Average Subjective Decisiveness Probabilities across Rounds by Session**




comprehension of the decisiveness rules and payoff possibilities in the belief treatment. They earned slightly more on average ($15.75) than subjects in the control treatment, but the differences are easily accounted for by the additional payments subjects received for the accuracy of their beliefs.

Our main interest in the belief elicitation treatment is to assess whether subjects vote when their decisiveness beliefs exceed the cost of voting, $p > c = 0.18$, and abstain otherwise. We are also interested in whether subjects learn over time to adjust their beliefs toward the actual frequency of decisiveness.

# Results

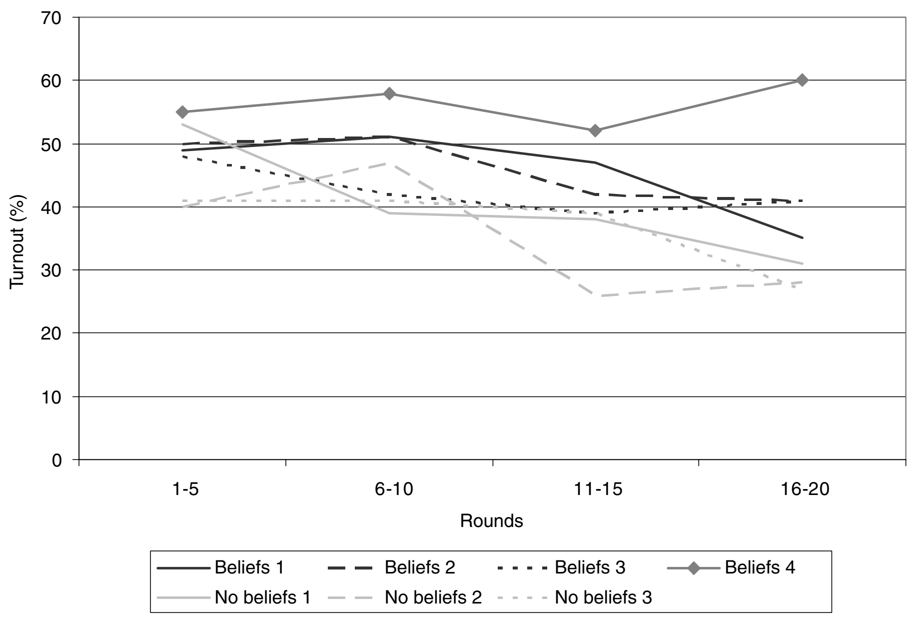
We report results from seven sessions—four belief elicitation (or treatment) sessions and three control sessions. Each session involved 20 subjects who made decisions in 20 rounds. Thus, there are 1,600 participation (or voting) decisions from the belief elicitation treatment and 1,200 from the control treatment, i.e., a total of 2,800 decisions. We begin with a discussion of whether changing instructions in the belief elicitation sessions altered subjects' behavior. This is followed by a brief review of aggregate results. The lengthiest part of the results section is devoted to the primary concern of this article—analyzing the individual-level behavior. Finally, we test the obtrusiveness of the belief elicitation procedure.

## Simple versus Complex Instructions

Figure 1 shows the average subjective decisiveness probability over all 20 rounds of the four belief elicitation sessions. If the more complex instructions systematically cause overestimation of the probability of being pivotal while simple instructions help avoid it, the series for the single session with simple instructions (session 4, represented by a black solid line) should stand apart from the lines representing sessions 1–3. However, this is not the case—the average subjective decisiveness probabilities in session 4 are very similar to those found in the other three sessions. Indeed, when looking at the rest of the graphs that present information by session and are discussed below (Figures 3–6), session 4 does not differ much from sessions 1–3. Including or excluding information from this session in the probit estimations (see Table 2) produces substantively very similar results. Given this, we can be rather confident that subjects' ability to estimate their pivot probabilities does not depend significantly on the complexity of instructions.[11]

[11]As the next section explains, the aggregate turnout in session 4 is higher than that in the other six sessions (see Figure 2). However, given that the turnout rate from session 4 represents a single case, it is hard to say whether this difference is systematic. If several additional sessions were run with simpler instructions, the average turnout across those sessions may still look similar to the average turnout from sessions with complex instructions. For our purposes, more important than the aggregate turnout is the original concern that complex instructions may introduce a bias into subjects' estimations of their pivot probabilities. The latter, however, appears not to be the case.

