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**Title:**

Off the fence onto the bandwagon? A large-scale survey experiment on effect of real-life poll outcomes on subsequent vote intentions

Abstract

Despite decades of scholarly inquiry, the debate on the existence of a bandwagon effect in politics remains undecided. This paper aims to overcome the limitations of previous experimental and survey research. We test to what extent success in real-life polling outcomes of the previous weeks influences subsequent vote intentions. To this end, we designed a large-scale survey experiment among a diverse cross-section of the Dutch electorate (n=23,421). We find that simple polling outcomes by themselves do not affect subsequent vote intentions. We do find evidence for a subtle but relevant bandwagon effect: An emphasis on growth in the opinion polls stimulates subsequent support. However, there is no evidence that the bandwagon effect is more apparent among people who were on the fence.

Key words

Opinion polls, survey experiment, bandwagon, momentum, party preference, vote intention, voters

## **Off the fence onto the bandwagon? A large-scale survey experiment on effect of real-life poll outcomes on subsequent vote intentions**

### **1. Introduction**

Is there such a thing as a bandwagon vote? Do “pre-election polls tend to handicap the losing side by influencing doubtful voters to vote for the winning candidate”? (Gallup & Rae 1940: 244). As early as 1940, George Gallup and Saul Forbes Rae argued that there are two main problems in the way the bandwagon effect had been tested comprehensively. First, experimental studies frequently study college students in non-realistic situations with non-existing candidates. Second, because the determinants of public opinion shifts are complex and interdependent, the causal mechanisms are hard to pull apart (Gallup & Rae, 1940: 248-249). A more convincing test of the bandwagon vote and its intricacies therefore requires an assessment of the effect of *actual* poll results on *real* voters. To date, the problems sketched by Gallup and Rae have not been solved. Even though the topic has received ample scholarly attention, the existence of a bandwagon vote remains hotly debated (cf. Irwin & Van Holsteyn 2000). Gallup, for one, was highly sceptical: “No amount of factual evidence seems to kill the bandwagon myth. (...) After thirty years, the volume of evidence against the bandwagon theory has reached staggering proportions, and yet many writers continue to allude to this theory as an accepted fact" (Gallup 1965: 546).

Yet, the current body of scholarship on the existence of a bandwagon vote is far more mixed than Gallup’s early assessment implies (Donsbach 2001; Hardmeier 2008). Survey-based research has led to paradoxical outcomes. Most people, when asked directly, are convinced that polls have the potential to induce bandwagon effects, but deny that they themselves are sensitive to such influences (Lang & Lang 1984; McAllister & Studler 1991; Sonck & Loosveldt 2010). Subjective perceptions of electoral success influence party choice (Meffert et al. 2011). Generally, though, surveys are not well equipped to assess whether actual success in the opinion polls causes a bandwagon vote to occur, as the mechanisms underlying the bandwagon effect can be emotional and subconscious in nature, rather than cognitive and conscious (cf. Marsh 1985; Mutz 1992, 1998; Meffert et al. 2011).

To address these concerns, many studies on the bandwagon vote have applied an experimental design (e.g. Hardmeier 2008). There have been various attempts at natural experiments, for instance by comparing early voters to late voters – the latter being more likely to have been exposed to exit-polls –, by comparing people who watched television broadcasts exposing the likely election outcome at mid-day to those who did not, or by comparing voters who were interviewed before successful polls were presented to those who were interviewed afterwards (e.g. Tuchmann & Coffin 1971; Mendelsohn 1966; Fuchs 1966; Blais et al. 2006). These studies generally found no evidence for bandwagon effects, but were unable to achieve genuine randomization of the bandwagon ‘treatment’. Experimental research in controlled environments with randomized groups was used to isolate bandwagon effects more abstractly, by using fake candidates and/or fake polls, generally among a small and selective population such as students (e.g., Morwitz & Pluzinski 1996; Fleitas 1971; Navazio 1977; Goidel & Shields 1994; Cotter & Stoval 1994; Daschmann 2000; Mehrabian 1998). However, these experiments do not tell us to what extent bandwagon effects take place in non-artificial settings as a consequence of actual poll results. Many of these experimental studies require quite a leap of faith from experiment to reality.

Moreover, the bandwagon effect need not be indiscriminate (cf. Traugott 1992). Success in the polls is most likely to attract voters who were already “on the fence” (Gallup & Rae 1940), i.e., voters who support candidates with similar characteristics rather than ideological opposites, and voters who previously considered voting for that candidate at least once. Moreover, politically sophisticated voters are less likely to change party preference because they do not easily yield to additional cues (Lazarsfeld et al. 1948; Meffert et al. 2011; Van der Meer et al. 2013).

For all these reasons, it is no surprise that previous studies reached conflicting results and that the existence and nature of the bandwagon vote remains hotly contested. Experimental tests have generally been strong on internal validity, but less so in generalizing their findings to real-life settings

and the general electorate. Survey-based studies, by contrast, have been strong in external validity, but unable to tap into the potentially unconscious nature of the bandwagon mechanism.

This study discusses the nature of the bandwagon vote, proposes a research strategy to overcome the main limitations of previous studies, and then tests to what extent there is evidence for the existence of a bandwagon vote. We employed a large scale panel survey experiment, where 23,421 respondents were randomly assigned to 10 different groups and confronted with the same outcomes of the most recently published nation-wide opinion poll – yet, crucially, framed in different ways – before we asked their current vote intention. Because our experiment does not use fabricated poll outcomes, the treatments are less likely to be experienced as outlandish, if only to those who already had a general idea of the relative party size in the polls. Survey experiments have proven their exceptional merit in studying a myriad of political science topics (see Gaines et al. 2007 for an overview) and could potentially bridge the experimental and survey traditions in the bandwagon literature. Moreover, we explore the conditionality of the bandwagon mechanism on the framing of party success in the polls and voter attributes such as volatility and political sophistication.

In sum, our design enables us to test to what extent the bandwagon mechanism occurs among actual voters when they are confronted with actual opinion poll outcomes.

Our survey experiment is situated in the Netherlands, early in the run-up towards the parliamentary elections of 12 September 2012. The Dutch case has some advantages that allow us to study various aspects of the bandwagon vote. First, the Netherlands has a proportional electoral system and a multiparty system in which no party comes close to reaching an absolute majority. This allows us to analyse one specific source of the bandwagon mechanism (momentum rather than static support, see below). In fact, our experiment contains vignettes with different reference points in time, so almost any party in this system can be framed as a winner or loser (Irwin & Van Holsteyn 2000). Second, the Dutch electorate has become increasingly volatile (Mair 2008), although this volatility remains largely bounded within one of two party blocks (Van der Meer et al. 2012). Some degree of volatility

is a necessary precondition for bandwagon effects to occur. However, any bandwagon effect in this system is likely to be subtle, as there as is rarely a single clear winner.

