The Effect of Tailor-Made Information on Electoral Participation: The Case of Voting Advice Applications

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Abstract. A new type of interactive online tool has just mushroomed in European democracies and beyond. Voting Advice Applications (VAAs) help users casting a vote by offering an explicit ranking of viable options with an implication that this ranking is tailored according to the user's political opinions. The wide amount of readily available information provided by VAAs to users has been shown to contribute to reducing the transactional costs involved in gathering relevant political information. Available evidence also supports the idea that VAA users are more likely to cast a ballot in elections as a result. The extent to which electoral participation is *caused* by using a VAA, however, remains unclear. Exclusive reliance on case studies, data limitations and methodological shortcomings plagued in a way or another virtually all previous studies. Against this background, we decided to reassess the mobilizing effect of VAAs by means of a multi-method approach. Our cross-sectional analysis of twelve national election study datasets provides further support to the idea that VAA usage increases users' chances of casting a ballot in elections as compared to non-users. This conclusion is strengthened by the results of a randomized field experiment conducted in the context of the 2013 Italian parliamentary election in cooperation with the Italian National Election Study. The scattered diffusion of VAAs in Italy provided the ideal conditions to test their effects in a sort of nationwide laboratory, and it further substantiated previous findings stemming from those countries where effects could be imputed to their widespread diffusion.

Introduction

The advent of the World Wide Web has profoundly altered the way political information is produced and digested by the wider public at election time. Internet's multiple links with the political process have been put under tough scrutiny by social and political scientists in recent years. The available literature has generated relatively ample evidence that in Western democracies the emergence of the Internet resulted in a significant change within political behavior (for a review, see: Chadwick and Howard, 2009). Research has focused on its possible impact on political engagement and participation, either directly – e.g., encouraging users to participate – or indirectly – e.g., providing them with the necessary information to do so (Norris, 2000). And indeed, web-based political information has been shown to bear a positive impact on broadly-defined patterns of political engagement (for a review, see: Boulianne, 2009) as well as more specific patterns of electoral participation (Tolbert and McNeal, 2003; Bond *et al.,* 2012) – this being especially the case with the younger generation (Hirzalla *et al.,* 2010).

One of the defining characteristics of online political communication lies with its interactive capabilities. Its peculiar effects, in turn, have been hypothesized to stem from the delivery of "more detailed information [that] can be customized to a greater extent" (Prior, 2005: 579) by users. The provision of *tailor-made information* is indeed a common phenomenon in today's online landscape. Social media relentlessly (re)shape the information environment by allowing users to manage information in a way that fits with their needs (Lau and Redlawsk, 2006). Facebook, for instance, provides its users solely with information about status and activities of persons and pages they decided to follow. In this way, users receive information – including political information – in the light of their own preferences. While the relationship between patterns of political communication on mainstream social media and electoral participation has been convincingly documented (see, most notably: Bond *et al.,* 2012), very little is yet known about the electoral impact of tailor-made political information

provided by online platforms of relatively smaller scale.

In the last decade, a new type of online tool has mushroomed in European democracies and beyond. Voting Advice Applications (thereafter: VAAs) help users casting a vote by comparing their policy preferences on major issues with the programmatic stances of political parties on such issues (for an overview, see: Garzia and Marschall, 2014). VAA respondents fill in a questionnaire with their opinion on a wide range of policies. After comparing the user's profile with that of each party/candidate, the VAA produces its "advice" in the form of a rank-ordered list, at the top of which stands the party/candidate closest to the user's policy preferences. Whereas the advice provided by the VAA is to be considered as a form of political communication, it must be also noted that it differs considerably from most of the campaign messages that citizens traditionally receive. Like traditional media, they relay information about parties' positions to voters. Unlike other sources, however, they provide customized political information. VAAs offer an explicit ranking of viable options with an implication that this ranking is tailored according to the user's political opinions. In other words, VAAs reveal to the user the structure of the political competition *in light of her own preferences*. The ability of VAAs to reduce the costs of information at election time is one of the keys to understand their growing success among voters (Alvarez *et al.,* 2014a). Nowadays, the existence of at least one VAA has been witnessed in virtually all Western democracies. In countries like Belgium, Germany, Switzerland and the Netherlands, the proportion of eligible voters resorting to VAAs at election time ranges between ten and forty per cent (Marschall, 2014). In Scandinavian countries, VAAs are mentioned as the *primary* source of political information during the campaign by a relative majority of voters, outnumbering traditional media such as newspaper and television (Ruusuvirta, 2010).

The massive spread of VAAs across countries and users and their increasing relevance in the electoral process have resulted in a fast-growing number of academic papers devoted to

the topic.[1] A significant stream within this literature shares a common interest in political behavior and, in particular, in the ways in which VAAs can affect voters' patterns of electoral participation. Available evidence supports the idea that VAA users are better informed and hence more likely to cast a ballot in elections as compared to non-users (Schultze, 2014). The extent to which electoral participation is *caused* by using a VAA, however, remains unclear. Exclusive reliance on case studies, data limitations and methodological shortcomings plagued in a way or another virtually all previous studies. Against this background, we decided to reassess the electoral impact of VAAs focusing on their actual mobilizing potential through the following research question: *Do VAAs increase the likelihood of their users to cast a ballot in elections?* We begin by sketching our theoretical framework. Then, we provide a critical review of the available works on the topic. After having outlined the uniqueness of our two-fold methodological approach, we present the results of our empirical analyses, focusing in turn on cross-sectional data and experimental evidence. The final section discusses the results as well as their major contribution to (and implications for) political communication research.