**FIGURE 2 Turnout Rates for All Sessions, Five-Round Averages**




## Aggregate Results

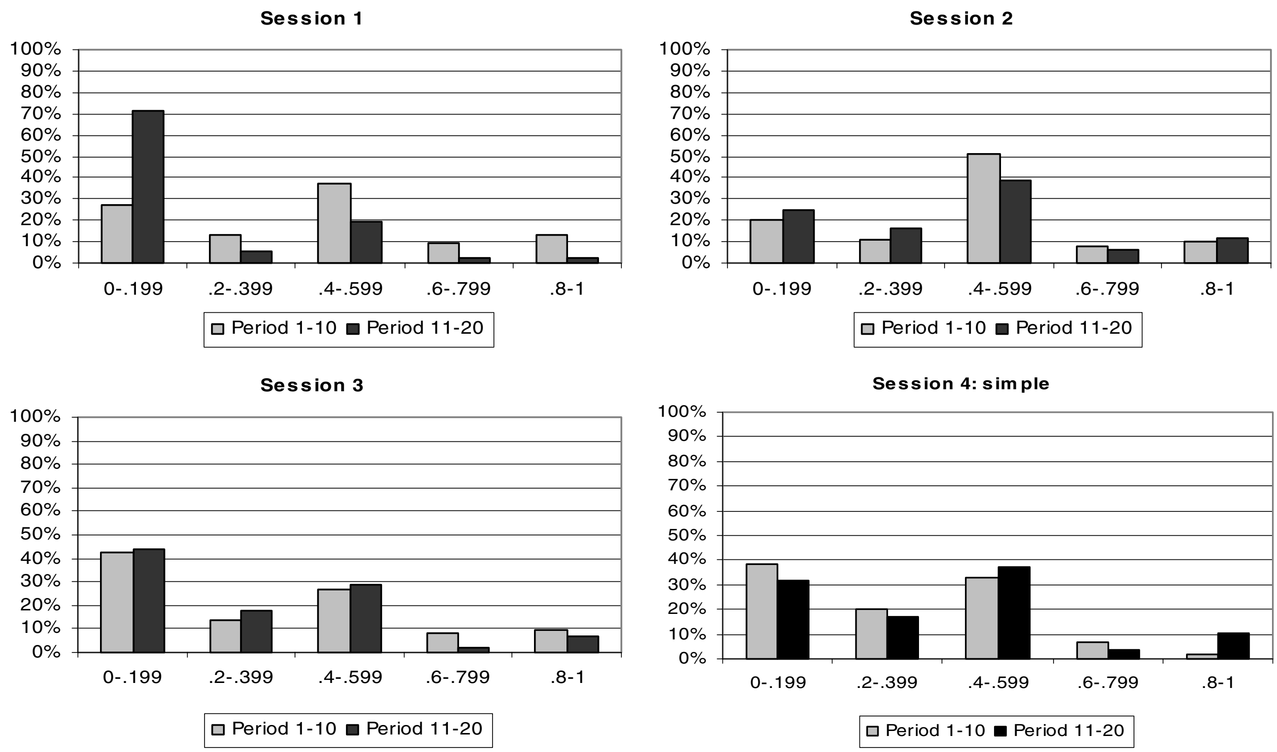
Figure 2 summarizes turnout using 5-round averages for each of the seven sessions, labeled beliefs or no-beliefs sessions 1, 2, 3, 4. The average turnout in rounds 1–5 is close to the theoretical prediction of 53%. However, in all sessions, except beliefs session 4, the turnout levels drop below 50% over time.[12] The differences in turnout between the two treatments are not large. Using data on 5-round averages as shown in Figure 1, and a nonparametric Mann-Whitney test, the null hypothesis of no difference in turnout rates between treatments can be rejected only in rounds 15–20 at the 0.05 level of significance. On the other hand, when the data are not grouped into 5-round averages, a Mann-Whitney test suggests that the overall average turnout rate (all rounds) is significantly higher (at the 0.05 level) for the four beliefs sessions (47%) than for the three no-beliefs sessions (39%). The latter finding is attributable to several factors, including the big drop-off in turnout in the no-beliefs sessions toward the end of those sessions; the high average turnout—56% (the closest to the theoretical prediction)—in beliefs session 4; and finally the fact that in no-beliefs session 2, one group became dominant, i.e., the same group won nearly every round, thus lowering participation rates in that session. The fraction of decisive games was very similar across treatments: in the beliefs sessions, 21 out of the 80 (14 out of 60 if beliefs session 4 is excluded) games resulted in a decisive participation, while in the control treatment the ratio was 12 out of 60. These aggregate findings do not allow us to draw strong conclusions about whether belief elicitation affected subjects' behavior. We will return to the issue of the potential obtrusiveness of the experimental treatment below. To foreshadow the conclusion, we find no significant differences in individual-choice behavior across treatments, suggesting the belief elicitation procedure was not obtrusive.

[12] Although this is not an entirely fair comparison given the differences in experimental design and theoretical predictions, other experimental studies of turnout or participation games in general, report participation levels similar to ours. For example, Schram and Sonnemans (1996b) use groups of 12 subjects to study turnout under different conditions. They do not give a theoretical prediction for aggregate turnout levels but report observed turnout of about 40%. (They use graphs rather than precise numbers to present aggregate turnout.) Großer et al. (2005) also use 12 subject groups and present graphs that report turnout levels of about 40–45%. Bornstein, Kugler, and Zamir (2005) use six subject groups and report average turnout of about 55%. Levine and Palfrey (2007) use groups of varying size and, unlike our study, an unequal number of players in each team. They report turnout levels of about 37% for sessions involving 27 or more subjects (the closest possible comparison to our 20-subject design). Similarly to our study, Levine and Palfrey observe turnout levels that are lower than theoretically predicted. However, Goeree and Holt (2005) have shown that for the type of binary choice games such as ours, observed participation rates tend, in general, to be lower than the theoretical prediction if that prediction is above 0.5, which is the case in our study.

