Can Biased Polls Distort Electoral Results? Evidence From The Lab*

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Abstract

In a series of experiments with 375 participants, we investigate the impact of biased polls on election outcomes, when voters have the opportunity to observe and learn about the bias by playing multiple voting rounds. While in control conditions, polls are unbiased, in treatment conditions, participants view only poll results where a particular candidate's vote share is the largest. We find that this candidate is consistently elected more often in the treatments than in the controls, because biased polls robustly distort voters' expectations about vote shares. Remarkably, this effect holds after eighteen election rounds, out of which the first three are practice rounds, and even in a treatment where voters are *explicitly* informed about the bias. Our results suggest that the anchoring effects of biased polls on participants' beliefs are stronger than potential reactance to biased information.

1 Introduction

The rise of populism in western democracies over the last few years has changed the political landscape, upsetting political balances that survived for decades and bringing new forces into the forefront. Populism has ramifications for both economic policy (Kaltwasser, 2018) and political stability. A key aspect of modern populism is distrust in democratic institutions, of which the most fundamental is elections under free media.

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The role of voting-intention polls, in particular, has been under heavy criticism. Questions have been raised about the reliability of polls (Shirani-Mehr et al., 2018) and their effects on democratic elections. A key reason for such scepticism is the widespread perception of poor predictive performance of polls in recent high-profile elections, most notably the US presidential elections of 2016 and 2020 and the UK general elections of 2015 and 2017.[1] Some prominent politicians, such as Lord Foulkes in the UK and Ron Paul in the US, have even claimed that polls may be manipulated by status-quo groups in an attempt to cling to power.[2] If people perceive polls to be biased in favour of a candidate or a party, this perception may erode trust in democratic institutions and reinvigorate populist agendas.

Thus, it is crucial to examine whether criticisms of polls, such as the ones presented above, are unfounded, or whether polls have the potential to skew election results in the current political environment. If the feedback that the public receives from opinion polls is not representative of the true preferences of the electorate, one may worry that this could distort the democratic process.[3] For instance, in a two-party election race, imagine that polls showing the left party ahead are more likely to be revealed to the public than polls showing this party trailing. Then, critical questions arise: would this systematic bias in exposure to poll results affect the electoral race? How significant is such an effect and how does it depend on what voters know about the bias? We find that biased polls have systematic and robust effects on electoral outcomes, and voters fail to account for the bias even when they are explicitly informed about it.

We present the results of an experimental study that examines the causal link between biased polls and two-party election outcomes. We are one of the first studies to perform this examination, and for good reasons. It is difficult to measure the degree of bias in polls and their electoral consequences with observational data alone. Ideally, one should conduct a randomised controlled trial, but it would not be ethical to distort a real electoral race. An alternative approach would be to use purely hypothetical surveys and to embed the study in a real election. However, if we chose to do this, we would be unable to incentivise thoughtful behaviour with real money and we would be unable to

[1] See also Whiteley (2016) on the reasons behind the failure of polls to predict the 2015 general election in the UK.

[2] For general economic models of such manipulation see Maniadis (2014) and Cipullo and Reslow (2019).

[3] In Appendix A, we provide real-life evidence that the public's exposure to polls is potentially biased. Using Twitter data from the UK and the US, we show that users of social media propagate polls in a selective way, thus leading to a systematic bias in the public's exposure to poll results.

examine the effects of repetition and learning. Summing up, we tackle an urgently important problem with the only methodological strategy – in our view – that establishes causality and allows for incentives and feedback.

In this paper, we conduct a series of experiments where we observe the outcome of fifteen electoral races (plus three practice rounds) between two parties (Party K and Party J) who field different candidates in every round. The two candidates differ in their 'valence', and the exact valences are known to some participants (the 'informed voters'). 'Uninformed voters' are only told the statistical distribution out of which the valences were drawn. Before each election, five voting-intention polls are generated by randomly sampling participants. In this manner, polls allow informed voters to provide a noisy signal regarding the valence of the two candidates.

In Experiment 1 (E1), we start by comparing a biased regime – where the results of only the *two polls most favourable for one candidate* (the candidate of party K, or simply *candidate K*) are revealed – to a natural control setting, where *all five polls* are revealed. In Experiment 2 (E2), the control setting entails revealing the results of *two randomly selected polls*, rather than all five polls. Finally, in Experiment 3 (E3), whereas in the control condition all five polls are revealed, in the treatment condition participants are informed beforehand about the (non-random) rule for selecting the two polls to be revealed.

If a party's popularity is systematically 'inflated' in the polls, does this result in an electoral advantage for that party? Our results suggest that this is indeed the case. Both in terms of the number of rounds that candidate K was elected and in terms of average vote share, candidate K performed better in the treatment than in the control condition in a robust manner. In particular, the biased feedback mechanism increased the average vote share of the favoured candidate K by 16 percentage points, 8.6 percentage points and 6.1 percentage points in E1, E2 and E3, respectively. These differences are consistent across sessions and their magnitudes are meaningful politically. Importantly, there is limited evidence that these effects go away as participants gain more experience.

Furthermore, our analysis of voting behaviour and elicited beliefs provides support for the mechanism of anchoring and (insufficient) adjustment. Beliefs are highly correlated with the average vote shares displayed in the revealed polls. Econometric results further indicate that beliefs do not increasingly deviate from revealed poll results as time passes. This means that voters do not disproportionally discard or discount biased poll results in later rounds. Moreover, average revealed poll results are a good predictor of electoral results, although these polls were selected in a biased manner.

Our methodology addresses research design issues that have plagued previous literature. Previous experiments with biased or manipulated polls exist only in the political science literature (Meffert and Gschwend, 2011; Gerber et al., 2017) and they are all conducted in one-shot election environments, which do not permit voters to infer the accuracy of polls through experience. Therefore, we are the first to show how voters behave when they compare biased poll predictions with actual election results through time. We provide evidence that learning may be limited in these environments. Since feedback in a laboratory setting is much more frequent than feedback in real-life elections, one may reasonably expect learning to be even more ineffective in the real world. Consequently, our experimental paradigm may inform the public debate on whether or not biased polls can skew behaviour in real election settings.

The result that biased polls can manipulate voting behaviour is not straightforward. In all our experiments, participants have ample opportunities to infer the bias and in our E3 they know explicitly the bias rule. However, this does not eliminate the electoral advantage of the 'favoured' candidate K. Even when participants are explicitly informed of the bias, they do not appear to rationally weigh the information content of polls. Instead, it seems that, in forming their expectations about the electoral results, voters use polls merely as judgemental anchors, so they overweight the reference point that polls provide and they underweight their informational content. This interpretation is consistent with the well-known process of anchoring-and-adjustment (Tversky and Kahneman, 1974).

It is important to emphasise that our results run counter to the predictions of some established theories. In fact, some political scientist colleagues, drawing on the theory of psychological reactance (Brehm, 1966, 1972), predicted that the bias, once publicly revealed, will backfire against candidate K. Namely, if voters realise that polls are biased, they could perceive it as an attempt to limit their freedom on political choice and so they could vote against the polls and in favour of the election 'underdog'. However, in our experiments we find no such evidence. Indeed, if anything, the anchoring effect of biased polls on participants' beliefs seems to dominate any reactance effect.

The rest of the paper is structured as follows. Section 2 places our findings in the relevant literature. Section 3 discusses the design of our three experiments. In

Section 4 we present descriptive results of our experiments, whereas in Section 5 we conduct regression analysis. Section 6 provides additional analysis on welfare effects and individual behaviour. Section 7 presents a short discussion of our findings and concludes.

2 Related Literature

The effects of polls on election outcomes have been the topic of both theoretical and empirical study. This large literature contains important experimental studies, but as far as we can tell, none of them considers biased feedback on actual polls along with opportunities for learning. Economic experiments have examined a variety of mechanisms that can drive poll effects on elections, with neutral phrasing and a theory-testing focus. An important mechanism examined in the lab is asymmetric information among voters (McKelvey and Ordeshook, 1984, 1985; Brown and Zech, 1973; Sinclair and Plott, 2012). This experimental strand finds that polls aggregate information reasonably well, although voters exhibit some robust elements of bounded rationality. A second studied mechanism has been coordination and strategic voting in multi-candidate elections (Forsythe et al., 1996; Plott, 1982), where the evidence indicates that polls can often be instrumental in coordinating voters' choices. An additional important mechanism is turnout under costly voting. Most studies (Klor and Winter, 2007; Agranov et al., 2017; Gerber et al., 2017) point to a failure of the standard prediction that polls discourage majority group voting and that they are welfare reducing (Goeree and Grosser, 2007), although the effects seem generally complex.

However, the economics literature is mainly focused on unbiased polls, whereas our paper is concerned with biased polls and their effects on voting behaviour.[4] This is closer to the approach taken in political science, where many experiments strategically manipulate the poll information that participants receive. Typically, these experiments

[4]We suspect that at least part of the reason for this omission in the experimental economics literature is reluctance to use what can be viewed as explicit manipulation in the lab. For instance, we refer to several studies in political science that expose subjects to different poll results (sometimes fabricated) and examine how this affects their behaviour. In our experiments, we avoid this approach that would unambiguously qualify as deception and we only provide truthful information. Still, some colleagues would count as deception any omission of information, as long as participants are expected to behave differently in the presence of this information. However, most experiments where information is a treatment variable can be considered problematic under this strict definition. Moreover, according to this strict approach, even information about other participants' behaviour, or about the research objectives, should be shared with all subjects, but of course this would sometimes jeopardise the research design. We argue that the question of whether and how people are able to identify biased information can and should be examined in the economics laboratory, and how participants form beliefs about whether information is biased or not should be an open research question, not a forbidden one.

are non-incentivised. The early study by Fleitas (1971) indicates that voting is not responsive to the quantitative information revealed in polls. Meffert and Gschwend (2011) present different versions of newspaper articles that report voter support for German parties in multicandidate elections, while Rothschild and Malhotra (2014) manipulate the ostensible public support for several important issues and examine how this affects subjects' stated preference on the issues. These studies find that manipulation affects beliefs and moderately alters behaviour. Gerber et al. (2017) conduct large field experiments where they selectively convey poll results to manipulate the ostensible closeness of the race. Again, beliefs seem to be affected by the manipulation, but behaviour is not affected much. As with previous experiments, rational choice theories, which predict voter turnout, do not perform very well.

The main difference between the aforementioned political science studies and ours is that these studies are not examining whether participants are capable of understanding that manipulation is taking place and accounting for it. In particular, in these studies participants face biased or manipulated polls only once, so they do not learn from past mistakes. Our design allows for multiple rounds of repetition where the predictions of polls can be juxtaposed with the publicly known election results in every period, so that we explore the participants' scope for learning. We believe that this is a critical aspect, as economists are typically interested in stable patterns of behaviour after the effects of learning have taken place. In addition, our experiments show that voters are influenced by biased polls even when they are aware that polls are biased, a test that is absent from the aforementioned papers. To the best of our knowledge, no other study has attempted to disentangle the factors driving the effects of biased polls on election outcomes. Finally, our study is conducted in a laboratory and decision-making is incentivised with real money.

Our paper also speaks to the literature on the manipulability of democratic elections. Much work has taken place on the role of traditional or social media (DeMarzo et al., 2003; Gerber et al., 2009; Chiang and Knight, 2011; Bond et al., 2012; Epstein and Robertson, 2015). In some sense, our paper sits on the intersection between the two discussed literatures: if pre-election polls can impact elections and if they are biased in some way, then they can distort democratic outcomes. We show that this is indeed possible, and that it can happen in a manner such that one particular party systematically benefits.

3 Our Experimental Environment

In general, the information conveyed by poll results can be relevant to voters for many reasons (for instance, voting is costly and voters need to estimate the closeness of the race, there are multiple candidates and voters need to focus on a viable candidate, or voters have bandwagon preferences). The particular environment we choose to study here is akin to Feddersen and Pesendorfer (1997), where voters assess candidates on two dimensions, their ideological position and their intrinsic quality (valence). In our setting, there are two political parties, party K and party J, each one of which fields a candidate. We refer to the candidates' identity by the name of the political party they stand for, hence the candidates are K and J.

All voters know the closeness of the candidates' political views to their own, i.e. the ideological position of the two candidates, but they differ in their knowledge of the candidates' valence. Some voters are informed and know precisely the valence of each candidate, while the remaining are uninformed and they know only the statistical distribution out of which each valence is drawn. Moreover, in our setting informed voters are on average left-wing leaning in terms of ideological positions, while uninformed voters are on average right-wing leaning, so the voting intentions of the informed voters are not representative of the overall population. As a result, elections across the entire set of voters (not within the set of informed voters only) are needed to aggregate the electorate's preferences, while pre-election polls convey valuable information to uninformed voters by helping them make inferences about candidates' valence. In our setting, we have five voting intention polls taking place prior to each election.

Our research question, then, focuses on whether election outcomes are *affected* by giving voters a biased sample of the total information (total information in every round consists of the results from five polls), which systematically depicts the candidate of party K performing 'better' than in reality. This 'biased selection' environment constitutes our experimental *treatment manipulation*. We define the concept of 'affected' italicised above relative to two control conditions as benchmarks. Our first control (in E1) is simply an environment where the total information is released to voters. Our second control (in E2) is a setting where an equal amount of information as in our treatment condition (two polls out of five) is conveyed, but in a random, rather than a systematically selective, manner. The second control allows us to test whether the difference between observing all five polls and two selected polls is due to disparate quantities of information, i.e. observing a smaller set of polls (two instead of five), or whether it is due to the selection per se.

In a final experiment (E3), we also test whether the effect of biased polls is due to subjects perceiving the polls as unbiased (despite the feedback that they receive in every round) or due to their inability of properly inferring from feedback which is known to be systematically biased. We perform this test by replicating experiment E1 with one important modification. In particular, in the treatment condition, participants are informed explicitly about the (biased) selection rule. All experiments are described in detail in Table 1.

Table 1 The experimental design

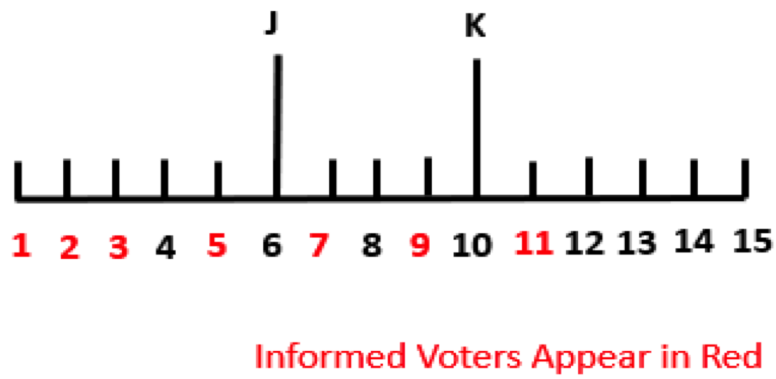
	Experiment E1	Experiment E2	Experiment E3
Treatment	The two polls (out of the five) with the greatest support for K are revealed.	The two polls (out of the five) with the greatest support for K are revealed.	The two polls (out of the five) with the greatest support for K are revealed. Subjects are a priori informed about this.
Control	All five polls are revealed.	Two out of the five polls are randomly revealed. Subjects are a priori informed about this.	All five polls are revealed.

3.1 Voters' Preferences on Candidates, Voter Information, Polls, and Elections

In each experimental session, there are fifteen human voters (the two non-human 'candidates' are inactive, hence they do not vote). Voters are ordered according to their ideological positions as illustrated in Figure 1. Voter 1 is the most left-wing voter, while Voter 15 is the most right-wing voter. The median voter is in position 8, while candidates of parties J and K are in position 6 and in position 10, respectively. Ideological positions of candidates are the same in all rounds[5] and all voters know it in advance. At the beginning of each round, the ideological position of each voter is randomly drawn from integers between 1 to 15 (inclusive) without replacement.

[5] The interpretation is that the two parties consistently pick candidates that share their ideological views.

Figure 1 Ideological preferences in the experimental interaction



Notes. This figure illustrates the distribution of preferences and information across the fifteen participants in any given experimental round. The positions on the line are occupied by different participants in every round.

