

Stopping the Evil or Settling for the Lesser Evil: An Experimental Study of Costly Voting with Negative Payoffs in a TRS Electoral System

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Abstract The strategic voting across families of various electoral systems is often depicted as “settling for lesser evil” (e.g. Cox in *Making votes count*. Strategic coordination in the world’s electoral systems. Cambridge University Press, New York, 1997; Gschwend 2004) in order to prevent the worst outcome of elections. In this paper we experimentally (13 sessions with 18–30 participants, 546 voting situations) explore the degree of strategic voting (as compared to non-voting) framed in situations with (non-compulsory) costly voting where voters face outcomes with positive and negative payoffs (“stopping the evil” framing) and/or with solely negative payoffs (“settling for lesser evil” framing). We explore voters’ decisions in rather complex environment of the two-round electoral system, with symmetric amount of private information about the preferences of electorate available to them and show that voters’ behavior does differ markedly in both the situations. The results have implications for the representation and democracy.

Keywords Elections · Framing · Experiment · Electoral systems

1 Introduction

After the first round of the Czech Republic’s first direct election of President, which was held using an absolute majority two-round system (TRS) in January 2013, there was a major shock when the center-right candidate, Karel Schwarzenberg, made it to the second round with 23.4 %, trailing <1 % behind the front-runner Miloš Zeman, >7 % ahead of the rest of the field. This was a truly unexpected result, as Schwarzenberg was never among the top two candidates in any of the 14

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pre-electoral polls conducted by Czech polling agencies through November and December 2012, after the field of candidates had been set. In those surveys, he on average polled a tiny 8 % of the vote and lagged not only behind Zeman, but also an independent and moderate candidate—Jan Fischer—and center-left Jiří Dienstbier. The media coverage of the polls was largely concentrated on the top two candidates (Gregor and Hrbková 2014: 111) while Schwarzenberg got little attention, spent only a modest resources and seemed virtually uninterested about the result of the vote.

At the same time, Schwarzenberg was *helped* by the polls. At the moment when the anything-but-moderate Zeman pushed past Fischer into first place in the polls, Schwarzenberg's campaign started to spend money heavily in order to define the election (at first without mentioning any specific candidate) as a contest between candidates who on the one hand were controlled by special interests that posed a danger to democracy and who divided the nation, and on the other hand candidates who are able to unite the people. Naturally, this included Schwarzenberg, the heir to the legacy of the late former Czech President, Václav Havel, and was the worthiest candidate who had the highest valence. This was sufficient for a considerable number of late-deciding voters or even those who were initially reluctant to go to the polls to support him in the days directly preceding the first round and catapult him into the runoff. He continued with this strategy in the second round; however, Zeman didn't follow suit and framed the contest as decision between the Left (him) and largely unpopular Right (Schwarzenberg) to proceed to a rather convincing win. One year later, the story was nearly repeated in Slovakia, where center-right candidate Radoslav Procházka also always polled in very modest single digits but his electoral result more than doubled, nearly making it into second round after defining himself as the only candidate who could beat the frontrunner.

The viability of the strategy of Schwarzenberg and Procházka was well documented in several empirical studies about the TRS voting and its effects. Cox (1997: 133) invokes two examples, Peru 1990 (Fujimori beating Vargas Llosa) and Russia 1996 (Yeltsin beating Zyuganov) where the common scenario was voter coordination on one candidate who was perceived to be a viable alternative to another (leading) candidate that should be beaten at any cost. Less elaborated is the analysis of events that lead voters to such prediction and enable the coordination. We believe that in such highly personalized $M = 1$ elections, it is the presence of multiple pre-electoral polls which reveal the choices of voters who have decided earlier that alters the nature of decision-making for undecided voters. This conceptually amends the nature of such elections—although technically held simultaneously, they are, in effect, sequential.

In the rest of the paper, we briefly discuss the properties of sequential voting procedures and their effects for voting. We tackle the issue from the perspective of the strategic reasoning of the undecided electorate. Consider a situation where voting is costly, abstention is possible, and the payoffs from the candidates vary. In such a situation, is the (personal) decision to vote and/or is the (group) ability to coordinate affected, whether the voting act is among several decent alternatives, or the goal is to prevent a lousy candidate from winning, or even if the vote is among

several bad candidates? Are the factors that can explain group decisions the same when choosing among several good options compared to settling for the lesser evil? We try to answer the question through a series of laboratory experiments with TRS voting.

