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Deliverable D9.1 – Practical Political Knowledge Final Report

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A. Abstract

This document summarizes my work within the ELECDEM network. I argue that politically uninformed people are less likely to provide information on their political attitudes but they are also more prone to have economically left-wing ideological leanings. This leads to the systematic overrepresentation of right-wing attitudes in surveys and may also be harmful to the interests of those who would benefit most from communitarian policies. Using linear and Poisson regression techniques on data from an experimental survey carried out in September 2009 in Hungary, I show that aggregate changes in political attitudes are likely to be revealed when correcting for item non-response missing data for attitudinal variables. Notwithstanding the relationship between political knowledge and opinionation, I argue that political knowledge has an inevitable ideological bias generated by the distribution of right wing and left wing arguments in the media and printed press. I find no conclusive evidence for this claim on cross-national data; however, null and substantively insignificant results are often found in the literature on political literacy, suggesting that it is either the theory of information effects that is wrong, or we fail at making reliable measurements of political knowledge. In order to shed light upon this matter, I investigate the sensitivity of the reliability of political knowledge scales to variations at the metric and conceptual level. We find that the format of the questions that are used for measuring political knowledge accounts for up to one third of the variance of reliability on data from the second module of the Comparative Study of Electoral Systems. The bootstrapped estimates show that this result is robust, and the same pattern holds for various measures of reliability and convergent validity. On Dutch Vote Compass data, we show that there may be issues of conceptualization as well that affect the results cited in the literature on information effects. We find that the classic triadic model of knowledge acquisition (capability-opportunity-motivation) needs to be revised to correctly specify capability as a moderator rather than a direct factor influencing political knowledge. Finally, we find with a correlational, incomplete confirmatory factor analysis, that the concept of political knowledge is not unidimensional, as the literature assumed to date. All these findings point to the conclusion that the utility of political literacy is likely to be understated in the empirical literature. Some of the null findings and weak relationships found thus far between political knowledge and indicators of participation, turnout, opinionation, tolerance or pro-democratic attitudes, are in fact stronger in reality than empirical results would suggest.
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C. Executive Summary

I investigate the informational component of the underrepresentation and overrepresentation of implicitly defined groups in democracies. My main expectations, rooted in the literature on information effects (Bartels, 1996) and normative theory of democracy, rest on the assumption that any information asymmetry has the potential to generate asymmetric benefits, regardless of the type of transaction that is taking place. People who are more knowledgeable about politics have a better chance to make political decisions that maximize their chances to have their interests politically represented. To the extent that knowledge-defined groups overlap with interest-defined ones, the interests of particular segments of society will be systematically underrepresented.

I argue that people who are less politically knowledgeable are not only less likely to be surveyed by academic and commercial pollsters, but they also have a higher propensity to abstain from expressing their opinions when prompted about their political attitudes. To the extent that the politically knowledgeable are demographically, economically and culturally different from their less knowledgeable peers, the differential voice given to knowledge-defined groups will generate skewed perceptions of the state of public opinion at any given time. This phenomenon contributes to the partial disenfranchisement of citizens with lower socio-economic status and increases political inequality whenever public policies are geared towards the pursuit of fair representation.

The acquisition of political knowledge is theoretically driven by a triad of interrelated factors: cognitive ability, opportunity and motivation (Luskin, 1990). While I argue for a revised triadic model in which cognitive ability moderates the effects of the other factors rather than having a direct effect on knowledge, opportunity and motivation appear to directly affect political knowledge. Low SES translates into low “opportunity”, which partially explains the overlap between knowledge and SES-defined groups, introduced previously. Motivation, however, influences one's level of political knowledge through newspaper readership and television news viewership. News media deliver political information with an inevitable ideological bias, thus, citizen level knowledge gains are likely to be associated with an attitudinal change in the direction of such biases. I argue that a nontrivial amount of the political inequality fostered by informational asymmetries occurs through the unequal articulation of information with left- and right- ideological biases.
The effects generally found in the literature, and in my work in particular, are rather weak; thus, I allocate a significant part of my research efforts to investigating whether the null results often cited stem from the empirical triviality of information effects or from the suboptimal measurement of political knowledge. To this end, I researched the reliability and convergent validity of political knowledge scales constructed from questionnaire items of various formats and types. Finally, I shed light upon the empirical structure of the construct by evaluating the assumption of unidimensionality that has become commonplace in the scholarly literature.

My results are manifold, both significant and null results are enumerated below with bullet points.

- The same variables that successfully predict political knowledge also predict item non-response in attitude variables: low SES respondents are less knowledgeable and more likely to provide “do not know” responses to attitude variables. They are also more likely to endorse economic redistribution.
- No conclusive evidence was found for an effect of the ideological leaning of the media environment on the direction of information effects (in member states of the European Union)
- Cognitive ability moderates the effects of opportunity and motivation on the acquisition of political knowledge. Education appears to be a poor proxy for cognitive ability, thus justifying our direct measurement of cognitive ability.
- Closed-ended questions (true/false or multiple choice) systematically overestimate the knowledge of respondents, both by allowing random guessing and by offering more cues than open-ended ones.
- The format of questions (closed vs. open ended) account for up to one third of the variance in the reliability of political knowledge scales (in CSES Module 2)
- Open-ended questions foster higher reliability and convergent validity for the resulting scales
- Political knowledge is not one-dimensional; the reliability of issue domain-specific scales is higher than that of general knowledge scales. This applies to different types of knowledge as well: chronic (knowledge about durable political matters), current (knowledge about recent developments in domestic and international politics), or name/office recognition (the ability to associate names of politicians with the positions they hold).
D. Full Report

Objectives/Aims

a. Conceptual Linkages: Evaluating Electoral Democracies

The workpackage I assumed responsibility for, WP9: Practical Political Knowledge, lies at the intersection between the fields of political and voting behavior, cognitive science, social psychology, normative democratic theory, and the philosophy of knowledge. The training requirements, research activities and outputs encapsulates elements specific to all these fields of research, in addition to the advanced statistical tools necessary for any scientific endeavor.

The main objective of my research is to discuss, evaluate and contextualize the alleged causal links between citizen political knowledge and the functioning of democratic representation. Studies to date have focused on the effects of political literacy on various indicators of political participation, attitudes and cognition mostly within a polity rather than across countries (there are few notable exceptions, mentioned in the following sections). Others have argued that knowledge about politics among citizens is necessary for an unbiased representation, as a prerequisite for and logical entailment of the idea that democratic citizens must choose their own fate and elect politicians who are likely to cater for their needs. While normative democratic theory is teeming with credible arguments for such assertions, the empirical evidence in their support is scarce at best. My research activities are geared towards finding evidence for the connection between what people know about politics and their chances to have their political interests represented in democratic institutions; between political literacy, responsiveness and accountability. Such effects are unlikely to be direct; the relationship between knowledge and top-down political outcomes can only be mediated by the behavioral and attitudinal manifestations of knowledge within the context of specific democratic institutions (formal or informal).

Evaluating the link between political knowledge and various indicators of quality of democracy necessitates close attention to political communication, due to its role in the exchange of information between political actors; formal and informal institutions, due to their power to regulate and coordinate political action; cognitive psychology for its role in the crystallization of political attitudes and evaluations and in translating them into behavioral outputs; and comparative politics, which provides the statistical tools, research designs and body of literature to build on.
My initial approach was in line with the literature on “information effects”: we compute an estimate for the effect of a systematic increase in political knowledge in a given sample on an aggregate-level outcome of interest (Bartels, 1996), be it vote choice, turnout, the distribution of various political attitudes or behaviors. We repeat the procedure on a large number of subsamples drawn at random from the initial sample and we aggregate the results with Rubin's (1987) rules. Other times, we compared the performance of socio-economic variables in predicting political knowledge to their effect on opinionation, and drew conclusions by paralleling the conclusions from one model to the conclusions we drew from the results of the other model. However, we found weak to very weak effects of knowledge on variables that were theoretically expected to be causally linked to it; this comes as no surprise given the overwhelming amount of existing literature that failed regrettably at finding substantively significant information effects.

This, however, may come as a major surprise to elitists, to normative theorists of democracy and to common sense: how come that knowing more about the subject does not help one make better decisions? One way out of the conundrum is to conclude that no decision is better than the other, thus citizen input in general and the vote in particular are meaningless products of a system desperate to legitimize its existence. If this is true, democracy is a scam. Another solution is to conclude that, due to the cognitive limitations of humans (confirmation bias, representation bias, availability bias, etc.) we are simply not capable of distinguishing between otherwise distinct political alternatives. If this is true, democracy is too good a system for us; we do not live up to its standards. There is, however, a less cynical, more anthropologically optimistic solution to our problem: we fail at measuring the impact of political knowledge on our variables of interest.

I took this latter stance, and focused most of my research efforts on evaluating the extant conceptualizations, operationalizations and measurements of political knowledge in pursuit of more reliable and valid political knowledge scales. If we measure something with a significant amount of random error (low reliability), its estimated effect is likely to be weak enough to obscure empirically true relationships. Low validity, on the other hand, can cripple any research to the point where no empirically true conclusions can be drawn from the statistical results. To the extent that measurement error underlies the weak effects found in the literature on information effects and in the early stages of my research, the assertions made by the normative theories of democracy may, in fact, be accurate. Political knowledge may enhance democratic representation, responsiveness and accountability; we just fail on methodological grounds to find the true size of such effects. I started out by trying to
contribute to the advancement of a line of research, but ended up working on an attempt to save it.

b. Key Methodologies and Data

As already implied in the previous section, I made use of state-of-the-art statistical tools to test all my hypotheses. The scope of all my theoretical expectations is rather broad; I aimed at drawing conclusions about individuals in general, rather than citizens of specific countries. It is improbable from the outset that a given increase in political knowledge would have the same impact regardless of the institutional context in which the citizen dwells. In order to grasp such variations and to model them accordingly, I made use of the training on hierarchical linear (multilevel) modeling provided through the ELECDEM network. By using this method I was able to obtain unbiased estimates for individual level predictors and avoid violating the assumption of independence of errors, but it also allowed me to estimate the effects of macro level, institutional variables (the ideological bias of the media environment, for instance) on individual level outcomes (self-positioning on the left-right ideological dimension).

In order to assess and compare the quality of various operationalizations and measurements of political knowledge, I used and collected survey experimental data, aided by the training in experimental methods also provided within the network. The survey experiment helped me isolate the effects of the measurement tool from the more substantive factors influencing data quality, thus allowing me to compare the reliability of measurements done with different survey tools. The experimental part of the survey consists of randomly assigning political knowledge questions of three different formats: multiple choice questions, where respondents are invited to find the correct answer out of four possible ones; true/false questions, where the respondents would evaluate if a statement is true or false; and open-ended ones, where the respondents type in their answers if they can retrieve it from memory. On this occasion, together with Andre Krouwel of Kieskompas, we also managed to collect data on cognitive ability, which was not publicly available before in any large N samples. More details on the design are found in the following sections of the report.

All cross-national analyses were done on secondary data: the second module of the Comparative Study of Electoral Systems (CSES, Module 2) and the 2009 wave of the European Election Study in 2009 (EES 2009) for individual-level data, and the PIREDEU
2009 Euromanifestos for country level data. The choice of data was made on two crucial criteria: the availability of practical political knowledge questions and other relevant variables from a large enough sample of polities, and a clear documentation of the wording and coding of the data.

The Comparative Study of Electoral Systems was particularly useful for analyzing the effect of question format on the quality of political knowledge measurements, because it provides cross-national variation at the level of the format of the questions the respondents were asked. Having variation at the level of format allowed us to make meaningful comparisons between the quality of measurement obtained with various formats; in that regard the CSES is the only existing database that allows for such analyses on a cross-national basis. The European Election Study has a standardized battery of knowledge question, rendering it unsuitable for studying format-effects. However, I was able to test my hypotheses regarding the heterogeneity of information effects on ideology as a function of the dominant ideology at the national level by combining the individual-level EES data with the PIREDEU party-level data.
D. Full Report


Introduction

Finding a link between political information and ideology has been a great challenge to voting behavior scholars ever since the proper statistical tools were developed. While the early voting behavior researchers (Berelson et al., 1954; Campbell et al., 1960; Converse, 1964) did not focus specifically on how political information may influence attitudes and participation, they did imply that the low levels of sophistication that they found had catastrophic potentials.

These assertions were most likely influenced by the early normative theories of democracy (Mill, 1892; Lipmann, 1920) which linked the success of representative democracies to the vivid interest and keen engagement of citizens in political affairs. However straightforward it may be that those who know the rules of the game play differently from those who don’t, there are certain theories in the cognitive psychology literature that may lead us to different conclusions (Kahneman et al., 1982; Popkin, 1993; Lupia, 2006).

While the efficient use of cognitive heuristics may significantly curb information effects on voting behavior, some authors claim that it is the political attitudes and the citizens’ issue positions that are more likely to be affected by political knowledge (Bartels, 1996; Althaus, 1998; Berinsky, 1999). This happens due to the fact that the availability of heuristics makes one’s articulation of their own political opinions rather redundant for the purposes of elections – citizens are asked to turn out to vote for candidates or parties and not for actual policies (even when it comes to referenda, political elites make sure that the citizens can easily identify the supporters of the two sides of the issue at hand). Increasing the citizens’ levels of political awareness, thus, will most likely have a much bigger effect on their political attitudes than on their voting preferences (Fishkin, 1997; Althaus, 1998).

Another problematic issue is how to measure attitudes and political information. One of the most fundamental concerns regarding attitude research is the seemingly volatile,
unstable character of the attitudes displayed by respondents in surveys. This does not happen only due to the weak structure of the belief systems of citizens (Converse, 1964) but also due to the fact that most respondents do not usually spend much time thinking about politics in their everyday life and give answers to survey items off the top of their head (Fishkin & Luskin, 1999). Furthermore, the attitudes expressed by respondents tend to be rather sensitive to details related to survey design and questionnaire building (Piazza, Sniderman & Tetlock, 1989); there can be substantively and statistically significant differences between the information contained in two variables that differ only in the wording of the questionnaire items they are based on. In order to deal effectively with this issue, half of the items used for the measurement of political attitudes in this section encouraged non-responses (DKs) and the other half discouraged DKs.

There are also debates on what and how to measure political knowledge. Out of the multiple operationalizations and conceptualizations of political knowledge, I chose to focus on what is often referred to as “practical political knowledge” (Somin, 2006; Popescu & Toka, 2007). As opposed to the ideological knowledge (Somin, 2006) this concept refers to the respondents’ awareness of whom their representatives are and what are their legal prerogatives. By using this subcategory of political information as the main independent variable I eliminated the risk of endogeneity and autocorrelation that can arise when having a dependent variable related to political ideologies and an explanatory variable that touches upon ideological matters.

Finally, in order to cope with the differences that can stem from the different types of questionnaire items, I used knowledge variables based on 6 types of knowledge questions: open ended, multiple choice and true-false questions, with “don’t know” answers alternatively encouraged and discouraged. In line with already existent literature on political information and survey research, these small variations in how the items are designed have systematic effects on the rate of correct responses and on the number of DKs (Miller & Orr, 2008).

As shown empirically in this section, high levels of political knowledge tend to increase the response rates to questions related to political attitudes and, at the same time it is a significant correlate of right-wing political leanings on economic issues. These relations remain significant even when education, political interest and media consumption are included in the models; showing that political information itself has an effect on ideology and non-response that is worth paying close attention to. The conclusion that can be drawn from the results is that left-wing economic attitudes tend to be underrepresented in surveys due to
the increased relative likelihood of left-wing supporters to have low levels of political information.

What we know about political information and attitudinal variables

The field of research concerning political knowledge and public opinion emerged together with the development of the statistical tools necessary for survey research. One of the main purposes of surveys in the early history of survey research was to provide reliable sketches of the needs, preferences and grievances of citizens, thus enabling political representatives to design policies that match the preferences of the populace better (Gallup & Rae, 1940).

However, the underlying assumptions of these expectations are that people have real and fairly stable attitudes towards politics, that they are able to articulate their opinions and, ultimately, that they have a minimal level of political literacy (Fishkin, 1997). What researchers have shown repeatedly over the last 60 years is that most people are severely uninformed (Campbell et al, 1960; Carpini & Keeter, 1996; Andersen et al, 2005) and their attitudes are highly volatile and unconstrained (Converse, 1964; Converse, 1975; Piazza et al, 1989). This being the situation, Gallup’s and Rae’s optimism is no longer justified.

That surveys are not particularly representative also due to the inability and/or unwillingness of less knowledgeable citizens to provide information about their political opinions is not new (Turgeon, 2009). However, there are certain issues related to the political inequality fostered by traditional surveys that were not sufficiently approached yet. Which opinions are represented in surveys? Which groups are underrepresented; which groups are overrepresented; how representative of the target population are surveys on political attitudes? Apart from the distortions laid forward by survey researchers (self-selection bias, impossibility of contact, low response rates, etc.), how different are the respondents of political attitude items from non-respondents? These are the main questions that this section is trying to address.

As shown by Adam Berinsky in an article published in 2002 in the American Journal of Political Science, those people who are the expected beneficiaries of welfare policies tend to abstain from stating their opinion on welfare issues in surveys. This happens because they are often unable to voice their political interests due to their lower levels of education, political knowledge and material welfare (Berinsky, 2002). Socio-economic variables, as
demonstrated by the author, are good predictors of missingness in variables related to the attitudes towards welfare policies; at the same time, poverty is the raison d’être of such policies.