Each candidate's valence is drawn at the start of every round from a uniform distribution with values between 1 and 120.[6] At the time of the polls and the elections, the two drawn valences are known to voters in ideological positions $\{1, 2, 3, 5, 7, 9, 11\}$ who are the *informed voters*. The remaining voters, i.e. the ones in ideological positions $\{4, 6, 8, 10, 12, 13, 14, 15\}$, are the *uninformed voters*. They only know the distribution out of which the quality (valence) of the candidates is drawn.

The utility that voter $i \in \{1, 2, ..., 15\}$ obtains in the case where candidate $h \in \{J, K\}$ wins the election is given by $U_{ih} = X_i - \alpha d_{ih} + Q_h$, where U_{ih} is voter i's overall utility from candidate h being elected, X_i is voter i's utility from having a candidate with the same ideological position as herself being elected, while d_{ih} is the distance between the ideological positions of voter i and candidate h. Q_h is the valence of candidate h, and α is a parameter that measures the utility loss per unit of distance in ideological positions between i and h. For the purposes of our experiments, we set $X_i = 100$ and $\alpha = 5$ (for all voters, rounds, and sessions) and, as stated previously, $Q_h \sim U[1, 120]$.

After the valence is drawn for both J and K and informed voters receive this information, five polls, each inquiring four randomly chosen voters, take place. Sampled subjects are asked for whom they would like to vote in the upcoming elections (they may choose not to participate).[7] Given the number of drawn subjects who choose to

[6] To reduce noise across sessions, we drew these valences once and for all before the start of the first session and used the same random draws for every session and for all experiments. In rounds 1, 5, 9, 10, 11, 12, 13, 14 and 15, K has a higher valence, while J's valence is higher in rounds 2, 3, 4, 6, 7 and 8.

[7] Voters choose from the following three options: 'K', 'J', and 'Prefer not to participate'. Experimental poll results do not show the information on 'non-participation', as this corresponds to the salient information that voters receive in real polls, especially when multiple polls are presented. For instance, Figure 2

participate, a poll reports the fraction of those in favour of K and in favour of J, respectively. For example, a poll revealing the following fractions: [25% for J, 75% for K] indicates that out of the four voters, all of whom chose to answer, three expressed support in favour of candidate K and one in favour of J.[8] Note that a single voter may participate in multiple polls. After the five polls are created by the above process, some subset of the results (depending on the experimental condition) is presented to all voters. The summary of each experimental round (as it was provided to participants) is illustrated in Figure 3. The winner of the election is determined by simple majority, with ties broken by a random draw. Subjects participated in eighteen election rounds like the one described above, that is, three practice rounds and fifteen incentivised real ones.

Figure 2 YouGov Voting Intention Sample Screenshot (source: https://yougov.co.uk)

YouGov

Voting Intention Tracker (GB)

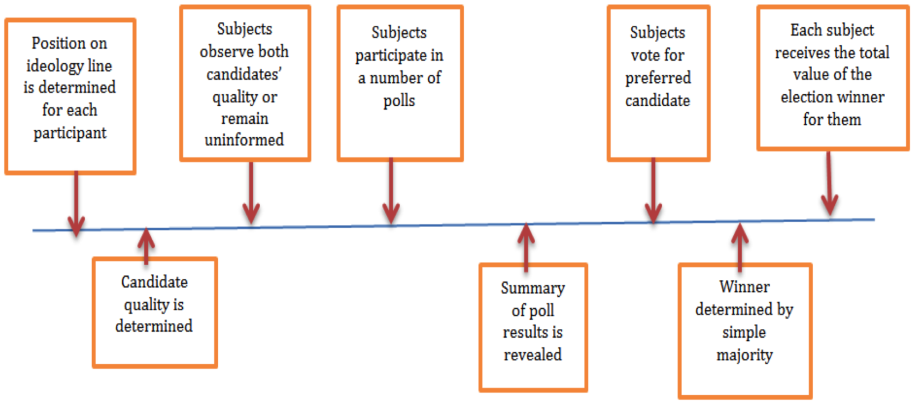
From 2019 General Election - Present

		Con	Lab	Lib Dem	SNP	Green	Brexit Party	Other	Con lead over Lab
		%	%	%	%	%	%	%	%
Start of Fieldwork	**End of Fieldwork**								
2020									
29/05/2020	30/05/2020	45	35	6	5	5	2	2	***10***
25/05/2020	26/05/2020	44	38	6	5	4	2	1	***6***
18/05/2020	19/05/2020	48	33	6	5	5	2	1	***15***
05/05/2020	06/05/2020	50	30	7	4	5	3	1	***20***
16/04/2020	17/04/2020	53	32	5	4	3	1	2	***21***
01/04/2020	02/04/2020	52	28	8	5	5	1	1	***24***
09/02/2020	10/02/2020	48	28	10	4	6	2	2	***20***
31/01/2020	02/02/2020	49	30	8	4	5	2	2	***19***
24/01/2020	26/01/2020	49	29	10	5	4	2	1	***20***
DECEMBER GENERAL ELECTION		44	32	12	4	3	2	3	***12***

illustrates the format of presentation of UK polls used by 'YouGov.co.uk'. This format of presentation is common for almost all online media appearing in an online search for 'voting intention polls', such as Financial Times tracker, 'ukpollingreport.co.uk' and 'markpack.org'. Accordingly, our experimental approach substantially simplifies the feedback that participants observe about the results of polls, while keeping in line with the presentation structure used in real life elections. This is especially important, since participants need to infer overall support for each candidate on the basis of results from multiple polls, without being cognitively overwhelmed.

[8] If, out of the four sampled voters, three opted to support K and one chose not to participate, the poll would be presented as 0% in favour of J and 100% in favour of K.

Figure 3 Sequence of actions in each experimental round



Since some voters are uninformed about the difference in valence between the two candidates, pre-election polls can be socially valuable in this setting. In particular, they can be utilised to transmit information about the candidates' valence from informed to uninformed voters. It is theoretically important that the distribution of informed voters is not symmetric in the ideological spectrum. If the distribution was symmetric, then the socially efficient outcome would be for uninformed voters to abstain from elections and let voting among informed voters determine the election outcome. In such an environment, polls would not perform a politically valuable role, because participation of uninformed voters would not be necessary. Instead, polls are meaningful in our setting, because they aggregate information about candidate valence when the ideological preferences of informed voters do not represent the ideological preferences of uninformed voters.[9]

To illustrate the hypothesised inference process of participants who do not account for the bias, let's assume that some uninformed voter observes substantial support in favour of K in the polls. If she perceives polls as unbiased and other subjects as rational, she will infer that K's valence is higher than J's, since some informed voters who are close to J's ideological position prefer to vote for K. These voters would do so only if K is of significantly higher valence than J. Accordingly, the uninformed voter, who observes the polls herself, infers from them the higher valence of candidate K and she may herself change her voting intention from J to K, depending on her position in the political preferences spectrum.

Note that the bias in exposure to poll results can in principle be detected through learning in all of our treatment conditions. Voters may perceive polls as unbiased in the

[9] However, this does not impact our experimental design, since even if the distributions of political preferences of informed and uniformed voters were identical and symmetric, a biased sample of polls (if not appropriately discounted) would still tilt the election result in the favoured candidate's direction.

early rounds of our experiments. However, informed voters know the true valence parameters of the two candidates and, if they are motivated primarily by pecuniary incentives, they will vote for the candidate that gives them the highest experimental payoffs. If this behaviour persists, poll results will systematically overstate candidate K's vote share in comparison to the election results. Voters able to learn from experience should detect this systematic difference, and adjust for it in their beliefs and behaviour. In short, our research design allows for polls to alter subjects' beliefs in favor of candidate K, but also for participants to detect the bias through experience.

3.2 The Three Experiments

The only stage that differs across the two experimental conditions in each of our three experiments is the one where the summary of poll results is revealed (see Figure 3). Table 2 illustrates how the information on poll results is revealed to subjects in the two experimental conditions of E1 and E3. Finding meaningful differences between 'control' and 'treatment' would indicate that biased polls can skew elections. The first benchmark (the control condition in our first experiment), which we use to judge whether 'skewing' takes place, is a perfectly transparent regime where all existing information (all five polls) is available to the public. This is a natural starting point. We also consider another benchmark (the control condition in our second experiment) where two out of the five polls are revealed in a random manner.

Table 2 Example presentation of poll results in each condition

Treatment					
COMPANY		B			E
Candidate K		75%			100%
Candidate J		25%			0%

Control					
COMPANY	A	B	C	D	E
Candidate K	33%	75%	25%	67%	100%
Candidate J	67%	25%	75%	33%	0%

Notes. There are five polling companies, A to E. The result of each company is represented in terms of the two fractions measuring support for each candidate. In the control of E1 and E3, all five results are revealed, in a format similar to the example of the table. In addition, if the above table represented an actual set of poll results, then, in the treatment condition of all three experiments, companies B and E would be revealed, since these polls yield the highest support for candidate K.

In terms of the treatment conditions, our natural point of departure (in E1 and E2) is an environment where voters observe the revealed information and have no a priori knowledge concerning how the two polls out of five are chosen to be revealed. In our view, this corresponds to many natural election environments of interest, where voters are not provided with any 'manual' describing the possible biases or agendas of those that reveal poll information. Instead, they have the chance to infer such biases and agendas through experience. In our experimental setting, this is accomplished because voters can compare poll predictions with actual election results (which they observe at the end of every round in all of our experimental conditions). In Experiment 3, we examine the consequences of providing a priori information about the exact nature of the bias to voters.

Experiments E1 and E2 had 120 participants each,[10] with eight 15-subject sessions (four control sessions and four treatment sessions).[11] Experiment E3 had 135 participants, with four control sessions and five treatment sessions. Participants in E1 and E2 were students at the University of Southampton and Newcastle Business School, and the experiments took place between May and November 2018. Participants in E3 were students at the University of York, and the experiment took place in June 2019. Our objective was for each experimental block (of 30 subjects) to achieve perfect randomisation by containing one control and one treatment session, with participants being randomly allocated between the two.[12]

In each session, subjects read instructions from their computer screens.[13] After the instructions, subjects participated in 18 rounds of play, including three practice rounds. At the end of the session, they were asked to complete a short questionnaire and were informed about their final score and monetary earnings. The core design of each round has been summarised in Figure 3. The only aspect that was not described there is the 'belief elicitation' stage. In particular, after the release of information on poll results, participants were asked to state their beliefs about the vote shares of the two candidates in the elections. The information on poll results revealed to participants took the form

[10]We shall use the words 'session' to denote each experimental interaction among 15 subjects who vote in the same election, and 'block' to denote the two sessions (one control and one treatment) taking place at the same time in the lab. A block has 30 subjects.

[11]We denote individual sessions as Ei_Cj or Ei_Tj where $i \in \{1, 2, 3\}$ denotes experiment, $j \in \{1, 2, 3, 4, 5\}$, denotes session , 'C' stands for control, and 'T' for treatment. For instance, $E1\_C1$ denotes the first control session in $E1$ and $E2\_T1$ the first treatment session in $E2$.

[12]The only three exceptions in this approach were sessions $E1\_C2$, $E1\_T2$ and $E3\_T5$, which were the only sessions of their block because of insufficient subject participation or lab capacity constraints.

[13]We programmed the experiments using O-tree (Chen et al., 2016) and recruited subjects via ORSEE (Greiner, 2015) in the University of Southampton and via *hroot* (Bock et al., 2014) in the Universities of Newcastle and York.

of a single probability distribution for each result, as shown in Table 2. Participants' beliefs at the elicitation stage were also expressed in terms of this binary probability distribution.

4 Results

Let us first provide an overall summary of the primary treatment effect across the three experiments: the rate of electoral success. Table 3 illustrates (for all three experiments) the number of rounds won by each candidate in each session of the two treatments. In addition, the table shows - in the parentheses - the total number of votes that each candidate received in each session. Adding up across all sessions in a given treatment, we can see that in E1 party K won 60% of all rounds in the control condition but 80% of the rounds in the treatment condition. In E2 party K won 61.6% of all rounds in the control but 73.3% of the rounds in the treatment, while in E3 party K won 56.7% of all rounds in the control but 64% of the rounds in the treatment. These differences are relatively homogeneous in their magnitude and consistent in their sign, both across sessions of a given treatment and across rounds of a given session.

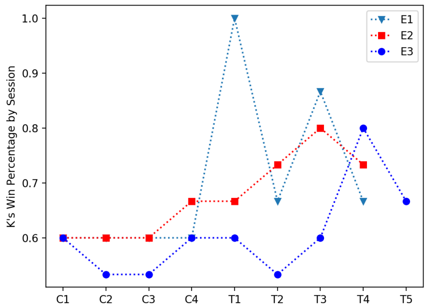
Table 3 Number of elections won and votes received by each party across sessions and experiments, and results of Mann-Whitney U Test

	E1		E2		E3	
	K	J	K	J	K	J
C1	9 (137)	6 (87)	9 (123)	6 (92)	9 (126)	6 (93)
C2	9 (118)	6 (97)	9 (135)	6 (85)	8 (139)	7 (82)
C3	9 (125)	6 (88)	9 (136)	6 (80)	8 (134)	7 (85)
C4	9 (131)	6 (82)	10 (126)	5 (82)	9 (123)	6 (97)
T1	15 (195)	0 (30)	10 (142)	5 (77)	9 (141)	6 (79)
T2	10 (151)	5 (72)	11 (143)	4 (70)	8 (138)	7 (77)
T3	13 (165)	2 (55)	12 (148)	3 (66)	9 (141)	6 (78)
T4	10 (150)	5 (63)	11 (163)	4 (56)	12 (158)	3 (65)
T5					10 (143)	5 (80)
P-value	0.010 (0.015)		0.018 (0.015)		0.120 (0.018)	

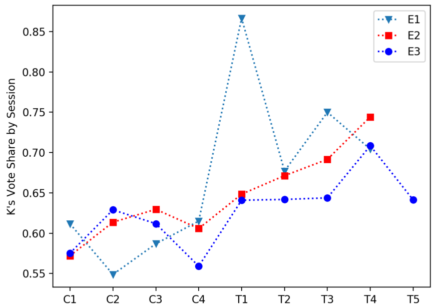
Notes. The numbers in the parentheses correspond to the total votes for each party in different sessions. The p-values are calculated as follows. In each session, the number of elections won by K (number of votes received by K) constitutes our continuous measure. Each of the three tests examines the null hypothesis that the probability that this continuous measure in a random control session is larger than the analogous measure in a random treatment session is equal to 0.5.

We examine whether these differences are statistically significant using a Mann-Whitney test. For the three experiments, we treat each experimental session as an observation, and the continuous variable we compare across the two treatments is the number

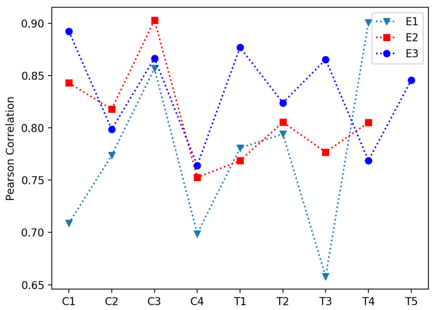
Figure 4 Comparison of descriptive results across three experiments



(a) Fraction of rounds won by K in each experimental session



(b) Average vote share for K in each experimental session



(c) Statistical correlation between average beliefs and average revealed polls

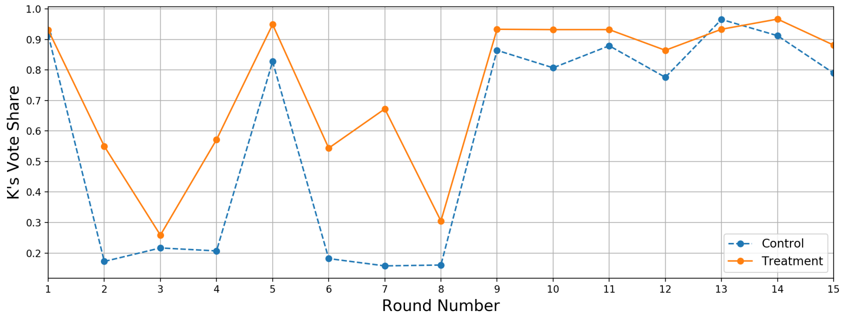
Notes. Figure 4a: In E1 and E2, the first three control sessions (C1, C2 and C3) have identical winning percentages for K, thus the relevant data points in the figure overlap.

of rounds won by K in a session. As we can see from Table 3, the difference in rounds won by K is statistically significant for Experiments 1 and 2 but not for Experiment 3. We also compare the total election votes for K in a session, and the difference is statistically significant for all three experiments. In general, the differences are sizable and consistent, as we shall illustrate below.