## Individual-Level Results

The crucial independent variable in this study is the subjective decisiveness probability. Subjects could state a probability with an accuracy of up to three decimal places.

**FIGURE 3 Average Frequency Distribution of Subjective Decisiveness Probabilities over Ten Rounds by Sessions**




In all sessions, 0 and 0.5 were modal values, though many other values were chosen. The mean subjective probability that an individual is decisive is rather high: 0.33. It varies slightly by session, equal to 0.29 for the first and the third beliefs sessions, 0.41 for the second, and 0.32 for the fourth.

Figure 3 shows frequency distributions for the subjective decisiveness probabilities by session averaged over the first and last 10 rounds. As the graphs illustrate, subjects' decisiveness probabilities in the first 10 rounds are spread more uniformly over the interval [0,1] than the last 10 rounds, where the distribution is more skewed to the left of the interval.

On average, 63% of subjects across all four belief elicitation sessions stated a probability of being decisive that was higher than 0.18. Recall that $c = 0.18$; thus, the decisiveness probability of 0.18 serves as the theoretical cutpoint for participation. These subjective probabilities of being decisive can be compared to the actual probabilities, or the frequencies of past decisiveness. The actual mean frequency of decisiveness (all 20 rounds) averages out to be 0.149 across all four beliefs sessions. This average frequency is 0.05, 0.21, 0.13, and 0.21 for each session 1 through 4, respectively.[13] The difference between the historical average objective frequency of decisiveness and the subjective frequency of decisiveness is rather substantial. Figure 4 illustrates this difference by session; notice that the difference is always positive, but decreases with experience.[14] The convergence is especially visible in the case of the first session (solid line) where in the last two rounds the objective and average subjective probabilities are equal. This suggests that individuals *can* learn over time to adjust their subjective probabilities of decisiveness in response to histories of voting outcomes in the direction of the true *ex post* frequency of decisiveness. The positive values of the series in Figure 4 indicate that subjects are almost without an exception *overestimating* the probability.

Figure 5 compares average subjective decisiveness with the average actual decisiveness in each *round* of a session. It appears that subjects condition their beliefs on their actual experience of being decisive. Consider session 1: Here, actual decisiveness is a rare event that occurs only twice early in the session and subjects' stated beliefs

[13]In the mixed strategy equilibrium, the frequency of decisiveness would average 0.18.

[14]The average historical decisiveness at the start of round $t$ is the average frequency with which subjects have been decisive in all prior rounds $t = 1, \ldots, t-1$. Figure 4 plots the average difference between subjects' stated subjective probability of decisiveness for round $t$ and the average historical decisiveness at the start of round $t$, beginning with the second round, as average historical decisiveness cannot be ascertained prior to that round.

FIGURE 4 Difference in the Subjective Probability and Average Historical Frequency of Being Decisive