## **2. Theory**

The bandwagon mechanism has a solid foundation in various micro-economic subfields in the form of functional externalities due to increased preferences by other consumers (e.g. Becker 1991; Brown & Link 2008). In political science, it refers to a tendency among voters to vote for parties that are perceived to be successful ('winners') in pre-election polls of vote intentions. This is conceptually distinct from strategic voting, where voters aim to facilitate or prevent a coalition outcome by voting for parties of lower preference based on expected election outcomes (Blais et al. 2006; Hardmeier 2008; Irwin & Van Holsteyn 2002).

Political bandwagon effects have been difficult to define, identify and test. The definition of success in the opinion polls is a point of discussion by itself, especially in a multiparty context (Irwin & Van Holsteyn 2000). The label 'winner' might refer to having support from a majority of the electorate; to having the most support (i.e., from a plurality, that is, being larger than any of the other parties); or to having momentum (i.e., gaining support the fastest). The definition of momentum depends, in turn, on the reference point: A candidate may have lost support over several months, but (re-) gained it more recently over several weeks. Both size and momentum, however, need to be separated from general media effects, i.e. gains in vote shares due to articles and broadcasts that set parties or candidates in a positive light.

In our empirical test we focus exclusively on the bandwagon vote related to momentum. In the media cycle leading up to elections, the image of a winner generally relates to this momentum rather than to static size, as changes carry more news value. Nevertheless, the cues on which party or candidate is winning are not necessarily given by the poll outcomes themselves. Rather, they are contained in the message that is conveyed to the general public (Cotter & Stoval 1994; Daschmann 2000). Without

information as to which party is gaining momentum (cf. Fleitas 1971; Navazio 1977), polling results are thus unlikely to produce bandwagon effects.

The very same poll outcome can result in vastly different narratives. Two aspects seem to be especially relevant: (1) the point of reference, and (2) the frame. The point of reference determines the size and direction of the change in support, especially in the face of a volatile electorate. Yet, there is no single, self-evident reference point to interpret polling outcomes. Journalists have tremendous leeway in assigning winner-loser status to parties. One may consider the vote/seat share at the last elections, the last poll outcome, or the historic high point or low point (probably under the current leadership). Emphasis on growth might bring about bandwagon effects (Kleinnijenhuis et al. 2007), emphasis on decline a so-called ‘Titanic effect’ (a flight away from the losing party).

Similarly, the same poll outcome can be framed positively and negatively, regardless of the point of reference. Imagine, for instance, a party of which the support in the polls remained stable after an earlier period of growth. This outcome can be framed positively (‘retained growth’) or negatively (‘stagnated’). Such choices affect the degree to which a party is framed as a winner.

Besides the dry polling results, the point of reference (growth vs. decline) and the frame (positive vs. negative) therefore function as input to the treatments we deliver in our survey experiment.

#### *Four mechanisms to explain the bandwagon effect*

The nature of the supposed bandwagon is unclear, but we can distinguish various potential mechanisms to explain why people would tend to vote for candidates that do well in the polls (e.g. Bartels 1988, 1996; Mutz 1992; Hardmeier 2008). First, some use polls as “a crude indicator of other voters’ judgments about the candidates’ strengths and weaknesses” (Ansolabehere & Iyengar 1994) to reassess reasons to vote for winning candidates (Mutz 1992). Second, voters might use polls of the preferences of the collective as a useful heuristic cue, especially if other political knowledge is lacking (Mutz 1998; Meffert et al. 2011). Third, voters may be inherently attracted to being on the

side of a winning candidate (Abramowitz 1987, 50; Bartels 1988), for instance to raise their self-esteem (Mutz 1992). Finally, the bandwagon vote may be caused by an even more basic, affective and unaware herd instinct (Hardmeier 2008).

These mechanisms differ in the extent that voters make cognitive use of information and the extent to which they appeal to emotional responses (Irwin & Van Holsteyn 2000). Voters may, but need not, be aware of their motivation to vote for the winning candidate. Consequently, self-reported motivations for electoral behaviour and within-person comparisons of different treatments tend to underestimate bandwagon effects, as they emphasize an awareness of cognitive processes.

#### *No indiscriminate effects*

The more cognitive mechanisms imply that some groups of voters are more likely to give in to the bandwagon mechanism than others (Traugott 1992; Mehrabian 1998). A first evident group that is likely to be attracted to success in the polls is made up by voters who were “on the fence” anyway (Gallup & Rae 1940), i.e., voters who already contemplated voting for the successful party (Bartels 1987). Similarly, voters who more easily change party preferences between elections (volatile voters), should be more likely to jump on the bandwagon. Finally, there is some evidence that bandwagon effects are more likely to occur on undecided voters (Ceci & Kain 1982; Mutz 1992).<sup>1</sup> Hence, we would expect the bandwagon vote to occur particularly among voters who had contemplated voting for that party before, and among more volatile voters.

A second group of voters who are likely to give in to bandwagon effects are those with low levels of political sophistication. Politically sophisticated voters are more resistant to the influence of opinion polls (Popkin 1991; Meffert et al. 2011). Their political opinions are less susceptible to new cues (Lazarsfeld et al. 1948), given equal levels of exposure to such cues (Albright 2009). As political sophistication and education are positively related, we would expect that higher educated citizens are less likely to experience bandwagon effects.<sup>2</sup>

### *Hypotheses*

Summing up, based on the foregoing, we formulate the following hypotheses

- H1. Reading dry poll outcomes (without reference point or frame) do not affect subsequent vote intentions compared to reading no poll outcomes.
- H2. Poll outcomes that emphasize growth of a single party in the polls compared to an earlier point of reference stimulate voters to subsequently intend to vote for that party.
- H3. Poll outcomes that contain the frame that a single party is doing well in the polls stimulate voters to subsequently intend to vote for that party.
- H4. The bandwagon vote is more pronounced (a) among voters that had contemplated voting for that party before, (b) among volatile voters, and (c) among voters that are less sophisticated.