While he does mention political information in his article, Berinsky understates the importance of political sophistication in this distortion of representation; my main claim is that socio-economic variables are merely distal variables to the missingness witnessed by the author. Adam Berinsky presented the lack of political information as being incidental to the greater phenomenon of political inequality; what I argue in this section is that there are significant information effects on the ideological leanings of citizens; the issue regarding people’s attitudes towards welfare policies is only a particular case of a much more general pattern.

The link between economic wealth and the ability and willingness to articulate political grievances is not straightforward at all, but there are no logical difficulties in seeing the link between political literacy and the articulation of political attitudes; we have no reason to believe that political information is different from any other skill until someone demonstrates the contrary – people who read a certain book are more likely to be able to articulate an opinion about it; people who know more about politics are more likely to articulate a political attitude.

There is one more issue that needs to be settled at this point: what counts as political information and what does not? How should we measure political information? There are multiple answers to this question. The pioneers of political knowledge research (Berelson, Lazarsfeld, McPhee, Gaudet, Campbell, Miller, Stokes and, to a lesser extent, Converse) refered to “political knowledge” as being the citizen’s ability to name their representatives, to understand their issue positions and to relate their own positions to that of the parties or candidates (Campbell et al, 1960). To this “practical political information”, an ideological dimension was added later on to the measurements of political knowledge (Converse, 1964; Converse, 1975). While the debate at that time focused mostly on issues of conceptualization, much newer concerns in the field are directed to problems of operationalization, measurement and even to its utility in the study of public opinion and elections (Lupia, 2006).

There are at least two major arguments that were formulated against the agenda of this field of research. The theory of cognitive heuristics (Popkin, 1993) claims that citizens do not base their political decisions on information as much as they employ mental shortcuts, rules of thumb that help them make fairly good evaluations of things political without the need of costly and time consuming processes of gathering information. People can successfully
compensate their lack of political knowledge with such heuristics, making it useless to look at their level of political sophistication for the purposes of empirical research in political behavior. The second argument that needs to be mentioned is that regarding the complexity of the issue at hand. Most people have substantive knowledge of things political related to their field of expertise, often without even knowing that the issues they are aware of are in fact political (Dalton, 2002). For instance, most school teachers know who the candidates for the school board are and most farmers know about the available subsidies for agriculture much better than the average political scientist does. However, all the research that was published so far in political science journals failed to measure or even to acknowledge these aspects of political awareness (for a notable exception see Smith, 1989; and Iyengar, 1990). It can be argued, though, that the only source of disagreement on this matter lies in the definition of the term “political”; which takes us back to paradigmatic differences that will most likely never be settled.

Throughout this paper, the economic ideological leanings will be understood in Berinsky’s terms: preference for communitarian (redistributive) vs. individualistic (market) economic policies. For measurement purposes, I used variables related to attitudes towards private property, free market competition, state intervention in the economy and redistribution of wealth. The average of the scores on these variables was used as a measure of economic ideological leanings.

Data and method

The data used for the empirical testing of my hypothesis is drawn from a phone survey experiment conducted in September 2009 on a random sample of Hungarians living in Hungary. The experimental part of the survey consisted in randomly assigning respondents into groups differing only in the type of question that they are asked. The political information items are divided into two batteries. The first battery of items consists of 6 questions, each of them having three different versions: an open-ended version, a multiple choice one and a true-false one. The last battery includes 6 variables with two versions each: one version in which the DK is encouraged and one in which it is discouraged. For each item in the first set, there was roughly one third of the sampled population that responded to each of the three types of questions. The versions (a, b and c) were rotated after each response, so that there was no single respondent who received only questions of type a or of type b. Similarly, the second set of questions had versions “a” and “b”, with roughly 50% of
the sampled population answering to one version of a certain question and roughly 50% answering to the other version. The set of 12 items on political attitudes also contains two versions for each question. For each item formulated in a positive manner there is another with the same meaning but formulated in a negative manner (ex: “The state should…” and “The state shouldn’t…”). The assignment to one group or another was done at random, in the same manner that was mentioned in the previous paragraphs.1

The dataset also contains information on the respondents’ level of education, their media consumption (TV news, newspapers and internet), their sex, age, income and interest in politics; all these variables were used as controls in the models. With a sample size of 1705 and an item non-response (NA, excluding DK) rate averaging below 1% on the variables included, all the parameter estimates are likely to be statistically unbiased and representative of the target population.

As mentioned above, there are issues related to measurement that fuel the lively debate on the matter of political information. In order to demonstrate the differences between the results yielded by different types of questionnaire items (multiple-choice, open ended, true-false), I calculated the expected results of the multiple choice and true-false questions based on the distribution of correct, incorrect and “don’t know” answers in the open-ended questions.

The expected results, thus, are calculated based on two assumptions that need to be rejected in order to demonstrate that the differences between the three types of items are indeed related to something else than random guessing. The first assumption is that people who don’t know the correct answer to multiple choice or true-false questions can and will try to give a random answer, thus increasing the number of false positives (correct answers by chance). The second assumption is that the open-ended questions are the ones that give us the correct percentage of knowledgeable respondents.

1 The randomization was made without any randomizing tool; however ad hoc it may be, no variable in the database correlates significantly with the patterns of assignment to the treatment groups.
Since the calculations are based on random samples of equal sizes and the only difference between the treatments assigned to the three samples is the type of question they received, the results shown in Table 1 below show that random guessing cannot be the only factor behind the differences in outcomes. Different types of questions set different standards for what is expected of the respondent; in order to give a correct answer to an open ended question one often needs to have a little more than a hunch about the topic of the question, whereas the true-false and the multiple choice questions rarely require more than a vague awareness of the issue. On the other hand, in multiple choice and true-false questions one can include information that may mislead the respondents, thus countering the effect mentioned previously.

The apparently surprising results shown in the second half of the table below point in this direction; people may have heard, for instance, that a great percentage of the EU budget

<table>
<thead>
<tr>
<th>Topic</th>
<th>Type</th>
<th>N</th>
<th>Correct (%)</th>
<th>Don’t know (%)</th>
<th>Expected correct (%)</th>
<th>Difference (obs.-exp.)</th>
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<td>21.7</td>
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<td>73.4</td>
<td>-</td>
<td>-</td>
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<td>16.8</td>
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<td>34.9</td>
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<td>22.7</td>
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The expected percentages were calculated using the following formula:

\[
\text{Expected} = \frac{\% \text{ Incorrect in open ended} + \% \text{ DK in open ended} - \% \text{ DK in multiple choice or true-false}}{\text{number of available options} + \% \text{ correct in open ended}}
\]

The table above shows that people may have heard, for instance, that a great percentage of the EU budget...
is directed to agriculture and give a correct answer to an open-ended question on this topic. However, when having to decide whether the EU spends more on agriculture than industry (as it happened in the penultimate multiple-choice question), the respondent may feel that “agriculture” is unlikely to be the correct answer. While agriculture seems to be a rather cheap activity, industry is generally thought of as being very costly, and, by applying faulty heuristics, respondents may conclude that agriculture cannot be the main consumer of European funds.

Based on these observations, one may expect to find that multiple choice and true-false questions perform worse in terms of validity and reliability than open-ended ones. I constructed separate scales counting the number of correct answers to the three types of questions and checked how they correlate with traditional predictors of political knowledge: interest in politics and education. Surprisingly enough, the three types of questions perform almost equally well. In fact, the scale based on the multiple choice questions correlates with both education and interest slightly better than the other two ($r=.238$ with education and $r=.138$ with interest, whereas the other two scales correlate at .122 and .127 with interest and at .160, respectively. $151$ with education). Format-effects are unlikely to contaminate our results with bias or excessive random error, yet further tests are needed.

In order to predict the ideological leanings of the respondents and the number of DK answers to attitude questions I use regression models (Ordinary Least Squares for ideology and Poisson regression for DKs). The same explanatory variables are used in all models: political information (the total number of correct answers to the 12 political information items), education (highest education cycle graduated by the respondent measured on a 4-point scale), interest in politics, frequency of watching news on TV and newspaper consumption (recoded into 0-1 continuous variables), income, religiosity, sex, age, talkativeness (how often the respondents talk to family and friends).

In addition, I controlled for the total amount of guessing enabled by the specific combination of knowledge questions that the respondent was asked. This variable was constructed based on the expected results presented in Table1: I summed up the expected inflation of each multiple-choice and true-false scale that was used for the knowledge measurement of the respondents. For instance, if a certain respondent was assigned the multiple-choice version of the first question, the true-false version of the second and the open-ended version of the third, she would accumulate 46.23-29.4=16.83 on the guessing variable for the first question, 45.9-22.9=23 for the second one and 0 for the third one, which adds up to a total score of 39.83. The score, thus, corresponds to the expected
amount of guessing that accounts for the inflated scores found on the multiple-choice and true-false questions.

The control for talkativeness was included due to the increased likelihood of talkative people to express opinions regardless of their actual level of political sophistication (Fishkin & Luskin, 1999); it is expected, thus, that the “talkative” variable will significantly improve the model fit in predicting the number of DK answers. Furthermore, as a personality trait, talkativeness may also have less straightforward effects on our variables of interest through complex paths that are beyond the reach and scope of this article.

In addition, I tested additional models using the same independent variables and the number of middle observations (undecided answers) on the 5-point scales of the attitude variables as dependent. The purpose of these last models was to point out that the DK non-responses are more than mere undecided answers, they are “non-opinions” rather than tokens of moderation (Turgeon, 2009). The results successfully demonstrate this claim.

Finally, in order to assess the importance of the question type in influencing and shaping the results of models employing political information as an explanatory or response variable, I added two more OLS models that predict political knowledge. While the first model only looks at the main effects of the variables presented previously, the second model estimates the joint effects of guessing with gender, education and political interest for purposes of comparison. I chose to include these particular interactions due to the fact that gender, political interest and education tend to be significant predictors of political knowledge; thus, when using multiple choice or true-false questions it is plausible that those groups that are generally found to be less politically knowledgeable (namely the uneducated, the less politically interested and women) will be disproportionately advantaged by the opportunity to guess the correct answers. Given the coding of these three variables (higher values on the interest variable correspond to high levels of political interest; highly educated people have high values on the education variable; the gender variable has value 0 for women and 1 for men), the estimates for these interaction terms are expected to have a negative sign.

Note that I used two models for predicting each of the dependent variables described above. The first model of each of these pairs does not contain “guessing” as a control variable, whereas the second ones do. The reason for doing this is to grasp the effects that the type of question has on the results we get from models that have political knowledge as an independent variable. An alternative to this method would have been to control for the number of true-false and multiple choice questions that were used to construct the knowledge score for each of the respondents. Such a strategy may seem rather appealing, especially due
to the underlying normalcy of the distributions of count variables based on random assignment; however, it is rather apparent in Table 1 that such variables would be rather poor estimators of the guess-related inflation of the knowledge variable.

Finally, the models used are described below:

Model 1:
Knowledge = \beta_0 + \beta_1 \times \text{interest} + \beta_2 \times \text{education} + \beta_3 \times \text{TV} + \beta_4 \times \text{newspaper} + \beta_5 \times \text{religiosity} + \beta_6 \times \text{family income} + \beta_7 \times \text{age} + \beta_8 \times \text{gender} + \beta_9 \times \text{talkativeness} + e

Model 2:
Knowledge = \beta_0 + \beta_1 \times \text{interest} + \beta_2 \times \text{education} + \beta_3 \times \text{TV} + \beta_4 \times \text{newspaper} + \beta_5 \times \text{religiosity} + \beta_6 \times \text{family income} + \beta_7 \times \text{age} + \beta_8 \times \text{gender} + \beta_9 \times \text{talkativeness} + \beta_{10} \times \text{guessing} + \beta_{11} \times \text{guessing*education} + \beta_{12} \times \text{guessing*interest} + \beta_{13} \times \text{guessing*gender} + e

Model 3:
\log(\text{E (Attitude DK)}) = \beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{interest} + \beta_3 \times \text{education} + \beta_4 \times \text{TV} + \beta_5 \times \text{newspaper} + \beta_6 \times \text{religiosity} + \beta_7 \times \text{family income} + \beta_8 \times \text{age} + \beta_9 \times \text{gender} + \beta_{10} \times \text{talkativeness} + e

Model 4:
\log(\text{E (Attitude DK)}) = \beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{interest} + \beta_3 \times \text{education} + \beta_4 \times \text{TV} + \beta_5 \times \text{newspaper} + \beta_6 \times \text{religiosity} + \beta_7 \times \text{family income} + \beta_8 \times \text{age} + \beta_9 \times \text{gender} + \beta_{10} \times \text{talkativeness} + \beta_{11} \times \text{guessing} + e

Model 5:
Ec.Conservatism = \beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{interest} + \beta_3 \times \text{education} + \beta_4 \times \text{TV} + \beta_5 \times \text{newspaper} + \beta_6 \times \text{religiosity} + \beta_7 \times \text{family income} + \beta_8 \times \text{age} + \beta_9 \times \text{gender} + \beta_{10} \times \text{talkativeness} + e

Model 6:
Ec.Conservatism = \beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{interest} + \beta_3 \times \text{education} + \beta_4 \times \text{TV} + \beta_5 \times \text{newspaper} + \beta_6 \times \text{religiosity} + \beta_7 \times \text{family income} + \beta_8 \times \text{age} + \beta_9 \times \text{gender} + \beta_{10} \times \text{talkativeness} + \beta_{11} \times \text{guessing} + e
Model 7:
\[
\log (E(\text{Undecided})) = \beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{interest} + \beta_3 \times \text{education} + \beta_4 \times \text{TV} + \beta_5 \times \text{newspaper} + \beta_6 \times \text{religiosity} + \beta_7 \times \text{family income} + \beta_8 \times \text{age} + \beta_9 \times \text{gender} + \beta_{10} \times \text{talkativeness} + e
\]

Model 8:
\[
\log (E(\text{Undecided})) = \beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{interest} + \beta_3 \times \text{education} + \beta_4 \times \text{TV} + \beta_5 \times \text{newspaper} + \beta_6 \times \text{religiosity} + \beta_7 \times \text{family income} + \beta_8 \times \text{age} + \beta_9 \times \text{gender} + \beta_{10} \times \text{talkativeness} + \beta_{11} \times \text{guessing} + e
\]

**Results and discussion**

The estimates for all the models can be seen in Table 2 below. In spite of the weak model fit, some of the results are very significant and point mostly in the expected direction. Notice that all the variables that were found significant in model 2 have different signs in model 1 – with the exception of political interest which is highly significant and points in the same direction both in predicting non-opinions and right-wing economic leanings – which shows that the variables that predict conservatism also predict low item non-response rates in political attitude variables. Education and political knowledge are significant predictors of both ideological leaning and number of non-opinions. While the more educated and more politically informed are more likely to be economically right-leaning and to express valid opinions, people who watch the news on TV, women tend to be both more left-leaning and more able or willing to express political attitudes. Religiosity was found to be a deterrent of right-wing economic leanings but it is not a significant predictor of non-opinions, even though the estimate for this variable points in the expected direction (opposite to the one found in the model predicting economic conservatism).
### Table 2: Estimates for models 1-8. Determinants of knowledge, ideology, DK and middle answers

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While we did not manage to predict the ideological leanings sufficiently well (the model fit for these models is rather low), the prediction of missingness in attitude variables was much more successful. The non-opinionated tend to read the newspapers less, to watch the news on TV less often and they are generally older and, all other things being equal, they tend to be wealthier than those who were able to express valid political attitudes. These variables were not significant in predicting the ideological leanings of the respondents, which does not allow us to contrast the two sets of models on the basis of these variables. Finally, contrary to the expectations, talkativeness did not reach an acceptable level of statistical significance in predicting “don’t know” and it does not appear to be a significant predictor of left-wing ideological leanings. All in all, we can see that, at least on those variables that successfully predicted ideology, the non-opinionated are rather similar to the economic left-wingers and we have reasons to expect that, were we to induce systematic variation on these variables, it would have an effect both on the citizens’ ideology and on their willingness or ability to express valid political attitudes.

“Don’t know” does not mean “undecided”, that is the conclusion that can be drawn from the comparison between the estimates for models 3, 4, 7 and 8. Out of the five variables that reached an acceptable level of statistical significance in the last two models, only one points in the same direction both in predicting DKs and middle values: respondents from wealthier families, ceteris paribus, have a higher propensity of being undecided or moderate (i.e. to choose the middle value on the attitude scale) but are also more likely to express non-opinions when asked about their political attitudes. The undecided respondents are younger, they tend to be more politically knowledgeable, to read the newspaper more often and to be less religious than the rest of the respondents; unlike the non-opinionated who display the exact opposite traits in these respects (with the exception of religiosity which only reached an acceptable level of statistical significance in predicting middle values on attitude variables).

Furthermore, less educated people, women and respondents who are not very interested in politics and don’t watch the news on TV were also found to be less likely to express non-attitudes; these variables were not found to be significant predictors of undecided answers. These results do not show us that undecided respondents are the exact opposite of those who express non-opinions; this was not the purpose of the analysis after all. However, we can claim now with a reasonable level of confidence, that the non-opinionated are not merely undecided and, conversely, the undecided are not non-opinionated but rather moderate.
It would be a logical fallacy to conclude, based on the results, that people who do not provide their position on political matters are predominantly left-wingers. After all, the models do not explain the bulk of the variation of the dependent variables; furthermore, we cannot tell what would happen to the error terms if we were to combine the model explaining the DKs and the one explaining ideological leanings. However, the results show that the abstainers are indeed more likely to be leaning left than right; and this claim is also hinted at in previous research papers (Berinsky, 2002) and in line with the theoretical expectations described throughout this section.