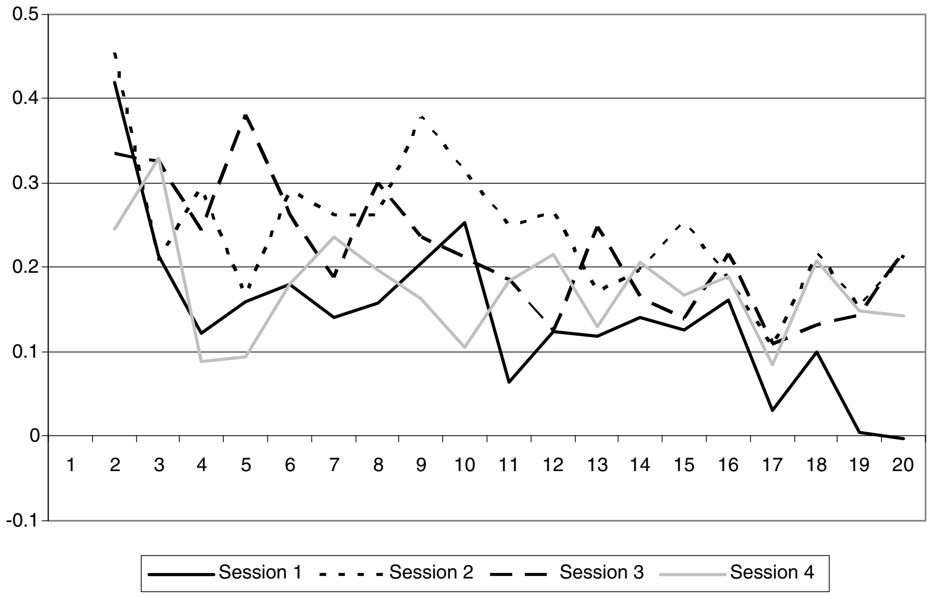
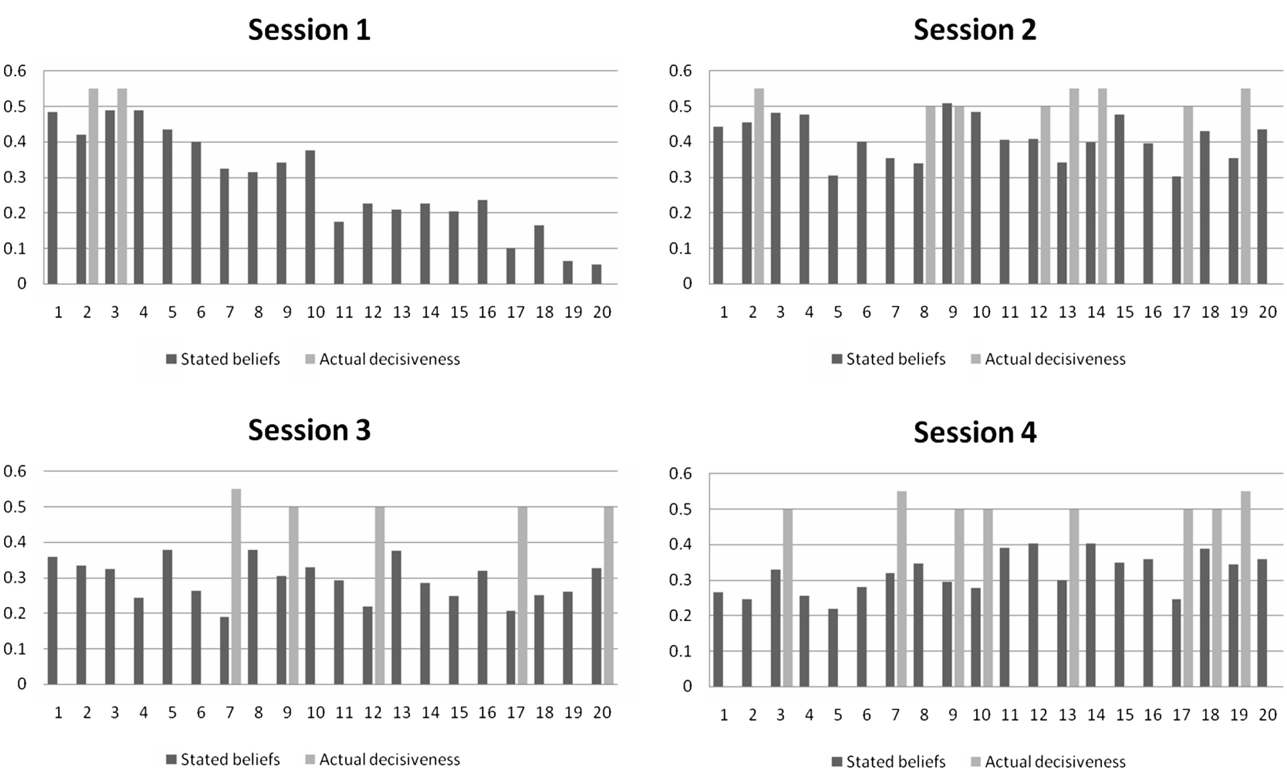



FIGURE 5 Average Subjective Decisiveness Probability and Average Actual Decisiveness in Every Round by Session



of being decisive decline correspondingly over time. In other sessions, especially in sessions 2 and 4, actual decisiveness occurs more frequently throughout the duration of the session. This helps sustain subjects' beliefs that they may be decisive at relatively high levels through all 20 rounds. As above, we can observe that subjects use the historical frequency of actual decisiveness to form their subjective beliefs about the probability of being decisive.

Given that subjects are overestimating the probability that they were pivotal, why is turnout not significantly greater than the 50% level predicted? Several explanations can be offered. First, while subjects were overestimating pivotality relative to the historical frequency of decisiveness which averaged 14.9%, the 50% turnout prediction is associated with a higher equilibrium frequency of decisiveness—18%. Second, as we emphasized below, subjects are not playing crisp best responses to their subjective beliefs, i.e., voting if and only if $p > 0.18$ and abstaining if $p < 0.18$. Once the notion of a strict best response is relaxed, the turnout prediction of 50% need no longer apply.[15] Finally and relatedly, we have not controlled for heterogeneity in risk attitudes and have assumed risk-neutral actors. By contrast, risk-averse types might abstain even if their pivotality beliefs were high, and risk-loving types might vote if their pivotality beliefs were low.