Theoretical Framework

The *civic voluntarism* model postulates that political resources, such as information and knowledge, are a key precondition for participation (Verba *et al,* 1995). With more information, citizens are better able to make sense of their own position relative to the electoral supply and thus more likely to cast their ballot in elections. Available studies of the impact of political knowledge on electoral participation confirm that higher levels of political information increase the likelihood of voting (Palfret and Poole, 1987; Delli Carpini and

[1] According to Google Scholar, 554 papers including the string "Voting Advice Application" have been published between 2004 and 2015. Together, they attracted 2535 citations (H-Index=26).

Keeter, 1996; Lassen 2005). Accordingly, the individual-level probability to cast a vote can be postulated as inversely proportional to the effort required to gather enough information. A number of costs are involved in the process of becoming sufficiently informed, namely: *procurement*, i.e., gathering the relevant data; *analysis*, i.e., undertaking a factual analysis of the data; and *evaluative,* i.e., relating data and/or factual analysis to specific goals (Carmines and Huckfeldt, 1996: 245). With several issues at stake and a multitude of parties and/or candidates running for office, the task of gathering information may augment the cost of voting up to a point that overcomes benefits, thus possibly keeping away citizens from the ballots. In the low-information rationality framework, voters are expected to minimize this effort by relying on whatever 'free' or inexpensive information can be picked up (Popkin, 1991). In this sense, the wide amount of readily available information about politics and political parties provided by the VAA contributes to reducing the transactional costs involved in gathering relevant political information and increasing the likelihood of voting in turn.

Available studies of VAA effects on users' political knowledge confirm the idea that VAAs improve users' knowledge about political matters during the campaign. Ladner (2012) reports over four *smartvote* users out of five indicating that using the VAA improved their knowledge of the 2011 Swiss election. Kamoen *et al.*'s (2015) analysis of the 2012 Dutch parliamentary election provides evidence that VAA usage increased users' factual knowledge of political parties and party standpoints. Similar figures are reported by Schultze (2014) for the case of Germany. These knowledge effects appear larger for young users (Ladner *et al.,* 2009) as well as among those who consider VAAs to be a "serious" advice instrument (Alvarez *et al.,* 2014a; Kamoen *et al.,* 2015).

For VAAs to bear an actual effect on electoral behaviour, however, improving knowledge is a necessary yet not sufficient condition. Discovering one's position *vis-à-vis* the political parties running in the election cannot be expected to lead the user to participate in a mechanical fashion. Her views need to be *echoed* to a reasonable extent by at least one of the

available alternatives. This is where the crucial role played by tailor-made political information kicks in. Alvarez *et al.* (2014b) developed the concept of *representative deficit* building on the degree to which voters fail to match the political supply – the lower the match between the parties' standpoints and the voter's preferences, the higher the representative deficit. The representative deficit is precisely the conditioning mechanism that makes users more likely to take their revealed preferences into account. A low representative deficit can be interpreted as a convincing political self-portrait. It shows the users that "their" party – that is, a party that greatly overlaps with their policy preferences – does indeed exist. In doing so, it might incite them to turn out and cast a vote for such party. On the other hand, users finding themselves on a corner of the political space where no party can be found may experience a sense of "political solitude" with possible negative effects on their willingness to take part in the election. To put it down using a simple commercial analogy, if the offer displayed in the vitrine does not match the demand, the context for entering the shop is unfavorable (Dinas *et al.*, 2014: 292).

Cross-Sectional Evidence: Review and Analysis

The first studies investigating the impact of VAAs on electoral participation were conducted by Stefan Marschall and his team on the case of the German *Wahl-O-Mat*. In both the 2004 and the 2009 German Federal elections, over one user out of ten declared to "feel more motivated to turnout because of having used [that] VAA" (Marschall, 2005; Marschall and Schmidt, 2010). In the same years, another research group led by Andreas Ladner began analyzing the electoral impact of the Swiss VAA *smartvote*. Their early analysis of the 2007 federal election found about forty per cent of respondents declaring that using the VAA had a "decisive or at least slight influence on their decision to go to the polls" (Ladner and Pianzola, 2010). On the basis of these data, Fivaz and Nadig (2010) concluded that the

overall turnout in that election could have been about 5 per cent lower had the *smartvote* platform not made available to Swiss voters.

A critical issue with the aforementioned studies lies with their exclusive reliance on opt-in surveys administered to users right after having been exposed to the VAA. In other words, the influence exerted by the VAA on users is measured through self-assessment and *only* among those who are willing to fill the opt-in survey. Apart from being subject to a heavy self-selection bias, these type of data do not even assure that subjective estimates of impact will match with actual changes in terms of preferences and behavior. Indeed, Walgrave *et al.* (2008) find that the reported intention of changing behavior as a result of having used a VAA is not always (nor often) matched with actual changes in voting behavior.