A credible diagnosis for our findings require that we rule out the possibility of format-effect contamination, as previously mentioned. In order to understand the effect of guessing on the performance of political knowledge in our models, one needs to pay close attention to the differences between the results for model 1 and 2 but also to look at how the estimates change in the other models once the guessing variable is introduced. The first things that come to our attention when contrasting model 1 to model 2 is that guessing mediates the effect of political interest on knowledge, it doubles the size of the main effect of political interest and significantly changes the size of the intercept. While the latter is less than surprising given the findings presented in Table1, the effect on political interest is worth paying close attention to. The negative sign of the interaction term between interest and “guessing” shows that the possibility of guessing in knowledge questions tends to inflate the scores on the knowledge scale by allowing uninterested respondents to give correct answers in spite of their generally lower levels of political knowledge. By separating this joint effect from the direct effect of political interest, the main effect of interest tends to increase in size while keeping its high level of significance (p = 3.2*10^{-15} for the second model and p = 1.6*10^{-15}).

It is reassuring to note the virtual absence of any effect of guessing on the slopes of political information and political interest in models 3 through 8. It can be argued that controlling for the possibility of guessing systematically decreases the slopes of political knowledge and interest – which may be rather counterintuitive – but the differences only appear at the 4th or even 5th digit, thus making these effects statistically uninteresting. The possibility of guessing, thus, inflates the scores on the political knowledge scale without biasing the results of models that employ political knowledge as an explanatory variable. At best controlling for the possibility of guessing may increase type 2 errors in models where the effects of political information are particularly low and dangerously close to zero.
One can go even further in interpreting these results: whether we measure political information using true-false, multiple choice or open-ended questions, the resulting scale seems equally reliable, and statistical results remain equally robust. However, given the apparent inflation of scores when using closed-ended (multiple-choice or true-false questions, researchers using cross-country analyses that focus on information effects need to account for the type of questions in their models. For instance, the Comparative Study of Electoral Systems contains knowledge variables based solely on true-false questions in countries such as France, Great Britain or Sweden and only open-ended questions in countries such as Korea, the Netherlands and The United States. While it is very likely that the type of questions will not have an influence on the slopes of political information in single-country studies, researchers who want to look at information effects cross-nationally should differentiate between the different types of questions.

Conclusions

The aim of this section was to demonstrate that the same variables that predict left-wing political leanings are also good predictors of non-opinions in political attitude variables. Political knowledge and education are the logical predictors of the absence of political attitudes; the link between political knowledge and ideological leanings is, though, much more debatable. Most of the studies that focused on explaining the link between political information and ideology have concluded, though, that high levels of political sophistication are associated with moderate right-wing ideological leanings. The matter is still far from being settled, because we know about political literacy that it tends to correlate with socio-economic variables and it is plausible that all the relationships found between political knowledge and attitudes or vote choice only reached statistical significance due to improper control for socio-economic or demographic factors. Precisely because of this situation, the results presented in this section should not be left completely unquestioned.

There are two main findings in this study that are worth mentioning. The first finding is that education, political knowledge, interest and gender predict the missingness in political attitude variables and the ideological leanings of respondents. Very much in tune with the theoretical expectations, these variables (with the sole exception of political interest), which were found to be highly significant in spite of their high potential of collinearity, predict left-wing political attitudes and the high propensity to reply “I don’t know” to attitude questions.
This leads us to conclude that it is very likely that opinion surveys, which had the initial purpose of improving political representation and enhancing political equality by providing additional channels of expressing the political needs of citizens, tend to overrepresent conservative attitudes and to underrepresent liberal or socialist ones. This distortion happens without the deliberate, ill-intentioned behavior of pollsters but rather through the inability of those who are less endowed with political resources (material wealth, education, political information, etc.) to articulate their political attitudes.

Finally, this article shows that DK answers are notably different from undecided answers. The number of DK answers and that of answers in the middle of the 5-point scales are predicted indeed by more or less the same variables, but the direction of the coefficients is opposite in 4 out of 5 cases. DK is a non-opinion; the middle of the scale is most likely a valid attitude.

This study calls for an increased attention to the interpretation of non-responses in attitude polls, especially when they have the potential to be used for designing public policies. Moreover, it calls into question the representativeness of survey data and provides further evidence of the right-wing bias of survey results. As mentioned previously, these distortions are not intentional, and that is just another reason to pay close attention to these issues.
D2. Where Have All the Socialists Gone? Unintentional Biases in Opinion Surveys

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Juraj Medzihorsky, Central European University

Introduction

Democracy is generally claimed to be a form of government that has the greatest potential of satisfying the needs of most if not all the population of a given polity. Representative democracy, as implied by the name itself, bases its normative claim to legitimacy on the accurate and unbiased political representation of the interests of all the citizens. Finding the apparent and hidden sources of distortions to political representation brings us one step closer to solving the problem of political inequality (the concept here refers to any individual-level inequality that determines disproportionate influence on political decisions).

The aim of my research is to find, quantify and describe the informational component of these distortions. This paper will investigate the relationship between specific institutional contexts and the existence, size and direction of information effects in the member countries of the European Union.

Dwelling mostly on the literature linking political information to political behavior and to economic attitudes (see especially Berinsky, 2002; Althaus, 2003; and Bartels, 2005); this paper argues that information gains tend to favor pro-market attitudes to various degrees on a cross-national basis. These cross-country variations are not entirely stochastic; they often follow theoretically predictable patterns outlined in the extant literature that is summarized in the following sections (we will refer to them as the sociological explanation). Alternatively, the sizes and directions of such information effects can be mediated by the existence, the direction and strength of pro-market or anti-market biases in the information environment of people (we call this perspective the propaganda explanation).

After a brief survey of some of the more important theories and findings in the field of information effects, we make a more detailed description of the proposed causal link between information environments and information effects on attitudes towards economic
redistribution. The theory is then tested using Markov-Chain Monte Carlo simulations of the effects of information gains for each country in the EES 2009 survey; the results thus found serve as input data (as dependent variable) for a country-level regression model that aims to estimate the effect of the informational-environmental bias on such information effects.

Finally, the results of the empirical tests are discussed in more detail before concluding with the final remarks and guidelines for future research.

Size and direction of information effects: the sociological and the propaganda explanations

Empirical studies on the matters of political information, vote choice and political attitudes have been carried out before (Bartels, 1996; Carpini & Keeter, 1996; Althaus, 1998; Gilens, 2001; Sturgis, 2003; Toka, 2002; Andersen et al, 2005; Sturgis and Smith, 2010), but the cross-national study of the links between these issues is still in an emerging state. While most of these studies focused on one single polity and had empirical findings that may reflect the idiosyncrasies of specific countries; the primary objective of this article is to provide empirical support for a much more general and generalizable theory of political sophistication and political attitudes.

Previous studies have found that political information tends to correlate with rightwing attitudes towards economic issues and with liberal attitudes towards social ones (Johnston et al., 1996; Berinsky and Cutler, 1998; Berinsky, 2002, Fournier, 2002; Althaus, 2003). Furthermore, more informed people, as mentioned before, had a higher propensity of voting for right-wing parties in most countries where this relationship was studied (Oscarsson, 2007; Hansen, 2009; Crampton, 2009), suggesting that the trend may not reflect country-specific peculiarities but rather a more universal pattern. However, such studies were only performed in a few countries and require further replication.

We can thus hypothesize that political information will correlate positively with pro-market attitudes and with liberalism on social issues (support for alternative lifestyles, abortion, euthanasia, etc.). While the latter may be very context specific and the cross-national data on such matters is rather scarce, the former are issues that are relevant in most if not all political contexts one can conceive of. Even though the economic axis (market-redistribution) may not be central to the political debate and to electoral competition in some countries (Benoit and Laver, 2005), citizens are affected by economic policies regardless of
the saliency of the issue on the political scene. As a result, analyzing the impact of political information on the economic attitudes of citizens is likely to be relevant across national political contexts.

Why would political information correlate with right-wing attitudes towards the economy? There is no consensus in the scholarly literature on this matter, regardless of the numerous studies that consistently revealed the same directionality of the relationship between information and economic attitudes (Luskin, 2003). One of the plausible causes of this situation is related to the predominant socio-economic profile of the natural supporters of economic redistribution. The natural supporters of economic redistribution are the economically worse off, who often happen to be less educated and, thus, to have less means or fewer channels of access to the mainstream political discourse. This being the situation, the supply of political information will be skewed in favor of the supporters of right-wing policies, who are generally economically better off and, more often than not, more educated than the likely supporters of economic redistribution.

Consequently, exposure to political information tends to be more strongly associated with the socialization of pro-market ideas than with pro-redistribution ones. Thus, the underlying preferences of the uninformed citizens that would benefit most from economic redistribution are more likely to remain dormant than the preferences of the uninformed who would benefit more from pro-market policies (Gilens, 2001; Lau and Redlawsk, 1997). However, fully clarifying this causal chain is an overly ambitious goal that is beyond the scope of this article; what is highly relevant for our research is to investigate the degree of generality of the aforementioned relationship (is the direction of the relationship consistent across countries?) and to estimate the size of the direct and indirect impact of information on the support for policies that are traditionally associated with the economic right.

Further discussion here is needed. In case there is a systematic right-wing bias of the information environment of citizens (as previously discussed), more exposure to political information would determine the (previously) uninformed to systematically endorse rightwing economic policies regardless of their underlying “true” preferences. I call this the propaganda explanation, because it refers to the unequal articulation and socialization of

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The term here refers to the preferences citizens would have if they were fully aware of all the possible implications of the political alternatives facing them. It is generally assumed that knowledgeable citizens approximate this ideal more closely than their uninformed peers (Lau and Redlawsk, 1997; Lau and Redlawsk, 2001)
political ideas due to the economic affluence, increased capabilities and privileged positions of certain groups (here, the supporters of pro-market policies).

Further, uninformed citizens tend to be less able or willing to provide information about their political attitudes when prompted by survey interviewers (Berinsky, 2002; Miller and Orr, 2008; Turgeon, 2009). If political ignorance predicts both support for economic redistribution and the probability of item non-response in attitude surveys, it is highly probable that surveys depict an image of collective preferences that is skewed in favor of the supporters of pro-market policies. I hypothesize, thus, that political information tends to increase the citizens’ propensities to give valid answers to questionnaire items related to political attitudes.

Finally, I predict that the predominance of low-information citizens tends to bias our survey-driven understanding of collective preferences in favor of pro-market views. This would imply an involuntary yet systematic and potentially consequential pro-market bias in our survey-driven understanding of public opinion. We call this the “missing socialist” theory (see previous section).

The dominant view in the literature on information effects is that political knowledge (which is assumed to be generally unbiased) tends to bring the political attitudes and decisions of citizens closer to their true preferences (Luskin, 1990; Fishkin and Luskin, 1999; Lau and Redlawsk, 2001). An increase in the level of political knowledge, according to this view, would not systematically favor one side or the other of the political spectrum; it would make the natural supporters of redistributive policies more likely to support redistribution and natural opponents of it more likely to oppose it. To the extent that the natural supporters of redistributive policies outnumber the natural opponents of it (at least among the politically uninformed), information gains should have an aggregate effect opposite to the one we would expect if the propaganda explanation holds true. We call this alternative (yet currently mainstream) theory the sociological explanation, because it refers to political ignorance as being part of the structural disadvantages that impede the ability of certain groups to articulate political views in line with their actual preferences. Based on this view, an increase

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3 The word here refers to the likely beneficiaries of such policies. Generally it is expected that people who are economically worse off would benefit more from redistributive policies; however, a lengthy discussion of this issue is beyond the scope of this proposal.
in the level of political knowledge would reveal the “true” preferences of the politically uninformed, thus decreasing (or even reversing) the aforementioned right-wing bias.

The two theories are not mutually exclusive. Even in predominantly pro-market societies, the natural supporters of the left are likely to develop pro-redistributive attitudes as a direct effect of a hypothetic information gain. However, the natural supporters of pro-market economic policies are the ones who are likely to benefit most from information gains in such countries precisely because of the interplay between the sociological and the propaganda effects.

While I do not necessarily endorse this view, the propaganda effects can be seen as mere distortions of the sociological effects (however, I only propose this highly empiricist perspective for ease of argumentation). This being the situation, the propaganda effect is less apparent whenever the sociological effect is in place; the two competing theories make identical predicaments for the natural supporters of the market – information gains will push them closer to the economic right – and opposite ones for the natural supporters of redistribution – the effects of information gains cancel each other out, making any increase of information virtually inconsequential. Not accounting for the sociological effect in our empirical assessment of the propaganda explanation will only lead to an increase in type II errors. However, the two effects cannot be easily separated empirically without constraining the macro-level equation of my model and limiting the number of degrees of freedom, thus rendering my tests powerless. With these tradeoffs in mind, I will proceed at focusing solely on the propaganda theory and leaving the sociological one for future reference.

Hypothesis: The size of information effects and the pro-market influence of information gains are dependent on the availability of redistributive ideas in the information environment. The better the leftist economic views are represented at the level of the mainstream political discourse, the lower the right-wing biases of information gains and the weaker the information effects.

The propaganda theory: macro level operationalizations

Are citizens more likely to hear one side of the political debate or the other? Are they equally exposed to redistributive arguments as they are to pro-market ones? Are certain views
more likely to be articulated than others? All these matters are likely to have an effect on how new information influences the partisanship and beliefs of citizens.

Information environment

There are three factors that I will use to account for the potential partisan character of the information environment of different countries. The distribution of the positions of political parties or candidates on certain policy dimensions related to welfare is indicative of the relative partisanship of the supply of political information in a certain polity. The farther away the average position of the politicians is from the average position of the informational underdogs (in this case, if the propaganda explanation holds, I am referring to the supporters of economic redistribution), the more likely it is that information increases would determine the uninformed to support right-wing economic policies.

In addition to this, the existence of a publicly funded television channel and the trade union density are also expected to reveal the likelihood of exposure to leftist economic views. Thus, the increase of political information in polities with high trade union densities in which there is a publicly funded television channel is less likely to lead to an increase in the support for pro-market policies than it is in countries with lower trade union densities that do not have publicly funded television channels. All these explanatory variables will be used in our final models, either as independent of each other or as part of an index of pro-market environmental bias.

Controls

Lau and his associates (2008a; 2008b) made, arguably, the most ambitious studies on the effects of institutional variables on what they call “correct voting”, which is conceptually very close to “information effect”. While “correct voting” refers to the degree of correspondence between the values and political preferences of citizens and those of the candidates they vote for, earlier operationalizations of the term (Lau and Redlawsk, 1997; Lau and Redlawsk, 2001) consider whether citizens vote for the party or candidate they would have if they were fully informed as a token of “correct voting”. This being the case, the hypotheses that apply successfully to the concept of “correct voting” can easily be transferred to our study of information effects with the sole assumption that the vote is just a mere reflection of the political attitudes that people hold.
As Lau and his associates suggested, volatility (in this case we refer to the aggregate rate of change in the public’s political attitudes across time) is likely to be positively related to our quantities of interest. After all, our theoretical model requires citizens to have political attitudes that respond to environmental or individual factors which are not always rigidly stable over time. In our final analyses, attitudinal volatility will be accounted for.

Data and method

In order to test our hypothesis, we used the 2009 wave of the European Election Studies (EES) for the individual level data and the PIREDEU 2009 Euromanifestos Data for the wealth of country level information that it provides. Our country level dataset includes all 27 EU countries as of 2009, each of them with individual level samples roughly equal to 1000. List-wise deletion of cases sensibly reduced our sample sizes to 700-800.

The method that we employ for testing the effect of political information on attitudes is OLS regression with survey data as input. Political knowledge, based on the count of correct answers to various political quiz questions (7 true-false items were used, with a reliability of Alpha = 0.73), is the main independent variable and the attitude towards redistribution constitutes the dependent variable. The latter is measured on a 5-point scale that ranges from 1 (fully disagree with the statement that the government should direct more resources to reducing inequalities in society) and 5 (fully disagree with the previous statement). Multiple demographic, attitudinal and behavioral variables were used as controls in the standard individual-level model that was eventually repeated for each country in the sample. Furthermore, all the possible two-way and three-way interactions with political knowledge were included in the model as well; thus rendering the parameter estimates for the main model hardly interpretable and generating multiple situations of collinearity. The final goal of these models is to compute aggregate marginal effects of information on the attitudes towards redistribution; thus, such assumption violations are unlikely to be consequential.

From a substantive point of view, it is apparent that information can influence political attitudes in a plurality of ways; the reliable assessment of the size and direction of information effects requires the specification of a large number of such plausible causal paths. The main hypothesis of this article is on the macro level; hence, it will require more elaborate testing. The method used to this end (the “Bartelsian” simulation model) was developed by Bartels (1996) and is now widely used for estimating the impact of political
information on aggregate outcomes. After regressing the attitude towards redistribution on political information, demographics, and all the possible two-way and three-way interactions, we estimate the “first difference” \((\exp(Y)|\text{knowledge} = \text{knowledge} + 1) - (\exp(Y)|\text{knowledge} = \text{knowledge})\). This approach results in having many regression coefficients that are sometimes hardly interpretable and a first difference the dispersion of which is not directly observable. However, the large number of parameters will much more accurately capture all the conceivable ways in which political information may influence attitudes. Furthermore, Monte Carlo simulations allow us to create confidence intervals around our best estimate of the first differences, thus overcoming the aforementioned single-observation problem.

Hence, our next step is to simulate an increase in the observed scores of the respondents on the political information variable in order to estimate the impact of such an increase on the distribution of the attitudes towards redistribution. By simulating this aggregate estimate we obtain the margin of error for the impact of information on the distribution of economic attitudes, i.e. we can compute confidence intervals around the best estimate to estimate the size and significance of information effects. After repeating the procedure for all the countries in the sample, the information effects thus estimated are regressed on our macro-level variable of interest: the average position of the legislators on the issue of welfare in each country.