In addition to assessing the accuracy of beliefs, we can also examine the *payoff efficiency* of subjects' decisions in the beliefs treatment relative to best response and Nash equilibrium benchmarks. Specifically, we calculated the payoffs subjects *would have earned* had they played strict best responses to their subjective probabilities of pivotality in each round, i.e., if they had chosen to vote whenever their stated $p$ was greater than 0.18 and had chosen to abstain whenever their stated $p$ was less than 0.18 (no instances of $p = 0.18$ were found in the data). This hypothetical exercise will lead to different election outcomes and payoffs than are found in the actual data. In the event of ties, we used the actual, pre-announced tie-breaking rule for the round (which was announced *before* subjects submitted their subjective probabilities). We also calculated the payoffs subjects could have expected to earn if all had played according to the quasi-symmetric totally mixed equilibrium prediction, i.e., not only did they play best responses to their subjective beliefs but those subjective pivotality beliefs were correct.[16]

**TABLE 1 Ratios of Actual to Hypothetical Best Response and to Equilibrium Payoffs in the Four Belief Elicitation Sessions**

| Ratios of Actual to Best Response Payoffs | | | |
|---|---|---|---|
| **Session** | **Periods 1–10** | **Periods 11–20** | **Periods 1–20** |
| 1 | 1.112 | 0.952 | 1.024 |
| 2 | 1.149 | 1.168 | 1.159 |
| 3 | 1.059 | 1.070 | 1.064 |
| 4 | 1.023 | 1.060 | 1.041 |
| All 4 | 1.107 | 1.063 | 1.082 |
| **Ratios of Actual to Equilibrium Payoffs** | | | |
| **Session** | **Periods 1–10** | **Periods 11–20** | **Periods 1–20** |
| 1 | 1.00 | 1.04 | 1.02 |
| 2 | 0.998 | 1.037 | 1.018 |
| 3 | 1.024 | 1.042 | 1.033 |
| 4 | 0.972 | 0.974 | 0.973 |
| All 4 | 0.998 | 1.023 | 1.011 |

Table 1 reports the ratio of actual payoffs to hypothetical "best response" payoffs and to symmetric mixed equilibrium payoffs for each of the four beliefs sessions over the first 10, the last 10, and all 20 rounds.[17] We see that, with a few exceptions, subjects were generally earning slightly *higher* payoffs than they would have had they played either best responses to their subjective beliefs or according to the mixed strategy equilibrium. The average differences between actual and hypothetical payoffs are, in all instances, quite small—just a few cents. This finding

[15]One possible means of modeling noisy best responses is the quantal-response equilibrium approach of McKelvey and Palfrey (1995). Following Levine and Palfrey (2007), imagine that the probability of voting is characterized by a logit function,

$$q(c, p, \lambda) = \frac{1}{1 + e^{\lambda(c-p)}}$$

where $c$ is the cost of voting, $p$ is a player's subjective belief with regard to pivotality, and $\lambda$ is a parameter measuring the intensity of payoff considerations for voting decisions—or the "rationality" of subjects. If $\lambda = \infty$ as we have implicitly supposed, then subjects are perfectly rational and $q$ will equal 1 if $c - p < 0$ and $q = 0$ if $c - p > 0$. At the other extreme of complete irrationality where $\lambda = 0$ we have $q = 0.5$. Thus so long as $\lambda$ is finite (Levine and Palfrey 2007 estimate that $\lambda = 7$), this model of noisy best response moves voting probabilities and hence turnout closer in the direction of 0.5 and away from the higher turnout levels that would be associated with greater-than-equilibrium probabilities of pivotality.

[16]The expected payoff in the symmetric mixed equilibrium is calculated as follows: for the advantaged group, Pr (of at least a tie) = 0.5, so the expected per round payoff to members of this group from playing according to the equilibrium, where they vote with probability 0.47, is $0.5 - 0.47 \times 0.18 = \$0.4154$. For the disadvantaged group, Pr (of winning) = 0.5, so the expected payoff to members of this group in equilibrium is $0.5 - 0.53 \times 0.18 = \$0.4046$. Since a player is equally likely to be a member of either group, the expected symmetric equilibrium payoff per round is \$0.41.

[17]For simplicity, both the actual and hypothetical payoffs used in these ratios did not include the small payoff component that subjects earned for the accuracy of their stated beliefs.

suggests that, while subjects were not playing crisp best responses to their stated beliefs (more on this below), nor were their beliefs of pivotality consistent with the equilibrium prediction, they nevertheless appear to have been no worse off as the result, so their incentives to move further toward the rational choice, equilibrium prediction may have been weak.

## Multivariate Analyses

In order to further understand the effect of subjective beliefs of pivotality on the likelihood of buying a token, we have conducted a number of multivariate probit regressions. As individual decisions within sessions are not entirely independent, we have clustered the standard errors on subjects in all analyses. The results are presented in Table 2. Model 1 estimates the effect of the stated beliefs of being pivotal (continuous variable) on the decision to vote (binary variable) while Model 2 replicates the same analysis using a dummy variable coded "1" for those who stated a probability of being pivotal higher than 0.18 in order to test the exact predictions of the theory.

Both models include several controls. First, we control for whether the group of which the subject is a member will win in the event of a tie. This variable might also be thought of as proxying for a preelection poll announcing a lead to one candidate. The pivotal voter model predicts lower turnout for the "advantaged" group (see fn. 7; Levine and Palfrey 2007). Further, since we ran several rounds of "elections" and the group members stayed the same across rounds, we also control for various history effects. These include (1) whether a given subject was pivotal in the last round, (2) whether the subject bought a token in the last round, (3) whether the subject's group won the last round, (4) the number of tokens bought by the subject's group in the last round, (5) the subject's earnings from the last round, and (6) whether there was a tie in the last round. We also control for session effects using session dummies and for the round number.