In order to address this critical issue, VAA scholars have turned to mass survey data. Marschall and Schultze (2012) take advantage of a pre-electoral wave of the German Longitudinal Election Study (GLES) and find a 6 per cent increase in the probability to cast a ballot among VAA users as compared to non-users. However, their study suffers a low external validity because the dataset employed consists of a quota sample of the German online population. Moreover, the dependent variable is measured before the election, so one cannot be sure whether turnout intentions get actually converted into electoral participation.

To overcome these limitations, a growing number of studies have resorted to national election study data. Working with nationally representative samples increases substantially the external validity of the findings. At the same time, the structure of post-election surveys allows for factual measures of VAA usage (rather than subjective assessments of impact) and actual voting behavior. Gemenis and Rosema's (2014) analysis of 2006 Dutch Parliamentary Election Study (DPES) data estimates, by means of simulation, that the presence of VAAs was responsible for 4.4% of the reported turnout in that election. Another analysis by Dinas *et al.* (2014) on European Election Study (EES) data shows that even after controlling for a wide set of socio-structural, attitudinal and behavioral variables, the individual-level

probability to cast a vote in the EP election of 2009 was 14 percentage points higher for VAA users as compared to non-users.

This inventory of studies, by and large confirming the hypothesized positive association between VAA usage and electoral mobilization, highlights nonetheless commonalities in terms of their exclusive reliance on case studies. To put the mobilization hypothesis to a more demanding empirical test, the analysis that follows provides a comparative reassessment of the effect of VAAs on users' patterns of electoral participation across countries and time. To this aim, the analysis resorts to standardised cross-national measures of VAA-usage as made available by the growing amount of national election studies asking voters whether they used a VAA during the campaign. The present analysis expands on the number of elections included in Garzia *et al.*'s (2014) study, and employs eight datasets from four different European countries: Finland (2003, 2007, 2011), Germany (2009, 2013), The Netherlands (2003, 2006, 2010, 2012), and Switzerland (2007, 2011). We also analyse the European Election of 2009, since that year's ESS included a question on VAA usage. The dependent variable of the analysis is a dichotomous variable indicating whether respondents did take advantage of their right to vote in the election under analysis. Another dichotomous variable measures whether respondents used one (or more than one) VAA during the campaign.[2]

[2] In the Dutch and Swiss studies, the whole sample has been asked directly about VAA-usage during the campaign. However, in both the 2006 and 2010 Dutch studies, the "direct" VAA-usage question was asked only to those who declared in a previous question that they "know one or more tests of political preference on the internet, where people can find out which party they agree with the most". In this analysis, we coded '0' all those who answered negatively to the "filter" question. Also the German study of 2009 features a direct question of VAA usage. However, only a subsample of users has been asked this question – namely, all those who reported to have used (at least) once a week the Internet to inform themselves about political parties during the federal election campaign (i.e., Those who declare to have never used internet to gather information about political parties during the campaign have been coded '0'). With respect to Finland, there is no direct question

Table 1 presents the proportion of study respondents that declared to have used a VAA during the campaign in each dataset. VAA-usage appears, unsurprisingly, mostly spread in Finland and the Netherlands – that is, the two countries in which VAAs have appeared first. There, over one third of respondents declare to have used at least one VAA during the campaign. In Germany and Switzerland, this proportion amounts to about 10 percent. Interestingly, an unambiguous upward trend in the proportion of VAA-users across time can be observed in each of the countries under analysis.

A comparison of turnout rates across users and non-users in each dataset is also presented in the table. The bivariate analysis confirms that VAA users are systematically more likely to cast a vote in elections as compared to non-users. The statistical association between these two variables is indeed highly significant and signed as expected in each dataset, and so are the various t-tests.

< Table 1 about here >

As the major purpose of the present analysis is testing to what extent electoral mobilization can be correctly attributed to the act of having used a VAA, we also need to control for a number of alternative explanations of electoral participation within a multivariate setting. Given the relatively low number of countries and elections under analysis, we abstract from contextual (i.e., socio-structural and institutional) explanations and focus on individual-level determinants. Drawing on the useful meta-analysis by Smets and van Ham (2013), our analysis includes statistical controls connected to individuals' socio-

regarding VAA use. We then decided to resort to an indirect measure based on how much did respondents follow the election campaign on "Candidate Selectors on the Internet" (i.e., VAAs). The possible answers are: "A great deal", "Quite a lot" "Not very much", "Not at all" and "Can't say". All the respondents picking any of the first three answers are coded '1', with all others coded '0'.

demographic profile (age, age-squared, gender, and educational attainment), belonging to intermediary associations (religiousness), and political attitudes (strength of party identification, self-placement on the left-right scale, interest in political matters and sense of satisfaction towards democracy). We also include a variable tapping whether respondents did cast a vote in the previous election in order to control for the effect of voting habits. As the decision to turnout in the previous election may have been due to at least some extent by VAA-usage (something we unfortunately cannot control for due to the cross-sectional nature of the data at hand) the inclusion of this latter control serves also as a means to consider our results as a relatively conservative estimate of the impact of VAA usage on participation. Given the dichotomous nature of the dependent variable, logistic regression has been preferred to OLS estimation.