### **3. Survey experiment**

This study combines the strengths of the two main approaches in this particular field of bandwagon research - survey versus experiments - in a large-scale panel survey experiment. On the one hand, by randomly assigning respondents to treatment conditions, we circumvent endogeneity concerns that have plagued conventional survey-based studies. Furthermore, by specifically linking treatments to reference points and frame, we can disentangle some of the mechanisms that underlie the bandwagon vote. On the other hand, our design addresses the external validity issues that hampered previous controlled experiments. The use of the most recent, non-fabricated poll outcomes as treatment allows us to study the bandwagon vote as it occurs naturally rather than the more radical responses that fictional settings might induce.

### *IVOP*

We embedded our survey experiment in the EenVandaag Opinion Panel (1VOP), organized by the widely viewed Dutch public daily news programme EenVandaag. Joined via self-application, all respondents are invited by e-mail to participate in each poll. The 1VOP data cover nearly 100,000 unique respondents<sup>3</sup> that participated in one or more of the 103 waves on vote intentions between September 2006 and September 2012. Although the distribution of voters with regard to vote intention is biased<sup>4</sup>, there is hardly any bias in the extent and direction in which vote intentions change.<sup>5</sup> As the 1VOP has never included a survey experiment before, respondents cannot realistically be expecting to receive a treatment.<sup>6</sup>

The wave that included our survey experiment was held in the days after May 16<sup>th</sup> 2012. This is nearly a month after new parliamentary elections were announced but almost four months before they actually took place on 12 September. The timing of the experiment was relevant: vote intentions began to matter, although the campaign had hardly started and strategic considerations were unlikely to take place so early: information, predispositions and strategic considerations would still be relatively low (cf. Mutz 1992). We purposely set out the experiment in a relatively quiet week when the polls of the previous weekend hardly showed changes. Unfortunately, this aim was slightly upset as some content of the budgetary ‘Spring Agreement’ of the government (VVD, CDA) with three opposition parties (D66, CU, GL) leaked during that week. Nevertheless, because of successful randomization this should not impair the internal validity of our analysis.

This wave of the 1VOP covers a total of 23,421 respondents (approximately 60% of all panel members at that moment). Within the survey experiment, drop off was minimal (<0.5%); 98% had participated in at least one earlier wave; on average they had participated 27.9 times (median: 26).

### *Experimental design*

Our design is experimental in the sense that respondents were randomly (and automatically) assigned to one of ten treatment conditions. Each of the ten groups got a slightly different treatment (and

attention diverting question). Afterwards, all groups were presented with the same final question on current vote intentions. We confirmed that there were no significant or substantive differences between the ten groups on key characteristics: respondents' vote in 2010, their vote intention in April 2012 (only available to a subset of the respondents), and their level of education, gender, and religion (see Appendix A).

The relevant number of respondents per treatment does not only depend on the total sample size and the number of treatments, but should also be balanced to the expected effect size which is rather small (Blais et al. 2006; Hardmeier 2008). As we test the effects of actual opinion poll results in a single (non-repeated) treatment on a medium sized party, we expect the bandwagon effect to be in the range of 2 to 3 parliamentary seats (i.e. 1.3% to 2% of the vote intentions) at most. A reliable estimation of such an effect requires quite a bit of statistical power. We therefore aimed for circa 2,500 respondents per treatment, although we fell short of that by several hundreds.

The choice which party to focus on in this survey experiment is not straightforward in a multi-party system such as the Netherlands. The Dutch party system is characterized by two electoral blocks of parties within which most electoral volatility takes place (Van der Meer et al. 2012). Consequently, the parliamentary balance of power between the left and the right is rather constant. This limits the scope of potential bandwagon effects: few right-wing voters will be inclined to vote for a left-wing party that is successful in the polls, and vice versa.

Four criteria guided the selection of the political party that would be central to our vignettes. First, the party should not be a fringe party but have a large or moderately large appeal to voters. We were left with six options: VVD, PVV, CDA, PvdA, SP, and D66. Second, the party should be moderately successful compared to its potential vote share: A party that already reached the maximum of its electoral potential is unlikely to receive a bandwagon vote, while a party that bottomed out is too likely to grow with any bit of good news. Third, we required a party for which it was *factual* and *likely* to interpret the poll outcome both positively and negatively, and for which we could sensibly

describe the outcome with reference to a previously worse outcome (i.e., growth) and a previously better outcome (i.e., decline). Finally, the party should appeal to a broad range of voters of various similar parties (i.e., a moderate rather than radical party).

The Dutch Labour party (PvdA) provided the best match. Its voting potential was hardly reached (unlike the main rightwing parties PVV and VVD), but it had not hit rock bottom either (unlike the Christian democratic CDA). It had experienced a shift in party leadership three months earlier, and had grown since then (+6 seats in the polls). However, compared to the 2010 election results it still performed worse (-10 seats in the polls compared to parliament). The PvdA therefore seems suitable to disentangle some of the mechanisms behind the bandwagon vote without having to sacrifice our goal of employing realistic vignettes.

We distinguish between ten treatment groups (see Table 1). Seven of these make up our main analysis. The control group received no poll outcome, no interpretation, and no question; one group only received the question that we formulated to divert attention from the treatment<sup>7</sup>; one group received a poll outcome and the diversion question; four groups also received an interpretation of the poll outcome for the Labour party PvdA (based on different reference points and frames).

We included three more treatment groups as robustness checks. They focus on a different party for which bandwagon effects were less likely due to ceiling effects (governing party VVD) and a different polling agency (TNS Nipo). We discuss these results after our main analyses.

All respondents were asked for their vote intention directly after the diversion question.

### *Vignettes*

To assess the effect of opinion poll outcomes and the way these outcomes are discussed, we expose five of the seven groups to the actual opinion poll results of the previous week. We rely on the poll outcomes of peil.nl, the polling agency that is most prolifically covered in Dutch media. These outcomes are published every Sunday morning, i.e., more than three days before the start of our

experiment. Consequently, some of the potential bandwagon effects may already be accounted for in the preferences of all respondents. We did not mention the name of the polling agency or the pollster himself in our survey experiment in order to keep the setting as neutral as possible. Parties were ordered by government status and size, so that the score of the PvdA was mentioned as fourth in a list of eleven parties.