For taking into account the varied confidence intervals of these estimates across contexts, this analysis needs to be replicated for each bootstrapped estimate for the dependent variable, and the results for the bootstraps need to be aggregated using Rubin’s (1987) rules. Our hypothesis on the country level, thus, is tested using linear regression with countries as units of analysis.

Due to scarcity of data, we were only able to run preliminary tests of our hypothesis, leaving important parts of our theory untested at the moment. This paper should only be read as work in progress until further tests are run.

**Results, First Trials**

Our results show notable variance in the size of information effects across the 27 EU countries. Table 1 shows the distribution of the simulated information effects for each country in the sample. Information gains are generally associated with less support for redistribution;
however, there are large deviations from this pattern (see especially Portugal). The distributions of simulated information effects average sensibly above 0; what is even more disconcerting is that the direction of information effects is very inconsistent across simulations (Figure 1 below makes this graphically apparent).

Information effects do not seem significantly correlated with the pro-market biases of the information environments. Neither the expected values of the dependent variable, nor the slope of information or the first difference can be predicted with acceptable levels of certainty using our independent variable of interest. The results are summarized in Table 2 below. With the exception of the first model in the table, where the dependent variable is the expected value of the public’s attitude towards redistribution, the estimates for the slopes are smaller than the ones for standard errors. This makes it apparent that the lack of statistical significance is not an artefact of the small number of cases that we have in our country level sample.

**Table 1.**

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Figure 1.
Discussion and Conclusions

As stated previously, the information effects that we found are not sufficiently robust in most of the countries in our sample; and our proxy for the informational bias of the environment did not manage to account for the cross-country variations in information effects.

There are multiple explanations that are equally plausible for this situation. Unlike most of the authors that have used the Bartelsian models for estimating information effects (see especially Bartels, 1996; and Sturgis and Smith, 2003), we included several attitude variables in our level1 model specification and interacted them with political information. Some of these variables are (at least in theory) logically endogenous to our dependent variable.

Consequently, our estimates for the first differences do not only measure the impact of knowledge on the public’s attitude towards redistribution, but they also measure the effect of the former on the level of agreement between the attitudes of the public towards issues that are expected to be internally constrained (Converse, 1964). This being the case, our models give increased leverage to the sociological explanation previously described, thus reducing our ability to see the information effects that can be attributed to what we called the propaganda theory.

It is also possible that the positive results in the literature are only caused by certain peculiarities of the relatively small number of polities in which the respective studies were conducted. However, our study is not the first one to address the issue of information effects on a cross-country basis (Popescu et al, 2010; Lau et al, 2008a; Lau et al, 2008b), leading us to the conclusion that the it is our model that requires more elaborate testing.

Finally, the study presented here is, allegedly, underdeveloped. In order to reduce type 2 errors and get more reliable estimates of the effects of interest, the empirical tests need to account for all three explanations described in the text: the “sociological”, “propaganda”, and the “missing socialist” one. We are determined to incorporate these in more advanced versions of the current paper.
Summary of Results: Size of Information Effects by Ideological Leaning of the Environment

Introduction

Political information is generally measured by adding up the number of correct answers given by respondents to quiz-like questions related to political knowledge. The shortcomings of this approach to measuring awareness, ability or skill have been discussed in the psychometric literature for more than half a century, but only recently have they started to arouse the interest of political scientists. To mention only a few of these shortcomings – which will be discussed in this article, the questions that are used for constructing ability scales are not of equal difficulty; the format of the questions (open ended/ multiple choice/ true or false) yield different rates of correct responses by allowing the respondents to guess at unequal rates; the items may not always fit on a single dimension. The latter may entail that knowledge is not one-dimensional, but the more inviting conclusion is, certainly, that the data fails to conform to the empirical reality.

The question that we address in this article is whether the format related inflations of the averages of knowledge scales affect the reliability and validity of inferential statistical findings obtained with models in which knowledge is used as an independent or dependent variable in a multivariate fashion. In other words, assuming that we construct a political information variable by adding up or averaging the number of correct answers to quiz-like questions; do different question formats yield different slopes or error terms for knowledge or for its predictors in regression-based models?

We start by constructing an individual-level format-inflation variable that accounts for the possibility and plausibility that correct answers were obtained by random or informed guessing using data from a survey experiment carried out in Hungary in 2009. Then, we compare the results of regression models in which guessing is not accounted for with the results of nested models in which guessing is controlled for. Political knowledge is alternatively used as independent (when the dependent is the number of “don’t know answers” to political attitude questions and an independently constructed knowledge scale, respectively) and as dependent (for the purpose of assessing the effects of format on the mean of knowledge).
The experimental data used for all tests also includes a battery of questions that allows us to create an alternative scale of political information. These questions have a standardized format and content, hence the scores for different individuals are equally valid on the additive of correct answers to these questions. This alternative scale was used for directly assessing how format variations affect the reliability and convergent validity of our initial variable, using the same approach described previously.

Finally, in order to provide additional empirical support to our findings and to rule out the possibility of having results that only reflect the idiosyncrasies of the Hungarian public opinion, we use similar regression models as the ones described previously for the countries in the second module of the CSES database.

**The measurement of political knowledge: question format**

There are certain aspects related to reliability that cannot be accounted for mathematically; the quality of measurements is always bounded by the actual data. In this regard, there are things that cannot be changed after the data was collected: the wording of questionnaire items, the format of the questions (open ended, true-false, and multiple-choice); the topics of the questions (policy specific, ideological, campaign-specific knowledge or chronic, general), the ordering of the questionnaire items, etc. Such issues received very little attention in the field of research concerning political knowledge even though it seems apparent that this is precisely the area in which significant improvements can be made both regarding the validity and the reliability of the measurements.

This article focuses on a specific aspect of questionnaire design, while leaving further inquiries open for subsequent research. Can the reliability and/or validity of multivariate statistical findings in the study of political information effects be questioned on grounds related to question formats? Do we have reasons to encourage the use of a certain question format instead of another when constructing political information variables based on quiz-type questionnaire items?

Open ended questions tend to foster what the literature calls false negatives: a nontrivial proportion of relatively knowledgeable respondents either give incomplete answers according to the standards of the coders/interviewers (Gibson and Caldeira, 2009) or they simply shy away from giving an answer due to personal insecurity (Lupia, 2009). True-false questions and multiple choice ones, on the other hand, register false positives, meaning that a
noticeable proportion of people who are not particularly knowledgeable take the opportunity to guess the answers to the questions, thus seeming more knowledgeable than they are in fact (Mondak, 2001).

These issues related to questionnaire design are particularly important due to questions of validity and reliability. It is noticeable that open ended questions systematically underestimate the level of political knowledge of respondents due to the aforementioned false negatives, whereas true-false and multiple choice questions tend to overestimate it. Furthermore, it can be argued that open-ended questions and multiple choice or true-false questions in fact measure different things: while open-ended questions test the ability of respondents to reproduce information gathered prior to the survey interview; multiple-choice and true-false questions merely capture their ability to recognize certain pieces of information that they may normally not be able to reproduce (Kubinger et al, 2010).

This fact would not pose any kind of problems were the underestimations and overestimations random – homogeneously spread throughout the sample. However, as previous findings have shown, there are statistically significant differences between those who are willing to attempt at guessing the answers to the questions and those who shy away from doing so. Generally, men, people who are less educated, those who are interested in politics and those who are more self-confident are more likely to try to attempt at guessing when they don’t know the answers (Schuman and Presser, 1980; Steele and Aronson, 1995; Sturgis and Smith, 2010).

Our results (see table 2) bring further corroborating evidence to these hypotheses. Moreover, the literature shows that the total amount of random guessing tends to be highest when the political interest question precedes the set of knowledge items (Sturgis and Smith, 2010). This being the case, validity and reliability cannot be assumed for measurements of political information, proper assessment and careful consideration for measurement quality should be common practice.

**Extant approaches to dealing with format effects**

The general purpose of the existing literature on scaling, format effects and guessing is to create reliable rankings of the abilities of test-takers based on the latent structure of their response patterns (see Rasch, 1960; Kubinger et al, 2010). The necessity for such studies can be understood best in the context of the educational psychology literature, where improving
the assessment of skills and fine-tuning grading methods are particularly relevant as endpoints of the research endeavor. In this case it is rather unsurprising that little attention has been given to the performance of the estimates of personal ability (skill) in inferential statistics or to how guessing affects the conclusions we can draw from models where ability/skill is used as an independent variable. Not only does this study try to shed some light on these fairly novel issues, but it also tries to bring some important psychometric concepts and concerns closer to the political science literature.

The literature on psychometric scaling often reports large and significant differences in the rates of correct responses to test items as a direct function of question formats: for instance, Kubinger and his associates (2007; 2010) found that mathematics students score similarly on tests with free response format and on tests with 2 out of 5 multiple choice questions but they tend to score much higher on multiple choice tests with 1 out of 6 format. The interpretation of these results is rather straightforward: in open ended questions the respondents have virtually no chance of guessing the correct answer; on 2 out of 5 questions their chance of guessing the correct answer equates to 1/10, whereas on 1 out of 6 questions this probability increases to 1/6. As the likely returns from trying to guess drop, the attempts at guessing also drop, rendering the 2/5 format more similar to the open ended one in this respect.

A method of assessing political knowledge that has recently started to gain coverage in the political science literature is the Rasch model (see Selb and Lachat, 2009), which is similar in structure to the two-parameter logistic IRT model but differs from it significantly in method. While they both aim at estimating two latent traits underlying the performance of respondents to quizzes or tests – the ability of the respondent and the difficulty of test items – the Rasch model uses a simple logistic function that pools together both respondent and item dummies as explanatory variables predicting performance (Rasch, 1980). Rasch models do not explicitly account for the phenomenon of random guessing; nevertheless, guessing is likely to be responsible of situations in which respondents with a certain estimated level of ability provide correct answers to test items of higher difficulty (Smith, 1991). Such information is certainly not sufficient for constructing and using estimates of format induced guessing as controls in multivariate models.

4 There are 5 response options out of which one single combination of two is correct. The answer will be considered correct if and only if both responses are ticked.
The three-parameter logistic IRT model, on the other hand, utilizes explicit controls for guessing (Martin, del Pino & Del Boek, 2006); however, the standard approach is to equate the expected amount of guessing to 1/k for all test items with k response categories. Alternatives to this include accounting for the ability of the respondents or for the difficulty of test items (Martin, del Pino & Del Boek, 2006). The approach employed in this article substantively falls in the latter category, in that it takes into consideration the number of response categories and the empirical difficulty of the questionnaire items at hand. Nevertheless, the main contribution of this article is not intended for the field concerning psychometric scaling; our approach to estimating format related inflation has the sole purpose of creating a computationally straightforward and mathematically simple control for format-effects that is easily applicable to the context of this particular research project.

**Data, methods and results**

The data that was used in this article is drawn from a phone survey experiment conducted in September 2009 on a random sample of Hungarian citizens. The experimental part of the survey consisted of randomly assigning respondents to groups differing only in the type of question they were asked. The political information items are divided into two batteries. The first battery of items consists of 6 questions, each of them having three different versions: an open-ended version, a multiple choice one and a true-false one. Roughly one third of the sampled population responded to each of the three types of questions. The versions (a, b and c) were shuffled after each response, so that there was no single respondent who received only questions of type a or of type b. The questions in the second battery of questions had versions that discouraged “don't know” answers and versions that discouraged “don't know” answers; roughly 50% of the sample were assigned one version of each question and roughly 50% answered to the other version. Unlike the questions in the other battery, the format of these questions was identical for all respondents.

The dataset also contains information on the respondents’ level of education, their media consumption (TV news, newspapers and internet), their sex, age, income and interest in politics; all these variables were used as controls in the models. Furthermore, respondents were asked to rate their level of talkativeness, thus allowing us to use this as a control variable as well. With a sample size of 1705 and an item non-response (NA, excluding DK)
rate averaging below 1% on the variables included, all the parameter estimates are likely to be statistically unbiased and representative of the target population.

If we consider that question formats should affect the reliability and/or the convergent validity of political information scales, it is reasonable to expect that knowledge variables created based on questions with different formats would associate differently with the most common correlates of political information. To this end, I constructed separate scales counting the number of correct answers to the three types of questions and checked how they correlate with traditional predictors of political knowledge: interest in politics and education. Surprisingly enough, the three types of questions perform almost equally well. In fact, the scale based on the multiple choice questions correlates with both education and interest slightly better than the other two (r=.238 with education and r=.138 with interest, whereas the other two scales correlate at .122 and .127 with interest and at .160, respectively.151 with education).

Furthermore, we intuitively expect more talkative people to be more prone to attempt at guessing when they do not know the correct answers, thus artificially increasing their political information scores. Thus, if open-ended questions are indeed conducive to false negatives (people who are fairly knowledgeable shy away from giving an answer), people who are less talkative should be less likely to provide correct answers to such questions than more talkative respondents. Hence, the association between talkativeness and the performance of respondents on open-ended questions should be informative in this respect. However, we found no relationship between talkativeness and the propensity to give a correct response to any of the open ended questions; the only logit parameter estimate that came close to traditional standards of statistical significance (p<0.08) does not even point in the expected direction.

We then created an individual level variable capturing the expected inflation of the mean of the knowledge scale that is due to the possibility and plausibility of guessing. This

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5 Possibility refers to the actual chance of being able to give a correct response by guessing at random. The possibility of guessing will be equal to 1/k for multiple choice items with k response categories. Plausibility refers to how likely it is that a certain response is obtained through guessing; it takes into consideration the empirical difficulty of the question by taking into account the rate of correct answers given by respondents who received open-ended versions of the item at hand. For questions in which the open ended format yielded 100% correct responses, we estimate 0% guessing for multiple choice or true-false items (given that the content of the items is held constant). For questions in which the open ended format produced 0% correct responses, we will expect 100/k % correct responses for multiple choice questions that have k response categories.
variable was constructed based on the expected results presented in Table 1: I summed up the expected mean inflation of each multiple-choice and true-false scale (relative to the open-ended versions) that was used for the measurement of the respondents’ knowledge. For instance, if a certain respondent was assigned the multiple-choice version of the first question, the true-false version of the second and the open-ended version of the third, she would accumulate 46.23-29.4=16.83 on the guessing variable for the first question, 45.9-22.9=23 for the second one and 0 for the third one, which adds up to a total score of 39.83. The score, thus, corresponds to the expected amount of format-induced inflation on the multiple-choice and true-false questions.

This slightly deviates from the common practice in IRT literature of estimating the probability of guessing by simply dividing 1 by the number of correct answers and distractors in each multiple choice item (Kubinger, 2007). Instead of having a priori expectations about the inflations due to guessing, we compute the guessing variable here by taking into account the difficulty of each item as well (which is given by the percentage of correct answers in open-ended questions). Thus, we would expect no guessing inflation in multiple choice questions if their respective open-ended versions yielded 100% correct responses; but we would expect very high levels of guessing for variables in which the open-ended versions returned no correct responses.
We expect to find a significant positive association between “format-inflation” and knowledge; we expect to observe higher levels of political knowledge for respondents who had the opportunity to guess the answers to the knowledge items. However, the fact that the average scores of political information tend to be higher when respondents have this opportunity does not necessarily entail that the reliability or validity of different scales would be affected.

We count the correct responses given by each subject to the entire set of knowledge questions to obtain the respondents’ level of political information. Using linear regression, we try to predict this score using variables often cited as predictors of political knowledge (education, political interest, gender, income, etc.) and other controls. In an alternative model, we also include the format-inflation variable, which is expected to be positively related to the observed level of political knowledge but also to affect some of the slopes initially found. In

<table>
<thead>
<tr>
<th>Topic</th>
<th>Type</th>
<th>N</th>
<th>Correct (%)</th>
<th>Don’t know (%)</th>
<th>Expected correct (%)</th>
<th>Difference (obs.-exp.)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Minister of education in Hungary</td>
<td>Open ended</td>
<td>599</td>
<td>29.40</td>
<td>67.3</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>Multiple choice</td>
<td>577</td>
<td>66.40</td>
<td>20.1</td>
<td>46.23</td>
<td>20.17</td>
</tr>
<tr>
<td></td>
<td>True/False</td>
<td>529</td>
<td>62.00</td>
<td>21.7</td>
<td>54.65</td>
<td>7.35</td>
</tr>
<tr>
<td>President of the EU Commission</td>
<td>Open ended</td>
<td>576</td>
<td>22.9</td>
<td>73.4</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>Multiple choice</td>
<td>557</td>
<td>53.0</td>
<td>24.1</td>
<td>40.56</td>
<td>12.44</td>
</tr>
<tr>
<td></td>
<td>True/False</td>
<td>572</td>
<td>56.8</td>
<td>31.1</td>
<td>45.9</td>
<td>10.9</td>
</tr>
<tr>
<td>Number of EU member states</td>
<td>Open ended</td>
<td>582</td>
<td>10.1</td>
<td>44.2</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>Multiple choice</td>
<td>565</td>
<td>40.4</td>
<td>16.3</td>
<td>34.63</td>
<td>5.77</td>
</tr>
<tr>
<td></td>
<td>True/False</td>
<td>558</td>
<td>58.6</td>
<td>16.8</td>
<td>46.65</td>
<td>11.95</td>
</tr>
<tr>
<td>Parliamentary support needed for Prime Minister</td>
<td>Open ended</td>
<td>563</td>
<td>34.3</td>
<td>37.1</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>Multiple choice</td>
<td>570</td>
<td>24.4</td>
<td>9.6</td>
<td>53</td>
<td>-28.6</td>
</tr>
<tr>
<td></td>
<td>True/False</td>
<td>572</td>
<td>32.0</td>
<td>17.5</td>
<td>58.4</td>
<td>-26.4</td>
</tr>
<tr>
<td>Budget of the EU</td>
<td>Open ended</td>
<td>592</td>
<td>32.4</td>
<td>54.1</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>Multiple choice</td>
<td>556</td>
<td>36.0</td>
<td>14.6</td>
<td>50.06</td>
<td>-14.06</td>
</tr>
<tr>
<td></td>
<td>True/False</td>
<td>557</td>
<td>27.6</td>
<td>16.0</td>
<td>58.2</td>
<td>-30.6</td>
</tr>
<tr>
<td>MSZP on budget deficit</td>
<td>Open ended</td>
<td>578</td>
<td>40.3</td>
<td>34.9</td>
<td>-</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>Multiple choice</td>
<td>571</td>
<td>43.3</td>
<td>25.2</td>
<td>51.8</td>
<td>-8.5</td>
</tr>
</tbody>
</table>

The expected percentages were calculated using the following formula:

Expected = \( \frac{\% \text{ Incorrect in open ended} + \% \text{ DK in open ended} - \% \text{ DK in multiple choice or true-false}}{\text{number of available options}} + \% \text{ correct in open ended} \)

We expect to find a significant positive association between “format-inflation” and knowledge; we expect to observe higher levels of political knowledge for respondents who had the opportunity to guess the answers to the knowledge items. However, the fact that the average scores of political information tend to be higher when respondents have this opportunity does not necessarily entail that the reliability or validity of different scales would be affected.