The results of Model 1 show a strong effect of the stated probability of being decisive on the probability of buying a token. Substantively, the predicted probability of buying a token is 0.15 when the stated probability of being pivotal is 0 (i.e., at its minimum) and 0.34 when it is 1 (at its maximum), holding other variables at their mean (for continuous variables) or median (for categorical variables; session dummies are held at 0). Model 2 produces similar results—the predicted probability for buying a token is 0.15 when the stated probability of being pivotal is higher than 0.18 and only slightly higher, 0.26, when it is lower than 0.18, all other variables at their mean or median.[18]

These results suggest that the subjective probability of being pivotal plays a significant role in people's decision to participate: the higher the subjective probability the greater the likelihood of buying a token. The results are not, however, as crisp as the theory would predict: a subjective probability of 0.18 does not function as a clear cutpoint for the decision to participate. If subjects were playing according to the crisp cutpoint prediction of the theory, those who stated a probability of being pivotal greater than or equal to 0.18 should participate, while those who stated a lower probability should abstain. However, only 52% of the former participated and 60% of the latter abstained. Further, although the decisiveness probabilities of participants are usually higher than those of nonparticipants, there does not appear to be a clear average cutpoint for participation. Thus, there is only weak support for the specific prediction of the theory. Few participants use the exact deterministic cutpoint strategy predicted by the theory. However, there is evidence that subjects' behavior tends toward the theoretical prediction with higher subjective probabilities increasing the likelihood of participation. Furthermore, as discussed above, subjects' payoff efficiency is already approximately equal to that of a rational choice voter.

## Additional Findings

In addition to the main findings, some of the variables measuring the effects of history or past behavior are also significantly related to the decision to participate. First, round number or trend has a significant negative effect on the probability of buying a token: all else equal, subjects were less likely to buy a token in later than in earlier rounds. This may indicate a certain learning effect in terms of cumulative disappointment in low payoffs from buying a token, or the emergence of a free rider problem (see Bendor, Diermeier, and Ting 2003; Kanazawa 2000 for

[18]We also estimated models that included the average historical frequency of being decisive in addition to the other variables reported in Table 2, Models 1 and 2. This did not diminish the effect of the subjective probabilities of being decisive. Rather, the objective frequencies had a negative and no statistically significant effect on turnout while the effect of subjective beliefs remained significant and in the predicted direction. This underlines the importance of subjective beliefs of being pivotal in turnout decisions and challenges the use of some objective measures of this probability, such as closeness of an election, when testing the pivotal voter model. As we saw, although over time the subjective probability of being pivotal tends toward the actual frequency, the differences can be substantial.

**TABLE 2 Probit Models of the Effect of Subjective Decisiveness Probability on Turnout**

| | Model 1: beliefs b(SE) | Model 2: beliefs b(SE) | Model 3: no-beliefs b(SE) | Model 4: all sessions b(SE) |
|---|---|---|---|---|
| Beliefs elicited | | | | 0.151 |
| | | | | (0.187) |
| Subjective Pr(Decisive) | 0.615*** | | | |
| | (0.184) | | | |
| Subjective Pr(Decisive) >0.18 | | 0.389*** | | |
| | | (0.116) | | |
| Historical frequency of decisiveness | | | 1.219 | −0.024 |
| | | | (0.935) | (0.386) |
| Group wins tie | 0.231** | 0.239** | 0.549** | 0.365*** |
| | (0.111) | (0.113) | (0.129) | (0.079) |
| Decisive $t-1$ | 0.153 | 0.164 | 0.218 | 0.257*** |
| | (0.124) | (0.128) | (0.143) | (0.099) |
| Participate $t-1$ | 1.011*** | 0.995*** | 0.861** | 0.912*** |
| | (0.132) | (0.123) | (0.152) | (0.102) |
| Win $t-1$# | −0.010 | −0.012 | | 0.075 |
| | (0.083) | (0.083) | | (0.082) |
| Number of group tokens $t-1$ | −0.109*** | −0.107*** | −0.112** | −0.105*** |
| | (0.025) | (0.025) | (0.026) | (0.019) |
| Earnings $t-1$ | 0.042 | 0.018 | 0.322** | 0.077 |
| | (0.110) | (0.109) | (0.142) | (0.085) |
| Tie $t-1$ | −0.041 | −0.018 | 0.581** | 0.084 |
| | (0.127) | (0.130) | (0.185) | (0.103) |
| Round (trend) | −0.008* | −0.007 | −0.044** | −0.023*** |
| | (0.005) | (0.005) | (0.007) | (0.004) |
| Beliefs session 2 | 0.091 | 0.041 | | −0.009 |
| | (0.240) | (0.234) | | (0.183) |
| Beliefs session 3 | 0.203 | 0.156 | | −0.107 |
| | (0.322) | (0.322) | | (0.191) |
| Beliefs session 4 | 0.738** | 0.657* | | 0.277 |
| | (0.377) | (0.377) | | (0.219) |
| No-beliefs session 2 | | | 0.147 | 0.158 |
| | | | (0.181) | (0.135) |
| No-beliefs session 3 | | | −0.071 | −0.075 |
| | | | (0.177) | (0.163) |
| Constant | −0.320* | −0.348* | −0.341* | −0.249* |
| | (0.190) | (0.201) | (0.172) | (0.132) |
| $\chi^2$ | 279.37*** | 265.01*** | 123.56** | 173.28*** |
| Pseudo $R^2$ | 0.12 | 0.12 | 0.12 | 0.11 |
| N | 1520 | 1520 | 1140 | 2660 |