Of the five groups that were exposed to the opinion poll results of peil.nl, one group got to read a basic introduction line: ‘Last weekend opinion pollers have polled on which parties voters would vote.’ The other four groups got an additional brief passage, emphasizing the role of the PvdA (‘The most notable is the development of the PvdA’). The first vignette was designed to signal growth compared to an earlier point of reference: it emphasized in neutral terms that the PvdA had grown (+6 seats compared to 3 months before). The second vignette signals a decline compared to an earlier reference point (-10 seats compared to current distribution in parliament).<sup>8</sup> These allow us to test hypothesis 2. The third vignette had an extensive, positive frame, i.e., a bias in favour of the PvdA (keeps on doing well in the polls). The fourth vignette had an extensive, negative frame, i.e., a bias against the PvdA (stagnated). These allow us to test hypothesis 3. We ended each of the six treatment groups (not the control group) with a question to divert attention from the specific nature our treatment (the precise poll outcome/interpretation) and reduce potential priming effects: ‘What do you think of such opinion polls?’ Finally, all respondents received the same question on their current vote intention: ‘Which party would you vote for if parliamentary elections were held today?’ This exact question is commonly asked at the end of surveys in the 1VOP. This vote intention question forms our dependent variable.

Table 1 presents a systematic overview of the similarities and distinctions between these comparisons including the three treatment groups we included for robustness checks, as well as the literal translations of the survey questions. Theoretically, we are interested in two sets of comparisons: (1) an assessment whether the mere act of viewing opinion poll outcomes in vignette 3 affects vote

intentions (compared to not viewing these outcomes in vignettes 1 and 2); and (2) an assessment whether the reference point and framing of opinion poll outcomes in vignettes 4-7 affect vote intentions (compared to the neutral outcomes in vignette 3).

[Table 1 somewhat here]

### *Conservative test*

Our survey experiment is a conservative test for the existence of bandwagon effects. First, the use of actual poll results itself will have a dampening effect, as changes in these polls were rather modest in the period we study. To some extent, the poll results will already be accounted for in the attitudes and vote intentions of all our respondents, as they had been published in the media three days before the start of our experiment. Second, the treatments we offered were rather subtle. They were embedded at the tail end of a lengthy questionnaire. Consequently, the respondents may have been less likely to read the vignettes intensively, the more so because the survey was not conducted using an interviewer. Third, we offered a single trigger. Vignette 3 and vignettes 4 and 5 only differ with regard to a single sentence. These triggers are not repeated across various media outlets, as conventionally happens in the Dutch case, or by the same outlet at different stages of the news cycle). Hence, there is no cumulative effect that may occur as time goes by, making our experiment a one-shot game. Fourth, there may be panelization effects: respondents in panel surveys tend to become more stable in the vote intention they report (Warren & Halpern-Manners 2012).

Nevertheless, the position of the treatment in the questionnaire might undermine the conservatism of our test: the vote intention question is posed directly after the treatment and the diversionary question. Finally, the timing of the survey experiment - after the announcement of elections but before the intensified campaign - can both stimulate and hinder the bandwagon vote. Respondents might be more susceptible to the bandwagon mechanism, as their political predispositions may not have been

too structured yet. However, winning and losing in the polls might have mattered less to respondents so far ahead of the actual elections, thus hampering the bandwagon mechanism. Ultimately, this is an empirical question we cannot answer in this study.

### *Setup*

We present our analyses as follows. After showing the descriptive results we estimate logistic regression models to test to what extent the intention to vote for the PvdA differs across each treatment compared to the relevant control group. In line with our directional hypotheses, we performed one-tailed tests. In these multivariate analyses we eliminated those respondents who refused to answer and those who were not allowed to vote. Unfortunately, 1VOP does not allow us to distinguish between those who refuse to answer (and are coded as ‘missing’) and those who simply do not know at the moment, as both groups fall in the same category in the survey. We therefore had to exclude this category.<sup>9</sup> Respondents with other non-substantial preferences (non-voters, blanc voters) remained in the analyses: their in- or exclusion did not affect the outcomes.

We checked to what extent these differences may be explained by six control variables: gender, level of education (an eight category variable ranging from elementary school to an academic grade), participation rate in the 1VOP before May 2012, electoral volatility in the 90 1VOP waves before May 2012 (dichotomized between constant vote intentions and shifting vote intentions), and intention to vote for the PvdA in these 90 waves (both as a dichotomous measure and relative to participation rate). Missing values on earlier participation, gender, level of education, and electoral volatility were dealt with through pairwise deletion. No other theoretically relevant political measures – such as ideological predisposition or issue preferences – were available in 1VOP. Nevertheless, randomization ensures that this omission does not bias the causal estimates.

Finally, we expand on these models for the conditional hypotheses by interacting the treatments with (1) previous PvdA-preference, (2) electoral volatility, and (3) level of education<sup>10</sup>.

## 5. Results

### *Descriptive analysis*

Figure 1 shows the share of vote intentions in favour of the PvdA for each of the seven main groups. The dark lines represent the point estimates, the grey areas the 95% confidence interval. Support ranges from 16.5% in the control groups to 18.5% in the group that was triggered to consider that the PvdA had grown in the polls. Being exposed to dry poll results did not affect intention to vote PvdA compared to having no information about the polls at all (difference in point estimate: 0.1%). This holds up in multivariate analyses.<sup>11</sup>

The vignette that emphasized growth (18.5%) had a significant, positive effect on subsequent vote intention for PvdA. However, an emphasis on declining support did not have an inverse effect on party choice: PvdA-support in that group is 16.8%, statistically indistinguishable from the control group. The simple fact that the PvdA got attention in the trigger might have boosted the intention to vote PvdA, regardless of whether or not it was positive or negative. Yet, it cannot explain why only the growth vignette had a significant effect. The other two treatments groups – who received vignettes that framed the poll outcomes negatively (stalled growth) and positively (consolidated growth) without emphasizing momentum – score somewhat in between (18.2%), though not significantly different from the control groups.

This implies, as we had expected, that the effects are small. The existence of the bandwagon vote only receives empirical support with the growth-vignette that was designed to capture (positive) momentum. Hence, we should not overemphasize bandwagon effects caused by a single poll outcome in a multiparty system: regular polls in the Netherlands hardly ever show party shifts of more than 1 or 2 seats on a weekly basis. Yet, even such small differences could be consequential in societal terms: a 2 percentage point difference in support equals 3 seats in the 150 seats Dutch Lower House and could easily be the difference between the largest party and the runner-up.