We count the correct responses given by each subject to the entire set of knowledge questions to obtain the respondents’ level of political information. Using linear regression, we try to predict this score using variables often cited as predictors of political knowledge (education, political interest, gender, income, etc.) and other controls. In an alternative model, we also include the format-inflation variable, which is expected to be positively related to the observed level of political knowledge but also to affect some of the slopes initially found. In
other words, if we construct a political information variable by adding up or averaging the number of correct answers to quiz-like questions; do different question formats yield different slopes or error terms for knowledge or for its predictors in regression-based models?\(^6\)

A second set of models are used for this purpose. Model 3 predicts the number of “don't know” answers given by respondents to the battery of political attitude questions; the political information variable described earlier is used as independent. We then (Model 4) include the format-inflation variable in the specification and compare the slopes and standard errors obtained with the two models. An additional set of models (5 and 6) have an alternative political information variable as dependent; this variable was constructed using a different battery of political information items in the same dataset. While the count of DK answers to attitude questions is logically and theoretically linked to the respondents’ levels of political knowledge, the relationship between the two variables is merely a proxy for the reliability and convergent validity of our measurement of political information. The estimate for the relationship between our former and latter measurement of political information, on the other hand, offers us a direct estimation of the reliability and convergent validity of the initial variable.

Furthermore, given the admittedly very novel and arguably underdeveloped nature of the “format-inflation” variable that was described previously, we used the actual count of MC and TF questions as predictors in these last pairs of models instead of the “guessing” variable that is used in model 2 and 4. The only difference between model 3 and model 7 and between model 4 and 8, respectively, is that “format-inflation” was replaced by the two format count variables. All eight models are formally expressed below:

Model 1:

\[
\text{Knowledge} = \beta_0 + \beta_1 \cdot \text{interest} + \beta_2 \cdot \text{education} + \beta_3 \cdot \text{TV} + \beta_4 \cdot \text{newspaper} + \beta_5 \cdot \text{religiosity} + \beta_6 \cdot \text{family income} + \beta_7 \cdot \text{age} + \beta_8 \cdot \text{gender} + \beta_9 \cdot \text{talkativeness} + e
\]

\(^6\) The descriptive statistics for the knowledge items in our database show that true-false and multiple choice questions tend to produce higher estimates of the political knowledge of citizens; however, these differences are not fully consistent across items, which suggests that other effects (content related ones, or effects related to the different cues offered by different question formats) may be in place.
Model 2:
Knowledge = $\beta_0 + \beta_1 \times \text{interest} + \beta_2 \times \text{education} + \beta_3 \times \text{TV} + \beta_4 \times \text{newspaper} + \beta_5 \times \text{religiosity} + \beta_6 \times \text{family income} + \beta_7 \times \text{age} + \beta_8 \times \text{gender} + \beta_9 \times \text{talkativeness} + \beta_{10} \times \text{guessing} + \beta_{11} \times \text{guessing} \times \text{education} + \beta_{12} \times \text{guessing} \times \text{interest} + \beta_{13} \times \text{guessing} \times \text{gender} + e$

Model 3:
$\log (E (\text{Attitude DK})) = \beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{interest} + \beta_3 \times \text{education} + \beta_4 \times \text{TV} + \beta_5 \times \text{newspaper} + \beta_6 \times \text{religiosity} + \beta_7 \times \text{family income} + \beta_8 \times \text{age} + \beta_9 \times \text{gender} + \beta_{10} \times \text{talkativeness} + e$

Model 4:
$\log (E (\text{Attitude DK})) = \beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{interest} + \beta_3 \times \text{education} + \beta_4 \times \text{TV} + \beta_5 \times \text{newspaper} + \beta_6 \times \text{religiosity} + \beta_7 \times \text{family income} + \beta_8 \times \text{age} + \beta_9 \times \text{gender} + \beta_{10} \times \text{talkativeness} + \beta_{11} \times \text{guessing} + e$

Model 5:
Knowledge_alternative = $\beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{education} + \beta_3 \times \text{TV} + \beta_4 \times \text{newspaper} + \beta_5 \times \text{religiosity} + \beta_6 \times \text{family income} + \beta_7 \times \text{age} + \beta_8 \times \text{gender} + \beta_9 \times \text{talkativeness} + \beta_{10} \times \text{interest} + e$

Model 6:
Knowledge_alternative = $\beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{education} + \beta_3 \times \text{TV} + \beta_4 \times \text{newspaper} + \beta_5 \times \text{religiosity} + \beta_6 \times \text{family income} + \beta_7 \times \text{age} + \beta_8 \times \text{gender} + \beta_9 \times \text{talkativeness} + \beta_{10} \times \text{interest} + \beta_{11} \times \text{count\_MC} + \beta_{12} \times \text{count\_TF} + e$

Model 7:
$\log (E (\text{Attitude DK})) = \beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{interest} + \beta_3 \times \text{education} + \beta_4 \times \text{TV} + \beta_5 \times \text{newspaper} + \beta_6 \times \text{religiosity} + \beta_7 \times \text{family income} + \beta_8 \times \text{age} + \beta_9 \times \text{gender} + \beta_{10} \times \text{talkativeness} + e$

Model 8:
$\log (E (\text{Attitude DK})) = \beta_0 + \beta_1 \times \text{knowledge} + \beta_2 \times \text{interest} + \beta_3 \times \text{education} + \beta_4 \times \text{TV} + \beta_5 \times \text{newspaper} + \beta_6 \times \text{religiosity} + \beta_7 \times \text{family income} + \beta_8 \times \text{age} + \beta_9 \times \text{gender} + \beta_{10} \times \text{talkativeness} + \beta_{11} \times \text{count\_MC} + \beta_{12} \times \text{count\_TF} + e$
The first two models were estimated using Ordinary Least Squares regression, whereas for the third and fourth we used Poisson as link function due to the distributional properties of the dependent variable. Fully consistent with our expectations, guessing was found significant in predicting political knowledge both as main effect and as part of the joint effect with political interest (see Table 2). In addition to this, the slope of political interest almost doubles in size with the inclusion of the interaction effect. This finding suggesting that part of the direct effect of interest on the knowledge that we can observe in traditional surveys takes place through the noisy impact of the random guessing of less informed respondents. The negative sign of the estimate for the interaction effect indicates that the respondents who are not very interested in politics can display significantly higher levels of knowledge compared to their true score if guessing is allowed to take place. Other slopes changed too upon the inclusion of guessing in the model specification; however, these changes are small enough and substantively uninteresting.

Models 3 and 4, on the other hand, show that the inclusion of the format-inflation variable as control makes virtually no difference on the parameter estimates when political information is an independent variable and the count of “don’t know” answers to attitude items is the dependent. None of the effects estimated in the model lost their statistical significance or changed by more than a one-digit percentage of their initial size. The intercept dropped by approximately 9% of its initial size and is no longer highly statistically significant; which is by far the most substantive change induced by the inclusion of the format-inflation variable in the model. The fit of model 3 is not different from that of model 4 and, most importantly, political information associates with the rate of DK answers equally well in the two models. These results seem to point to the following conclusion: the reliability and convergent validity of political information does not depend on the format of the questions used for measuring information. If there are any format effects in place, they are certainly not visible in this particular multivariate setting.
The second part of Table 2 contains the estimated parameters for models 5 – 8; they have the same basic specifications as models 1 – 4, with the major exception that the format-inflation variable was replaced with the count of true/false and multiple choice questions that were used for the construction of the political information variable. A political information
variable exogenous to the one in models 1 – 4 was used as dependent in models 5 and 6. The format of the 4 questions used for the construction of this variable\(^7\) is the same for every respondent; the final scores represent the count of correct answers given by the respondents to the 4 questions. The old political information variable is used as independent for the purpose of assessing the reliability and convergent validity of the measurement. If the reliability and convergent validity of our political information variable are indeed invariant to format effects (as suggested by our previous findings), controlling for the number of TF and MC questions should have no significant effect on any of the other parameters estimated with our model.

All 8 models are paired. The difference between the first and the second model in each of the pairs is that the latter have controls for the count of true-false and multiple choice questions (for “guessing” in the first sets of models) used for the construction of the knowledge variable (the one used as independent in all models but 1 and 2, where it is the response variable). Not unlike the results discussed previously, the ones summarized in the second part of table 2 show little or no variation of parameter estimates upon the inclusion of format controls in the model specifications.

The only substantively and statistically significant difference that can be noticed is between the intercept of model 5 and that of model 6. The estimate for the intercept of model 6 is roughly twice the size of the one for model 5. This finding is in line with the expectation that MC and TF questions tend to yield higher estimates for the averages of knowledge due to two main considerations. In order to avoid redundancy, the count of open-ended questions was not included in the specification of model 6; thus, the intercept reflects the average score on the dependent variable of the hypothetical respondent whose political knowledge (independent) was estimated using only open-ended questions. What the variation of intercepts shows us is that if we standardize the measurement of political information across all respondents, the subjects that would have high scores on open ended questions tend to fare better than the ones who would appear equally knowledgeable on MC or TF questions.

Even though the coefficients for the two format effect variables are significant in models 4 and 5, their inclusion in the model did not alter the estimates for any of the other parameters. The same invariance can be observed in the last pair of models presented in the second part of table 2. This appears to lead to the conclusion that the format of the questions

\(^7\) The original battery included 6 political knowledge variables; we eliminated two because there was no unequivocable threshold to distinguish between correct and incorrect answers.
that our measurements of political information are based on is virtually irrelevant from the perspective of reliability of validity.

**Further tests: CSES**

It can be argued that our results are merely indicative of certain idiosyncrasies related to the Hungarian public opinion and politics. Our data, after all, was collected from a sample that is representative of the Hungarian voting age population as of November 2009. Thus, in order to provide additional empirical support to our findings we will use similar regression models as the ones described previously for the countries in the second module of the CSES database. Due to the limited number of knowledge items and the non-experimental nature of the data, the results are not expected to be as robust as the ones we obtain using the Hungarian survey experiment.

Nevertheless, not only can the CSES data corroborate our results with further empirical evidence, but it can also cast some doubt on our previous findings. The Comparative Study of Electoral Systems dataset contains a large number of variables relevant for studies in political science and is composed of representative samples from electoral democracies on all continents. Compared to other databases of similar magnitude, CSES provides considerably more diversity without making major compromises that would hinder cross-country analyses. For the purpose of this study the second module of the database was used; the only case selection employed was lead by the occasional unavailability of the relevant variables in some of the sampled countries.

The database does not have a battery of political attitude questions; hence, the dependent variable that we use instead is a dichotomous one that takes the value 1 for those respondents who successfully managed to position themselves on the left-right political scale and 0 for those who failed to do so. Furthermore, there are only three political information variables for each country, and the non-experimental nature of the data does not allow us to construct a “format-inflation” variable that would make this stage of the research a full replication of the previous one. Finally, some of the variables that were used at the previous stage are not available for the countries in the CSES database; namely the media consumption ones, the talkativeness variable and the interest in politics.

In some of the sampled countries only open ended political knowledge items were used (Germany, Brazil, Italy and Romania, for instance), whereas in others all three items
were of true-false format (Great Britain, Hong Kong, Sweden, etc.). In most countries, however, the knowledge items were more diverse in format, thus allowing us to create relevant macro-level question format variables for subsequent use as macro-level predictors of the association between political information and the likelihood of item non-response on the left-right self-placement variable.

<table>
<thead>
<tr>
<th>Polity</th>
<th>Slope for knowledge</th>
<th>Standard error</th>
<th>t-test</th>
</tr>
</thead>
<tbody>
<tr>
<td>Albania</td>
<td>0.46**</td>
<td>0.17</td>
<td>2.72</td>
</tr>
<tr>
<td>Brazil</td>
<td>0.30**</td>
<td>0.10</td>
<td>3.07</td>
</tr>
<tr>
<td>Finland</td>
<td>0.63***</td>
<td>0.14</td>
<td>4.47</td>
</tr>
<tr>
<td>Germany</td>
<td>0.46**</td>
<td>0.17</td>
<td>2.58</td>
</tr>
<tr>
<td>Great Britain</td>
<td>0.81***</td>
<td>0.11</td>
<td>6.95</td>
</tr>
<tr>
<td>Hong Kong</td>
<td>-0.17</td>
<td>0.12</td>
<td>-1.37</td>
</tr>
<tr>
<td>Hungary</td>
<td>1.21***</td>
<td>0.20</td>
<td>5.82</td>
</tr>
<tr>
<td>Israel</td>
<td>0.97**</td>
<td>0.32</td>
<td>2.99</td>
</tr>
<tr>
<td>Italy</td>
<td>0.71***</td>
<td>0.14</td>
<td>5.04</td>
</tr>
<tr>
<td>Korea</td>
<td>0.66***</td>
<td>0.12</td>
<td>5.31</td>
</tr>
<tr>
<td>Mexico</td>
<td>0.30***</td>
<td>0.06</td>
<td>4.95</td>
</tr>
<tr>
<td>New Zealand</td>
<td>0.30***</td>
<td>0.10</td>
<td>2.99</td>
</tr>
<tr>
<td>Romania</td>
<td>0.39***</td>
<td>0.08</td>
<td>4.71</td>
</tr>
<tr>
<td>Russia</td>
<td>0.58***</td>
<td>0.07</td>
<td>7.69</td>
</tr>
<tr>
<td>Spain</td>
<td>0.69***</td>
<td>0.19</td>
<td>3.61</td>
</tr>
<tr>
<td>Sweden</td>
<td>0.69*</td>
<td>0.30</td>
<td>2.30</td>
</tr>
<tr>
<td>United States</td>
<td>0.44***</td>
<td>0.11</td>
<td>3.78</td>
</tr>
</tbody>
</table>

For the first stage of the analysis we fitted models with identical specifications to the data from each separate country in the CSES sample. With three exceptions (Russia, Romania and Mexico), the models had AIC below 1000 and the parameter estimates for political knowledge were highly significant (with the exception of Hong Kong). We then collected the slopes, standard errors and t values from the models into separate vectors (see Table 3 below). We bound the vectors together with the count of multiple-choice and true-false questions for each country and tried to predict the size of the slopes, the size of the errors and the t values using these count variables separately and together. As shown in Table 4, none of the results is significant.
We find that the slopes and errors are not sensitive to variations of format, which leads to the conclusion that it is virtually irrelevant which type of questions we use for measuring political knowledge as long as the purpose of our measurement is to use political information as an explanatory variable in multivariate models. The intercepts are highly significant in all models, suggesting that the convergent validity of the political information scales used is consistent across subsamples (polities) regardless of the heterogeneity of question formats. Furthermore, we ran analyses of variance for the categories of the TF, MC and OE variables and found no differences between the slopes of knowledge across the categories of our independent variables. The post-hoc tests revealed one significant effect\footnote{The standard error of the slope of political information is statistically significantly higher in countries where one multiple choice variable was used compared to the countries in which no multiple choice questions were used} out of 27; which is less than what we would expect due to chance alone.

**Conclusions**

The purpose of this study was to determine whether the format of the questions used for constructing political information scales influences the reliability and validity of political knowledge variables operationalized as the count of correct answers to quiz-type questionnaire items. We tested this on experimental data from Hungary and used the CSES Module 2 database for partial replication of the results. We found no significant format effects on the slopes and errors of political information neither on the Hungarian database nor on the CSES data. Regardless of the type of questionnaire items (i.e. regardless of the amount of guessing that the questions allow) that are used, the resulting knowledge scale is equally reliable and valid. Scales constructed based on true-false questions associate with traditional

\begin{table}
\centering
\caption{OLS coefficients for question format (intercept in parentheses)}
\begin{tabular}{|l|c|c|c|}
\hline
\textbf{Independent} & \textbf{No. of TF} & \textbf{No. of MC} & \textbf{No. of OE} \\
\textbf{Dependent} & \textbf{questions} & \textbf{questions} & \textbf{questions} \\
\hline
Slopes for knowledge & -0.037 & 0.032 & 0.019 \\
& (0.58*** ) & (0.54*** ) & (0.52** ) \\
Standard errors & 0.009 & 0.004 & -0.010 \\
of the slopes & (0.144*** ) & (0.149*** ) & (0.170*** ) \\
t-tests & -0.519 & 0.192 & 0.370 \\
& (4.350*** ) & (3.904*** ) & (3.286*** ) \\
\hline
\end{tabular}
\end{table}
correlates of political information (political interest and education) just as well as those based on multiple-choice or open-ended questions.