< Table 2 about here >

The results presented in Table 2 provide strong confirmation of our research hypothesis. VAA-usage is systematically related in a statistically significant way to electoral participation. The relationship holds even after the introduction of our extensive set of statistical controls. The results of our analytical effort are summarised in Table 3, where we present the increase in the predicted probability of casting a vote in the election under analysis moving from a value of '0' (i.e., did not use a VAA during the campaign) to a value of '1' (i.e., did use at least one VAA during the campaign) of our key independent variable.

< Table 3 about here >

Robustness

To test the robustness of our findings, we estimated a set of likelihood ratio tests comparing for each election the constrained model that excludes an effect of VAA usage with the

unconstrained one considering it in the same subsample of voters. In every single case we are led to reject the hypothesis that VAA usage can be excluded from the empirical models of turnout. However, even if we established that the inclusion of VAA use systematically improves our models of electoral participation, we cannot still exclude that the positive association that appears to link VAA use and electoral behavior might be actually driven by relatively small categories of influential observations. Narrow clusters of voters may be responsible for the most part of the statistical association in a context in which, if excluded, the relationship would be moderate or even absent. For instance, one could think of a cluster of young, highly educated voters with Internet access and a strong interest in political matters. For these strata of voters the hypothesized relationship may be somewhat greater in strength. Hence, it is opportune to evaluate it also after having excluded potential influential observations (i.e., those affecting the magnitude of regression coefficients). Overall, we find no changes after excluding influential observation – as defined by those voters reporting a value of the Cook's statistics greater than 0.5 – from the estimation sample, as the average marginal effect of VAA usage on turnout remains 9.5% (on a pooled dataset). Differently, when estimating our models excluding outlier observations, the marginal effect of VAA decreases to 7.5%. However, in all cases we are still able to find positive and significant coefficients.

In spite of these demanding tests, it must be kept in mind that cross-sectional data has its inherent limitations when it comes to causal inference. VAA use is not randomly assigned to individuals. If the decision to become a VAA user and the decision to go to the polls have common determinants that are either unmeasured or unknown, estimates from a regular regression model will be biased. Recent notable attempts to tackle the shortcomings of cross-sectional data in the study of VAA effects on electoral participation include Heckman selection models (Garzia *et al.,* 2014; Pianzola, 2014a) and approximate matching techniques (Gemenis and Rosema, 2014). Previous scholarship has already pointed out the key problem

inherent to the application of Heckman selection models to the topic at hand, namely, their inability to satisfy the necessary assumption that the factors affecting the treatment variable – i.e., using a VAA – do not affect the outcome variable – i.e., casting a vote in elections (*ibid.*). Approximate matching techniques, to the contrary, do not need the separation of outcome and treatment assignment equations, the exogeneity of observables pre-treatment covariates, nor the adoption of specific functional forms for the outcome equation (Heckman and Navaro-Lozan, 2004). However, their application leads to include more observations than might be appropriate, relying on the specific chosen metric to match all treated units, considering also those who do not have proper comparable controls. The application of approximate matching methods hides long cycles of balance checks and re-checks. And yet, there is no insurance that improving balance on one variable leads to greater imbalances on others. Against this background, we opted for a specific type of exact matching algorithm called Coarsened Exact Matching (Iacus and King, 2012). The results of this further robustness check, which we present in Appendix, are in line with our expectations, highlighting only minimal differences as compared to the estimates provided by logistic regression.

Experimental Evidence: Review and Analysis

Notwithstanding the methodological advances allowing better control of respondents' data in a context plagued by self-selection into the treatment (i.e., using the VAA), it is evident that the ideal scenario for a causal assessment of VAA effects on users' patterns of electoral mobilization remains the *random assignment* of the treatment in a proper experimental setting. As of now, only a small bunch of experimental studies of VAA effects have been made available in the literature. Vassil's (2012) analysis of the 2009 Estonian election to the European Parliament finds very weak effects of VAA usage on participation. As his study population consists exclusively of university students, however, the findings are of limited

external validity. A similar problem afflicts the study by Maheo (2014) who administered her "treatment" only to a subsample of voters in a low-income voter neighborhood in Montreal during the 2014 Quebec provincial election campaign.

An experimental analysis of VAA effects involving a nationally representative sample of voters is that by Pianzola (2014b) in the context of the Swiss Federal election of 2011. Unfortunately, this study suffers of a very low "first stage". If access to a VAA is open to the public (as it was the case with the Swiss VAA *smartvote* employed in the study) one cannot exclude the possibility that subjects in the control group could take the treatment independently from the experiment. Indeed, over 70 percent of participants in the control group reported to have used *smartvote*. The same troubling issue – although in reduced magnitude – is to be found in the experimental design employed by Enyedi (2015) in his analysis of the 2010 Hungarian parliamentary election.