[Figure 1 somewhat here]

[Table 2 somewhat here]

*Multivariate tests: direct bandwagon effects*

In Table 2 we perform logistic regression analyses to explain the intention to vote for the PvdA. After including additional controls (model 1), the positive effect of a growth trigger is even stronger. The vignette with the positive frame has a positive effect on the intention to vote PvdA as well. These findings support hypothesis 2 (point of reference) and hypothesis 3 (framing).

The six control variables primarily explain differences *within* the treatment groups in support for the PvdA. It is self-evident that the intention to vote PvdA is higher among respondents who had intended to vote PvdA at least once before, especially when they intended to do so relatively often. Support is lower among voters who changed their vote intention at least once between 2006 and 2012. There are no effects of participation rate, gender and education; probably any effect of the latter variables has already been picked up by a preference for the PvdA at earlier time points.

*Conditional effects: off the fence?*

Finally, we test to what extent these treatment effects are conditional on the characteristics of the respondents. Theoretical perspectives on the bandwagon vote imply that voters who had previously intended to vote PvdA would be more likely to jump on board. However, none of the interaction effects in models 2a and 2b are significant, despite plentiful variation on all of the characteristics. We thus fail to find support for hypothesis 4a. The lack of effects is not due to ceiling effects (cf. Joslyn 1997): even when we estimate models in which we separate respondents who always vote(d) PvdA, none of the interaction effects come out significant.

Model 2c tests whether volatile voters – who change vote intention more often – are more likely to be affected by the bandwagon effect. Again, we find no evidence: all the interaction effects are non-

significant. Hypothesis 4b is not supported. In additional analyses, we checked whether more evident results originate from interactions with respondents who had considered one of the prime electoral rivals of the PvdA, i.e. D66, GreenLeft, and the Socialist Party. This was not the case.

Finally, we tested whether the lower educated are more susceptible to bandwagon effects, once they are exposed to the relevant triggers. Model 2d shows that none of the interaction effects of the treatments with level of education are significant. This fails to support hypothesis 4c. Higher educated voters do not resist the bandwagon trigger more strongly than the lower educated. Assuming that higher educated are more exposed to such information, in real life bandwagon mechanisms may therefore even be more common among them. Alternatively, lower educated respondents might have gone over the paragraphs leading up to the survey question more quickly than higher educated, and therefore been less exposed to the treatment in our experiment.

All in all, though, we find no indication that bandwagon effects are more common among voters that are on the fence, i.e., voters who were already attracted to this party beforehand, voters who were already more volatile beforehand, and voters who are less likely to resist new information. Of course we should allow for the possibility that the statistical power of our test is insufficient to demonstrate the conditionality of the bandwagon effect.

#### *Robustness I: alternate measures for the PvdA*

We performed several robustness checks. To improve statistical power, we reran our models after combining the two treatments that were favourable to PvdA (i.e., growth and positive frame) and the two that were not (i.e., decline and negative frame). This did not affect any of the conditional effects substantively. In a second check, we included people's voting behaviour in 2010 as a control variable to model 2b. This did not affect any of the other effects substantively.<sup>12</sup> Finally, with regards to the measurement of electoral volatility and the share of past vote intentions for the PvdA, the use of a

measure that is continuous or consists of three (instead of two) categories did not affect our conclusions.

### *Robustness II: VVD*

Finally, our survey experiment included three treatments based on recent - but already 2 weeks old - outcomes of the second most prolific Dutch polling agency (see Table 1). Two of these treatments are accompanied by a header and text that focuses attention on the VVD (either positively or negatively). We should emphasize that the VVD was in a vastly different electoral position than the PvdA. It was the largest governing party, and the party of the Prime Minister. Despite the breakdown of the government coalition, the VVD remained the largest party in the polls, at a historical high (that the party had not obtained since 2002) at the level of the successful 2010 elections. Hence, the positive vignette mentions consolidated growth, the negative vignette stalled growth. Given the historical high of the VVD and consequent ceiling effects, a bandwagon effect based on momentum is far less likely than it was for the PvdA at the same time.

Table 3 displays the results of the analyses for the VVD. Model 1 finds no support for an unconditional bandwagon effect. The effects of the control variables are similar to those found for the intention to vote PvdA, with the addition that more common participants in the 1VOP are more likely to vote VVD. Yet, we do find evidence for conditional bandwagon effects. Model 2a signals ceiling effects. Reading a vignette that emphasizes the success of the VVD does stimulate the intention to vote VVD, but only among two extreme groups: voters who already tended to vote VVD very often and among voters who never tended to vote VVD before. To ease interpretation we re-estimated model 2a by focusing on four nominal categories in model 2b: respondents who never intended to vote VVD, who did so rarely (0.1-25%), who did so regularly (25-90%), and who did so often (90%-100%). The conclusions are similar: there is a bandwagon effect of reading about success of the VVD, but not among those who are in doubt (on the fence).

Apparently, the best a party at a historical high can hope for is retaining voters. “Rather than doing better and better (or worse and worse) in an unbroken cycle, candidates may reach plateaus of support” (Bartels 1985: 814). Once the VVD reached this plateau, the bandwagon effect was unlikely to attract even more undecided voters, but was able to retain already decided voters.

## **6. Conclusion**

Citizens, politicians and journalists tend to consider opinion polls on vote intentions as relevant sources of information (Wichmann & Brettschneider 2009; Irwin & Van Holsteyn 2008). They are used by strategists as tools to frame campaign reports or to decide on the direction of election campaigns, and by voters to assess the usefulness of a strategic vote. Opinion polls may also affect elections through cognitive and/or affective responses by voters caused by the (changing) success of parties in these polls. The bandwagon vote constitutes perhaps the best known of these responses. Although the bandwagon mechanism has been hotly debated for more than half a century, its relevance and even its existence remains unresolved.

In this study we proposed a methodological setup to alleviate some of the drawbacks of the two standard approaches commonly employed in bandwagon studies. Experimental research tended to find evidence for the existence of a bandwagon vote, but with low external validity due to the use of fabricated poll outcomes on a small and selective subsample of the population. In survey research, by contrast, evidence in favor of the bandwagon vote was less common; yet, these studies tended to trigger cognitive responses and evaluations. Hence, it remained unclear to what extent real poll results on real parties affect a broad segment of the electorate. We therefore set up a survey experiment in a panel dataset among 23,421 respondents that were randomized over 10 treatments that in turn were based on actual poll results of the previous week.