*Note*: Table entries are probit coefficients with robust standard errors, clustered on subject, in parentheses. Dependent variable is whether or not a token was bought. $^*p\leq0.1$, $^{**}p\leq0.05$, $^{**}p\leq0.01$ #In Model 3, Win $t-1$ is dropped due to colinearity.

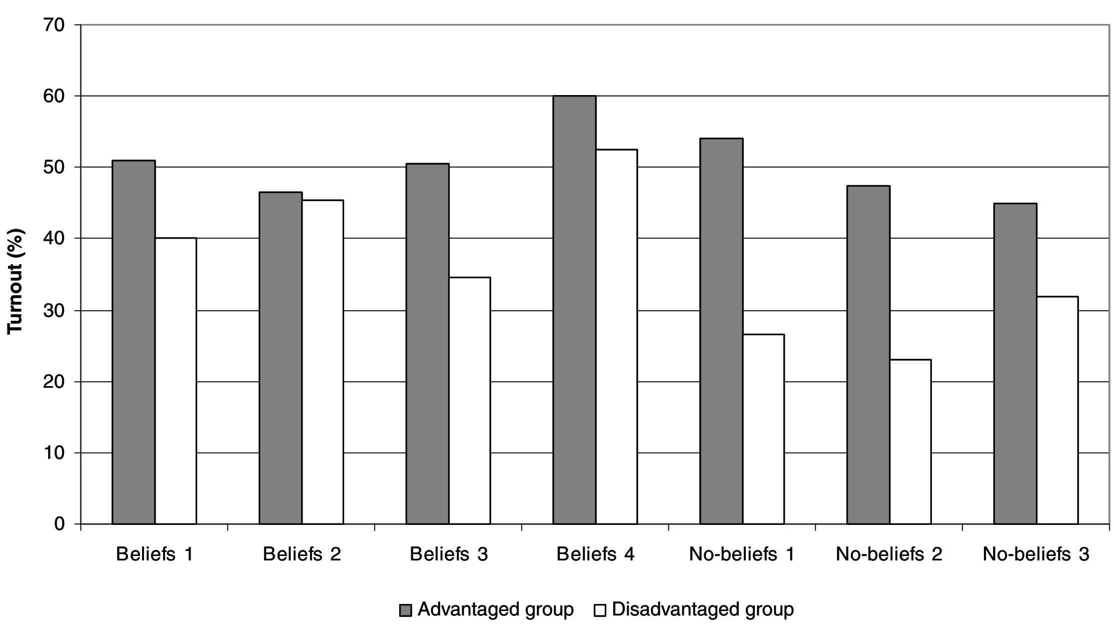
learning effects). This result also reflects the observation that turnout declines when democracies mature, i.e., as a result of repeated elections (Kostadinova 2003). Second, subjects are more likely to participate when they have participated before. This result reflects the argument about the "habitual voter" (Gerber, Green, and Shachar 2003; Plutzer 2002) made in a previous empirical literature on turnout.

Further, the participation rate of one's group members also significantly influences an individual's decision to buy a token: a subject is less likely to participate if the general participation rate in his or her group was high, indicating the emergence of a free rider problem. Prior high participation rate in one's own group may generate expectations of a similarly high participation level in the subsequent round. Given such an expectation, it is rational for a subject to abstain and avoid the cost of voting yet still expect to benefit from one's group winning. Interestingly, our result contradicts the argument that in situations that require voluntary contributing to public goods, conditional cooperation or reciprocity is the best strategy for each player (Axelrod 1984). Our experiment, however, is not only limited to such intragroup conflict but also includes intergroup conflict, which may explain why conditional cooperation does not occur. Past group success or failure does not play a significant role in decisions to participate.

Models 1 and 2 also allow us to test a further implication of the theory. Recall that in the unique quasi-symmetric mixed strategy equilibrium the turnout probabilities for the two groups are not the same (see fn. 7). Rather, we predicted an underdog effect (Levine and Palfrey 2007), where the group winning a tie should have higher turnout than the disadvantaged group. The *Group wins tie* variable in Models 1 and 2 captures this relationship. Contrary to the theory, the finding indicates that the tie-breaking rule acts as a coordination device for voters, mobilizing (rather than demobilizing) them behind a "leading" candidate. Substantively, in Model 1, the predicted probability of buying a token is 0.24 for a member of the group that is announced to win a tie compared to 0.17 for a member of the other group; in Model 2, the respective predicted probabilities are 0.31 and 0.23. Additionally, we also estimated an interaction effect between the group winning a tie and the subjective decisiveness probability. This multiplicative interaction term was negative and statistically significant at the 0.01 level when included into Model 1, indicating that the effect of the subjective belief of being decisive on participation decision is significantly different across the advantaged and disadvantaged groups. Furthermore, given that the coefficient for the interaction effect was negative, the effect of the subjective decisiveness probability on participation decision was weaker for the disadvantaged group (conditional coefficient equals 0.32) than for the advantaged group (conditional coefficient equals 0.95).