2 Properties of Sequential Voting

Battaglini et al. (2007) distinguished between elections where all voters make their choices simultaneously, and those which are sequential in nature, with some voters knowing the choices of earlier ones. They found a surprising amount of votes which use a sequential procedure (US primaries, roll-call votes, EU referenda). They also argue that voters may be helped by various polls and surveys which inform them about the choices of early voters. The mechanism behind the sequential processes, *learning by voting* (Morton and Williams 2001; Morton 2004; Knight and Schiff 2010) allows voters to gather information about the quality of the candidates during the campaign, when voters in later stages attempt to uncover the personal information of early voters from their voting returns. The studies usually document that candidates benefit from momentum effects when their performance in early rounds of voting exceeds expectations.

Most studies on sequential voting concentrate on its full-fledged variation—contests where the information available to late voters comes in the form of results from actual choices of early voters, mostly US primaries. As a rare exception, in their natural experiment with French overseas territories, Morton et al. (2015) found that exit poll information decreases voter turnout, with a substantial bandwagon effect among those who still turn to vote. Deltas et al. (2015) discuss the sequential nature of US presidential primaries and compare sequential and simultaneous voting procedures from the perspective of candidate selection, with the former having much better coordination properties, yet with the trade-off of the possibility of early voters not properly learning about candidate qualities and coordinating around a low-quality candidate. Similarly, Knight and Schiff (2010) see a trade-off between sequential election systems, which place too much weight on early information but enable late voters to make quality decisions, and simultaneous procedures where all voters weigh the information equally but rely too much on their priors.

3 Highly Personalized TRS Elections—Sequential or Simultaneous?

Most—even if not all—presidential elections held using a two-round voting system use the majority principle as a threshold of legitimacy (Perez-Liñán 2006). Is it possible to extend the concept of sequential vote to this type of elections, which are—

strictly speaking—simultaneous in nature? Does the information from polls influence voters to the extent that we could analytically treat such elections as an “as if” sequential contest? As the classical study of Blais et al. (2006) documents, the polls induce strategic voting for some voters, prompting them to desert lower-ranked candidates. On the other hand, the contagion effect (similar to the “learning by voting” mechanism) was not found in the 1988 Canadian data. Strictly speaking, the polls inform voters (decided or undecided) only about the intention to vote of some of them, who moreover can later change their minds. Although several recent studies indeed document some intra-personal variability at the individual level during elections (Blumenstiel 2014; Blumenstiel and Plischke 2015) caused especially by priming effects, there is also literature (Kleinnienhuijs and Walter 2014) that shows that at the aggregate level, the situation and support for alternatives is relatively stable. On the other hand, the discourse of the polls, explicitly distinguishing among those voters who have already made up their minds and those who are undecided, clearly supports the sequential character of highly personalized contests. Moreover, undecided voters are helped even more by such surveys and polls that not only measure actual support for candidates, but often also their chances in various runoff setups.

Majority as a minimum threshold not only guarantees a minimum level of support for the victor; it also allows the electorate to select the ultimate winner of the contest. In presidential and semi-presidential regimes, it also in theory strengthens prospects for democratic accountability through pre-electoral (or pre-second round) alliances and broader support for presidential administrations. These expectations have been empirically questioned in some cases. Payne et al. (2002) show that a runoff often merely offers voters the opportunity to choose “the lesser evil” and vote *against* the least preferred candidate; the implications for governability are in fact negative. The lesser evil argument is empirically under-researched however. We have found only one study (Burlacu 2012) documenting that voters (regardless of electoral system) carefully choose among the alternatives even when presented with several poor options.