Our results are consistent with the existence of bandwagon vote. The dry results by themselves do not affect subsequent vote intentions. However, when accompanied by a qualifying text in which the

party is said to be winning – that is, growing compared to an earlier point of reference – that party will subsequently obtain more votes than if the party had not been framed as a winner. This effect is consistently significant under various tests. Being explicitly framed as a winner in rather positively biased terms *without* a point of reference to suggest growth may also stimulate subsequent support, but that effect is less reliable. We find no evidence that framing of parties as electoral losers or as parties in decline led voters to flee that party. Ultimately, it is not the poll outcome itself that matters but the emphasis on who is winning. Bandwagon effects are less likely when a party reaches a historical high in electoral support. Hence, we found unconditional bandwagon effects for the then less successful PvdA and conditional bandwagon effects for the then historically successful VVD. All in all, these bandwagon effects are subtle (as they require specific circumstances and lot of statistical power), but societally substantive (cf Hardmeier 2008): our one-shot treatment leads to 2 to 3 additional seats in the 150 seat Dutch parliament.

Still, support for the bandwagon mechanism is certainly not overwhelming. All bandwagon effects are borderline significant at the .05-level. One reason is a lack of statistical power, despite the large number of respondents per treatment. Given the small effect size that could be expected from a single treatment based on actual polling outcomes, and due to our focus on one middle-sized party that generally does not attract right-wing voters, the effective number of respondents got rather small. Power analyses suggested that our study required more power than the 23,421 respondents we had. This is obviously important for future research that follows our empirical strategy of employing realistic stimuli to actual voters. Moreover, these conclusions hold under statistical controls, under various model estimations and despite the conservative setup of our test.

Yet, we did not find the conditional effects that we had expected. Theoretically, voters who are on the fence and voters who are less knowledgeable should be more likely to be affected by the bandwagon mechanism. Yet, none of the cross-level interaction effects in our main analyses were significant. We found significant cross-level interaction effects in our robustness check (on the

VVD), but they point to ceiling effects instead. The lack of theoretically interpretable conditional effects seriously challenges the validity of the bandwagon mechanism. At the very least, the lack of differentiation does not make sense if bandwagon effects would be due to the cognitive mechanism (Ansolabehere & Iyengar 1994) or the cue taking mechanism (Mutz 1998), leaving us with the gratification mechanism and/or contagion mechanisms: an indiscriminate herd instinct.

Although we failed to reject the null hypothesis, we cannot conclude that there are no conditional effects either. Ideally, one would assess the effects of actual polls by following respondents over a longer period of time. That would risk diffusion, though. Alternatively, we could aim for stronger treatments in more controlled settings. Yet, that would come at a high cost: a sacrifice of the ecological validity that our study attempted to bolster.

## **7. Discussion**

Ultimately, the question is to what extent the effects we found in this survey experiment point to actual bandwagon effects of real-life polling outcomes. We are confident that they do.

That does not mean that there are no threats to our design. The first concerns the inflation of the Type I error rate as a result from the seven treatment groups we discern. However, this cannot explain our results. Theoretically, we deduced three main hypotheses (expecting two significant effects, and one insignificant one), and found evidence in favour of precisely those three hypotheses. Even when we combined different treatment groups, we obtain the same results.

Second, the press leak of the content of the Spring Agreement (between coalition parties and three minor opposition parties) in the week of the experiment was an unforeseen interference. Yet, because respondents were randomized over treatments, any effect of the event would have affected all treatment groups and at best obscured any treatment effect due to increased support for the PvdA.

Finally, our findings cannot be caused by priming effects, i.e., the mere attention specific parties get in the vignette. Although priming might explain why we find no negative effects of decline or failure,

it cannot explain why we only find significant, positive effects of growth and success. If attention for a party in the vignette is the main driver of our findings, the vignettes with the most extensive frames (positive *and* negative) should have created the strongest positive effects. That is neither the case in our study of the PvdA, nor in our study of the VVD.

The subtle but significant bandwagon effects of actual polling outcomes are relevant. The emphasis of growth in reports on the polls matters. Even a one-shot treatment led to a 2pp increase in vote share. The subjective choice for a point of reference by journalists has consequences. Less accurate media reports on polling outcomes in which margins of error are ignored are cause for concern (Lavrakas & Traugott 2000; Sonck & Loosveld 2010; but see Kirchgässner & Wolters 1987): non-significant growth reported in the media might lead to a self-fulfilling prophecy. In the short run there is a risk of cumulative effects, especially when signals in different media are not mixed.

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**Table 1. Overview of the vignettes**

Experimental group	Party	Basic	Reference / Framing	Origin of poll result	Question	n
1 (control)	-	-	-	-	-	2391
2	-	V1 <sup>a</sup>	-	-	Yes <sup>k</sup>	2306
3	-	V2 <sup>b</sup>	-	Peil.nl <sup>i</sup>	Yes <sup>k</sup>	2306
4	PvdA	V2 <sup>b</sup>	Ref: growth <sup>c</sup>	Peil.nl <sup>i</sup>	Yes <sup>k</sup>	2353
5	PvdA	V2 <sup>b</sup>	Ref: decline <sup>d</sup>	Peil.nl <sup>i</sup>	Yes <sup>k</sup>	2379
6	PvdA	V2 <sup>b</sup>	Frame: neg <sup>e</sup>	Peil.nl <sup>i</sup>	Yes <sup>k</sup>	2275
7	PvdA	V2 <sup>b</sup>	Frame: pos <sup>f</sup>	Peil.nl <sup>i</sup>	Yes <sup>k</sup>	2296
8	-	V1 <sup>a</sup>	-	TNS NIPO <sup>j</sup>	Yes <sup>k</sup>	2400
9	VVD	V2 <sup>b</sup>	Ref/Frame: pos <sup>g</sup>	TNS NIPO <sup>j</sup>	Yes <sup>k</sup>	2317
10	VVD	V2 <sup>b</sup>	Ref/Frame: neg <sup>h</sup>	TNS NIPO <sup>j</sup>	Yes <sup>k</sup>	2398

a 'In the media there are repeatedly reports on the popularity of political parties according to opinion pollers.'

b 'Last weekend opinion pollers have polled on which parties voters would vote. (...) The complete result is as follows:'

c 'The most notable is the development of the PvdA. Two months ago the party was still at 14 seats in the polls, today that would be 20.'

d 'The most notable is the development of the PvdA. The PvdA would now obtain 20 seats. That is fewer than the 30 seats the party currently holds in the Lower House.'