Figure 6 further illustrates the different behavior of the advantaged and disadvantaged groups by presenting the average turnout in all seven sessions for both groups. In all sessions, the turnout is higher for the former, the difference being statistically significant (using a nonparametric Mann-Whitney test and each of the seven sessions as a unit of analysis) in all but the second and fourth beliefs sessions. Although our theory does not predict such a result, it is consistent with the *bandwagon effect*, according to which voters favor a party that is doing well in the

**FIGURE 6 Turnout in Advantaged and Disadvantaged Groups**



polls, reported in several voting studies (McAllister and Studlar 1991). Here, this finding may have occurred as an artifact of our particular experimental design, i.e., the use of ex ante coin toss (or status quo) rule for breaking ties and fixed group membership. An alternative, yet strategically equivalent design might declare in advance that one group always wins a tie and employ random rematching of subjects into groups as opposed to our fixed group membership design. If the bandwagon effect disappears, then we might attribute the current result to our design.[19]

### Testing the Obtrusiveness of the Belief Elicitation Procedure

Models 3 and 4 in Table 2 replicate Model 1 with data from the control treatment and from all sessions respectively, using historical frequency of decisiveness instead of the stated beliefs of being pivotal. The goal here is to determine whether subjects behaved significantly differently when beliefs were elicited compared to the control group. Most importantly, the dummy variable differentiating between treatment and control sessions (variable name *Beliefs elicited*) in Model 4 is not statistically significant. This allows us to conclude that there are no significant differences in the behavior of subjects across treatments and that our belief elicitation procedure was not obtrusive in terms of making subjects more aware of the rationality of participating.

Furthermore, Model 3 produces roughly similar results as Model 1. As above, we find that the tie-breaking rule influences turnout, prior participation increases while high level of group participation depresses the likelihood of current participation, and turnout decreases significantly over time. Further, as above (see fn. 18), the effect of the historical frequency of decisiveness falls short of the conventional level of statistical significance. These similarities across treatments further suggest that the belief elicitation was unobtrusive in terms of influencing subjects' decision making.

There are also some differences, however: both prior earnings and history of ties significantly and positively influence turnout. These history effects are likely influencing one's subjective probability of being pivotal, and as such, may reflect some of the effects otherwise captured by the subjective beliefs.

## Conclusions

The pivotal voter model that builds on Downs' (1957) rational choice theory of turnout is the most intuitive, yet also the most controversial formal theory in political science. Empirical tests of the theory to date have mostly relied on proxies or have been partial. The concept around which much of the controversy revolves—the probability of being pivotal—has received the least empirical attention. Indeed, pivotality is simply inferred from the closeness of the election, and the individual-level calculus of voting is never unpacked.

This study has provided the first direct test of the pivotal voter model, with a specific focus on whether and how voters' beliefs of being pivotal factor into their voting decisions. We used a context-free laboratory setting that allows us to control voting benefits and costs and to elicit voters' beliefs about pivotality.

On the aggregate level, we find support for the pivotal voter model with aggregate turnout rates that are close, though slightly below theoretical predictions. The main focus of this study, however, is on the individual-level behavior, where we find mixed support for the theory. On the one hand, we find that subjects' beliefs do inform us as to their likely voting decision. Subjects who believe that their participation will be pivotal for the outcome of a game (an election) are more likely to participate. This relationship is strong and robust, but it is not deterministic—there is no single cutpoint strategy of participation that applies to all voters as predicted by the theory. On the other hand, we have found that subjects systematically overestimate the probability that their voting decision will be pivotal. The weakness in the empirical support for the theory comes not from the failure of subjects to best respond to their beliefs; rather, it comes from the failure of subjects to hold the correct beliefs in the first place, a distinction that could not have been understood prior to conducting this experiment.

Additionally, we have found that subjective beliefs are more important for the participation (turnout) decision than the actual frequencies of being decisive. Indeed, the subjective probabilities tend to be considerably higher than the actual ones—undermining the common view that closeness is a useful proxy for pivotality in testing rational models of turnout. Yet, we also find that, on average, beliefs become somewhat more accurate over time, indicating a learning effect in the turnout decision. The

[19] Interestingly, a recent study presents the results of an experimental participation game with random rematching design without eliciting beliefs and reports a similar bandwagon effect (Bornstein, Kugler, and Zamir 2005). When beliefs are elicited, a random rematching design might make it more difficult for subjects to assess or adjust their subjective probability of decisiveness. We leave this exercise to future research.

overestimation of pivotality may, thus, provide a solution to the paradox of voter turnout—voting happens because people systematically think that their vote counts more than it actually does, though this overestimation declines with experience.

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