To overcome all the limitations stemming from the existing studies, we have set up an experiment in the context of the most recent parliamentary election held in Italy, on February 24th 2013. This election provides an extremely interesting setting for testing our mobilization hypothesis. For the first time in the history of Italian parliamentary elections, turnout fell below 80 per cent – the actual figure being 75.2 per cent. Although in line with the general trend of declining turnout rates across established democracies, the magnitude of decline in the 2013 election (i.e., minus 5.3 percentage point as compared to 2008) was more marked than one could expect based on the last 20 years' trend line. Possible explanations included the growing disaffection towards politics on behalf of Italian voters, the weakening ability of traditional agents of political mobilization (e.g., religious organizations, trade unions, political parties) and the declining trust in "newer" sources of mobilization such as political leaders, whose image capital reached an all-time low in that election (Barisione *et al.*, 2013). Against this background, there is room to believe that the provision of relevant, easily accessible political information may ignite a cognitive-based pattern of (re)mobilization. The

Italian case can also be considered an ideal "laboratory" for the assessment of VAA-effects in the context of real-world elections. The country is in fact characterized by a surprising lack of VAAs made available to voters (Marschall, 2014). Concerns with respect to the first-stage are further minimized by our decision to resort to a "mock" VAA platform. Through an invited accessibility design**,** the experimental VAA platform was in fact accessible only to the respondents in the treatment group. In this way, we were able to overcome the main shortcoming of inherent to the existing studies without the need to indulge in the unpractical (as well as unethical) exercise of denying a group of citizens access to a VAA while forcing others to use it.

The issue statements at the core of our mock VAA platform were based on the salient issues of the campaign. The final selection of 30 statements was guided by the aim of maximizing variation across parties (that is, we excluded all those questions with a lower discriminating power across party positions) as well as comprehensiveness in terms of policy domains.[3] The positioning of parties on the various statements was achieved on the basis of a hierarchy of available data sources. Party manifestos were obviously the main source of information. When information about specific issues was not available in party manifestos, we resorted to party websites' content and declaration of party leaders. If none of these sources proved useful, we made use of previous expert positioning endeavours conducted on the Italian case (i.e., ITANES Expert Survey 2011). All parties already represented in Parliament, as well as those with a reasonable chance to attain representation in the 2013 legislature were coded by the research team, for a total of 14 parties included in the VAA. The experimental VAA platform invited respondents to offer their reaction to the 30 issue statements with one of five responses, ranging from "completely agree" to "completely disagree" plus a "no opinion" option. The calculation algorithm was based on the *city-block*

[3] The wording of the 30 statements is provided in Appendix.

method. The visual outcome was the classic match-list, at the top of which stands the party closest to the respondent's policy preference (see Figure 1).

< Figure 1 about here >

The experiment was embedded in a multi-wave CAWI panel of the Italian National Election Study (ITANES). The panel design of the study was especially useful for the purposes of the experiment as it allowed not only to measure the outcomes of interest after the election, but also to measure baseline attitudes and behavior before participants' exposure to the treatment.[4] The experimental protocol consisted in three stages:

1. Pre-treatment measurement. The pre-treatment measurement was carried out on the entire sample population (N=908) on January. The survey included items about respondents' baseline political attitudes and behaviour (i.e., willingness to participate in the forthcoming national election and voting intention).

2. Randomization and treatment assignment. The sample has been randomly split in halves (N=454). Only the treatment group received, on February 15th, an invitation to take part in the experiment. Upon acceptance, respondents were redirected to our server and asked to perform the VAA test. Response rate was a noteworthy 95.6 percent (N=434). Users' perception of the usefulness of the VAA were widely positive: 35 percent of the respondents

[4] The main drawback of CAWI technology lies in the slightly biased demographics of those who tend to respond to online questionnaire invitations. Indeed, youngsters were slightly over-represented in our sample (mean age is 45.5 as compared to the 49.4 in the CATI post-electoral survey fielded simultaneously by ITANES) and so were respondents with high educational level (university graduates are 23.4 per cent of the sample as opposed to 12.9 per cent among CATI respondents).

rated it 'very useful' and 45 percent 'fairly useful'. Table 4 presents the result of the balance test we performed through multivariate logistic regression of treatment assignment based on a parsimonious set of typical predictors of VAA usage. The results show that the sample is well balanced: none of the predictors discriminates the two groups in a statistically significant way. This confirms that the random assignment was performed correctly, so it is possible not to include control variables when comparing treatment and control groups.

3. Post-treatment measurement. The post-treatment measurement was carried out in late February/early March. This involved again the entire sample population. The key attitudinal questions remained identical from those in the previous wave in order to achieve full comparability. As to turnout, the voting intention measure was replaced with its behaviour-recall counterpart (i.e., "did you vote in the last national election?").

< Table 4 about here >

The focus of the analysis is on the mobilization potential of VAAs. The dependent variable is thus *Mobilized* – the difference in the intention to participate in the forthcoming national election as measured in pre-treatment survey [*TurnoutInt*] and the reported turnout as measured in the post-treatment survey [*Turnout*]. Note that in the pre-treatment survey, respondents' intention to cast a ballot was measured through a 4-point scale ranging from "very likely" to "not at all likely". In order to achieve comparability with the actual turnout variable (dichotomous) we recoded the turnout intention variable in two ways. The first variable [*TurnoutInt*] codes '1' only those respondents declaring themselves "very likely" to participate in the forthcoming election, and '0' all others. To put under test the robustness of the estimates, we created a second variable [*TurnoutInt_Alt*] where respondents declaring themselves either "very likely" or "fairly likely" to participate in the forthcoming election are

coded '1'.[5] As a result, we created two dependent variables:

(1) *Mobilized = Turnout – TurnoutInt*

(2) *Mobilized_Alt = Turnout – TurnoutInt_Alt*

Both variables measure the VAA's capacity to mobilize those who intend to abstain from the elections but subsequently still vote. Therefore, both dependent variables are coded '1' in all those cases in which the respondent aims to abstain from the elections at t_{-1}, but then decides to participate in elections. The variable is coded '0' for those to whom the intention to participate was equivalent to the reported behaviour after elections (that is, planned to vote and voted, and correspondingly, did not plan to vote and did not vote). There were few observations who intended to vote, but subsequently did not. These 'demobilized' voters are coded '–1'. Summary statistics of the two dependent variables are presented in Table 5. Regardless of the operationalization of the mobilization variable, one can easily note that treatment takers are more likely to be mobilized as compared to non-takers [+10.9 percent for *Mobilized*, +7.8 percent for *Mobilized_Alt*].