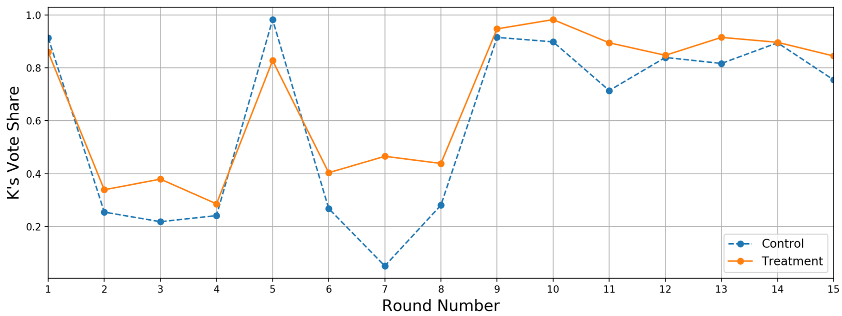
4.1 Experiment E1

Recall that in Experiment E1, the 15 participants in each control session voted every round after having been exposed to the results of all five polls, while in the treatment condition, the respective 15 participants were exposed to the two polls that had the greatest voting intention for the candidate of party K (but this was not explicitly stated). The most important general finding is that the treatment did offer a considerable advantage to party K. Biased exposure to polls increased both the likelihood of party K winning the election and its vote share. Figures 4a and 4b juxtapose the fraction of

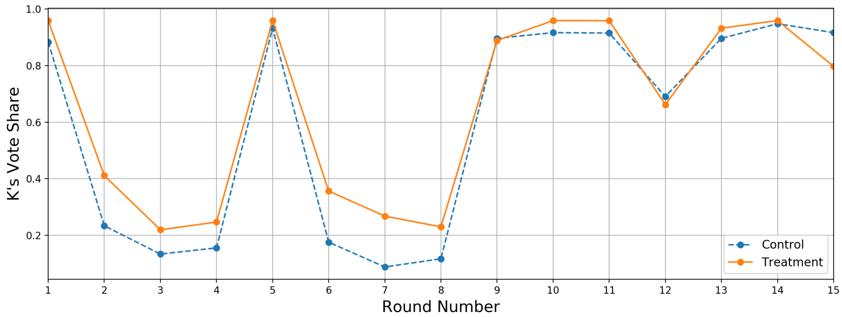
Figure 5 Comparison of vote share round-per-round in all experiments



(a) E1



(b) E2



(c) E3

election rounds won by K and vote shares for K in treatment versus control sessions. It is clear that for E1 the electoral performance of K is consistently better in all treatment sessions relative to any control session. K won more rounds than J in both the treatment and the control condition. This is, however, to be expected since (by pure chance) in most rounds the randomly drawn valence for K was higher than the drawn valence for J. In fact, in 9 out of the 15 regular rounds K had higher valence than J, and in 7 of these the difference in favour of K was over 20 points.[14]

Furthermore, the difference in vote shares does not appear only at the average level, but also for each individual round. Figure 5a shows the vote share of candidate K in the treatment and the control condition for each round (averaging across the four sessions of each condition). The figure indicates that 'treatment' rounds have consistently higher vote shares for K than 'control' rounds. In fact, in E1, vote shares in 'treatment' are higher than vote shares in 'control' for 14 out of 15 rounds. This is important because it does not seem to be the case that the difference vanishes in the last few rounds. Since the existence of stochastic valences renders optimal behaviour dependent on beliefs, the data are consistent with the hypothesis that participants do not sufficently discount poll results in the treatment condition, even after several opportunities for learning. We shall now delve deeper into this important issue.

Evidence from Beliefs

As we explained previously, after the 'summary of polls' stage and before elections, participants were asked to state their beliefs about the vote shares of the two candidates in the upcoming elections. We use this elicitation of subjects' beliefs to examine whether they are in alignment with the poll information that participants received. At this point, we need to define two measures that we shall use frequently in the subsequent analysis. First, 'average revealed polls' in a given round is the share of voters supporting K that can be inferred by the revealed polls in this round. For instance, in E1, in every round of the treatment condition, this share is derived as the average of two polls, while in rounds of the control condition this share is derived as the average of five polls. Second, 'average beliefs' in a given round will refer to the elicited expected vote share for K averaged across session participants. If participants in the treatment

[14] Figure C.1 in the appendix indicates that there is enough heterogeneity in the findings of the five polls, so that revealing a biased selection of poll results is meaningful. For almost all rounds of the treatment sessions, the vote share of K differs substantially across polls, so selecting the ones with the highest share gives a non-representative image of the average vote share of K.

condition perceived polls to be biased, then they should predict different vote shares for the election than the analogous poll information revealed, and this could potentially lead to a low correlation between average beliefs and average revealed polls. However, Figure 4c shows that the correlation is clearly not larger in the control sessions relative to the treatment sessions.

Averaging all sessions and rounds of the treatment condition together yields a grand mean of average beliefs equal to 74.7%, while the grand mean of average revealed polls is 76.7%. While this points to negligible discounting of revealed polls, it is worth examining this relationship in more detail with the help of appendix Figures C.2 and C.3. In particular, these figures juxtapose (in each round and session) average revealed polls and average beliefs. In both conditions, average beliefs closely follow the average revealed polls, with no clear pattern of differences. This is consistent with the idea that participants do not discernibly discount poll information neither in the treatment nor the control condition.[15]

4.2 Experiment E2

Experiment E1 compared the electoral results in a 'biased regime', where there is a systematically biased selection of poll results revealed to the public, to a 'full information' regime. This full information regime is a natural benchmark to consider: the public is informed about the totality of relevant evidence for democratic decision-making. However, a weakness of this benchmark is that it provides more information than the control condition (the results of five polls instead of two). For this reason, it is important to also employ a control condition where the amount of information is similar to the treatment (the 'biased regime'). For this purpose, we conducted the same number of randomised blocks (four 30-subject blocks) in an additional experiment (Experiment E2) where the control condition revealed the results of only two out of the five polls, and these two polls were chosen randomly.

As we can see from Figures 4a and 4b, the evidence points consistently to the direction observed in Experiment E1, but the treatment effects are smaller. This seems to be driven mainly by different behaviour in the treatment, rather than in the control. In turn, this disparity is likely caused by the different evolution of beliefs. The following

[15] As we will show in Section 5, this is compatible with the actual feedback that participants receive. In particular, in the treatment condition the biased polls seem to predict electoral behaviour well, presumably because they become self-confirming prophecies.

subsection explains that in the treatment condition of E2 participants appear to partly discount the results of polls, in the sense that they expect meaningfully lower vote shares for K than what average polls predict.

Figure 5b illustrates the vote share differences round-by-round. Once more, in E2, the pattern is that the vote share for K is uniformly higher in the treatment than in the control, while the difference does not seem to disappear with learning. These differences appear somewhat smaller than in E1. However, the difference is still meaningful: pooling sessions within each experimental condition of E2, the number of rounds won by J in the control is nearly 50% larger than in the treatment (23 vs. 16). Overall, the consistency of the pattern indicates that the biased release of poll information has relatively robust and predictable effects on electoral results.[16]

Evidence from Beliefs

There is no a priori reason to expect that subjects in the treatment condition of E2 would behave differently than in the treatment condition of E1, since these conditions are practically identical.[17] However, some systematic patterns of discounting poll results exist in the treatment condition of E2. In particular, the average beliefs for K in the treatment sessions (grand mean: 71.6%) tend to be lower than average revealed polls (grand mean: 77.6%). This indicates that subjects seem to somewhat discount the (inflated because of bias) advantage in favour of K presented in the polls. This pattern does not seem to hold for the control sessions, since the grand mean of average beliefs for K is 59.5% and the grand mean of average revealed polls is 58.8%.[18] Figure 4c tends to confirm this pattern. In particular, looking at the line pertaining to E2, it appears that the correlation between average beliefs and average revealed polls is systematically higher in the control (where information is unbiased) than in the treatment (where information is biased). Yet, our econometric analysis in Section 5 provides very little support for the claim that participants discounted biased polls in E2.

[16]In our study, the qualitative effects are robust across several dimensions (experiment, session, round). However, as is common in social sciences, the exact effect sizes may depend on the context (Kessler and Vesterlund, 2015). For instance, we expect that if we were to conduct experiments with 10 polls instead of 5 (keeping other aspects of the experimental environment constant), we would likely find larger treatment effects. However, implementing this would be burdensome for participants in the current experimental environment.

[17]However, as we shall see in Section 5, despite the similar experimental structure, the feedback that participants received in these two treatment conditions was different. Only in the treatment condition of E2 do participants observe polls to systematically overpredict K's performance.

[18]Figure C.6 in the appendix shows that this small discounting of polls is apparent for most sessions and rounds of the treatment condition, while Figure C.5 shows no pattern of such discounting in the control condition.

4.3 Experiment E3

It may be argued that in actual democratic elections people have enough experience with the political process and the media in order to gauge the agendas and incentives of those who reveal poll information. In particular, it is likely that some voters have a strong prior about the 'biased feedback' rule. Accordingly, our environment in the treatment conditions of E1 and E2 might be criticised as capturing only the special case of elections with young or inexperienced voters, especially in early rounds of play. Moreover, the structure of the treatment conditions of E1 and E2 makes it difficult to pinpoint exactly the mechanism that drives the treatment effect. In particular, the effect may be either because of the inability of voters to understand that the information is selected in a systematically biased manner, or due to their difficulty in deducing information from a biased set of results even when they know the biased process that generates it.

To address these concerns, we run a third experiment (E3) where the treatment condition entails using the same biased rule as in the treatment conditions of E1 and E2, but with full clarity about this biased rule. In particular, the instructions mentioned that: "After polls have taken place in each round, the findings of the two companies which exhibit the greatest support for candidate K will be revealed to you. All participants will observe the fraction of votes that each of the two candidates received in the polls of these two companies" and then provided an example to illustrate the biased rule. In this environment, a rational participant would observe the results of these two companies and then try to gauge information about the valence of the two candidates accounting for the selection rule underlying these results. Once more, the issue is whether subjects sufficiently discount the information (typically) in favour of K having the higher valence, and thus whether society avoids the swaying of election results due to the biased reporting rule.

The basic results of E3 (which had five treatment sessions with the 'known biased rule' and four control sessions with the 'transparent democracy' information environment) are illustrated in Figures 4 and 5c. As can be seen, even in this case, the biased feedback rule seems to offer an advantage to candidate K. In particular, the four sessions with the best electoral performance for K (as measured by the fraction of elections won) are all sessions with the 'known biased rule'. The difference is – once more – politically meaningful: the number of rounds won by J per session in the control is about 20% larger than in the treatment (6.5 vs. 5.4). Again, it is the consistency and robustness of the effect of the biased release of poll information on electoral results that is striking. A similar message is conveyed by examining the average vote share of K in each session. In particular, in all treatment sessions K has a higher vote share relative to any control session. Figure 5c shows that the difference exists for most rounds, and that it is rather sizable when 'ceiling effects' are not binding.[19]

One interpretation of this finding is that polls create a judgemental anchor for voters' beliefs regarding election outcomes. Voters do not seem to have the capacity to account for the bias in the polls to its full extent. Instead, they seem to use poll results as anchors, which they adjust until they reach an acceptable range for their beliefs. The use of such a heuristic is reasonable, given the complex setting and its cognitive implications for participants. The following section provides futher evidence for such partial adjustment.

Evidence from Beliefs

The comparison between average revealed polls and average beliefs becomes interesting, especially compared to E2 and E1. In the treatment sessions of E3, there is a weak tendency for average revealed polls to exceed average beliefs (grand means are 74.5% and 68.7%, respectively). This is not true in the control sessions (grand means are 59.7% and 60.4% for average revealed polls and average beliefs, respectively).[20] However, correlational analysis shows no systematic patterns (see the line corresponding to E3 in Figure 4c). In particular, average beliefs do not seem more strongly correlated to average revealed polls in the control condition than in the treatment condition. This lack of strong support in favour of subjects' discounting of biased polls is corroborated by the analysis of session 5. Explicit models of learning are introduced, but they do not detect meaningful discounting of poll results in E3. In conclusion, despite the fact that participants are fully informed in E3, belief adjustment is small and insufficient, so that biased polls end up affecting electoral results.

[19]In the last five rounds of E3, the treatment effect appears small. However, in these rounds the vote share of K is so high that the treatment does not have much scope for increasing it further. This is referred to as a 'ceiling effect' in the behavioural literature, and the small treatment effects could be an artefact of this.

[20]Figures C.8 and C.9 in the appendix indicate that small discounting takes place in most rounds of the treatment condition, but such discounting not discernible in the control condition.

5 Econometric Models

The preceding descriptive analysis shows that biased exposure to polls increases the likelihood of 'favoured' candidate K being elected. This is our main treatment effect, and the key question is why this is happening. Because of the relatively complex environment we are studying, it is unlikely that voters use the strategic structure of the environment to predict behaviour deductively, thus we shall focus on the effects of feedback and learning on beliefs and behaviour. If voters in the treatment condition (many of whom are uninformed) believe the (biased) evidence from polls that K is popular, they infer that K has a high valence and wish to vote for him.

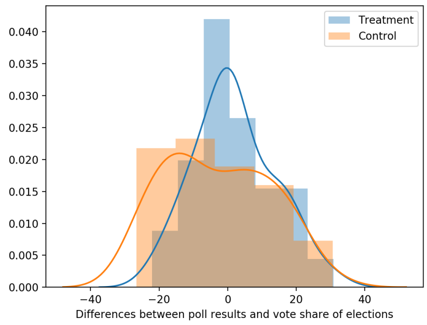
Accordingly, is the main treatment effect driven by voters' failure to account for the biased nature of polls? Our descriptive evidence points to partial and insufficient adjustment for biased polls in E2 and E3 and to no discounting at all in E1, but this needs to be investigated further econometrically. Our modelling strategy can be summarised as follows: if participants have enough opportunities to learn the biased nature of polls in the treatment conditions, beliefs should deviate from average poll information as participants gain experience. In the following analysis, we shall employ our measures of participants' beliefs to econometrically examine whether this is happening, hence complementing the descriptive results and the correlational evidence on beliefs.

Opportunities for Learning

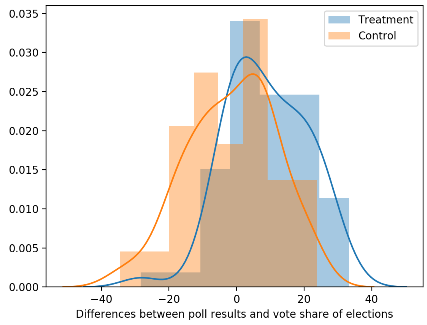
First of all, we need to examine whether it is reasonable to expect at all that participants in the treatment conditions (especially in of E1 and E2) will learn the biased nature of polls. This expectation comes from the fact that participants observe both the poll predictions and the electoral results in every experimental round. So, do participants have opportunities for learning? Figures 6a-6c illustrate the distribution of the differences between average revealed polls and the actual election results of the same round (both of these results are represented by the voting share for K). If the illustrated distribution of differences in a given condition is concentrated on positive values, this means that poll results in this condition systematically overpredicted K's vote share, and participants had the opportunity to observe this for a number of rounds.