The majority requirement and the possibility of runoff thus alters the sequential logic of the vote considerably. When information about a frontrunner which may be seen as a very unwelcome alternative by undecided voters becomes available, the remaining candidates can make credible claims about their electability when directly pitted against him in the runoff, thus diminishing incentives for bandwagon effect, reminding voters that a vote cast for candidate who would lose the runoff will be wasted. As several studies (Abramowitz 1989; Rickershauser and Aldrich 2007; Burden and Jones 2009) show, electability is indeed an important factor affecting voters’ choice, and the voters’ choice becomes more complex with the effective number of candidates after the first round being considerably higher than in plurality elections (Wittrock and Lewis-Beck 2011; Kacprzyk et al. 2013). This all should hint at the fact of the extreme importance of the runoff setup considerations. The change of frontrunner can—rather than bandwagon—generate an underdog effect, providing nonmyopic voters with incentives to vote for a candidate who is running lower than second or third place before the election. Highly personalized

elections, with numerous polls and a “horseshoe” discourse (Patterson 1994; Hahn and Iyengar 2002; Strömbäck and Shehata 2007; Matthews et al. 2012; Pétry and Bastien 2013), are an especially suitable event for this type of effect to occur. Note that here we are in partial disagreement with Cox (1997: 129), who considers polls and momentum of a given candidate to be a necessary conditions for a bandwagon effect to take place. We think that both scenarios are possible and we thus put more weight on voters’ considerations about the frontrunner and his chances when matched against various opponents in the runoff.

On the other hand, for the underdog effect for the trailing candidate to occur under the TRS, intense support from undecided voters and/or a shift among decided voters is required. This is especially vexing problem when voting is not compulsory. Underdog situations present voters with mixed incentives: on one hand, they can believe that they are personally efficient (Finkel et al. 1989; Opp 1999; Blais 2001: 130–132) because they bring resources and personal expertise to bear in changing the outcome; on the other hand, they can be skeptical about the chances that group will succeed in the task as a whole. This can be especially true in “lesser evil” situations, where voters can doubt the group success and they therefore refrain from voting, especially compared to the situations when there emerges a worthy opponent in the race which is capable of beating the “evil” frontrunner. Is there empirical support for this line of reasoning among anonymous electorates? Our experimental design tries to discover whether there will be equal or different group behavior in the above-mentioned situations. We also try to answer the question concerning which factors influence the conjectures about individual and group efficiency in designs that vary in the presence of “evil.”

4 Experimental Design

As we are primarily interested in voting and abstention in highly personalized TRS elections with different degrees of candidate support among decided voters and concomitantly the different amount of momentum they enjoy among undecided voters, our design differs markedly from previous experiments on TRS (Blais et al. 2007, 2011). What we do have in common with them is the goal of representing the concepts of “bonus from the elected candidate” and “electability.” We do not, however, embed the categories in the concepts of position and distance, but have been trying to represent them in another way. The reason is that in highly personalized elections, positional considerations—if present—are often overshadowed by the valence factors connected directly with the personalities of candidates or simply with the momentum that candidates are able to enjoy (or not). Moreover, we are not primarily interested in the choice of the electorate as a whole but only of those late, undecided voters, who are confronted with a candidate who is enjoying momentum among them but still casts some doubts about his electability because of low support among those who decided early.

In our design, we introduce several properties which are associated with real world TRS elections: information from polls, costly voting with the possibility of abstention, and the various level of momentum for the candidates running among the undecided voters. The “polls” were represented by the group of “decided voters,” their aggregate preferences always being known to the subjects who had yet to decide whether to vote, and if so, for whom. Voting was costly, with separate costs for the first round and the runoff. The momentum among undecided voters was represented through symmetric information about payoffs the subjects would receive (should they vote or not) after one candidate wins the majority. These payoffs accordingly differed for each candidate.

We investigate voter behavior in three types of situations: (1) choosing “the lesser of evils”—and election act among several candidates with negative payoffs; (2) the possible victory of a candidate with positive reputation which is pitted against a poor, yet leading, candidate; and (3) elections with several decent candidates, with one of them enjoying momentum among undecided voters. Similarly to the framing experiments, there was no obvious baseline in the experiment and group behavior was compared for all three situations.