Furthermore, controlling for the amount of random or informed guessing that the battery of knowledge questions allows did not affect the performance of the knowledge variable (used as independent) in any of the regression models that were tested. In this respect, we can claim that true-false, open ended and multiple choice item based scales perform similarly – in fact, almost identically – in regression models when used alternatively as independent variables. This leads us to the conclusion that most of the concerns that were mentioned earlier are not as well grounded as we would intuitively expect. These conclusions seem consistent across the countries (subsamples) in the second module of the Comparative Study of Electoral Systems.

Due to the unavailability of data for a full replication of the study presented in the first part of the present article, our tests on the CSES database are merely tentative. A full replication of the Hungarian survey experiment is necessary for a robust corroboration of our results. Moreover, while the validity and reliability of political information measurements do not seem to depend on the format of the questions used, the variations at the level of the content difficulty of questionnaire items may be more consequential. This, however, will be the topic of subsequent research.
**D4. The Influence of Question Format on the Reliability of Political Knowledge Scales**

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**Introduction**

Political information has been a central topic in the political science literature for more than half a century. Since the pioneering work by the Columbia school (Berelson, 1954) and the Michigan school (Campbell, Converse, Miller, and Stokes, 1960), numerous claims about the causes and effects of political information have been made over the years. The main normative thesis according to which citizen political literacy is paramount to the functioning of liberal democracy was severely challenged by the empirical results found over the years. Together with the parallel developments in inferential statistical methodology and survey design, revisions and alternatives to the initial thesis were set forth at increasing rates. This lead to a deepening of the historical divide between the elitist concerns about an ever more uninformed electorate and the populistic denials of the importance of a politically literate citizenry.

We contend that all scientific truth is procedural in origin; thus, the debate on the utility of knowledge can only lead to empirically true conclusions to the extent that the methodological decisions made in the process are internally consistent and externally valid. We argue that some of the decisions that researchers routinely make in their scholarly work are often insufficiently scrutinized and possibly consequential. Our paper investigates the effects of specific decisions at the level of data collection on the reliability of the measurements of citizen political knowledge.

The standard approach to measuring political knowledge is the additive one. The correct answers given by respondents to quiz-like political knowledge survey items are counted, the resulting scale being a count-type variable that is often used as interval or ratio in statistical analyses. The questionnaire items used in surveys for tapping into the political literacy of respondents either have an open-ended, true-false or multiple choice format. It is widely accepted in the scaling literature that open-ended questions foster false negatives: people shy away from providing an answer to questions because of personal insecurity and/or
because of the incomplete information they hold. Conversely, closed-ended questions encourage false positives: respondents with no knowledge or little knowledge of the subject matter have the opportunity to attempt at guessing the correct answer to the questions at hand. To the extent that the question format allows for guessing, some respondents will appear knowledgeable despite their lack of or partial knowledge. Whether the reliability of such measurements is affected by question format is an empirical question that we set off to illuminate with our present analysis.

We use the second module of the Comparative Study of Electoral Systems (CSES module II) as input data for our empirical analysis. It is important to note that the country collaborators of CSES were instructed to design the three political knowledge items in a way that would match predefined difficulty levels. More often than not, this requirement was met: roughly 25% of the respondents were able to give a correct answer to the most difficult question in their respective country; about 70% were able to answer the easiest one; and half were able to answer the question of moderate difficulty. In order to address this issue we complement our reliability analysis with nonparametric tests and customized alphas that do not rely on assumptions our data do not meet.

In addition to the classic Cronbach's alpha that we use for assessing the reliability of our scales, we use Loevinger's H coefficient of scale consistency. As a coefficient designed for diagnosing Mokken scales, H assumes a gradual increase in item difficulty that is consistent across individual performances: if respondent A answers correctly to item “a” but not to item “b”, respondent B must only answer correctly to item “b” if her answer to item “a” was correct as well. Under the assumption of one-dimensionality, political knowledge scales should display such patterns; any deviation from this regularity will translate into a decreased scale reliability with regard to H.

Political Knowledge

A minimal level of political awareness among all citizens is often claimed to be necessary for the functioning of democracy. By design, function and structure, the normative claim to democratic legitimacy rests on a few crucial assumptions. Citizens are expected to know best what their own political interests are and to have all the necessary expertise and will required for pursuing their interests through meaningful and targeted political action (Berelson, 1954; Campbell et al, 1960; Dahl, 1989; Bartels, 1996). Such assertions were
commonplace in earlier theories of democracy (see Lippman, 1922); however, later studies, backed by empirical results, have found little or no alarming consequences for political illiteracy. Democracies seem to function well despite widespread political ignorance (Converse, 1964; Carpini and Keeter, 1996), and the effects of knowledge on political decisions seems minimal at best (Sturgis, 2003).

Political information refers to the factual knowledge about politics stored in the conscious memory of citizens (Carpini and Keeter, 1996). As opposed to political attitudes or personal evaluations of things political, political information concerns the degree to which the beliefs held by citizens conform to an objective or intersubjective political reality. Such knowledge can be domain-specific (Smith, 1989; Iyengar, 1990; Krosnick, 1990), or general (Zaller, 1986; Carpini and Keeter, 1996), thus one-dimensional.

It is worth noting that under the methodological assumption of one-dimensionality, any empirically multi-dimensional construct will fail at explaining most of the variance it was theoretically expected to. Similarly, different operationalizations of the same concept will perform differently in statistical models; unreliable measurements will obscure existing relationships by elevating noise and invalid measurements are likely to lead to unsubstantiated conclusions. We thus depart from our normative concerns to discuss this closely related and highly consequential issue of measurement.

There are different approaches to political knowledge in the existing literature. The most common practice is to measure the level of “encyclopedic knowledge” (Lupia, 1994) of the respondents in order to assess their general level of practical political knowledge. As alternative approaches, one can rely on the survey interviewer’s assessment of the respondents’ levels of general knowledge about politics (Zaller, 1992; Bartels, 1996), refer to the policy-specific knowledge of the respondents (Gilens, 2001) or include measures of the subjects’ ideological literacy (Wolak, 2009). Most measurements are based on counting the correct answers that subjects manage to give to quiz-type questionnaire items; however, an increasing number of research articles use factor analysis or related methods for creating more reliable scales based on such quizzes. Most notably, IRT models (Carpini and Keeter, 1993), Rasch models (Selb and Lachat, 2009) and Mokken scales (Vettehen, Hagemann, and Snipenburg, 2004) are considered superior to additive scaling due to their decreased sensitivity to assumption violations. Based on the rationale that if there is a common variance of such variables it must be that of the “general political knowledge” that each of the individual variables are trying to assess – which is what we expect if the assumption of one-dimensionality holds, such methods not only assess the reliability of the measurement of
knowledge but they are often considered to shed some light on the empirical validity of the resulting scale (Boudreau and Lupia, 2011).

Three qualitatively different direct measures of political knowledge are concurrently used in the literature. Some political information questionnaire items are of the true-false type, others have multiple response categories (multiple choice questions) and others have open ended responses. There is no scholarly consensus as to which of these measures performs better in terms of reliability.

Open-ended questions require the respondent to be able to retrieve information from their memory without helping cues; whereas closed-ended items contain additional information that the respondent can refer to when answering the question (the distractors in multiple choice questions) and/or allow people to guess the correct response (both multiple choice and true-false items). Thus, open ended questions not only measure the respondents' levels of political knowledge but they also test the quick recall ability of respondents (Lupia, 1994); those people who shy away from providing an answer due to personal insecurity or self-consciousness will appear less knowledgeable than they are in fact. Non-responses induced by such psychological phenomena that are theoretically orthogonal to political knowledge will be referred to as false negatives; conversely, correct responses to closed-ended questions will be referred to as false positives to the extent that they were induced or favored by guessing in closed-ended questions. These issues related to questionnaire design are particularly important due to questions of validity and reliability. It is noticeable that open ended questions systematically underestimate the level of political knowledge of respondents due to the aforementioned false negatives, whereas true-false and multiple choice questions tend to overestimate it through false positives (Carpini and Keeter, 1993). This fact would not pose any kind of problems were the underestimations and overestimations random – homogeneously spread throughout the sample. However, as previous findings have shown, there are statistically significant differences between those who are willing to attempt at guessing the answers to questions and those who shy away from doing so. Generally, men, people who are less educated, those who are interested in politics and those who are more self-confident are more likely to try to attempt at guessing when they don’t know the answers (Schuman and Presser, 1980; Steele and Aronson, 1995; Sturgis and Smith, 2010). Furthermore, the total amount of random guessing tends to be highest when the political interest question precedes the set of knowledge items (Sturgis and Smith, 2010). This being the case, validity and reliability cannot be assumed for measurements of political knowledge, proper assessment and careful consideration for such issues should be common practice.
If, for instance, gender is a predictor of guessing, it means that in every model where political knowledge (the additive of correct answers to closed-ended questions in this case) is used as a predictor, we will have increased heteroskedasticity. The average of political knowledge for men will converge to \(1/k\) (\(k\) represents the number of response options for each political knowledge item) whenever the difficulty of the question approaches maximum, whereas the estimate for women will approximate their actual level of political knowledge. Assuming that all the covariates of guessing could be included in the model specification, some estimates for the effects of political knowledge may remain relatively unbiased as long as all the meaningful interaction terms are specified. This, however, does not circumvent the overarching problem of measurement unreliability (high reliability for women and low reliability for men) and invalidity (the knowledge scale measures guessing in men, which is, arguably, orthogonal to political knowledge).

**Measurement error and question format**

Surprisingly little research was published on the matter of format effects on the quality of political knowledge measurements. However, the psychometric scaling literature gives us important insights for evaluating some of the assertions made by researchers with regard to political knowledge question formats. To this end, we will consider political knowledge as a specific type of ability or skill (Carpini and Keeter, 1993).

Not unlike other ability tests, the political knowledge items in any given survey represent a sample of all the possible items tapping into a more general skill – political literacy in this case. The population of politically relevant data is potentially infinite, thus, there is no conceivable way of establishing the randomness of any sample of items (Carpini and Keeter, 1993). Under the assumption of uni-dimensionality (Zaller, 1986; Smith, 1989) and to the extent that our selection of items approximates a random sample from the population of possible political information items (Nunnally, 1978), the validity of our measurement can be assumed without concerns for question contents. We thus conclude that as long as our construct of interest is unidimensional, the topical content of political knowledge items need not be a matter of concern for the present study. We thus shift our attention to more technical aspects of the survey item structure.

The uni-dimensionality assumption does not settle the debate on format. Kubinger and his associates (2007; 2010) noticed a significant decrease in the rate of correct answers to test
items with more response options. While the one out of six multiple choice format fosters significant amounts of guessing, the two out of five\(^9\) format abruptly decreases the rate of random guessing by making it very hard for respondents to choose the correct answer by chance (Kubinger et al., 2010). If the same relationship between the number of response options and the amount of guessing holds true for items with fewer response options, we expect the true-false format to register the highest amount of guessing, followed by the multiple choice and the open-ended format.

The phenomenon of guessing affects reliability by increasing random error and elevating type II errors. There is little doubt that some of the null or substantively insignificant findings in the literature were partly due to format related measurement noise. Furthermore, the heterogeneity in the patterns of guessing point to the potentially more stringent problem of measurement bias (Sturgis and Smith, 2010). Certain studies on political knowledge, especially those discussing the gender gap in political knowledge, or the knowledge gap between low and high socio-economic status citizens, are certainly affected by the heterogeneity in the patterns of guessing. The observed levels of political knowledge for men and for lower educated people (Schuman and Presser, 1980; Steele and Aronson, 1995; Sturgis and Smith, 2010) are likely to be artificially increased by format effects. Survey items will overestimate the knowledge of men, thus overestimating the knowledge gap between men and women; conversely, guessing will increase the scores obtained by lower educated people (who appear to have high propensities to guess according to the aforementioned studies) thus decreasing the observed knowledge gap between the educated and the uneducated. The non-independence of measurement errors severely affects the validity of the measurement and contributes to violations of basic assumptions of most test statistics.

At this point we could be tempted to conclude that closed-ended questions should be replaced by open-ended questions to ensure a more reliable and valid measurement of political knowledge. However, previous studies have shown that open-ended questions are also plagued with measurement error (Gibson and Caldeira, 2009; Prior and Lupia, 2008). A notable proportion of relatively knowledgeable respondents shy away from giving answers to

\[ \text{9 There is a unique combination of two responses that are counted as correct answers to the question. Every other combination is incorrect; thus, the probability of guessing the correct answer is reduced from 1/6 (in one out of six formats) to 1/10.} \]
open-ended questions; such behaviors are likely correlated with personality traits and individual differences between respondents (Mondak, 2001).

Furthermore, apart from measuring political literacy, open ended knowledge items also test the respondents' memory by rewarding quick recall abilities (Lupia, 1994). People who have a good memory of names are more likely to be able to retrieve information about the name of the current holder of various governmental offices. While knowledge is likely to be among the covariates of recall ability, the two constructs are conceptually distinct and empirically only partially interdependent. To the extent that political information is not a perfect function of recall ability, boldness and self-confidence, open-ended questions are bound to induce measurement error.

In the absence of objective measures of political knowledge, we can only make comparisons between one imperfect scale and another. Both open ended and closed ended questions display considerable levels of unreliability and invalidity. However, it is an empirical question whether open-ended questions perform better than closed-ended ones for measuring political knowledge.

Data

Our empirical analysis was done on the Comparative Study of Electoral Systems (CSES), Module 2. The dataset contains a large number of variables of interest for social science research and is composed of representative samples from electoral democracies on all continents. Compared to other databases of similar magnitude, CSES provides considerably more diversity without making major compromises that would hinder cross-country analyses. For the purpose of this study the second module of the database was used; and the only case selection employed was lead by the occasional unavailability of the relevant variables in some of the sampled countries. Four elections were removed from the database (Bulgaria, 2001; Denmark, 2001; the mail-back stage of the German study; Iceland, 2003) due to lack of valid data on either of the political knowledge variables or due to the unavailability of information about the format of their respective questionnaire items.

In some of the sampled countries only open ended political knowledge items were used (Germany, Brazil, Italy and Romania, for instance), whereas in others all three items were of true-false format (Great Britain, Hong Kong, Sweden, etc.). In most countries, however, the knowledge items were more diverse in format, thus allowing us to create
relevant macro-level question format variables representing the count of open ended, multiple choice and true-false questions used in each polity. For socio-economic status we included variables on age, gender, education and family income; we also included an additive index of political participation based on the participation variables in the initial dataset (see Appendix 1 for a verbatim presentation of the items). The only consideration behind our selection of variables was their empirical and theoretical connection to political knowledge.

The three political knowledge items used in each polity were purposefully designed to match a predefined level of difficulty. Unsurprisingly, the rate of correct answers varies from one item to the next one in a consistent fashion across elections: there is an easy question that roughly 70% of the respondents answered correctly, a moderate one that approximately half of the respondents answered and a harder one on which about 25% appeared knowledgeable. This pattern holds regardless of the format of the respective questions, which means that the most apparent of all format effects – the inflated means for closed-ended questions – will not be easily observable on the CSES data. However, the differences in reliability between open ended and closed ended questions will be noticeable in their differential association with other variables, with the other knowledge questions or with the additive of correct answers across the three knowledge items.

**Empirical Analysis**

We divided the initial CSES database into separate election survey data for the sampled countries; all computations were made on the separate databases in an automated fashion using the open source statistical software R. We computed item by item statistics: item difficulty, pairwise polychoric correlations between knowledge items, correlations between each political knowledge item and demographics, correlations between each item and the knowledge scale, Mokken scalability without each of the items, Cronbach's Alpha without each of the items and standard errors around all estimates. Finally, we collected all results and regressed our measures of reliability on the count of open-ended, true-false and multiple choice questions used in each polity for the creation of the political knowledge scale.

In line with our expectations, the three knowledge items in each country differ significantly in difficulty. While the overall average rate of correct responses is just below 0.5 (0.44), the separate items vary in difficulty between an average of 0.24 for the most difficult items and 0.64 for the least difficult ones. In a few cases the rate of correct responses is
alarmingly low, thus rendering the quality of the measurement questionable at best (Nunnally, 1978; Carpini and Keeter, 1993). In Chile (2005), Ireland (2002) and Korea (2004) the most difficult item returned less than 5 percent correct answers, whereas in Taiwan (2001), Finland (2003), Israel (2003) and Romania (2004) the same statistic is within the 0.05-0.1 range. This leads to a lower power of discrimination for these items and is bound to affect the reliability of the measurement in a negative way (Nunnally, 1978). Finally, the difference between the ratio of correct to incorrect answers varies very little from the least difficult to the most difficult item in Japan and Korea, yet in the case of the latter no further abnormalities were found.