Figure 6a refers to E1 and it indicates that in the treament condition the distribution is slightly more concentrated on higher values than in the control condition. The mean difference between average revealed polls and election results is 1.94 for the

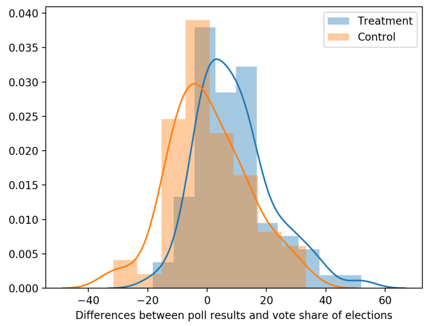
Figure 6 Distribution of differences in the vote share of K: revealed poll results vs. elections



(a) E1



(b) E2



(c) E3

Notes. The figures present the distribution of differences between average revealed poll results (presented as K's voting shares) and the actual election results in the same voting round. Data are pooled across rounds and sessions of an experimental condition. Experimental conditions where these differences tend to be large are conditions where subjects have the opportunity to infer that polls are biased.

treatment and -2.97 for the control. Accordingly, in this experiment participants had limited opportunities to observe a discrepancy between average revealed polls and election results. On the other hand, Figures 6b and 6c indicate that the distributions for the treatment conditions of E2 and E3 are concentrated on positive values much more than the respective control conditions. In E2, the mean difference for the treatment and control condition is 8.9 and -1.4, respectively. The analogous mean differences in E3 are 9.1 and 0.47, respectively. This indicates that, in E2 and E3, there was a pattern whereby in the treatment – but not in the control – average revealed polls systematically overpredicted K's vote share. Hence, in the treatment conditions of E2 and E3, participants were exposed to systematic positive discrepancies between poll predictions and the actual performance of K. Consequently, in these experiments participants had the opportunity to infer the biased nature of polls, and this should impact on the evolution of their beliefs.

Models of Polls, Beliefs and Voting

We will now examine in detail the evolution of beliefs and voting behaviour at the session level. We consider one round as the unit of observation. Since the same subjects participate repeatedly in a given session and the number of sessions is relatively small, we cluster errors at the session level and use wild bootstrapping for estimating standard errors (Cameron and Miller, 2015). The first model we estimate (Model 1) takes the following form:

$$\begin{aligned} &Belief_t = \alpha + \beta_1 Poll_t + \beta_2 Late + \beta_3 Treatment + \\ &(\textit{all possible two-way interactions}) + \beta_7 Poll_t \times Late \times Treatment + \epsilon_t \end{aligned} \tag{1}$$

The dependent variable $Belief_t$ is 'average beliefs' about candidate K's vote share in round t. $Poll_t$ is 'average revealed polls' in round t.[21] $Late$ is a 'late rounds' dummy variable taking the value 0 for early rounds (rounds 1 to 10) and 1 for late rounds (rounds 11 to 15). $Treatment$ is a treatment dummy (1 if the session is in the treatment condition, 0 otherwise). Model 1 examines in a formal manner the effect of the revealed poll information on subjects' beliefs in order to shed light on whether they perceive the polls as biased or not and on whether there are learning effects.

We are principally interested in the interactions of the dummy variables with $Poll_t$. A significant negative coefficient in the interaction term $Late \times Poll_t$ could indicate that the degree to which revealed poll results manipulate beliefs weakens through time. This would be consistent with the notion that subjects distrust polls at the treatment condition as time passes by (but we would not expect the same for the control condition). On the other hand, a significant negative interaction between $Poll_t$ and $Treatment$ could imply that in the treatment condition there is a weaker relationship between beliefs and average announced poll results. We would strongly expect such a negative interaction to exist in E3, since participants are explicitly informed about the bias.

As Table 4 indicates, the results of the model do not support the notion that subjects in E1 are able to learn and account for the bias in the treatment condition. In particular, there is no significant interaction between the 'late rounds' dummy and average revealed polls, although the respective coefficients are negative in both the treatment

[21] Thus, this specification models voters as rather unsophisticated, forming inferences about each candidate's support by merely taking the average of the polls revealed to them.

and the control. Results of E2 and E3 show a similar pattern. Interestingly, the interaction between $Late$ and $Poll_t$ is positive in the control but negative in the treatment condition in both E2 and E3. However, none of this is statistically significant. On aggregate, there seems to be weak, if any at all, evidence that subjects somewhat discount poll information in late rounds. The experimental condition also does not seem to make a difference: the estimated coefficient of $Treatment \times Poll_t$ is negative in all three experiments but none of the estimates is statistically significant; the estimated coefficient of the three-way interaction term does not have a consistent sign across the three experiments and it is not statistically significant in any of them.[22]

The second model we examine (Model 2) takes the form:

$$\begin{aligned}(Belief_t - Poll_t) = \alpha + \beta_1(Poll_{t-1} - Vote_{t-1}) + \beta_2 Treatment \\ +\beta_3 Treatment \times (Poll_{t-1} - Vote_{t-1}) + \epsilon_t\end{aligned} \quad (2)$$

$Vote_t$ is the vote share which K received in the election of round t. We use Model 2 to explicitly examine whether there is evidence for learning. Again, we focus on reinforcement-type learners, who observe the model's variables through time. If they observed that $(Poll_{t-1} - Vote_{t-1})$ was large, this means that (in the previous round) polls overestimated the performance of K relative to the election outcome. We expect that if subjects learn, this will result in adjusting their beliefs in the current round (for K's share) downwards conditional on the poll results, hence we expect a decrease in $(Belief_t - Poll_t)$.

As Table 5 indicates, the coefficients for $(Poll_{t-1} - Vote_{t-1})$ are small and not significant. This indicates that we do not find support in favour of the assumed belief adjustment mechanism. However, the treatment dummy has a negative sign and is statistically significant for all three experiments. This indicates that beliefs are closer to poll results in the control condition (especially in E2 and E3).

We conclude that the overall evidence (including descriptive and correlational evidence discussed in earler sections) indicates some weak tendency for beliefs in the treatment conditions of E2 and E3 to adjust for biased polls. However, when a learning mechanism is considered explicitly in Models 1 and 2, adjustment through time is hard to detect.

[22]The results of Model 1 are robust with respect to the exact specification, in the sense that in versions of Model 1 with fewer interaction variables, the results do not change (this analysis is available upon request).

Table 4 Effect on Beliefs (Model 1)

	E1			E2			E3		
	Pooled	Treatment	Control	Pooled	Treatment	Control	Pooled	Treatment	Control
Avg. poll info.	0.856***	0.848***	0.856***	0.815***	0.787***	0.815***	0.897***	0.847***	0.897***
	(-0.073)	(-0.038)	(-0.079)	(-0.04)	(-0.025)	(-0.043)	(-0.053)	(-0.059)	(-0.058)
Late rounds	16.819*	3.823	16.819	-2.863	14	-2.863	-0.342	7.117	-0.342
	(-8.458)	(-8.625)	(-9.097)	(-2.544)	(-6.408)	(-2.737)	(-10.386)	(-3.625)	(-11.299)
Late rounds*Avg. poll info.	-0.166	-0.021	-0.166	0.036	-0.092	0.036	0.044	-0.015	0.044
	(-0.102)	(-0.087)	(-0.109)	(-0.027)	(-0.08)	(-0.03)	(-0.141)	(-0.042)	(-0.154)
Is treatment*Avg. poll info.	-0.008			-0.027			-0.049		
	(-0.081)			(-0.046)			(-0.077)		
Is treatment	-0.118			-3.027			-2.158		
	(-4.939)			(-3.166)			(-4.762)		
Is treatment*Late rounds	-12.996			16.863**			7.459		
	(-11.655)			(-6.478)			(-10.947)		
Is treatment*Late rounds*Avg. poll info	0.145			-0.128			-0.059		
	(-0.13)			(-0.079)			(-0.147)		
Constant	9.094**	8.975*	9.094	11.694***	8.667**	11.694**	5.750**	3.592	5.75
	(-3.777)	(-3.424)	(-4.062)	(-2.764)	(-1.662)	(-2.973)	(-2.458)	(-4.273)	(-2.674)
Observations	120	60	60	120	60	60	135	75	60
R-squared	0.945	0.931	0.942	0.965	0.954	0.969	0.961	0.962	0.958

Robust standard errors clustered by session are in parentheses

$*p < 0.1, **p < 0.05, ***p < 0.01$

Table 5 Effect on the Differences between Beliefs and Average Poll Information (Model 2)

	E1			E2			E3		
	Pooled	Treatment	Control	Pooled	Treatment	Control	Pooled	Treatment	Control
ΔPV_{t-1}	0.034	-0.086	0.034	0.051	-0.032	0.051	-0.031	-0.064	-0.031
	(0.056)	(0.080)	(0.060)	(0.086)	(0.094)	(0.093)	(0.007)	(0.028)	(0.007)
Is treatment	-4.662**			-6.826***			-6.042***		
	(1.872)			(1.030)			(0.731)		
Is treatment *ΔPV_{t-1}	-0.12			-0.083			-0.033		
	(0.093)			(0.123)			(0.028)		
Constant	3.083***	-1.579	3.083***	1.304	-5.523***	1.304	1.118**	-4.924***	1.118
	(1.118)	(1.615)	(1.202)	(0.965)	(0.389)	(1.037)	(0.447)	(0.606)	(0.486)
Observations	112	56	56	112	56	56	126	70	56
R-squared	0.137	0.029	0.005	0.229	0.004	0.009	0.217	0.016	0.004

Notes. ΔPV_{t-1} is the difference between the revealed poll information and the actual vote share of K in the last round.
Robust standard errors clustered by session are in parentheses.
$*p < .1, **p < .05, ***p < .01$

6 Welfare Effects and Individual Behaviour

Welfare Effects

In terms of the welfare effects of biased polls, a rough measure of utilitarian welfare is the average experimental payoffs in each condition. One reason that biased polls should have a negative impact on this measure is that they introduce noise in the information conveyed by polls to voters. Moreover, if voters do not discount the information contained in biased polls properly, then they will tend to vote more frequently for candidate K even if he is of lower valence than candidate J.

Table 6 Average payoffs in each session

Session in E1	Average Experimental Payoffs	Session in E2	Average Experimental Payoffs	Session in E3	Average Experimental Payoffs
E1_C1	171.27	E2_C1	171.27	E3_C1	171.27
E1_C2	171.27	E2_C2	167.40	E3_C2	170.80
E1_C3	171.27	E2_C3	171.27	E3_C3	170.80
E1_C4	171.27	E2_C4	169.33	E3_C4	171.27
E1_T1	159.87	E2_T1	169.93	E3_T1	171.27
E1_T2	168.93	E2_T2	166.93	E3_T2	170.80
E1_T3	164.27	E2_T3	165.93	E3_T3	168.73
E1_T4	168.93	E2_T4	166.93	E3_T4	165.67
				E3_T5	169.27

Notes. These payoffs are the average individual experimental points across all rounds. In a particular round, a voter's payoffs depend on the distance of her ideological position to the winning candidate's position, and on the winning candidate's valence (see experimental instructions in Appendix B for details).

Indeed, our findings confirm these conjectures, as can be seen in Table 6. In particular, sessions in the treatment condition were generally associated with lower payoffs per subject than sessions in the control condition. In fact, average individual payoffs across conditions were 171.3 (control) vs. 165.5 (treatment) in E1, 169.8 vs. 167.4 (respectively) in E2 and 171.03 vs. 169.15 (respectively) in E3. This disparity resulted from the fact that the high-valence candidate lost in the treatment condition more often than in the control condition.

Specifically, in the control of E1 the high-valence candidate always won. In contrast, in the treatment condition of E1 there were 12 elections where candidate J lost, despite having the higher valence (candidate K never lost when their valence was higher). In E2, while in the control condition there were three elections where the high-valence candidate lost, this increased to eight elections in the treatment condition.[23] In E3, in

[23] Out of all these instances, only once did J win when K had the higher valence (it happened in the control condition).

the control condition, out of 60 elections, there were two cases where K was the high-valence candidate but J won in the end. The opposite never happened. In the treatment condition, out of 75 elections, there were two times when K was the high-valence candidate but J won in the end, and four times when J was the high-valence candidate but K won in the end.

Behaviour at the Individual Level

It also worthwhile to provide some insights on the behaviour of informed voters. We should note that in our experiments, informed voters face an easy decision: they should simply vote for the candidate that gives them the highest payoff, which they can easily calculate.[24] Accordingly, if these individuals' votes deviate from 'optimal behaviour' this would indicate that the assumption of rational, money-maximising political agents is violated. Table 7 illustrates the behaviour of informed voters in the election stage, depending on whether they are J-voters or K-voters. For instance, in 8.57% of the 420 election vote decisions that informed voters made in the control condition of E1, informed voters chose candidate K although the money-maximising choice was candidate J. Similarly, in 34.05% of the 420 decisions that informed voters made in the treatment condition of E2, informed voters chose candidate J and their money-maximising choice was also candidate J. As can be seen, most decisions by informed voters are consistent with the money-maximising model.

Table 7 Behaviour of informed voters

Preferred/ Voted for	E1		E2		E3	
	Percent of total choices		Percent of total choices		Percent of total choices	
	Control	Treat.	Control	Treat.	Control	Treat.
K/J	5.24	3.33	4.29	3.81	3.10	1.90
J/J	38.33	27.86	37.86	34.05	38.57	39.05
J/K	8.57	19.76	8.81	13.81	8.81	9.14
K/K	44.52	47.38	46.43	47.38	46.38	49.52

Notes. This table presents the voting behaviour of informed voters in the final elections. The data are pooled across rounds and also at the experiment level. 'Preferred' stands for the money-maximising choice of candidate, while 'voted for' signifies the actual voting choice in the elections. Please note that the fractions do not add up to 100%, because abstention is allowed at the election voting stage. In total, there are 420 decisions by the seven informed voters in the four sessions of each condition of each experiment (except E3, where in the treatment condition there are five sessions and thus 525 such decisions).

Nonetheless, a non-trivial fraction of decisions, slightly lower than 15% for the

[24]For simplicity, we shall call 'h-voter' an informed voter whose money-maximising choice is candidate h, where $h \in \{J, K\}$.

controls and ranging between 11% and 23% for the treatments, deviates from the prediction of the model of selfish money-maximising agents. A possible explanation for this behaviour is 'bandwagon preferences', i.e. a genuine willingness of the participants to vote for the likely winner, which is not captured by monetary payoffs. Interestingly, J-voters are more likely to vote for candidate K in the treatment condition than in the control, and within the treatment condition this type of behaviour is more common than the opposite (i.e. K-voters voting for candidate J). Thus, 'bandwagon preferences' are likely to be relevant, and in particular they seem to amplify the effects of biased polls.

Table 8 Comparison of individuals' voting at the polls vs. the final election

	E1		E2		E3	
Poll/Election	Treatment	Control	Treatment	Control	Treatment	Control
J/J	58.00%	76.06%	72.43%	78.35%	77.50%	78.69%
J/K	40.80%	22.01%	26.75%	18.90%	20.63%	19.67%
J/A	1.20%	1.93%	0.82%	2.76%	1.88%	1.64%
K/J	4.68%	13.99%	5.40%	12.57%	5.82%	14.04%
K/K	94.55%	84.55%	94.03%	86.03%	93.32%	85.67%
K/A	0.78%	1.46%	0.57%	1.40%	0.86%	0.29%

Notes. The table juxtaposes voting at the poll stage with the respective vote in the elections for the same individual and the same period. Behaviour at treatment vs. control conditions is compared and data are pooled at the experiment level. 'A' stands for abstention in the final elections. Only the decisions of individuals who voted for some candidate at the polls are considered.