5 The Experimental Procedure

The experiment was conducted at the end of September and the beginning of October 2014 at Masaryk University’s computer rooms. The participants were randomly chosen from the ORSEE database of students of Masaryk University and were invited to the experimental session. The actual amount of participants was 290 students, including 170 women and 120 men. Without any knowledge of the purpose of experiment or the knowledge of the experimental conditions in each session, participants were assigned to the sessions according to their time preferences. The self-assignment poses little problem in this particular experiment as there were no control groups and all participants received the same treatment in a combination of between-subject and within-subject designs. The subjects were financially compensated, and their average payoff was 200 CZK + 50 CZK show up fee.

There were 13 experimental sessions with a various number of subjects (5×18 , 1×19 , 2×21 , 1×24 , 1×26 , 1×29 , 2×30) with each session lasting approximately 60–90 min. The capacities of sessions varied in order to observe turnout in different sizes of electorates to test another hypothesis that we do not report in this paper. At the beginning of each session, the participants were randomly seated at the computers which they used the whole time. In order to prevent interaction and the communication between the subjects, the workplaces were physically separated by cardboard boxes. The experiment was conducted using Z-Tree software (Fischbacher 2007).

Prior to the actual experiment, the participants were informed about the conditions of the experiment and how the final payoffs would be paid out. The first part

of the experiment was subsequently initiated and the participants were asked to read the short text about the voting systems. A test comprising five questions followed. The questions in the test were based on the information from the short text about the voting systems, and the purpose of the test was to verify the participants' understanding of the voting procedures. The responses provided by subjects, however, had no impact on the final payoff.

Participants were then acquainted with the experimental procedure and with the calculation of the final payoffs. All the information was provided on computers and was simultaneously presented by an administrator. Since voters in real elections do not decide at the same time, the experiment was designed to imitate such a situation. The subjects were also given this information and it was explained that some people may make their decision to vote sooner or later than others. The electorate was therefore divided into two groups: the group of voters who decided to participate and had already determined their preferences (decided voters) and the second group which consisted of all experimental subjects (undecided voters). However, all participants were at the beginning of the experiment told that they were in the group of undecided voters and that the group consists of half of all possible voters. The whole electorate therefore had the size: the number of the participants $\times 2$. The experiment hence used some inevitable but minor deception, because voting decisions of decided voters were set in advance to meet certain criteria.

Each participant was given starting capital of 350 ECU (experimental currency unit) which changed after every voting situation in relation to the decision made by the participant himself and the decisions made by other participants. The actual amount of money which participants received after the experiment was determined by an exchange rate (its value was set according to the overall participants' profits to ensure that the average profit in Czech crowns would be approximately 200 CZK). Participants didn't have any information about the exchange rate calculation; they were told only that its value is approximately $4.5 \text{ ECU} = 1 \text{ CZK}$ and could thus expect conversion process with a linear relationship between CZK and ECU.

The subjects afterwards had an opportunity to partake in several voting situations; every of these corresponded to elections using a two-round voting system. The subjects were, however, provided no information about the number of repetition of voting or information about the length of the experimental session. They were only informed about the number of other participants.

The participants could have participated in up to 42 voting situations. During this sequential voting, in every situation they could choose from four candidates, labeled as A, B, C, or D. No further personal information about the candidates was provided. Participants only had knowledge about the size of each candidate's support among the group of decided voters and what the payoff would be if a candidate would have won the elections (the candidate had to gain an absolute majority of votes to win). The payoff for one voting situation could have been either positive or negative, ranged from -60 to 60 ECU and varied from small to high. The average difference between the highest and lowest payoff was 23.86 ECU, just short of three times the cost of voting for both rounds of the vote. 14 situations were

only positive payoffs, 14 only negative, and 14 were mixed positive-negative situations.

The support of candidates by decided voters was determined prior to the actual experiment by the researchers and these levels of support were chosen to show the differences in the voting situations. It was then the impact of the differences in possible payoffs and also the influence of the closeness of elections which could influence the voters' behavior. The distance between the first and the second candidate as well as between the second and the third was one of the most important independent variables. So as to prevent the participants from learning specific voting behavior, the 42 sequential voting situations were in random order (the order was the same in every session).