The average Cronbach's alpha for the additive knowledge scale is .44, with only one polity reaching traditionally acceptable levels of reliability for the measurement of knowledge (Poland, 2001 – alpha=.73) and four more – Italy, 2006; Kyrgyzstan, 2005; Norway, 2001 and Spain, 2004 – having alpha estimates between .6 and .7. The measurement appears particularly weak even by social science standards (Nunnally, 1978); however, it is worth noting that some of the coefficients may be highly unstable due to assumption violations. Not only that our data is binary, thus rendering variance-covariance based estimates inaccurate (Cronbach, 1951), but some of our items have a difficulty level far beyond the one recommended in the literature (Carpini and Keeter, 1993).

With all knowledge items being binary and displaying a consistent pattern of differential difficulty across elections, Loevinger's H coefficient for Mokken scales should provide a more realistic estimate of the internal consistency of our measurement (Mokken and Lewis, 1982). To this end, we computed the H coefficient for all elections and found an average score of 0.43, which shows that in the average election study, the CSES political knowledge items conform to the general pattern of Mokken scales (Garson, 2011). While the alpha coefficients correlate strongly with H, with r=0.86; the latter shows that in a majority of election studies the performance of respondents on one knowledge item is predictive of their performance on the other items. In 27 out of the 37 elections in our sample, the three knowledge items appear to display what we call hierarchical difficulty: respondents who answer correctly to an item of high difficulty \( p(1) \approx 0 \) are indeed very likely to answer correctly to easier items \( 0 < p(1) < 1 \). H exceeds the threshold of .4 (Garson, 2011) in all these cases, suggesting that the measurement of political knowledge is at least moderately internally consistent.

The reliability of the measurement varies considerably across polities, with a standard deviation of .177 for alphas and .193 for Loevinger's H. On average every additional
knowledge item increases alphas by approximately 0.08. However, in more than half of the elections sampled in CSES, there is at least one knowledge item that decreases the reliability of the scale measured with Cronbach's Alpha, yet it is not readily observable which property of the respective items affect the alpha coefficients. Whether the format of the questionnaire items is what drives these variations in reliability is an empirical question that we will discuss in the following section.

**Question formats and measurement reliability**

For a preliminary analysis we ran correlations between each knowledge item and the usual covariates of political knowledge: education, sex and age. The final table in the appendix summarizes our results separately for each election and item. All correlations of knowledge with education and sex are statistically significant, with some of the effects particularly large: some of the correlations with education are as high as 0.5 and even 0.6. Age is also a statistically significant predictor of political knowledge even though the sign of the relationship is not fully consistent across elections. These results suggest that the vast majority of the CSES knowledge items are convergently valid to different degrees; the correlations between the three knowledge items and education have a standard deviation of 0.14, 0.14 and 0.11 across elections. This variation may be caused by the differential reliability of the measurements across countries or by variations in validity, but further analysis is necessary to complement these findings.

We used the variable description publicly available on the website of the Comparative Study of Electoral Systems to classify the knowledge items as open ended, true-false and multiple choice questions. We regressed the reliability scores on these variables in order to see if there is a linear pattern linking question formats to the overall reliability of political knowledge scales. Our results are summarized in Table 1. [future versions of this paper will include simulation tests of the sensitivity of these effects to distributional and relational assumptions]

Question formats appear to account for roughly 1/3 of the variation in reliability. With only two explanatory variables, our model predicting the raw Alpha coefficients appears to fit the data surprisingly well. The average alpha for elections where only open ended questions were asked is 0.529; every additional true-false question will decrease reliability by an average of 0.06 points and each multiple choice item is expected to decrease alpha by 0.095.
The results are highly significant both statistically and substantively, considering that the maximum value of Cronbach's Alpha is 1.0. A scale constructed exclusively using true-false items is expected to have a reliability of 0.33; whereas a measurement based solely on multiple choice questions would have an expected reliability of 0.24, compared to the (admittedly low) reliability score of .529 found for open ended tests.

We also created a stacked dataset where each knowledge item is considered a separate case and the key variable is the average correlation of each political knowledge item with the other two items in the election study. We calculated interitem polychoric correlations between the three items and used the results for computing a revised version of the standardized alpha coefficient. We replaced the r coefficients in the original formula for standardized alpha with the three pairwise polychoric correlation coefficients to obtain a revised version of the standardized Cronbach alpha. We argue that this measure should provide us with more accurate estimates for the reliability of our scales because of its decreased reliance on the distributional assumptions of Cronbach's alpha.

<table>
<thead>
<tr>
<th>Table 1. Format effects on the reliability of the measurement</th>
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<tr>
<td>Intercept</td>
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<tr>
<td>True/False</td>
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<tr>
<td>Multiple choice</td>
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<tr>
<td>Adjusted $R^2$</td>
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</tbody>
</table>

We regressed our revised coefficient on the count of true-false and multiple choice items used in each election study. The results do not differ much from the ones found with the previous model: multiple choice and true-false questions tend to decrease the reliability of measurements; however, the pattern is even more evident than before. A test with only open ended questions is expected to produce a reliability score of .752; every multiple-choice question is expected to reduce reliability by 0.12 whereas true-false questions will decrease alpha by 0.92 on average. The model fits better than the previous one, with an adjusted $R^2$ of 0.38, which is particularly high considering the minimalistic model specification (only two explanatory variables) and the low predictive power of statistical models that appears to be the norm in the social sciences.

$$k \ast \frac{r}{(1 + (k - 1)\ast r)}$$

where $r$ is the average correlation between test items and $k$ is the total number of items in the test.
Our custom made alpha coefficient was built using polychoric correlations as input data; thus, if the format count variables managed to predict a nontrivial proportion of the variance in what we call polychoric alpha, they should also predict the average pairwise correlation between items. Does the degree to which political knowledge items measure the same thing actually depend on the format of the respective items? To address this question, we ran a final model in which we regressed the average interitem correlation on the count of multiple choice and true-false items used in the election study. The results are almost identical to the ones we found with our previous models: the more closed-ended questions we use, the lower the correlation between political knowledge variables.

Our conclusion seems inescapable at this stage. Open ended questions appear more reliable than closed-ended ones in the Comparative Study of Electoral Systems. The number of closed-ended questions in the test successfully predicts roughly 1/3 of the variance in the interitem correlation, internal consistency of the political knowledge scale, and measurement reliability. While both closed ended and open ended questions induce measurement noise and potential bias, it appears that the use of the former is more advisable than the latter if a more reliable measurement is sought.

Conclusions

Political knowledge items are often found in large-scale survey, yet little more than the best intuition of survey contributors is employed in the decision over one question format or another. While the potential unreliability of different formats is widely recognized, there is little evidence for the methodological superiority of one measurement or another. Our current study investigated the matter in an empirical fashion using the CSES module 2 data, which provides considerable cross-country variation in question formats for political knowledge items.

We found that the measurements utilizing closed-ended questions are by far less reliable than the ones based on open-ended items. Question format alone explains one third of the variation in measurement reliability, modeled under the linear assumption. We do not exclude the possibility of a non-linear relationship between our variables of interest; thus, it is conceivable that our findings in fact underestimate the real format effects. We leave this possibility open for further empirical investigations.
There are further possible limitations to our study. It is not inconceivable that the associations we found are in fact spurious; election studies that write better questions (regardless of the format) use more open-ended questions, yet the open ended format in itself may not increase reliability. Future studies could examine this possibility by adding to our models a proxy for how experienced the election study team in each country is. For instance, the year of the first election study in the country could be a proxy for this.

Finally, our findings hold under the assumption of the uni-dimensionality of knowledge. If the selection of question formats in each election study was in fact driven by concerns regarding the topics of the knowledge items, our results may have been partially caused by variations in question contents. This, of course, can only be the case if political knowledge is a multi-faceted construct, thus making variations of content consequential.

The contribution of our paper is manifold. The most apparent contribution from an academic standpoint is that it sheds light on important issues regarding the measurement and scaling of political knowledge. Our findings cast doubt on the accuracy of all null results in the study of information effects to the extent that their underlying measurements are based on closed-ended questions; at the same time, substantively trivial findings in the literature may have been induced by measurement error as well. The practical implications of our results are also worth noting. For commercial pollsters, the unreliability of closed-ended questions translates into an increased cost for data collection whenever such items are being used. The lower the reliability of a measurement, the more items will be needed to grasp an empirically real effect. What one can conclude based on our results is that the same reliability can be achieved with fewer items and smaller sample sizes if the right decisions are made at the survey design stage of any research endeavor.
D5. The ability-motivation-opportunity model revisited: The direct and indirect effects of two ability measures: education and cognitive ability

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Levente Littvay, Central European University
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Introduction

Normative theories of representative democracy claim that a politically knowledgeable populace is crucial for the functioning of democratic institutions, that democratic elections require informed decisions from the part of the citizens (Dahl, 1979). Our most basic intuitions tend to agree with such a claim; whoever knows the rules of the game, the players and their strategies, is more likely to play a good game or coach a good team. But even non-academics have raised awareness of this problem. The need for a cognitively engaged citizenry resonates strongly in many if not all social strata. In the words of Lisa Simpson from The Simpsons, “the price of freedom is eternal vigilance”.\(^{11}\) Whether political knowledge is indeed important for democracy is an empirical question that has been often addressed in the social scientific literature with various degrees of success.

Where does political knowledge come from? Who are the people who acquire political knowledge and what drives them to pay attention to political news and occupy their minds with political information? The classic model proposed by Luskin (1990) links the acquisition of political knowledge to a triad of interconnected individual-level factors: citizens have different cognitive abilities to aid their understanding of politics, they are unequally motivated and do not have equal opportunities to get informed. We propose to extend the model by theoretically arguing and empirically showing that cognitive ability has a substantively trivial direct effect on the acquisition of knowledge. More importantly, we also show that most of the effect of ability on the acquisition of knowledge is a moderation rather than a main effect. Finally, we question the validity of common operationalizations of

\(^{11}\) Often wrongly attributed to Thomas Jefferson, the source of the original quote could not be traced to date
the triad, namely the use of “education” as a proxy for cognitive ability (Carpini and Keeter, 1996) and argue that unlike cognitive ability, education does indeed have direct effect on knowledge. Its direct effect is also moderated by cognitive ability.

Considering plausible mechanisms through which ability can influence knowledge, we contend that there is no political information contained in or directly facilitated by any accurately measured cognitive faculties. Cognitive ability rests within the individual, while political information is external to them. The direct effect of education, consistently reported in the political science literature, is not indicative of the impact of cognitive ability on political knowledge. The pursuit of higher education causes greater exposure to democratic socialization, making educated people more prone to acquire political knowledge. We thus argue that the relationship between education and knowledge is falsely attributed to cognitive ability. Without denying that cognitive ability assists citizens in their decision to get politically informed and in the process of acquiring and understanding political information, we assert that its direct effect is overestimated by the education proxy.

We propose a revised version of the triadic model that highlights the moderating effect of cognitive ability on virtually all relationships between the elements of the triad and knowledge. To test our model, we designed a short cognitive ability test that we included in a panel survey conducted in the Netherlands with Kieskompas in March 2012 on a sample of 3466 respondents. We use regression to test the capability – opportunity – motivation model; show that the effect of education on political knowledge remains when controlling for cognitive ability, and that there is very little additional predictive power that the direct effect of cognitive ability adds to our models explaining political knowledge. We finally fit our revised version of the triadic model to the data and we find that the effects specified are not homogenous across ability defined groups. We conclude that cognitive ability needs to be modeled as moderator rather than direct effect if a more accurate understanding of the sources of political knowledge is sought.

Political Knowledge and the Capability – Opportunity – Motivation model

Political knowledge refers to the amount of factually correct information stored in the conscious memory of citizens (Carpini and Keeter, 1996). As opposed to political attitudes or personal evaluations of things political, political knowledge concerns the degree to which the beliefs held by citizens conform to an objective or intersubjective political reality. That the
European Union has 27 member states is a political fact that can constitute the content of a political knowledge quiz; however, that religion plays too strong a role in American politics is a subjective statement the correctness of which cannot be checked without resorting to subjective standards.

The utility of political knowledge is manifold and very well documented in the political science literature. More informed citizens are more likely to be aware of the political alternatives facing them (Fournier, 2002, Bartels, 2005; Sturgis and Smith, 2010) and to vote for parties and candidates who are politically closer to their personal political beliefs (Luskin, 1990; Bartels, 1996; Fishkin and Luskin, 1999; Lau and Redlawsk, 2001; Lau et al., 2008). Even more importantly from a normative democratic point of view, levels of political information were often found to be significant predictors of the strength and direction of political attitudes, beliefs, vote choice on the individual level and election results on the aggregate level (Johnston et al, 1996; Bartels, 1996; Althaus, 1998; Fishkin and Luskin, 1999; Crampton, 2009).

The recent literature in the field contends that political knowledge springs from a triad of nonorthogonal factors that jointly contribute to most of the variation in knowledge across individuals: capability or ability – cognitive ability; opportunity and motivation (Luskin, 1990; Carpini and Keeter, 1996). The capability – opportunity – motivation (henceforth the triad) triad was proposed by Robert Luskin primarily as a theoretical model with the main purpose of clarifying conceptual aspects related to political knowledge (Luskin, 1990); its empirical use is often hindered by the scarcity of available data and the operational complexity of the concepts it employs.

Opportunity refers to people's exposure to political news and newspapers; while it is theoretically an environment-level variable (Luskin, 1990), it is often operationalized with variables related to news media consumption and socioeconomic status (Jerit, Barabas and Bolsen, 2006). Motivation refers to the desire and ambition of individuals to acquire political knowledge and the diligence they exercise in their pursuit of political information (Carpini and Keeter, 1996). In line with the extant literature, we operationalize motivation as “political interest” which is, arguably, the main pathway through which the socio-political and economic environment can influence one's propensity to get politically informed (Lazarsfeld et al., 1948: 4; Campbell et al., 1960: 102; Van Deth, 2000; Miller and Rahn, 2002; and Delli-Carpini and Keeter, 1996).

Opportunity is a chiefly structural element of the triad; it incorporates the effects of the media environment (Hutchings, 2003) and of the position individuals hold in the social
structure: gender, income, education (Jerit, Barabas and Bolsen, 2006). The more available political information is in one’s environment, the more likely it is that they will become knowledgeable about political matters. Motivation may also overlap with opportunity and capability in its operationalization; while political interest is the measure of motivation generally agreed upon (Carpini and Keeter, 1996; Kwak, 1999), it is not uncommon to see it as a function of education and media consumption (Kwak, 1999).

Our modification of the model centers on ability and the mechanism through which it affects political knowledge. There is no established operationalization of the triad, especially ability. While some use education as a proxy for cognitive ability (Carpini and Keeter, 1996), others would look at SAT scores (Boudreau, 2009) or rely on the survey interviewers' assessments of the respondents' cognitive abilities (Luskin, 1990; Bennett, 2002). The status of education as an indicator of cognitive ability, opportunity and, arguably, motivation, complicates the interpretation of all estimates found for the effects specified in the triadic model and blurs the understanding of the sources of political knowledge. We discuss these aspects at length in the following section.

The effect of education

The association between educational attainment and various political behaviors, attitudes and the democratic desideratum is well documented in the political science literature. Educated people are more likely than their less educated peers to conform to most if not all democratic principles that the normative literature put forward throughout the years: they have a higher propensity to engage in all forms of political participation (Putnam, 2000), they express less prejudice towards minorities (Wagner and Zick, 1995) and are generally more politically knowledgeable than their less educated counterparts (Converse, 1964; Carpini and Keeter, 1996). In light of these well corroborated empirical findings, Converse even concludes that all “positive” values, attitudes and behaviors are fostered by high levels of educational attainment (Converse, 1972).

There is little doubt that higher education covaries with multiple democratic attitudes and behaviors; however, the evidence for a causal connection between the former and the latter is not conclusive. It can be argued that one's level of education is merely a reflection of their pre-adult socialization and experiences, social class and cognitive abilities. Hence, educational attainment may be merely a channel through which distal variables affect our
outcomes of interest (Kam and Palmer, 2008). It comes as no surprise, thus, that education stands out so often in political science studies as a strong predictor of any given political attitude or behavior.

The questionable causality is particularly consequential in modeling the acquisition of political knowledge. Alongside political interest and education, political knowledge tends to correlate positively with democratic values and SES, but there is no straightforward theory or robust evidence for the direction of the causality underlying the relationships between these variables. The use of education as a proxy for cognitive ability in the triad hinders our ability to distinguish between the direct, indirect and spurious associations between the elements of the triad and produces inaccurate estimates of the effects of ability, motivation and opportunity on the acquisition of political knowledge. We therefore need to strip the model of the endogeneity caused by the ambiguous status of education in order to see how cognitive ability interacts with opportunity and motivation in influencing the acquisition of knowledge. To overcome the problem, we propose a more direct measurement of cognitive ability that improves on the existing operationalization in terms of both construct and discriminant validity at the cost of a moderate loss of reliability.

Cognitive ability and the triad

The capability-opportunity-motivation theory finds cognitive ability to be one of the main determinants of political knowledge (Luskin, 1990) and, arguably, the most causally straightforward predictor of political literacy in general: political matters are often too complex and obscure for the population at large to grasp, understanding them often requires the assistance of cognitive faculties that are unequally distributed across individuals. But based on this, it appears from the offset that cognitive ability carries little or no substance directly linked to political knowledge; its main role is in translating external information into coherent knowledge retrievable from the memory of citizens. Conceptually the described mechanism is a textbook example of a moderator.

Cognitive ability measures were previously found to correlate positively with indicators of job performance, educational attainment and achievement, income (Jensen, 1998), prosocial attitudes, openness to experience, pro-democratic values (Hodson and Busseri, 2012) and negatively with religiosity (Nyborg, 2009) and delinquency (Jensen, 1998). It appears that what Converse concluded about education is true for cognitive ability;
most “desirable” outcomes in the social and political sphere seem to associate positively with various operationalizations of cognitive ability.