< Table 5 about here >

To test whether respondents' patterns of electoral mobilization across the campaign are statistically different across treatment and control groups, we estimated two univariate regressions (one per dependent variable) in order to isolate the *Average Treatment Effect* (ATE) of VAA usage on electoral mobilization. The results presented in Table 6 (panel A) demonstrate that the effect of the treatment on mobilization is statistically significant

[5] Due to missing values on either wave (i.e., DKs, N/A), 46 respondents have been excluded from the analysis.

regardless of the operationalization of the dependent variable

< Table 6 about here >

Robustness

A non-trivial problem with our experiment lies with those few respondents (N=20) assigned to the treatment group who decided not to take the treatment. A way to deal with this issue is to assign those respondents to the most unfavourable condition for our hypothesis to hold, namely, we assign them to the group of de-mobilized takers (score '-1' of the dependent variable). This conservative strategy puts the effect of the treatment under a stricter test, as it artificially conflates the number of de-mobilized respondents among the treatment group. The results, as presented in Table 6 (panel B), bring further support in favour of our hypothesis.

Concluding remarks

In our opinion, this paper contributes to the longstanding debate on information and elections focusing on a new type of interactive online tool: Voting Advice Applications. We provide further evidence for the positive impact of VAAs on electoral participation. Indeed, the delivery of readily available, tailor-made political information to users does not only appear to enhance their knowledge about party standpoints: it provides them with a clear overview of where parties stand compared to their own opinions, possibly motivating them to take advantage of their right to vote. As VAAs turn increasingly important in electoral democracies worldwide, their mobilizing potential calls for careful empirical assessment. Our cross-sectional comparative analysis shows that VAA users are systematically more likely to cast their ballot in election as compared to non-users, in spite of the country (among those we analysed) in which the election takes place and regardless of the first vs. second order nature of that election. Our experiment, through randomization of the treatment condition, supports

the idea that the VAA-impact takes place independently of whether voters self-select themselves into using the tool (as it is the case in our cross-sectional analyses). The experiment itself contributes to the research on the mobilizing potential of VAAs through an "ideal" design: an experiment on a nationally representative sample of voters in the context of a real election. The scattered diffusion of VAAs in the Italian context provided the conditions to test VAA effects in a sort of nationwide laboratory. The choice of Italy as a case study further substantiated previous findings stemming from those countries where effects could be imputed to the widespread diffusion of VAAs (e.g., Gemenis and Rosema, 2014). A note of caution is in order, though. In this analysis, we only estimated average treatment effects (ATE) assuming a constant effect among subgroups. Yet there are grounds to believe that certain subgroups are particularly "vulnerable" to the mobilizing effect of VAA usage. Further research in this direction is obviously called for if we are to establish a clear profile of a tentative VAA "target group" (van de Pol *et al.,* 2014).

As to the theoretical contribution of this study to the broader field of political communication research, one observes that VAAs produce identifiable media effects that are normally not easy to isolate within the complex informational dynamics of today's *hybrid* media systems (Chadwick, 2013). Interdependence between the usage of VAAs and other media can be assumed. This observation raises a number of questions that will in all likelihood be central in future studies on the topic: How does the use of VAAs influence the importance of other channels of political information? To what extent do the use of VAAs supplement (or substitute) the already existing media repertoire of recipients? And when it comes to effects: Are they independent or do they reinforce each other? By answering to these questions, VAA research will contribute in turn to a better understanding of the intervening effects of the media system in which they are located.

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Table 1 – Summary measures of VAA usage across datasets and mean turnout rate (VAA users vs. non-users)

	Finland			Switzerland		The Netherlands				Germany		EES
	2003	2007	2011	2007	2011	2003	2006	2010	2012	2009	2013	2009
Total N	1,249	1,408	1,293	4,377	4,379	2,556	2,521	2,247	1,677	2,072	1,905	25,238
VAA users (N)	276	415	556	358	496	819	903	897	672	200	273	1186
VAA usage %	22.1	29.5	43.0	8.18	11.3	32.0	35.8	39.9	40.1	9.65	14.3	4.70
% turnout (users)	93.10	91.80	91.70	86.00	87.10	98.30	97.00	96.10	95.40	96.00	92.30	89.60
% turnout (non-users)	77.50	80.20	83.30	67.50	72.40	94.80	90.40	88.00	79.70	77.60	82.40	70.70
Correlation rho	0.17	0.14	0.12	0.11	0.11	0.08	0.12	0.14	0.22	0.13	0.10	0.09
Chi^2	34.00	28.90	19.80	52.80	49.40	17.20	38.10	44.20	82.20	37.30	17.10	198.00
p-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Table 2. Cross-sectional analysis: Logistic regression estimates