Prior to each round of elections, the subjects could have decided whether they wanted participate or not. However, they did not know how many subjects had already decided to do so. If subjects decided to vote in the first round, they had to pay a cost of voting 5 ECU, or 3 ECU in case of the second round. By participating in elections, subjects could then influence the results of elections, which were calculated from the number of votes from both decided and undecided voters given to each candidate. However, this result (as well as the payoff) affected all participants (even those who decided not to vote). If no candidate received an absolute majority of votes, then the two candidates with the highest number of votes proceeded to the second round. If candidates had an equal number of votes, the candidate that proceeded to the next round was randomly selected. After the first round, the participants were informed about the turnout in both groups of voters (turnout among the decided voters was always 100 %), overall turnout, and the number of votes gained by each candidate. All voters from the "decided" group whose candidate had proceeded to the second round, participated in the second round as well and again voted for the same candidate. Even some decided voters, whose candidate was eliminated, participated in the second round and gave their votes to a different candidate. The number of those voters and also their preferred candidate was randomly generated. Participants were informed about their payoff shortly after one of the candidates had been elected. The value of this payoff was added or subtracted from every participant's account, regardless of whether they decided to vote or not. Subjects were also informed about the turnout in the second round, as well as about the number of decided voters whose candidate didn't pass to the second round. Participants were also told about the amount of decided voters, who decided to vote despite the fact that their preferred candidate had been eliminated in the first round. The experiment then proceeded with another voting situation according to the same principles (Fig. 1).

After the last elections, subjects were informed about their final payoff in ECU and also in CZK. The whole experimental session ended with an anonymous questionnaire mapping subjects' demographic characteristics as well as their political sophistication. The sophistication was measured by questions about the Czech political system and international politics. The very last part of the experimental session was a short debriefing providing general information about the purpose of the experiment and the payoffs were subsequently given to participants.

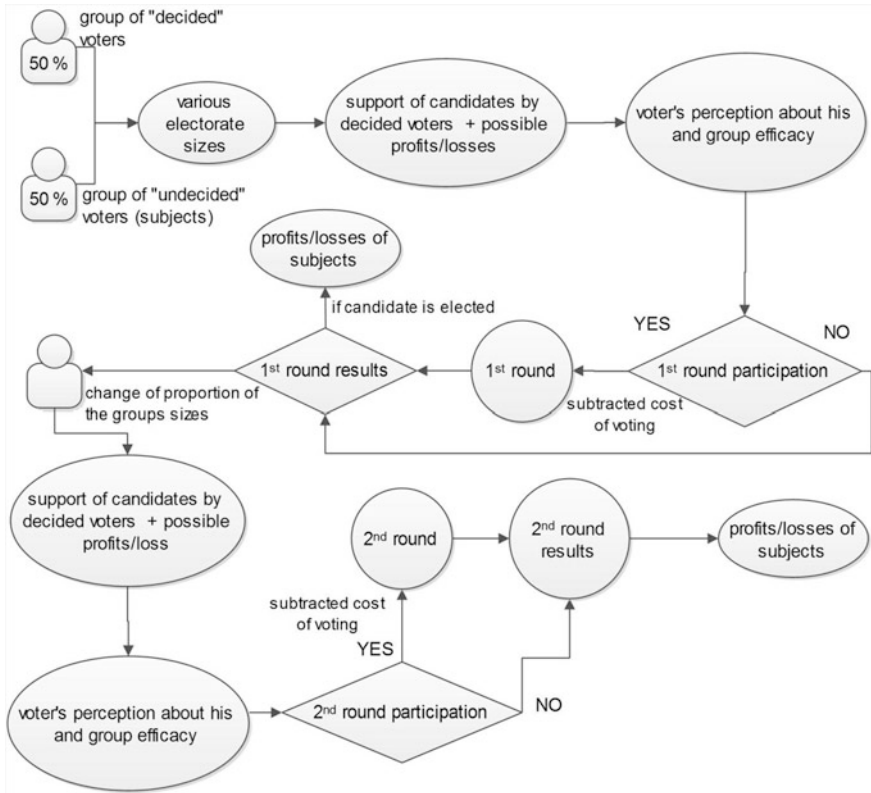


Fig. 1 The experimental procedure. The voting situation

During the debriefing the participant often made remarks that invoked the concepts of their personal efficiency, freeriding and group efficacy.