It is also useful to discuss the behaviour of voters at the poll stage. Table 8 compares the voting choice at the poll stage to the one at the actual elections.[25] The table indicates that, if subjects truthfully reported voting intentions in the polls, the treatment induced some voters to switch in the direction of voting for K in the elections. Moreover, the voting pattern for those that chose K in the polls is similar across experiments: in all experiments, about 8-10% of poll voters for K, who would otherwise depart from voting K in the elections (as indicated by behavior in the control condition), are induced by the treatment to stick to K. However, there are significant differences across experiments in the behaviour of those who chose J at the poll stage, and these can partially account for the heterogeneity of the primary treatment effect across experiments. In particular, as we move from Experiment 1 to Experiment 2, and then to Experiment 3, the effects of treatment in inducing those that voted for J in polls to switch to K in the elections falls from 19.2% to 7.85% to about 1%.[26] These were mainly uninformed voters who were

[25] Note that this table does not contain the behaviour of all subjects, since some were not randomly chosen to any poll, and some who were chosen opted not to participate. In total, Table 8 contains information for about 70% of overall election votes.

[26] These percentages are obtained as the difference between treatment and control in the J/K row in each

likely induced to switch to K in the elections because of the treatment.[27]

7 Discussion and Conclusions

In this paper we examined the existence and implications of biased mechanisms that propagate the results of voting intention polls. We presented results from a series of experiments with majority voting where participants received information regarding poll results in a systematically selective manner. The environment we considered is a two-candidate election contest with common values (concerning candidates' valence) and no voting costs. Our findings indicate that biased exposure to polls consistently skews the electoral outcome in a predictable way. In a robust manner, elections that took place in the 'biased polls' environment provided an electoral advantage to the candidate that was 'favoured by the bias'.

This effect was smaller when in the control condition two polls were randomly revealed, as opposed to all five polls being revealed, but the direction of the effect was consistently the same. Similarly, effects were smaller when voters were explicitly informed of the selection rule under which poll information was revealed, but the treatment effect was still sizable and consistent. Overall, the empirical results from E1 and E2 show limited evidence that the repeated opportunities for learning allowed voters to understand the systematic bias and account for it. The evidence from E3 indicates that it is especially the second part of this statement that matters (failing to account for the bias once one realises it).

A possible explanation for this behaviour is the genuinely complex environment where voting takes place. For instance, as Figure 6a indicates, pure feedback alone is unlikely to be sufficient for learning. In Experiment 1, in terms of comparing election results to average revealed polls, the biased treatment condition would not appear as particularly more 'suspicious' to an active learner than the unbiased control condition. Accordingly, voters who are unable to form inferences regarding the strategic nature of

experiment. Recall that the entries in this row correspond to the percentage of cases, out of all cases where someone voted both in the polls and the elections and chose J in the polls, that this voter voted for K in the elections. The higher occurrence of this in the treatment condition can be interpreted as a treatment effect.

[27] Tables D.1-D.3 in the appendix provide an overall summary of voting behaviour at the poll stage in the different sessions of the three experiments. The results are broken down by different status of voters (informed vs. uninformed). Certain insights can be inferred from Tables D.1-D.3: informed voters are more likely to participate to polls, while uninformed voters are more likely to vote for K rather than J in the polls (which makes sense, since their ideologies are closer to K). Moreover, there seem to exist no systematic differences between treatment and control, which is again unsurprising, since the treatment is different from the control only when voters observe poll results.

the interaction, but only learn from experience, are unable to adjust their behaviour.

However, in the treatment condition of E2, election results systematically assigned a lower vote share to K than average reveled polls. In E3, subjects had a priori information about the bias, and Figure 6c indicates that, in the treatment condition, elections also tended to diverge from average poll predictions in a systematic way. Despite all of this, in these experiments subjects only discounted the revealed poll information weakly in forming their beliefs. This indicates that even perfect a priori information, in conjunction with subsequent feedback, are not enough to ensure that voters sufficiently discount the results of biased polls. Participants seem to anchor their beliefs on average revealed polls and insufficiently adjust for the feedback they receive.

How applicable to real-world settings can results derived from our experimental environment be? We believe that our primary result, that people are unable to sufficiently account for biased polls and hence such bias might robustly distort elections, is likely to generalise to the real world. Our subjects participated in fifteen elections (plus three practice rounds). The number of rounds is reasonably high, given the length of the typical experiment in the literature. If anything, the time delay in real election environments might make learning more challenging. Obviously, our stylised environment simplifies important aspects of real elections. However, it seems to us that if subjects are unable to adjust for systematic bias in a stylised environment such as this, they are unlikely to do so in more complicated real elections settings. In addition, the results of E3 indicate that even intergenerational transmission of information about the incentives, agendas and biases of information providers is unlikely to undo the electoral effects of selection and bias in revealed poll results.

In summary, it seems that our laboratory paradigm can rigorously inform the public discussion on the risks of biased propagation of poll results for democratic outcomes. Our results are robust and consistent and indicative of a potential problem. More evidence using this paradigm in the lab and the field is needed (including replications of this study) before safe policy conclusions can be made. The stakes for electoral policy are high.

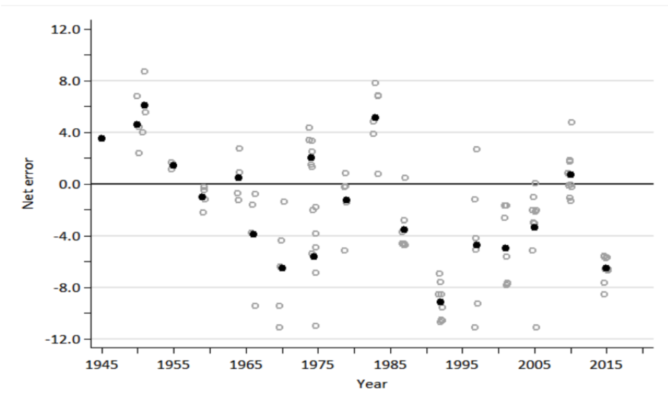
Appendices

A Bias in Propagation of Poll Results: Descriptive Evidence from the Field

Our experimental sessions took it for granted that systematic bias exists in the exposure of voters to pre-election poll outcomes. In this appendix we shall present evidence to support the relevance of this claim for real-life elections.

Leaving aside the possibility of conscious manipulation, several plausible mechanisms could generate systematic bias in the feedback that citizens receive about the results of voting-intention polls (Sturgis et al., 2016). First of all, pollsters have methodological flexibility similar to other empirical scientists (Ioannidis, 2005) and if they have strong priors about who is leading, they may choose methods that verify these priors (for example, turnout adjustments). Moreover, the traditional media reveal poll results selectively, either to pander to the expectations of their audience (Gentzkow and Shapiro, 2010) or to simply make interesting news (Larsen and Fazekas, 2019). Finally, the voters themselves may propagate results in a biased manner, especially via social media, and in this section we will provide empirical evidence for this mechanism.

Figure A.1 Net Error in Poll Estimates of the Conservative lead over Labour in the UK (source: Sturgis et al., 2016)



Notes. The net error measures the difference between the poll estimate and the election outcome for a party. The light grey markers indicate the net error for each pollster, and the solid black markers are the mean net error for all pollsters.

We use two sources of real-world evidence to illustrate that the public receives a biased idea of voters' political preferences from voting-intention polls. First, psephologists have long argued that polls in the UK systematically under-predict the vote share of the Conservative party and they over-predict the vote share of the Labour party. Figure A.1, originally included in the study of Sturgis et al. (2016), shows the net error in UK election polls for the last seventy years. The figure strongly indicates that the error is not random, but a systematic pattern emerges, especially in the last fifty years. For some reason, the performance of labour relative to the conservatives is systematically overestimated in UK polls.

We will now show that social media may also propagate polls in a biased manner, so that voters do not necessarily observe a representative sample of polls. In particular, we examine the biased propagation of published opinion poll estimates or trackers in the United States (US) and the United Kingdom (UK). We consider measures of voting intentions for US Presidential elections (reported by HuffPost pollster.com) and for UK parliamentary elections (YouGov's political tracker). This enables us to assess the spread patterns of the published poll results in two different countries. Our objective is to show that in some real-life electoral races a subset of voters is exposed to poll results in a manner that systematically depends on the results of the polls themselves.

From a theoretical perspective, people have various cognitive mechanisms that result in selective attention, such as negativity bias (Soroka, 2014), motivated reasoning (Taber and Lodge, 2006), cognitive dissonance (Morwitz and Pluzinski, 1996) or disproportionate responsiveness to outliers. Users of social media are also not demographically or politically representative of the general population (Mellon and Prosser, 2017), which could give rise to further biases in attention, via selective reporting. As a result, we expect that individuals attend to and, crucially, propagate to others, the results published by polling firms in a systematically biased manner. Indeed, as we demonstrate below, this seems to be the case.

A.1 Opinion Polling in the US and UK

While opinion pollsters in the US and UK ask a wide variety of survey questions on political issues, among the most prominent measures of political attitudes are for presidential elections (in the US) and Westminster voting intentions (in the UK). These are central to depictions of the 'horse race' by media (Iyengar, 1991; Matthews et al., 2012). In the US, George Gallup famously introduced random sampling methods to measure national voting intentions in the 1936 presidential election. Variants of the question "If the election were held today, whom would you vote for?" have been asked regularly ever since. During the 2016 presidential election campaign there were well over 400 national opinion polls of voting intentions for Donald Trump and Hilary Clinton, yielding a steady flow of information on the election horse race.

In the UK, pollsters have been asking people about their voting intentions for Westminster parliamentary elections since 1943 (Wlezien et al., 2013; Sturgis et al., 2016). YouGov has become one of the highest volume pollsters in the UK since their introduction of online methods in 2001, regularly fielding the question "If there were a general election held tomorrow, which party would you vote for?" During the first government of the UK's former Prime Minister David Cameron (2010-2015), it fielded a survey almost every other day.

A.2 Opinion Polling Data on Social Media (Twitter)

HuffPost Pollster and YouGov each report their latest poll estimates via their official accounts on the social media platform Twitter (in the case of HuffPost this involves polls conducted by other polling firms). This provides a regular stream of poll information that enables us to analyse patterns of selective reporting, by social media users, in an observational setting. With frequent estimations of public opinion (at least every other day), most fluctuations in poll estimates are attributable to noise due to sampling error (even where dampened by poll aggregators), and thus most users are (arguably) reacting to random short-term fluctuations, rather than systematic trends.[28] While it would in theory be possible to collect data on wider engagement with poll estimates on Twitter, this approach enables us to model a fairly stable source of poll information.

Table A.1 Twitter reporting of poll estimates in the US and the UK

	US – Presidential election voting intentions	UK – General election voting intentions
Choice	Trump/Clinton	Labour/Conservatives/Liberal Democrats
Pollster	All pollsters	YouGov
Start	8 September 2015	9 April 2010
End	8 November 2016	8 December 2017
Measure	Voting intention, by candidate	Voting intention, by party
N of polls	445	1,451
N of days	428	2,801
Polls per day	1.04	0.52
N of Δ in vote	444	1,450
Retweets	5,054	41,291
Source	@pollsterpolls	@YouGov
Search terms	"2016 General Election", "Trump", "Clinton"	"Lab", "Con", "Westminster voting intentions"

Notes. This table presents some general descriptive information regarding our empirical study of poll results on Tweeter.

We obtained relevant tweets of poll estimates from @pollsterpolls and @YouGov using an advanced Twitter search with terms corresponding to the standard form of poll

[28]We will show that a systematic bias in the propagation of poll results is even evident in responses to short-term noise, rather than more sizable long-term trends. With such trends we should expect such bias to play an even more important role.

reporting used by each organisation (removing all extraneous cases from the scraped data). Details of these search terms are provided in Table A.1. All the tweets report the current *level* of voting intentions for the relevant candidate or party. We calculate the change in voting intentions from the previous poll estimate in our dataset. This forms the independent variable of our analysis – the change in observed poll estimates.

Crucially, we also collected data on the number of 'retweets' for each tweet. This provides us with a measure of online propagation of the poll result, our dependent variable. On average, each poll estimate received 24.4 retweets, with an upward trend over time in the number of retweets as usage of Twitter grew. Our analysis undertakes an ordinary least squares regression of the number of retweets of a given poll estimate (*Retweets*) as a function of change in candidate or party support (Δ*Vote*). In the US we focus on change in the 'margin' between the candidates, i.e. the lead of Clinton over Trump. In the UK we focus on change in support for the Labour, Conservative and Liberal Democrat parties. This focus on *change* enables us to determine whether biased propagation of poll results can stem from mere short-term fluctuations, rather than structural differences between particular candidates or parties.[29] The estimated models therefore take the following form, where *Equation A.1* refers to the US, and *Equation A.2* to the UK.

$$US : Retweets = a_0 + b_1\Delta(Vote(Clinton) - Vote(Trump)) + \epsilon \tag{A.1}$$

$$UK : Retweets = a_0 + b_1\Delta Vote(Con) + b_2\Delta Vote(Lab) + b_3\Delta Vote(LD) + \epsilon \tag{A.2}$$

The results for this analysis are reported in Tables A.2 and A.3. These reveal largely consistent, and also interesting, patterns (both within and across countries) in the propagation of poll estimates. In the US (Table A.2), an one-unit increase in the Clinton-Trump lead in polls reported by HuffPost Pollster was associated with 1.0 additional retweets of the poll estimate. This might signify the partisan lean of the users of Twitter or the followers of this specific polling account, but it does hint at a selective reporting mechanism of poll estimates that fundamentally distorts voters' (salient) information on the popularity of candidates. We should emphasise that we wish to establish empirically the existence of this distortion, and we do not wish to claim causality.

[29]This focus on short-term dynamics enables us to show that even between adjacent days there is a systematic bias in propagation, and in particular selective propagation occurs regardless of how popular a candidate or party is.

Table A.2 Selective propagation of poll estimates of the US 2016 presidential election

	Retweets
Δ (Clinton-Trump)	1.021
	(0.148)***
Intercept	11.353
	(0.666)***
N	444
R-squared	0.10
Adjusted R-squared	0.10

$*p < 0.05; **p < 0.01; ***p < 0.001$

Notes. The table presents our regression analysis for the propensity to re-tweet poll outcomes in the US presidential race between Clinton and Trump, depending on the poll results.

Table A.3 Selective propagation of poll estimates of voting intention, UK

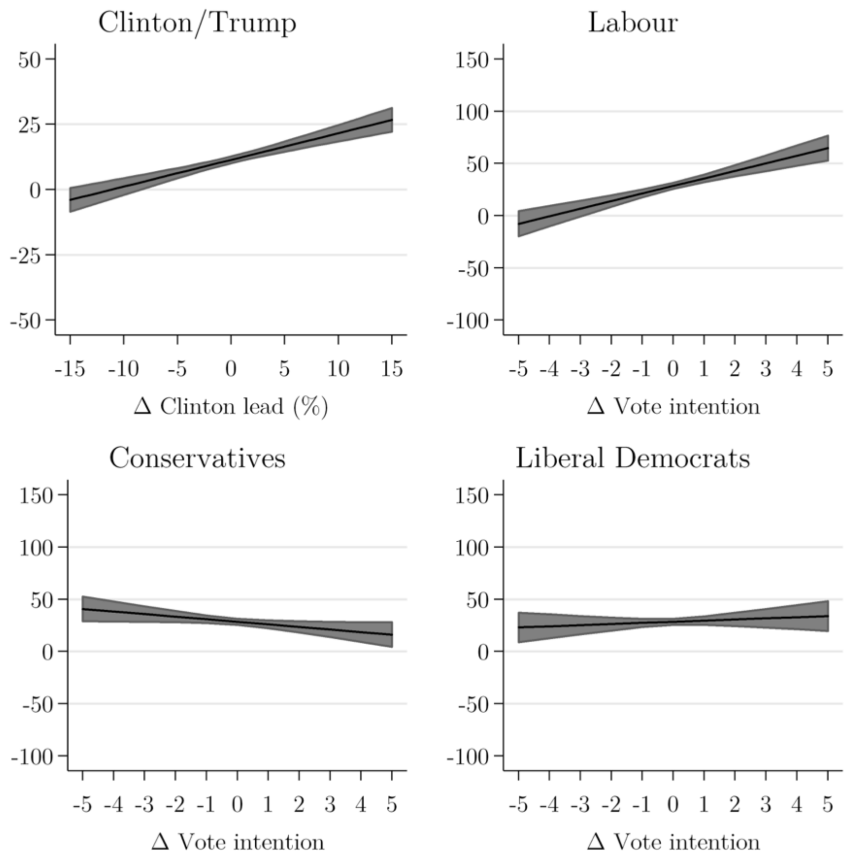
	Retweets
ΔVote(Con)	-2.464
	(1.162)*
ΔVote(Lab)	7.335
	(1.187)***
ΔVote(LD)	1.110
	(1.419)
Intercept	28.423
	(1.480)***
N	1,450
R-squared	0.04
Adjusted R-squared	0.04

$*p < 0.05; **p < 0.01; ***p < 0.001$

Notes. The table presents our regression analysis for the propensity to re-tweet poll outcomes in the UK general elections, depending on the poll results for each of the three major parties.