	Finland			Switzerland			The Netherlands			Germany		EES
	2003	2007	2011	2007	2011	2003	2006	2010	2012	2009	2013	2009
Age	0.016	0.042	0.035	0.023	0.022	0.089**	0.157***	0.014	0.010	0.003	0.115***	-0.000
	(0.034)	(0.032)	(0.033)	(0.018)	(0.020)	(0.041)	(0.040)	(0.026)	(0.029)	(0.028)	(0.025)	(0.006)
Age^2	-0.000	-0.000	-0.000	-0.000	-0.000	-0.001*	-0.001***	-0.000	0.000	0.000	-0.001***	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Education	0.227	1.248***	1.287***	0.700***	0.167	0.122	0.533	0.513*	1.016***	0.644*	1.269***	0.313***
	(0.363)	(0.395)	(0.423)	(0.170)	(0.170)	(0.451)	(0.379)	(0.310)	(0.301)	(0.371)	(0.404)	(0.057)
Gender	-0.244	0.068	0.083	0.101	-0.027	-0.210	-0.158	-0.017	-0.037	-0.238	0.301*	-0.037
	(0.187)	(0.191)	(0.207)	(0.095)	(0.096)	(0.244)	(0.226)	(0.181)	(0.180)	(0.164)	(0.174)	(0.036)
Religion	0.086	0.090	0.303	0.068	0.257**	0.640***	0.033	0.432**	0.008	-0.003	-0.016	0.125***
	(0.297)	(0.253)	(0.240)	(0.120)	(0.113)	(0.241)	(0.231)	(0.195)	(0.187)	(0.163)	(0.186)	(0.038)
Party Identification	0.160	0.353***	0.251**	0.302***	0.401***	-0.011	0.230	0.578***	0.672***	0.669***	0.446***	0.324***
	(0.108)	(0.099)	(0.107)	(0.049)	(0.051)	(0.180)	(0.176)	(0.160)	(0.232)	(0.105)	(0.094)	(0.020)
Interest in Politics	0.603***	0.380***	0.982***	0.772***	0.753***	0.940***	0.213*	0.305***	0.383***	0.991***	0.819***	0.497***
	(0.134)	(0.129)	(0.137)	(0.065)	(0.067)	(0.244)	(0.114)	(0.087)	(0.092)	(0.142)	(0.142)	(0.022)
Satistaction w/Dem	0.434***	0.065	0.122	0.020	0.038	0.229	0.292*	0.099	0.440***	0.589***	0.518***	0.233***
	(0.139)	(0.169)	(0.169)	(0.078)	(0.077)	(0.147)	(0.163)	(0.148)	(0.150)	(0.108)	(0.121)	(0.020)
Turnout (Past)	2.054***	2.606***	1.500***	2.146***	2.196***	3.310***	2.985***	1.980***	2.117***	2.013***	2.396***	1.429***
	(0.237)	(0.239)	(0.239)	(0.102)	(0.101)	(0.270)	(0.228)	(0.229)	(0.211)	(0.166)	(0.195)	(0.049)
Used a VAA	**1.176*** **	**1.470*** **	**0.527***	**0.887*** **	**1.118*** **	**1.244*** **	**1.016*** **	**1.285*** **	**1.842*** **	**0.997*** **	**1.080*** **	**1.116*** **
	(0.310)	**(0.331)**	**(0.271)**	**(0.237)**	**(0.230)**	**(0.341)**	**(0.303)**	**(0.252)**	**(0.273)**	**(0.496)**	**(0.339)**	**(0.113)**
Constant	-2.316***	-4.016***	-3.941***	-3.578***	-3.617***	-3.638***	-5.008***	-0.886	-3.031***	-2.733***	-5.921***	-2.353***
	(0.831)	(0.921)	(0.865)	(0.504)	(0.541)	(1.080)	(1.016)	(0.680)	(0.773)	(0.773)	(0.727)	(0.157)
AIC	0.70	0.58	0.56	0.80	0.75	0.24	0.31	0.44	0.55	0.59	0.55	1.00
BIC	-7261	-9087	-8244	-28058	-29130	-19032	-15780	-14781	-10655	-12687	-12598	-173877
Pseudo R-squared	0.28	0.34	0.28	0.34	0.33	0.29	0.35	0.23	0.31	0.39	0.39	0.17
Log-likelihood	-391	-389	-340	-1504	-1449	-296	-321	-444	-425	-536	-491	-9820
N	1152	1376	1261	3782	3886	2517	2151	2064	1574	1841	1820	19592

Note: Cell entries are logistic regression estimates, with robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

add odds ratios or
marginal effects

Table 3. Increase in predicted probability of casting a vote – users vs. non-users

Finland	**%**
2003	9.3
2007	9.0
2011	3.2
Switzerland	**%**
2007	12.0
2011	12.0
The Netherlands	**%**
2003	1.8
2006	2.1
2010	4.9
2012	10.0
Germany	**%**
2009	5.5
2013	5.4
EP Elections	**%**
2009	16.0

Figure 1. The VAA questionnaire (above); the 'voting advice' provided as a result (below)