6 Results

Each of the participants from the 13 groups participated in 42 voting situations, together for 546 voting situations (546 first round votes and 345 second round) and 19,798 individual decisions (12,180 in the first round and 7618 for the second round). In 201 cases, the candidate was elected in the first round, while in remaining 345 cases, there was a runoff. The candidate with the highest payoff was elected in 485 situations while in 61 situations the group failed to elect him (9 of them were in the vote that ended in the first round, while 52 in the second round). The amount of strategic voting (not selecting the candidate with the highest bonus in favor of supporting somebody else who was deemed more electable) was 7.5 %, unevenly

spread among three kinds of momentum situations, being as high as 9.5 % in situations with positive payoffs but a mere 4.5 % with situations with negative payoffs. In this paper, we only report voters' behavior in the first round, which is of primary importance for our research questions.

6.1 *Turnout in the First Round*

The average turnout in the first round was 42.90 % and in the second round 42.28 %. The amount of freeriding is consistent with levels reported by Blais (2001) for collective action problems. The lowest turnout was in situations with negative payoffs and the highest for negative-positive situations. The difference is about 15 %, significant both statistically and substantively; voting situations with only positive payoffs attract higher turnout than negative payoffs, but they are significantly lower than positive-negative ones. This pattern emerged in all but one session, in which the turnout for situation with negative payoffs was slightly higher than with positive ones. All this hints at different subjects' conjectures about group efficacy in all three types of situation. Subjects were prone to investing resources to simply increasing the probability of minimizing the extent of the loss, but participated most when there was also a result whose payoff was positive *and* within reach (Table 1).

To determine which variables affected turnout in voting situations, we ran OLS regressions with three independent variables, two representing a bonus from a candidate and one being a proxy for electability: (1) the payoff difference between the leading candidate in the group of decided voters and the candidate with the highest payoff for the subjects, (2) the vote difference between a leading candidate among decided voters and the candidate with the highest payoff for the subjects, and (3) the size of payoff from the most preferred candidate. We separately present models for voting situations with positive, negative and positive-negative payoffs (Table 2).

All three models (controlled for session ID) are robust. For the situations with positive payoffs, first round turnout is best explained by the combination of the size of payoff from the most preferred candidate (3) and vote difference between that candidate and the candidate leading among undecided voters (2). Simply put, to turn out and vote, the subjects were concerned about the absolute amount of payoff they would get if their preferred choice was elected; the more their favorite trailed the frontrunner, their propensity to vote increased. In situations with negative payoffs, the voting was determined by the payoff difference (1) and vote difference (2). Quite intuitively again, group turnout was higher when there was a greater difference in payoffs between the bad results as well as when the "less bad" candidate trailed the frontrunner to a greater extent. Particularly striking were the results in positive-negative payoff situations where the R² (0.57) was the highest and the turnout rate was best explained by a combination of payoff difference (1) and the size of payment from the most preferred candidate (3), while vote