In the UK (Table A.3), we see a similar pattern whereby a one-unit increase in voting intentions for the Conservative Party leads to 2.5 fewer retweets of the poll. In contrast, a one-unit increase in support for Labour leads to extra 7.3 retweets. There are no systematic differences for the Liberal Democrats, at least during this period. Predicted values of the regression models are depicted in Figure A.2. These confirm the findings: there are distinct partisan differences in the online promulgation of poll results, specifically a bias where increases in support for left-wing parties/candidates are propagated more in online platforms, whereas it is drops in that support that receive wider spread for right-wing parties/candidates. In the UK context, interestingly, the pattern is more pronounced for Labour than the Conservatives, so this is not a purely symmetrical relationship.

Figure A.2 Adjusted predictions (with 95% confidence intervals) of the number of retweets, by ΔVote



We have thus shown empirically that in modern democracies the public is likely to be exposed to the results of pre-election polls in a biased manner.[30] Now we may ask: what are the implications of such a bias for democratic elections? If biased exposure skews elections, then we should be concerned and maybe need to address this by policy changes. The problem is that establishing a causal relationship about such a complex phenomenon in the field can be difficult. For this reason, we employ the experimental method increasingly popular in economics and political science (Palfrey, 2016). The

[30] Of course, given the network structure of the social media, it is not true that the same biased sample of polls is revealed to every voter (as is the case in our experiment). For instance, if left-leaning people mostly re-tweet to other left-leaning people, right-leaning people may not be exposed much to those polls. In fact, there is evidence that "the network of political retweets exhibits a highly segregated partisan structure, with extremely limited connectivity between left- and right-leaning users." (Conover et al., 2011). Accordingly, we are not claiming that the empirical pattern established here always generates a biased propagation pattern similar to the one used in our experiments. However, our correlational results do establish that different subsets of voters are exposed more to a certain type of results rather than to another type of results. As stated before, we believe that there are other propagation mechanisms that could more plausibly lead to an information pattern similar to the one used in our experimental design.

virtue of this approach is that it can establish causality and discover general patterns of social behaviour in a controlled setting.

B Experimental Instructions

◊ Note that the instructions differ across conditions only in the section of the instructions labelled as **"Information about Poll Results"**.

Instructions

In this study you will be interacting with a fixed group of fourteen other participants for a number of rounds. In each round, the fifteen participants will have the opportunity to vote in an election. The study will consist of 3 practice rounds and 15 regular decision-making rounds. Your performance in the regular rounds counts towards your final earned amount, while practice rounds do not count. For each round, the sequence of actions is illustrated below. In every step, new information will appear at the top of the screen, so please have a look at it carefully before you make any decision or proceed to the next step.

Figure 3 here

In each period, you will have the opportunity to vote in an election. One candidate is of PARTY K and the other one is of PARTY J. *Your payoff in each round will depend on the distance of your ideological position from the ideological position of the election winner and on the quality of the election winner.*

Ideological Positions

At the start of each period you will be given a 'position number' between 1 and 15. This number affects how you value the positions of the two candidates. The candidate of Party J is in position 6 and the candidate of Party K is in position 10. These positions remain fixed for both candidates for the entirety of the study, but your position may change every period. In any given round, each of the 15 participants in your group takes a different position. So, every round some participant takes position 1, another participant takes position 2, another participant takes position 3, and so on, up to position 15. The distribution of participants to positions changes every round. Your 'ideological score' from the victory of each candidate is equal to 100 points minus 5 times the difference between your position and the candidate's position.

Candidate Quality

For each of the two candidates an integer number has been randomly drawn for every round. The possible values that this number can take are between 1 and 120 and each number is equally likely to be selected. This 'quality number' reflects the competency of the candidate in handling policy matters. The higher the number is the better the quality of the candidate is. A new quality number was randomly redrawn every period for each candidate. Only some participants in each round will have the opportunity to learn its value.

Informed and Uninformed Participants

In every round some participants are told the quality numbers that have been drawn for the two candidates, e.g. "Candidate J's quality is 100 and candidate K's quality is 24." These are the informed participants. The rest of the participants receive no additional information. Who receives this information is determined by the ideological positions. Participants with positions $\{1, 2, 3, 5, 7, 9, 11\}$ are informed. Participants with positions $\{4, 6, 8, 10, 12, 13, 14, 15\}$ are uninformed. This fact does not change across rounds.

Payoff Example

For example, assume that in a particular round you are in position 3, K's quality in this round is 75 and J's quality in this round is 13. Since candidate K takes position 10, your 'ideological payoff' from K's victory in this round is: $100 - 5 * |3 - 10| = 65$. You also earn an additional score equal to the winner's quality. So, if candidate K wins the election then your total payoff is: $65 + 75 = 140$. On the other hand, Candidate J has position 6. Then, if candidate J wins the election then your total payoff is: $100 - 5 * |3 - 6| + 13 = 85 + 13 = 98$. Please notice that if the difference in the quality between the two candidates in a given round is greater than 20, then you will always receive a higher payoff if the candidate with the higher quality wins, regardless of your ideological position.

Figure 1 here

Polls

After the 'informed voters' receive their information, five polling companies will conduct voting intention polls. In each poll, four out of the fifteen participants will be randomly chosen to state their voting preferences. *This means that you may be asked to state your voting intention by one polling company, or by many, or by none.* If you are contacted by many companies, you only have to state your answer once, and the same answer will be used for all of them. Notice that at the time that polls take place, seven voters are informed of the actual quality of the two candidates in the forthcoming election and eight voters are uninformed.

◇ **Information about Poll Results** - *All five polls are revealed (Control condition in E1 and E3)*

After polls have taken place, the findings of the five companies will be revealed. All participants will observe the fraction of votes that each of the two candidates received in the polls of these five companies.

◇ **Information about Poll Results** -*Two biased polls are revealed (Treatment condition in E1 and E2)*

After polls have taken place, the findings of two companies will be revealed. All participants will observe the fraction of votes that each of the two candidates received in the polls of these two companies.

◇ **Information about Poll Results** -*Two out of the five polls are randomly revealed. Subjects are a priori informed about this (Control condition in E2)*

After polls have taken place, the findings of two companies will be revealed to you. These two companies will be selected randomly out of all five that conducted polls. All participants will observe the fraction of votes that each of the two candidates received in the polls of these two companies.

◇ **Information about Poll Results** - *Two biased polls are revealed. Subjects are a priori informed about this (Treatment condition in E3)*

After polls have taken place in each round, the findings of the two companies which exhibit the greatest support for candidate K will be revealed to you. All participants will observe the fraction of votes that each of the two candidates received in the polls of these two companies. For example, consider some illustrative poll results for the five polling companies (A to E), in the following table. If those were the results of all five polls in a given round, then only the results of companies C and E would be revealed to you in that round. If there are ties, these will be broken with a random draw.

COMPANY	A	B	**C**	D	**E**
Candidate K	34%	50%	**100%**	50%	**75%**
Candidate J	66%	50%	**0%**	50%	**25%**

Beliefs about Election Results

After the poll results have been announced to you, and before elections take place, you will be asked to state the vote share that you expect each candidate to receive in the upcoming elections.

Elections and Payoffs

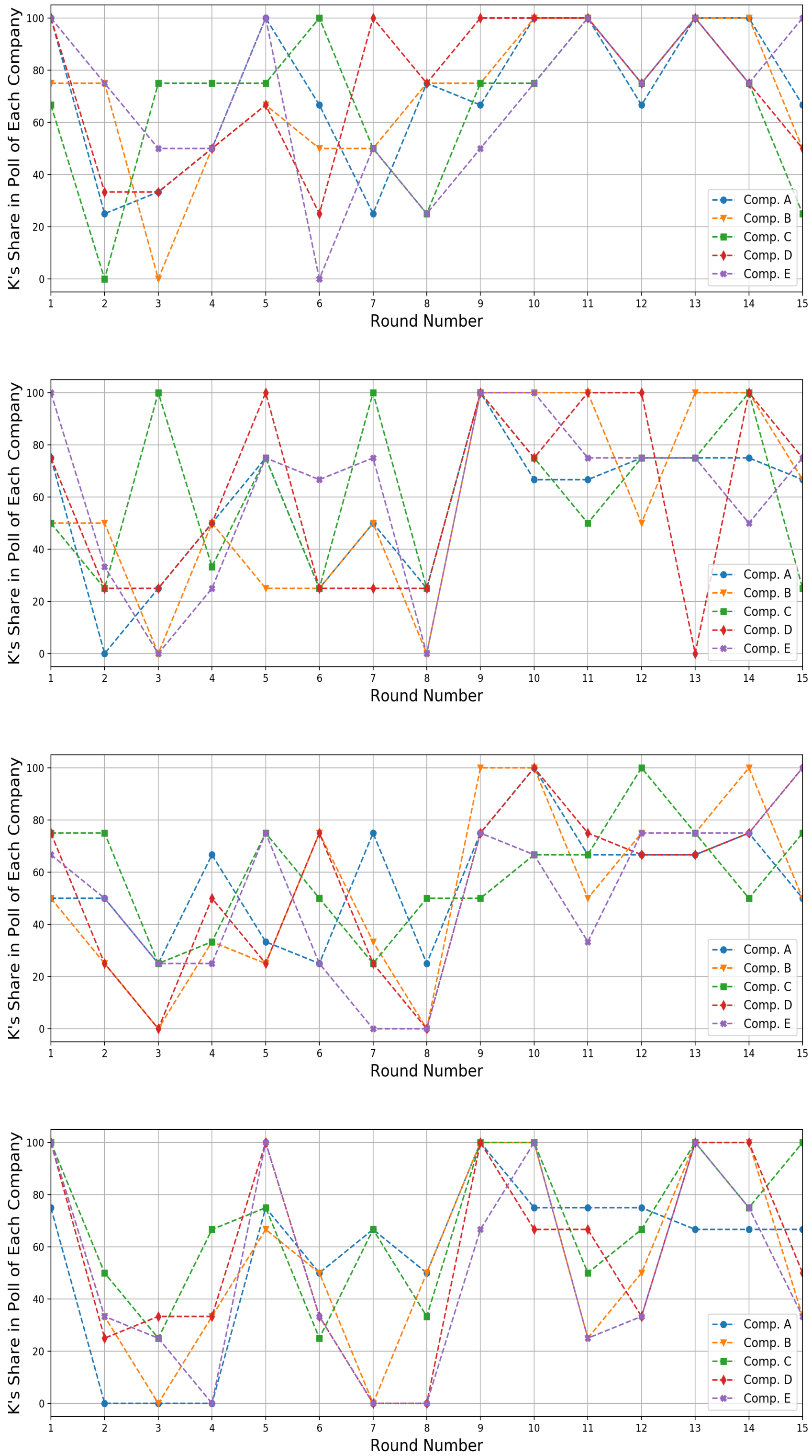
In the end of each period, elections take place, where each participant may vote or abstain. The winner of the election is determined by simple majority. In case of a draw, each candidate receives an equal chance of being selected as the winner.

Round Payoffs and Aggregate Payoffs from the Study

As described earlier, your total payoff from each round is the sum of the winner's quality and your 'ideological payoffs' from the candidate's victory. *Your total payoffs from this study will be the sum of all payoffs that you accumulate in each of the 15 regular rounds, plus your participation fee.* They will be paid to you in cash, at the end of the study. Each earned point will correspond to **half a penny**. You will now participate in three practice rounds. If you have any questions, please raise your hand and your question will be addressed individually.

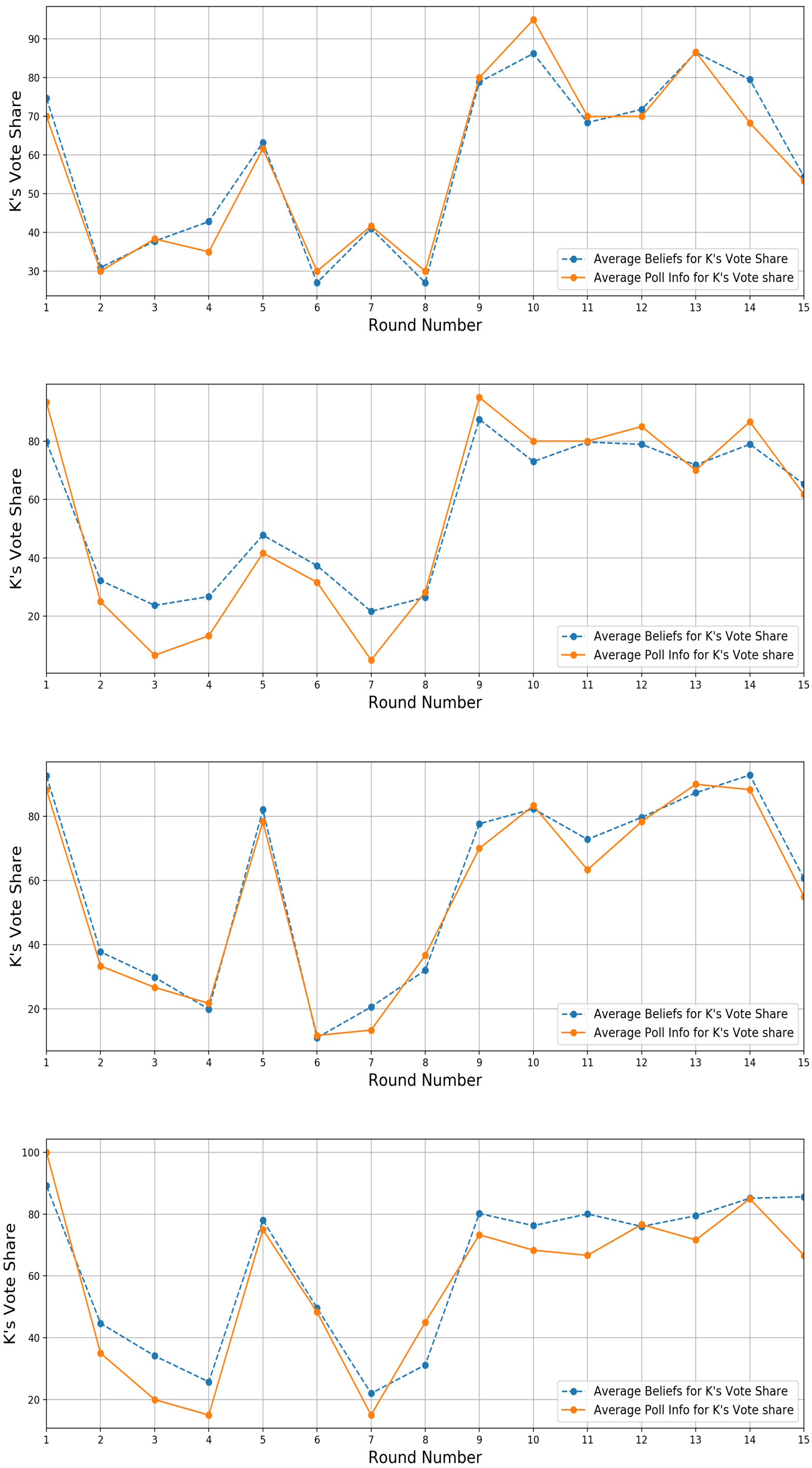
C Additional Graphs

Figure C.1 Poll outcomes in treatment sessions of E1 (T1-T4)