Table 4. Balance table. Logistic regression estimates

	b	S.E.
Male	.09	(.14)
Age (ref. 65+)		
18-24	-.10	(.29)
25-34	.14	(.25)
35-44	-.16	(.24)
45-54	-.10	(.24)
55-64	.24	(.24)
Educational level (ref: Low)		
Medium	.06	(.18)
High	-.07	(.22)
Interest in Politics		
Some	-.09	(.20)
High	-.33	(.22)
N	898	
Pseudo R-squared	0.01	

add notes

Table 5. Coding of the dependent variable

	Mobilized				*Mobilized_Alt*			
	Non-takers		Takers		Non-takers		Takers	
	%	(N)	%	(N)	%	(N)	%	(N)
De-mobilized (-1)	1.8	(8)	1.2	(5)	2.4	(11)	3.4	(14)
Not mobilized (0)	75.1	(341)	64.7	(264)	91.6	(416)	82.8	(338)
Mobilized (1)	23.1	(105)	34.0	(139)	6.0	(27)	13.8	(56)
Total	100	(454)	100	(408)	100	(454)	100	(408)

add notes

Table 6. Panel A. Average Treatment Effect

Dependent Variable	*Mobilized*		*Mobilized_Alt*	
	b	S.E.	*b*	S.E.
ATE	.11	(.03)***	.07	(.02)**
Constant	.21	(.02)***	.04	(.02)*
N	862		862	
R-squared	0.01		0.01	

Panel B. Average Treatment Effect (non-takers in the treatment group coded as demobilized)

Dependent Variable	*Mobilized*		*Mobilized_Alt*	
	b	S.E.	*b*	S.E.
ATE	.17	(.03)***	.12	(.03)***
Constant	.16	(.02)***	-.01	(.02)
N	862		862	
R-squared	0.03		0.03	

Note: Ordinary Least Squares estimation. *** $p < .001$, ** $p < .01$, * $p < .05$

add notes

APPENDIX. Coarsened Exact Matching (CEM) Estimation

Matching methods ground on the J. S. Mill's idea of comparing homogeneous units to provide solid inference. Matching methods work as a nonparametric tool of data pre-processing that aims at selecting only those observations that are comparable between the sets of treated and control groups, based on certain observable characteristics, and pruning the other – incomparable – observations (Ho *et al.,* 2007; Sekhon, 2008). The intuition behind matching is to pursue data reduction aiming at avoiding estimation bias. More specifically, matching in a non-experimental framework works focusing on the Average Treatment Effect on the Treated (ATT), considering all the treated units and dropping observations in the control group that are not characterized by a reasonable match based on pre-treatment observable covariates (Rubin, 1974). Matching methods do not need the separation of outcome and treatment assignment equations, the exogeneity of observables pretreatment covariates, exclusion restrictions (i.e., the set of observed treatment assignment factors does not need to be distinct from observed predictors of the outcome), nor the adoption of specific functional forms for the outcome equation. However, they rely on the assumption that the treatment variable is statistically independent – that is, ignorable – of potential outcomes after conditioning on the set of pre-treatment covariates (Heckman and Navaro-Lozan, 2004). The practice of approximate matching techniques (such as Propensity Score Matching) leads to include more observations than might be appropriate, relying on the specific chosen metric to match all treated units, considering also those who do not have proper comparable controls. The application of approximate matching methods hides long cycles of balance checks and re-checks. However, there is no insurance that improving balance on one variable leads to greater imbalances on others. To overcome these drawbacks we adopt the Coarsened Exact Matching framework (Iacus and King, 2012). CEM shares with the class of approximate matching methods the ignorability assumption, but avoids a set of potential pitfalls. It works with multiple-imputed data and avoids the misleading logic of setting ex-ante the least

important of the number of matched observations (variance), while having to carefully check and re-check the most important aspect of imbalance reduction (bias). The logic is to coarsen each variable to create a set of clusters of comparable pairs of treated and control units. In this way, it becomes possible to drop the unmatched units, recall the original values of coarsened variables and proceed with the analyses on the matched observations. CEM has been shown to eliminate imbalances including all multivariate nonlinearities, interactions, moments, quantiles, co-moments, and other distributional difference. Usual statistical moments can be implemented to estimate within strata remaining differences.

For the sake of clarity, only results from a pooled dataset including all NES data at hand are included in Table A.1, where we presents simple logit estimates (model 1), logit estimates on data preprocessed by CEM with listwise deletion of missing reported use of VAAs (model 2) and logit coefficients on Multiple Imputed data pre-processed by the CEM algorithm (model 3). Across the three models, we notice only very minor changes in the logit coefficients reporting the impact of VAA usage on electoral participation.

Table A.1 – Logit estimates on merged dataset, on CEM preprocessed data, and on MI-CEM

	(1)	(2)	(3)
Used a VAA	1.168***	1.132***	1.126***
	(0.0619)	(0.0598)	(0.0550)
Constant	-3.282***	-2.527***	-2.527***
	(0.156)	(0.168)	(0.168)
N	43,939	39,305	47,815
Average RVI	-	-	0.083
Largest FMI	-	-	0.529

Note: Standard errors in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Controls include: age, age-squared, education, gender, religiousness, strength of party identification, interest in politics, satisfaction with democracy, reported turnout in previous election, and election-specific dummies.