e Header: 'Effect leadership change PvdA appears played out'. Text: 'The most notable is the development of the PvdA. After Job Cohen was replaced by Diederik Samsom, the party had grown, but that growth has now stagnated. Just like last week, the party receives 20 seats. Those are still substantially fewer seats than the party currently holds in the Lower House'

f Header: 'PvdA retains growth'. Text: 'The most notable is the development of the PvdA. The PvdA keeps on doing well in the polls. Under the leadership of Diederik Samsom the party is much more popular than when Job Cohen led the party. Last week the PvdA was back at 20 seats. The party has been able to retain that considerable growth this week.'

g Header: 'VVD remains most popular'. Text: 'The most notable is the development of the VVD. The VVD remains the largest party in the opinion polls with 33 seats. Moreover, also in the latest poll the VVD has not lost any seat compared to the elections of 2010.'

h Header: 'VVD and Rutte losing their shine'. Text: 'The most notable is the development of the VVD. The PM bonus of the VVD keeps eroding. The VVD has gone down so far in the polls, that it ends up at 33 seats. That is only 2 more than at the elections of 2010.'

i Published May 13th. VVD 28 seats; PVV 20 seats; CDA 15 seats; PvdA 20 seats; SP 29 seats; D66 17 seats; GreenLeft 7 seats; ChristianUnion 6 seats; SGP 3 seats; Animal Party 3 seats; Other 2 seats.

j Published May 1st. VVD 33 seats; PVV 19 seats; CDA 16 seats; PvdA 22 seats; SP 28 seats; D66 15 seats; GreenLeft 5 seats; ChristianUnion 6 seats; SGP 2 seats; Animal Party 3 seats; 50Plus 1 seat.

k 'What do you think of such opinion polls?' (very interesting, rather interesting, not so interesting, not at all interesting)

**Table 2. A test of bandwagon effects**

	<i>Model 1</i>	<i>Model 2A</i>	<i>Model 2B</i>	<i>Model 2C</i>	<i>Model 2D</i>
<i>Treatment (ref: dry poll outcome)</i>					
* Reference point: growth	0.23 (0.13) *	0.22 (0.20)	0.55 (0.30) *	0.54 (0.29) *	0.03 (0.46)
* Reference point: decline	0.06 (0.13)	0.24 (0.20)	0.12 (0.32)	0.28 (0.29)	0.36 (0.46)
* Frame: negative	0.17 (0.14)	0.30 (0.20)	0.37 (0.31)	0.27 (0.30)	0.56 (0.46)
* Frame: positive	0.22 (0.14)	0.27 (0.20)	0.30 (0.32)	0.36 (0.30)	0.74 (0.45)
<i>Control variables</i>					
Vote intention 2006-2012: PvdA (share)	4.54 (0.16) ***	4.76 (0.32) ***	4.70 (0.34) ***	4.54 (0.16) ***	4.56 (0.16) ***
Vote intention 2006-2012: PvdA (dich.)	2.92 (0.16) ***	2.92 (0.16) ***	3.03 (0.31) ***	2.92 (0.16) ***	2.92 (0.16) ***
Electoral volatility (dich.)	-0.78 (0.13) ***	-0.78 (0.13) ***	-0.77 (0.13) ***	-0.58 (0.25) *	-0.78 (0.14) ***
Participation rate	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Level of education	-0.05 (0.03)	-0.05 (0.03)	-0.05 (0.03)	-0.05 (0.03)	0.00 (0.08)
Gender	0.14 (0.09)	0.14 (0.09)	0.14 (0.09)	0.14 (0.09)	0.13 (0.09)
<i>Interaction effects</i>					
<i>Ref: Treatment (dry poll outcome)</i>					
* Reference point: growth * PvdA (share)		0.08 (0.46)	0.46 (0.52)		
* Reference point: decline * PvdA (share)		-0.55 (0.43)	-0.66 (0.47)		
* Frame: negative * PvdA (share)		-0.39 (0.43)	-0.35 (0.46)		
* Frame: positive * PvdA (share)		-0.14 (0.45)	-0.11 (0.48)		
<i>Ref: Treatment (dry poll outcome)</i>					
* Reference point: growth * PvdA (dich.)			-0.57 (0.39)		
* Reference point: decline * PvdA (dich.)			0.20 (0.39)		
* Frame: negative * PvdA (dich.)			-0.10 (0.39)		
* Frame: positive * PvdA (dich.)			-0.06 (0.39)		
<i>Ref: Treatment (dry poll outcome)</i>					
* Reference point: growth * volatility				-0.40 (0.32)	
* Reference point: decline * volatility				-0.28 (0.33)	
* Frame: negative * volatility				-0.12 (0.34)	
* Frame: positive * volatility				-0.18 (0.34)	
<i>Ref: Treatment (dry poll outcome)</i>					
* Reference point: growth * education					0.04 (0.10)
* Reference point: decline * education					-0.07 (0.10)
* Frame: negative * education					-0.09 (0.10)
* Frame: positive * education					-0.12 (0.10)
N	10643	10643	10643	10643	10643

Source: 1VOP 2006|1 to 2012|11

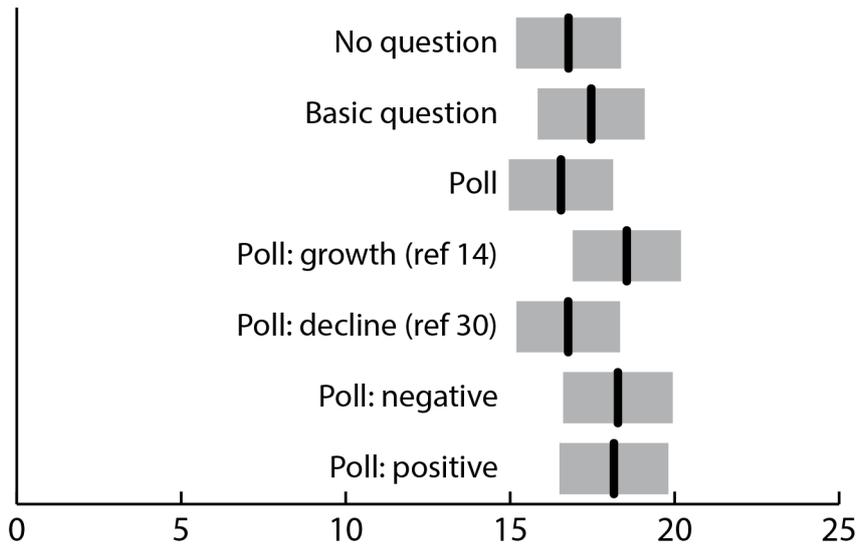
Logistic regression analyses. Standard errors between brackets; one-sided tests; \* p&lt;0.05