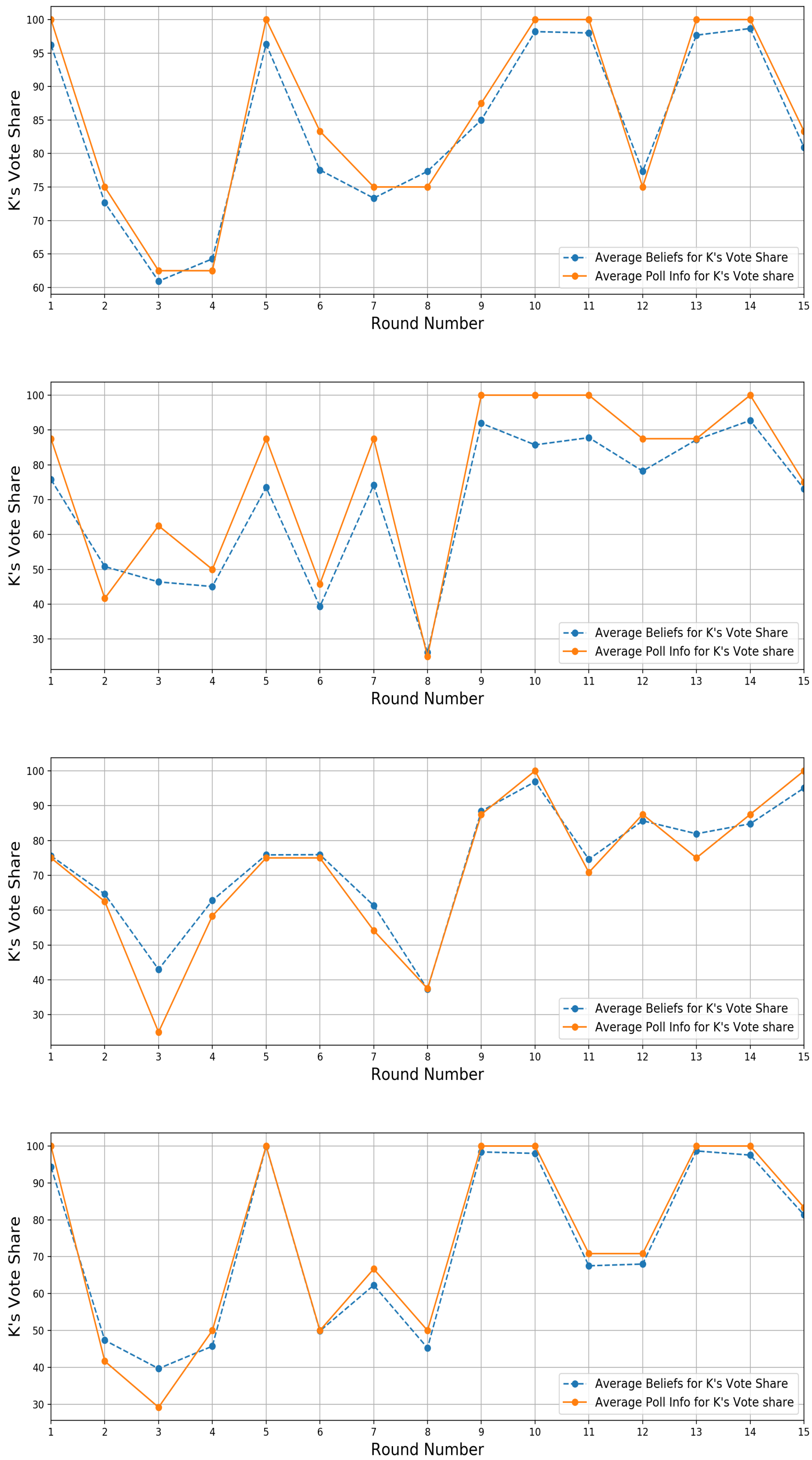
Notes. The graphs present the fraction of votes that candidate K receives according to the poll of each of the five companies in each period in treatment sessions of E1. The graphs for the four sessions T1-T4 are presented in sequence.

Figure C.2 Average beliefs vs. poll outcomes in control sessions of E1 (C1-C4)



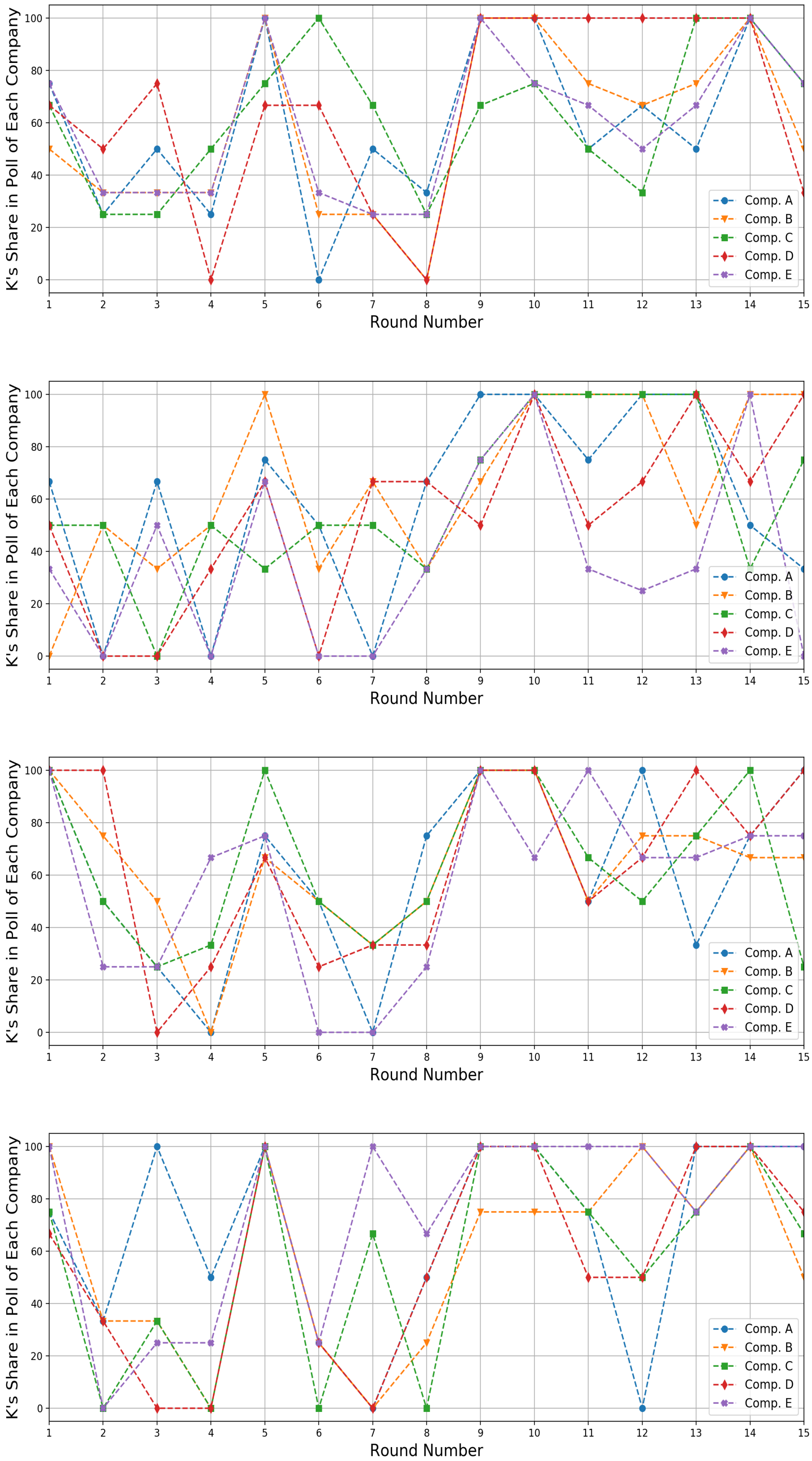
Notes. The dashed line presents the elicited beliefs on candidate K's vote share averaged over subjects. The solid line illustrates the vote share of candidate K according to polls, i.e. averaged across the five polling companies. The graphs for the four sessions C1-C4 are presented in sequence.

Figure C.3 Average beliefs vs. poll outcomes in treatment sessions of E1 (T1-T4)



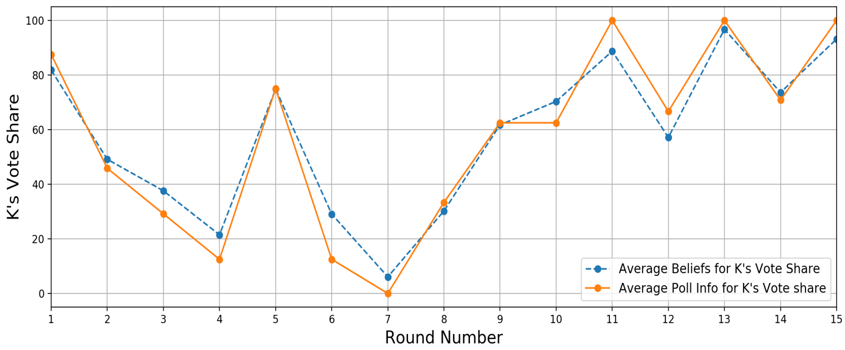
Notes. The dashed line presents the elicited beliefs on candidate K's vote share averaged over subjects. The solid line illustrates the vote share of candidate K according to polls, i.e. averaged across the five polling companies. The graphs for the four sessions T1-T4 are presented in sequence.

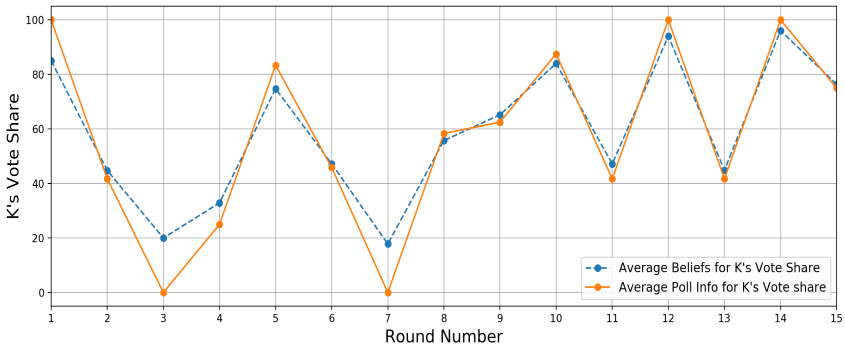
Figure C.4 Poll outcomes in treatment sessions of E2 (T1-T4)

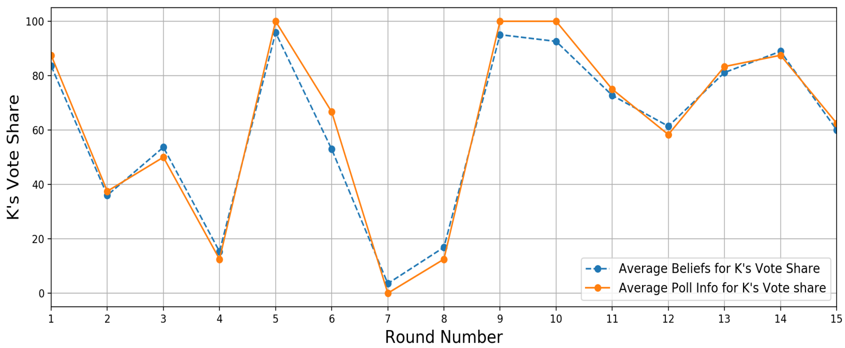


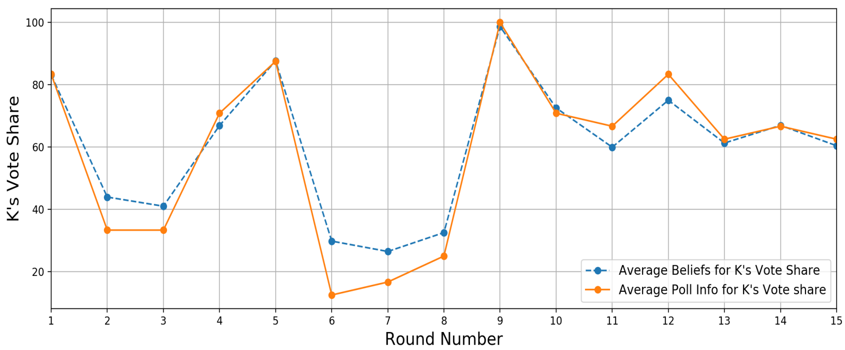
Notes. The graphs present the fraction of votes that candidate K receives according to the poll of each of the five companies in each period in treatment sessions of E2. The graphs for the four sessions T1-T4 are presented in sequence.

Figure C.5 Average beliefs vs. poll outcomes in control sessions of E2 (C1-C4)