Table 1 Group turnout 1st for all three types of voting situations by sessions

| Session ID | | 1st round turnout, structured by payoffs | | | |
|------------|---------------|--|----------|-------------------|--------|
| | | Positive | Negative | Positive-negative | Total |
| 1 | Mean | 42,857 | 43,253 | 50,793 | 45,634 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 13,855 | 14,807 | 18,978 | 16,070 |
| 2 | Mean | 50,714 | 36,428 | 55,238 | 47,460 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 13,280 | 14,527 | 16,523 | 16,590 |
| 3 | Mean | 52,380 | 38,888 | 57,936 | 49,735 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 15,208 | 17,702 | 20,868 | 19,398 |
| 4 | Mean | 49,261 | 38,669 | 52,463 | 46,798 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 17,448 | 14,950 | 22,066 | 18,904 |
| 5 | Mean | 51,984 | 26,984 | 52,380 | 43,783 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 13,363 | 9462 | 11,676 | 16,517 |
| 6 | Mean | 36,538 | 32,692 | 46,978 | 38,736 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 15,217 | 14,528 | 23,085 | 18,618 |
| 7 | Mean | 43,253 | 42,857 | 46,825 | 44,312 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 14,318 | 21,728 | 17,529 | 17,759 |
| 8 | Mean | 47,916 | 31,547 | 49,702 | 43,055 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 15,739 | 12,415 | 19,021 | 17,621 |
| 9 | Mean | 44,047 | 30,952 | 46,428 | 40,476 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 15,933,189 | 9171 | 19,196 | 16,475 |
| 10 | Mean | 46,240 | 38,345 | 46,616 | 43,734 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 16,014 | 21,986 | 21,833 | 20,015 |
| 11 | Mean | 45,238 | 30,952 | 50,340 | 42,176 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 13,436 | 12,354 | 19,741 | 17,267 |
| 12 | Mean | 33,673 | 28,571 | 37,755 | 33,333 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 9964 | 12,667 | 18,721 | 14,420 |
| 13 | Mean | 40,714 | 30,952 | 44,047 | 38,571 |
| | N | 14 | 14 | 14 | 42 |
| | St. deviation | 15,807 | 15,766 | 20,762 | 18,065 |
| Total | Mean | 44,986 | 34,699 | 49,038 | 42,908 |
| | N | 182 | 182 | 182 | 546 |
| | St. deviation | 15,226 | 15,608 | 19,427 | 17,881 |

Table 2 The factors influencing group turnout in the 1st round

| | Voting situations (payoffs) | | | | | | | | |
|--|-----------------------------|-------|---------|--------------------|-------|---------|-----------------------------|-------|---------|
| | Positive (N = 182) | | | Negative (N = 182) | | | Positive-negative (N = 182) | | |
| | B | SE B | β | B | SE B | β | B | SE B | β |
| Payoff difference between leading candidate and most preferred by the subjects | 0.034 | 0.061 | 0.046 | 0.298** | 0.046 | 0.393 | 0.385** | 0.068 | 0.408 |
| Vote difference between leading candidate and most preferred by the subjects | 0.198** | 0.070 | 0.174 | 0.496** | 0.068 | 0.431 | 0.036 | 0.082 | 0.025 |
| Maximum payoff from the candidate | 0.461** | 0.075 | 0.538 | -0.089 | 0.430 | -0.012 | 0.746** | 0.150 | 0.400 |
| Constant | 24.134* | 2.048 | | 20.029* | 2.329 | | 26.063* | 1.867 | |
| R ² | 0.434 | | | 0.431 | | | 0.579 | | |

** $p < 0.01$, * $p < 0.05$, controlled for: ID session

difference (2) was not significant. In other words, the readiness to coordinate and elect the trailing good candidate was influenced only by the fact how “good” the good candidate was and how he was “different” from the bad one, with no increased attempts of freeriding in situations when the contest was tight. The finding about different combinations of variables which are able to explain group behavior in all three scenarios is interesting but needs further elaboration.

7 Conclusion

Although one rather common school of thought (Taagepera 2007) is that with a TRS, all bets are off, our experiment especially succeeded in contrasting two kinds of situations. When there were only bad alternatives present as the voting choices, the willingness of the subjects to invest their resources and engage in costly voting that would bring them—and the group—only another negative result (although some alternatives were much worse than others), was the lowest. On the other hand, in the “stop the evil” scenario, turnout was consistently the highest and the vote difference between the frontrunner and a good candidate was not a factor that would prompt some subjects to freeride and abstain. The third scenario, with several decent options and with one of the candidates enjoying momentum, was somewhere in between the two extremes. As the payoff circumstances were evenly set in all three types of situations, we believe the subjects must have held different assumptions about group efficacy when considering whether to vote (or not).

Our findings are at the same time consistent with the empirical evidence from Czech and Slovak presidential elections and provide some support for the possibility of the underdog effect to occur. Clearly, we should not fully abandon the Coxian perspective about how events in TRS contests are most likely to unfold. But for highly personalized elections with their distinct properties, we can have more faith in nonmyopic voters who care about the runoff setup, are eager to stop the evil candidates from winning, and vote accordingly.

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