**Table 3. A test of bandwagon effects on a least likely case**

	<i>Model 1</i>	<i>Model 2A</i>	<i>Model 2B</i>	<i>Model 2C</i>	<i>Model 2D</i>
<i>Treatment (ref: dry poll outcome of TNS Nipo)</i>					
* Reference point: success	0.19 (0.15)	1.20 (0.58) *	0.95 (0.52) *	0.29 (0.36)	-0.49 (0.55)
* Reference point: failure	0.16 (0.15)	0.07 (0.71)	0.53 (0.45)	-0.18 (0.33)	0.52 (0.53)
<i>Control variables</i>					
Vote intention 2006-2012: VVD (share)	5.22 (0.26) ***	4.58 (0.36) ***	-	5.22 (0.26) ***	5.22 (0.26) ***
Vote intention 2006-2012: VVD (dich.)	3.46 (0.28) ***	4.20 (0.55) ***	-	3.47 (0.28) ***	3.47 (0.28) ***
<i>Share of support for VVD in previous polls (ref: Often (&gt;90%-100%))</i>					
* Never (0%)	-	-	-8.54 (0.57) ***	-	-
* Rarely (>0%-25%)	-	-	-3.90 (0.35) ***	-	-
* Regularly (>25%-90%)	-	-	-1.63 (0.33) ***	-	-
Electoral volatility (dich.)	-0.58 (0.19) ***	-0.60 (0.19) ***	-0.70 (0.20) ***	-0.72 (0.29) ***	-0.58 (0.19) ***
Participation rate	0.02 (0.00) ***	0.02 (0.00) ***	0.02 (0.00) ***	0.02 (0.00) ***	-0.00 (0.09)
Level of education	0.02 (0.05)	0.02 (0.05)	0.03 (0.05)	0.01 (0.05)	0.02 (0.05)
Gender	0.06 (0.15)	0.07 (0.15)	0.04 (0.15)	0.07 (0.15)	0.05 (0.15)
<i>Interaction effects</i>					
<i>Ref: Treatment (dry poll outcome)</i>					
* Treatment: success * VVD (share)		1.19 (0.52) *			
* Treatment: failure * VVD (share)		0.85 (0.50)			
<i>Ref: Treatment (dry poll outcome)</i>					
* Treatment: success * VVD (dich.)		-1.58 (0.64) **			
* Reference point: failure * VVD (dich.)		-0.20 (0.75)			
<i>Ref: Treatment (dry poll outcome), VVD (often)</i>					
* Treatment: success * VVD (never)			0.24 (0.78)		
* Treatment: success * VVD (rarely)			-1.12 (0.58) *		
* Treatment: success * VVD (regularly)			-0.96 (0.57) *		
* Treatment: failure * VVD (never)			-0.42 (0.84)		
* Treatment: failure * VVD (rarely)			-0.65 (0.51)		
* Treatment: failure * VVD (regularly)			-0.32 (0.50)		
<i>Ref: Treatment (dry poll outcome)</i>					
* Reference point: success * volatility				-0.11 (0.39)	
* Reference point: failure * volatility				0.44 (0.37)	
<i>Ref: Treatment (dry poll outcome)</i>					
* Reference point: success * education					0.16 (0.12)
* Reference point: failure * education					-0.08 (0.12)
N	6554	6554	6554	6554	6554

Source: 1VOP 2006|1 to 2012|11

Logistic regression analyses. Standard errors between brackets; one-sided tests; \* p&lt;0.05



Source: 1VOP, poll May 16<sup>th</sup> and further

Point estimates (black) and 95% confidence intervals (grey)

**Figure 1. Vote intention for the PvdA across seven experimental groups**

## End notes

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<sup>1</sup> Bandwagon effects might be self-reinforcing: success in the polls breeds more subsequent success. Yet, even self-reinforcing bandwagon effects are likely to top off, as the news of the party's increasing support grows stale or as the party reaches its electoral potential (Bartels 1985).

<sup>2</sup> Navazio (1977) and Schmitt-Beck (1996) suggest that the level of political sophistication influences which polling effect occurs. They argue that lower educated are more likely to vote for the underdogs, whereas the higher educated are more likely to jump on the bandwagon of a winner.

<sup>3</sup> The de facto number of respondents at each individual time point is smaller: the list of respondents gets updated on a regular basis to exclude dead and dormant accounts.

<sup>4</sup> Most notably, the 1VOP covers more supporters of the Socialist Party (+4%) and fewer of the Christian-Democrats (-10%). Controlling for party preference did not affect the outcomes.

<sup>5</sup> If we weight the respondents according to their voting behaviour in 2006, the election results and the *changes* in election results are strongly correlated ( $r=0.98$ ) to those in the 1VOP dataset.

<sup>6</sup> There was also an ethical reason for us to use non-fabricated opinion poll outcomes as our treatments: the respondents do not expect to be lied to in this survey.

<sup>7</sup> Theoretically, we expect no difference between the first two conditions. We included the second condition, though, as a check to rule out any treatment effect of the survey question itself.

<sup>8</sup> Note that the treatments were not symmetrical: given our emphasis on realism, we cannot formulate similar losses and gains reported in the exact time span.

<sup>9</sup> Inclusion of this group of respondents leads, as expected, to substantially the same effects but with marginally larger confidence intervals. The significance of the effect of the growth vignette in Table 2 would drop below .05 in model 1 (from  $p=.045$  to  $p=.071$ ), but not in the bivariate analysis.

<sup>10</sup> Ideally, we would have included a more direct measure of political sophistication or knowledge. Unfortunately, though, such a measure was unavailable in the 1VOP data set, except for a one time question posed 1.5 years earlier to less than half of the respondents.

<sup>11</sup> We cannot statistically conclude either that the dry poll is equivalent to the reference group: An additional test of equivalence (Wellek 2010) within a predefined interval of 0.3% provides no definite conclusion. We thus find no evidence that poll results without a 'qualitative stimulus' affect vote intentions (cf. Fleitas 1971; Navazio 1977), but no evidence either that they do not. A more powerful test is required.

<sup>12</sup> We left this effect out of the multivariate analyses in Table 2, as the voting behaviour question was asked only retrospectively for the most recent 1VOP participants concurrent with their current vote intention; inclusion could invoke endogeneity issues (Van Elsas et al. 2013).