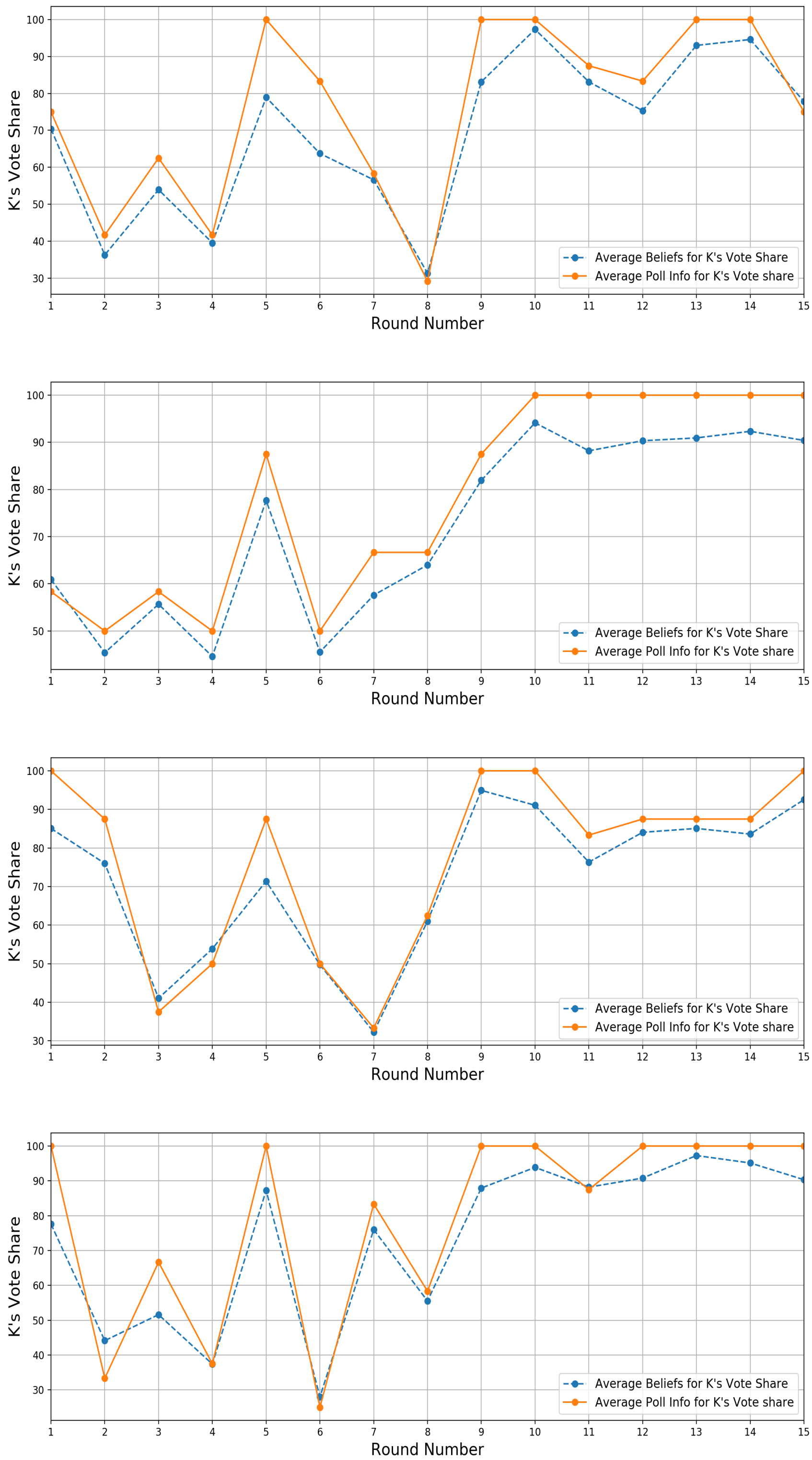






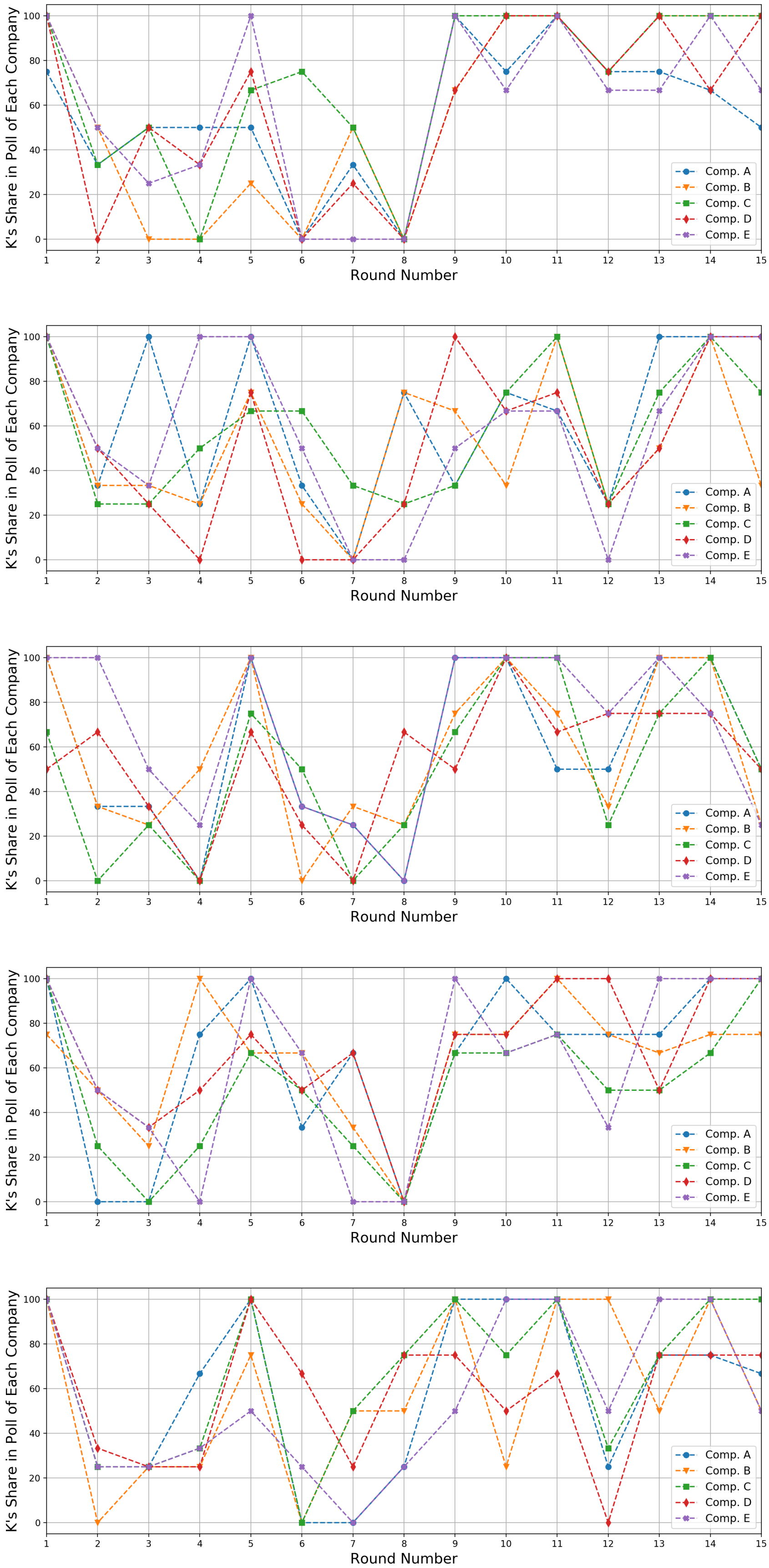
Notes. The dashed line presents the elicited beliefs on candidate K's vote share averaged over subjects. The solid line illustrates the vote share of candidate K according to polls, i.e. averaged across the five polling companies. The graphs for the four sessions C1-C4 are presented in sequence.

Figure C.6 Average beliefs vs. poll outcomes in treatment sessions of E2 (T1-T4)



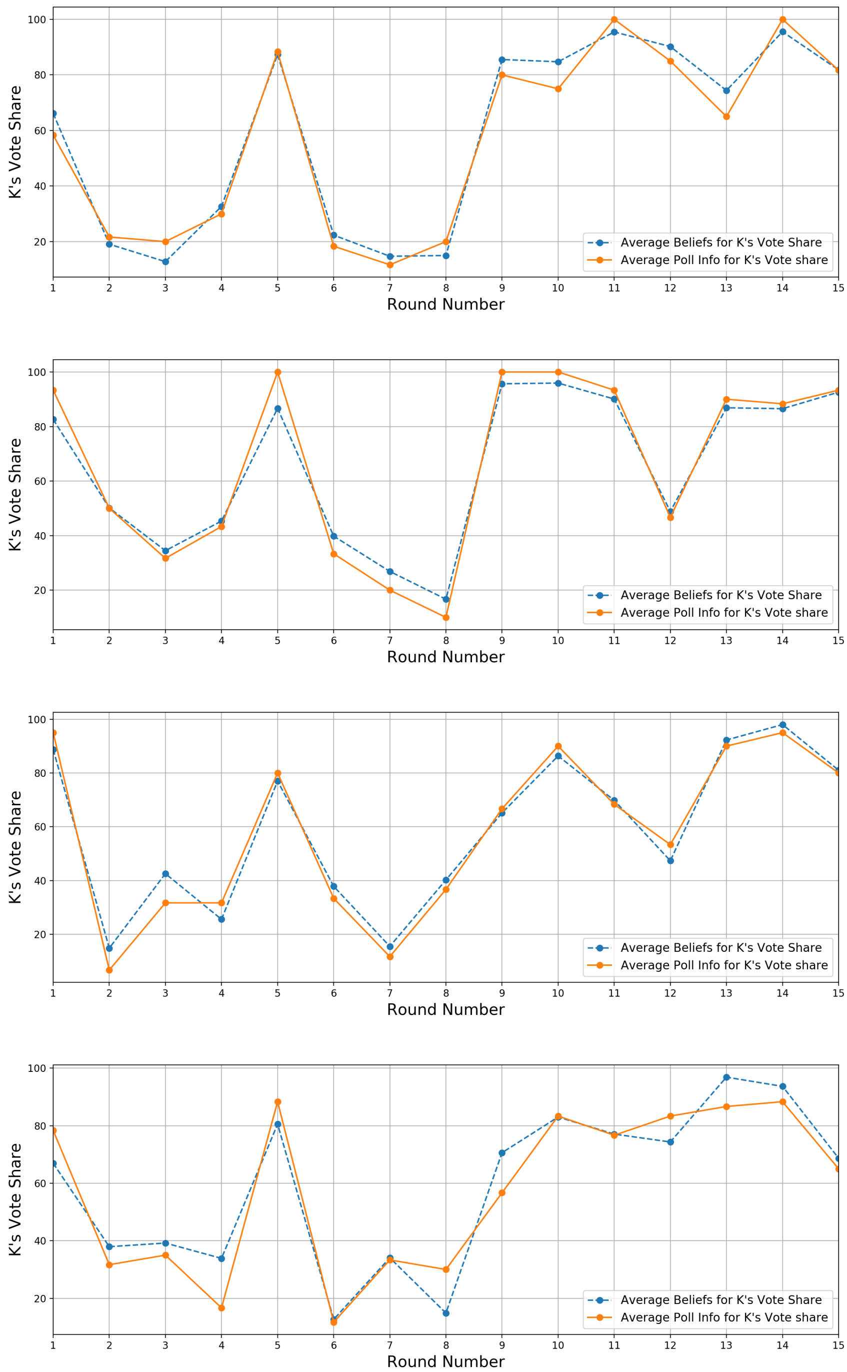
Notes. The dashed line presents the elicited beliefs on candidate K's vote share averaged over subjects. The solid line illustrates the vote share of candidate K according to polls, i.e. averaged across the five polling companies. The graphs for the four sessions T1-T4 are presented in sequence.

Figure C.7 Poll outcomes in treatment sessions of E3 (T1-T5)



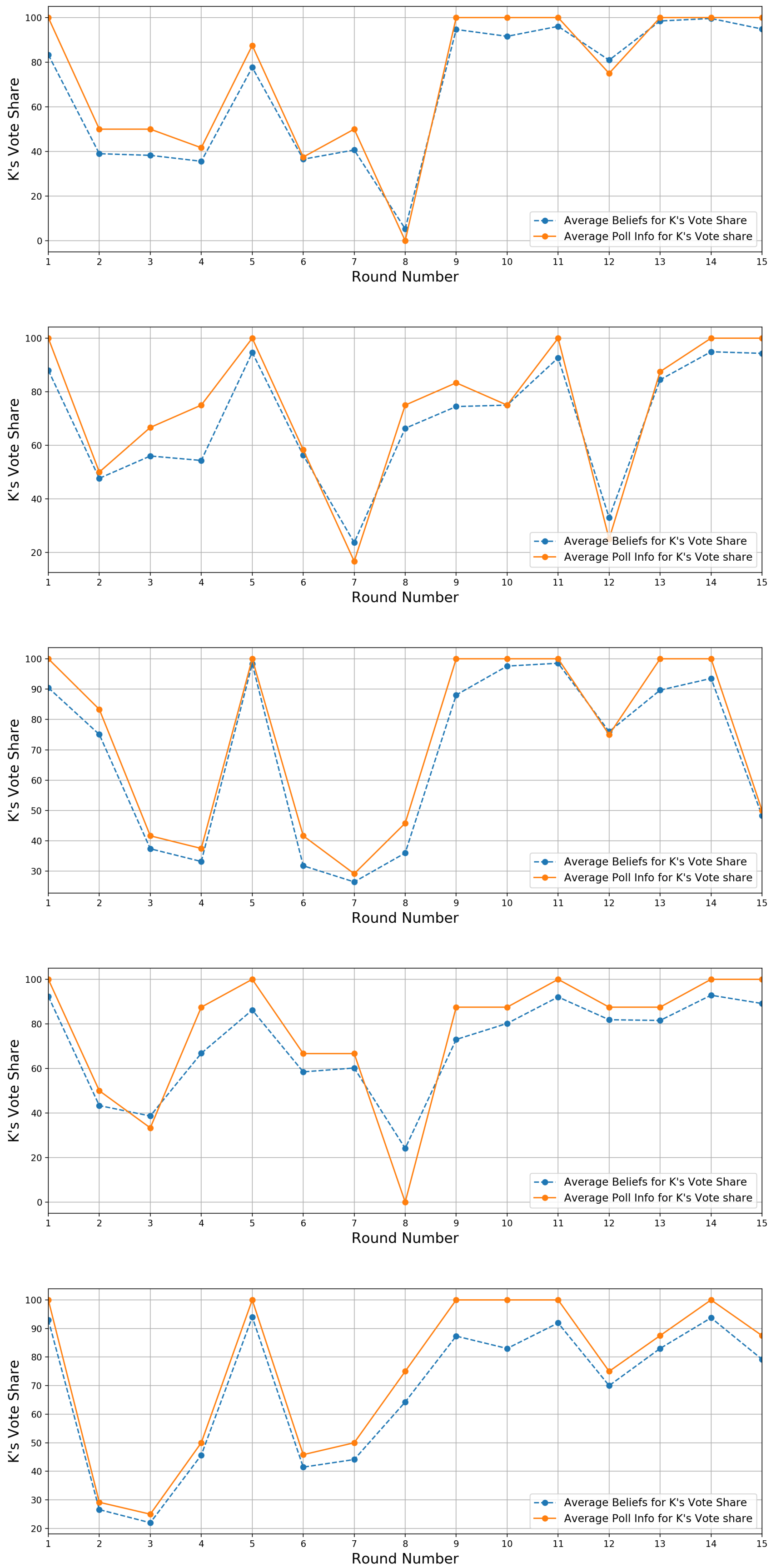
Notes. The graphs present the fraction of votes that candidate K receives according to the poll of each of the five companies in each period in treatment sessions of E3. The graphs for the five sessions T1-T5 are presented in sequence.

Figure C.8 Average beliefs vs. poll outcomes in control sessions of E3 (C1-C4)



Notes. The dashed line presents the elicited beliefs on candidate K’s vote share averaged over subjects. The solid line illustrates the vote share of candidate K according to polls, i.e. averaged across the five polling companies. The graphs for the four sessions C1-C4 are presented in sequence.

Figure C.9 Average beliefs vs. poll outcomes in treatment sessions of E3 (T1-T5)



Notes. The dashed line presents the elicited beliefs on candidate K's vote share averaged over subjects. The solid line illustrates the vote share of candidate K according to polls, i.e. averaged across the five polling companies. The graphs for the five sessions T1-T5 are presented in sequence.

D Additional Tables

Table D.1 Descriptive summary of voting behaviour at the poll stage, pooled at session level, E1

	session	E1_C1	E1_C2	E1_C3	E1_C4	E1_T1	E1_T2	E1_T3	E1_T4
uninformed	J	26.09%	29.21%	29.67%	38.30%	17.20%	30.85%	46.67%	21.74%
	K	42.39%	47.19%	37.36%	51.06%	67.74%	53.19%	43.33%	47.83%
	N	31.52%	23.60%	32.97%	10.64%	15.05%	15.96%	10.00%	30.43%
informed	J	44.05%	42.50%	44.44%	43.82%	34.94%	45.24%	38.37%	51.19%
	K	55.95%	50.00%	54.32%	55.06%	62.65%	54.76%	59.30%	47.62%
	N	0.00%	7.50%	1.23%	1.12%	2.41%	0.00%	2.33%	1.19%

Table D.2 Descriptive summary of voting behaviour at the poll stage, pooled at session level, E2

	session	E2_C1	E2_C2	E2_C3	E2_C4	E2_T1	E2_T2	E2_T3	E2_T4
uninformed	J	30.21%	29.21%	33.71%	22.92%	28.71%	26.32%	27.37%	29.21%
	K	43.75%	56.18%	39.33%	51.04%	48.51%	37.89%	47.37%	41.57%
	N	26.04%	14.61%	26.97%	26.04%	22.77%	35.79%	25.26%	29.21%
informed	J	38.75%	45.45%	44.19%	46.91%	45.56%	46.84%	37.50%	34.94%
	K	57.50%	53.41%	55.81%	50.62%	48.89%	50.63%	62.50%	61.45%
	N	3.75%	1.14%	0.00%	2.47%	5.56%	2.53%	0.00%	3.61%

Table D.3 Descriptive summary of voting behaviour at the poll stage, pooled at session level, E3

	session	E3_C1	E3_C2	E3_C3	E3_C4	E3_T1	E3_T2	E3_T3	E3_T4	E3_T5
uninformed	J	34.88%	21.98%	29.03%	23.76%	30.85%	20.83%	27.84%	23.96%	32.97%
	K	50.00%	43.96%	41.94%	39.60%	38.30%	43.75%	47.42%	54.17%	56.04%
	N	15.12%	34.07%	29.03%	36.63%	30.85%	35.42%	24.74%	21.88%	10.99%
informed	J	43.82%	37.18%	43.02%	48.10%	37.04%	52.33%	44.83%	42.35%	44.57%
	K	49.44%	60.26%	56.98%	50.63%	62.96%	47.67%	54.02%	56.47%	54.35%
	N	6.74%	2.56%	0.00%	1.27%	0.00%	0.00%	0.00%	1.18%	1.09%

Notes. The tables present the distribution of answers in the polls, for each individual session of our three experiments. 'N' Stands for non-participation. The choices are pooled across the fifteen rounds. They are further split into votes of the 'informed', those who know the valences drawn, and those who do not - the 'uninformed'.

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