Given its relative invariance and temporal precedence to socio-political, attitudinal, behavioral and economic indicators, we argue that cognitive ability is an exceptionally useful concept that is likely to aid our understanding of directional theories of political knowledge and reduce the inherent endogeneity of extant operationalizations of the triad.

The most commonly used test for cognitive ability (or Spearman's g, which is an equivalent concept) is currently the Raven's Progressive Matrices (Raven, 2000), with its variants Standard Progressive Matrices (SPM), Colored Progressive Matrices (CPM) and Advanced Progressive Matrices (APM). They measure two main components of cognitive ability: eductive and reproductive ability (Raven, 2000); in other words the ability to make sense of complex non-verbal information and to absorb and reproduce information. Cognitive ability tests are often contested for alleged problems of validity and reliability, yet correlations between cognitive ability scores obtained with different measurement tools are usually strong and always positive (Court and Raven, 1995).

We argue that political knowledge stems from a specific constellation of direct and conditional effects that fall in the broad categories of a revised capability – opportunity – motivation triad. We acknowledge the ambiguous status of education, separate it conceptually from cognitive ability and motivation and include it in the model as a direct predictor (though we also recognize that it is not independent of the other components of the triad either). Since the empirical model shows that cognitive ability only moderates all effects on the acquisition of political knowledge; we do not include a direct effect in the specification (Figure 1.).

We assert that the issues of endogeneity in the extant operationalizations of the theoretical model have thus far made it painstakingly hard to isolate the effects of individual variables on the acquisition of political knowledge. We contend that education is particularly problematic as a proxy for cognitive ability; interaction effects between cognitive ability and motivation or opportunity cannot be estimated accurately if ability is measured with education. A reduced reliance on proxies enables us to distinguish between the variation in political knowledge that is causally linked to cognitive ability and the variation that was previously wrongly attributed to it.

For illustrative purposes, we propose a brief discussion of the consequences of a mismeasure of cognitive ability in modeling the acquisition of political knowledge with the triad. If cognitive ability, operationalized as education, is found to be the strongest predictor of political knowledge, we can be tempted to conclude that the variation in political literacy is
mostly caused by individual differences that cannot be easily changed throughout one's life. While people's position in the social, political and economic community can be improved with affirmative action policies\(^1\), cognitive ability is mostly invariant within the individual and its effects on any given political outcome can hardly be manipulated without a radical reshuffling of the rules of functioning of the political community. Can we make politics easier to grasp to enable citizens with lower levels of cognitive ability to participate meaningfully in its workings? Rather unlikely.

Educational attainment is associated with motivation and opportunity as well, and its inclusion in the triadic model as proxy for cognitive ability would bias downwards the estimates for the effects of motivation and opportunity and bias upwards the estimate for the effect of cognitive ability. Of course, statistically equivalent outcomes are expected if we use educational attainment as indicator of motivation or opportunity in absence of a direct measure of cognitive ability.

Take, for instance, the theory of “knowledge gaps” (Tichenor et al., 1970), which is particularly relevant in the context of the triadic model. The theory states that there are systemic disadvantages that various social groups have, that make them unequally responsive to efforts made at improving their political literacy by feasible means. Knowledge is acquired as a result of the interactive effects of time-invariant indicators and less stable ones; the external manipulation of one of the main effects can have divergent impacts on the knowledge of individuals that differ on the other main effect. This applies very well to our situation: cognitive ability is largely invariant, however, one's interest in politics, their news consumption and even their income are likely to vary over the course of one's life (Prior, 2005). Since cognitive ability is likely to influence the knowledge of citizens by enabling them to understand and process otherwise highly abstract pieces of political information (Luskin, 1990), it is very likely that for any given level of opportunity, increases in motivation will cause unequal gains in political knowledge for cognitively unequal people. This enables a feedback loop that, in time, should broaden the gap in knowledge between the more informed and the less informed (Jerit, Barabas, and Bolsen, 2006). It is precisely this type of inferences that can be tested by specifying moderating effects and reducing the reliance on questionable proxies in the operationalization of the triadic model.

The effect of cognitive ability on political knowledge can only be indirect, and any model explaining the acquisition of knowledge needs to be specified accordingly. There is no

\(^{12}\) Of course, affirmative action policies will not change someone's gender or age, but they can reduce the structural advantage of currently dominant socio-demographically defined groups.
political information contained in cognitive faculties, thus, ability can only assist citizens in their efforts to understand political information but it cannot provide knowledge directly. We propose a model in which cognitive ability moderates the effects of all other variables on political knowledge (see Figure 1 below).

We used a panel of respondents - Dutch citizens of age 18 and over - who left an email address and indicated consent for being occasionally re-contacted after completing a Dutch online Vote Advice Application (www.kieskompas.nl, see Krouwel 2012 et al for a full description). Users of this website – over 3 million voters - were asked to opt-in during the Dutch local elections in March 2010, while the other respondents opted into our panel during the parliamentary elections in May/June 2010. Since these respondents consented to renewed contact and have used an election website, we expect above average political interest and also higher propensities to respond to survey participation.

Our data was collected by recontacting this panel between March 5 and March 15, 2012 as a separate wave, using the online services of SurveyGizmo (http://www.surveygizmo.com). Out of the initial sample of 8145 respondents, 3466 participated in this wave of our panel, 70 percent of them filled out the questionnaire before we sent the first reminder on March 11. A second reminder was sent on March 13, and the survey was closed on March 15 when the daily increase in the response rate fell below 1 percent.

In line with our expectations, our sample is highly educated and more interested in politics than a random sample of Dutch citizens, they are predominantly male. Education was
measured using a 6- category ordinal variable with labels corresponding to the educational qualifications specific to the Dutch education system. Education was used as continuous scale in all subsequent analyses. Sixty-four percent of our sample is in the highest two categories of education, twenty-six percent are in the middle categories and only ten percent in the first two. Six percent of our sample reported low and very low levels of political interest as opposed to fifty-eight percent who reported high and very high levels, while the remaining thirty-six percent are in the middle category of “moderately interested”. The average age in our sample is 49.7 and only 25.5 percent are female.

The questionnaire has four main batteries of questions. The first battery is a political knowledge quiz of seven questions with no variation of question formats across respondents. The second battery consists of eight political knowledge questions with formats (open ended, multiple choice and true/false) varying across respondents on a random basis. The assignment of question formats was done using the random generator tool provided by SurveyGizmo. The third battery of items includes socio-economic, demographic and attitudinal variables; additionally, we asked the respondents whether they remember what they were doing on September 11, 2011 (with an open ended follow up asking them to write a few words describing what they were doing) to tap into their memory skills.

Finally, the 7 cognitive ability items were custom made, following the patterns found in the Raven's Advanced Progressive Matrices (Raven, 1936). We employed an aggressive strategy of discouraging non-responses using the “soft-required” option provided by SurveyGizmo: respondents were not allowed to leave answer fields blank unless they ignored a warning message popping up on top of the page upon first clicking the “next page” button. However, clicking the “next page” button the second time would let the respondents navigate away from the respective page regardless of the number of fields left blank. This helped us maximize the number of guesses, thus increasing the variance in the success of guessing.

Due to the time constraints associated with large N surveys, we used a reduced item cognitive ability test. It is worth noting, however, that our study relies on the statistical power of large samples to overcome the loss of reliability that comes with measuring a given construct with fewer items than the established norm. All seven items are listed in part II of the Appendix.
Method and Results

We use the additive approach for constructing our political knowledge scales and our cognitive ability scale. Due to the relatively large number of political knowledge items in our database, we could construct separate knowledge scales for the first battery of items (range: 0-7) and for the battery of items with randomized format (range: 0-8). The vast majority of all respondents performed better on multiple choice and true-false questions than they did on open ended ones, which calls for the inclusion of a control for format (the count of open ended variables per respondent) in all subsequent fitted models.

Cognitive ability was measured by counting the correct responses to 7 items. We obtained a normally distributed scale with a mean of 2.9 and a standard deviation of 1.63. The Cronbach's Alpha of .51 shows that the internal consistency of the scale is less than optimal (Cronbach, 1951), yet well above the value we would intuitively expect from a scale that combines less than one fifth of the number of items that constitute the norm in cognitive ability measurements (Carroll, 1993). Our scale correlates positively with education ($r$=.23, see Figure 2. below)\textsuperscript{13}, the two TIPI (Gosling et al, 2003) “openness to experience” items

\textbf{Figure 2.}

Smoothed line describing the relationship between education and cognitive ability

\textsuperscript{13}The literature on cognitive ability usually reports stronger correlations with educational attainment (for a comparable estimate, $r$=.32, see Johnson, Deary and Iacono, 2009). We attribute the weakness of our correlation to the reduced number of items for measuring cognitive ability, the strict focus on non-verbal abilities and to convenience sampling.
(r=.08) and negatively with age (r=−.33) self-reported religiosity (r=−.12) and is completely independent from gender (p=.9).

We run OLS regression models, with political knowledge as dependent and cognitive ability, education, interest and news consumption as predictors while controlling for age and gender differences. In line with previous literature on political knowledge, we find a significant positive relationship between education and political knowledge (see Table 1). The significant association remains even when controlling for cognitive ability, and including the joint effect of cognitive ability and education does not render either of the coefficients insignificant.

We find that the same results hold if we use the other knowledge variables (the separate scales based on battery 1 and battery 2, introduced in the previous section) as dependents and they are consistent across various model specifications. Furthermore, we find that the direct marginal effect of cognitive ability on model fit is modest at best; education is the better predictor of political knowledge (which may seem rather counterintuitive if it is indeed a mere proxy for the “cognitive ability” element of political knowledge).

Our results cast doubt on the existence of a substantively relevant direct effect of cognitive ability on political knowledge. Furthermore, we interpret these results as corroborating evidence for our claim that the causal relationship between education and the acquisition of knowledge does not dwell in the association between education and cognitive ability, but rather in the intrinsic value of education in the formation of democratic citizens.

We find modest decreases in the effects of educational attainment when cognitive ability is controlled for; the effect of household income also drops, but the difference is substantively insignificant. The slope of age increases more sharply, which is likely due to the decrease in cognitive ability at old age and the positive relationship between age and political knowledge. The explanatory power of the models changes by less than one percent when cognitive ability is included in the specification, leading to the conclusion that much of the alleged direct effect of cognitive ability is already accounted for by the variance of educational attainment. The strong relationship between educational attainment and political knowledge is mostly due to other causal paths linking formal education to democratic citizenship, and not to cognitive ability.
We move on to fitting our revised version of the triadic model to the data for the different levels of cognitive ability separately. This will allow us to see whether the effects of opportunity and motivation are homogenous across ability-defined groups. We link this to the knowledge gap theory due to the relative invariance of cognitive ability across age (with the notable exception of old age, when steep decreases are likely to occur) and to its virtual insensitivity to environmental manipulation. The feedback loop mentioned in the previously in this section should manifest itself by having stronger effects of opportunity and motivation for higher levels of cognitive ability. This would imply that people with higher levels of

<table>
<thead>
<tr>
<th>Table 1. Standardized OLS estimates for the effects of education and cognitive ability on knowledge in 3 pairs of nested models</th>
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</thead>
<tbody>
<tr>
<td><strong>Total knowledge</strong></td>
</tr>
<tr>
<td><strong>Intercept</strong></td>
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<tr>
<td><strong>Cognitive Ability</strong></td>
</tr>
<tr>
<td><strong>Opportunity:</strong></td>
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<tr>
<td><strong>Gender</strong></td>
</tr>
<tr>
<td><strong>Age</strong></td>
</tr>
<tr>
<td><strong>Education</strong></td>
</tr>
<tr>
<td><strong>Income</strong></td>
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<tr>
<td><strong>Motivation:</strong></td>
</tr>
<tr>
<td><strong>Newspaper</strong></td>
</tr>
<tr>
<td><strong>TV news</strong></td>
</tr>
<tr>
<td><strong>Interest</strong></td>
</tr>
<tr>
<td><strong>Attention</strong></td>
</tr>
<tr>
<td><strong>Controls:</strong></td>
</tr>
<tr>
<td><strong>Memory</strong></td>
</tr>
<tr>
<td><strong>Open-ended count</strong></td>
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<tr>
<td><strong>Adjusted R²</strong></td>
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<tr>
<td><strong>df</strong></td>
</tr>
</tbody>
</table>

***^p<0.05
ability would benefit more from their opportunity to get informed, and their motivation will translate into knowledge more efficiently than in the case of subjects with lower capabilities.

We find mixed results. The effects of opportunity are generally strongest for respondents of lower cognitive ability; the impact of education and income on the acquisition of political knowledge drops sharply and almost monotonously from lower ability to higher ability respondents. The estimates are not significant in the last two models also because of the fewer degrees of freedom, but judging by the effect sizes relative to the standard errors of the estimates, we believe that the statistical insignificance is not just an artifact of sample size.

Rather interesting is that our indicators of motivation appear to have stronger effects for higher ability people. Both political interest and news consumption have steeper slopes

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**Table 2. OLS estimates for the triadic model by cognitive ability groups**

<table>
<thead>
<tr>
<th></th>
<th>Cognitive ability &lt;= 1</th>
<th>Cognitive ability = 2</th>
<th>Cognitive ability = 3</th>
<th>Cognitive ability = 4</th>
<th>Cognitive ability = 5</th>
<th>Cognitive ability &gt;= 6</th>
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<tbody>
<tr>
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<td>0.19</td>
<td>0.46*</td>
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<td>(0.13)</td>
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<td>(0.18)</td>
<td>(0.24)</td>
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<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gender</td>
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<td>-0.18*</td>
<td>-0.03</td>
<td>-0.11</td>
<td>-0.22*</td>
<td>-0.37*</td>
</tr>
<tr>
<td></td>
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<td>(0.08)</td>
<td>(0.07)</td>
<td>(0.08)</td>
<td>(0.08)</td>
<td>(0.16)</td>
</tr>
<tr>
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<td>0.11*</td>
<td>0.13</td>
</tr>
<tr>
<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.08)</td>
</tr>
<tr>
<td>Education</td>
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<td>0.12*</td>
<td>0.17*</td>
<td>0.11*</td>
<td>0.00</td>
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<td>(0.03)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.06)</td>
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<td>0.00</td>
<td>0.02</td>
<td>0.08</td>
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<tr>
<td></td>
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<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.04)</td>
<td>(0.07)</td>
</tr>
<tr>
<td><strong>Motivation:</strong></td>
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<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>Newspaper</td>
<td>0.08*</td>
<td>0.15*</td>
<td>0.08*</td>
<td>0.10*</td>
<td>0.07</td>
<td>0.15*</td>
</tr>
<tr>
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<td>(0.03)</td>
<td>(0.04)</td>
<td>(0.04)</td>
<td>(0.07)</td>
</tr>
<tr>
<td>TV news</td>
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<td>0.02</td>
<td>-0.04</td>
<td>-0.02</td>
<td>-0.00</td>
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<td></td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.03)</td>
<td>(0.04)</td>
<td>(0.07)</td>
</tr>
<tr>
<td>Interest</td>
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<td>0.14*</td>
<td>0.13*</td>
<td>0.22*</td>
<td>0.19*</td>
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<tr>
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\*p<0.05

14 Most effects are not significant due to the low number of degrees of freedom; standard errors and effect sizes are included in the table for ease of comparison.
for the right hand side of the distribution of cognitive ability, and attentiveness to the political questionnaire appears to render a better performance on the knowledge quiz mostly for the respondents with higher levels of ability. It appears that low capabilities can partially be compensated with an increased exposure to political information (opportunity), but motivation rewards the more capable in a disproportionate manner. Were we to motivate the population at large to acquire political knowledge, the gap is likely to broaden.

Yet the most intriguing finding is the change in model fit: if we compare the explanatory power of the triadic model for the lowest category of ability to that of the highest category, we find that the latter is increased more than two-fold. While model fit varies between .20 and .26 for the subsamples of ability higher than 1 and lower than 6, it is as low as .15 for the lowest ability group and as high as .35 for the highest ability group. This result entails that we can predict what drives the acquisition of political knowledge for people of high cognitive ability, but we can only guess what makes a low-ability respondent become knowledgeable.

Discussion and Conclusions

The acquisition of political knowledge is said to be driven by a triad of interrelated factors, broadly defined by Robert Luskin (1990) with the capability – opportunity – motivation model. We revise the initial model by specifying the moderating effects of cognitive ability on all other effects and changing the status of educational attainment from indicator of ability (Carpini and Keeter, 1996) to a direct effect. We discussed at considerable lengths the likely implications of the use of imperfect proxies, such as educational attainment for cognitive ability, in such interactive models and demonstrated that improving the validity of the triadic model and reducing its inherent endogeneity problem is in fact feasible using large N surveys.

We designed a short test of cognitive ability that we included in a panel survey on a convenience sample of Dutch citizens. We fitted the revised triadic model to our data and found that most of the relevant predictors of political knowledge differ significantly across ability-defined groups. While the impact of motivation is stronger for high-ability respondents, the effect of opportunity appears to drop sharply with the increase in cognitive ability. Furthermore, we find that the political literacy of the more cognitively capable is a lot more predictable than that of their less cognitively capable peers.
Our findings have significant methodological, conceptual and normative implications. From the methodological point of view, we conclude that the intercorrelations between the indicators of the three elements of the original triadic model make it virtually impossible with traditional statistical tools to discuss any causal relationships underlying the acquisition of political knowledge. Moreover, the use of education as a proxy for cognitive ability leads to the hasty conclusion that ability is the main driving force of political knowledge; furthermore, it prevents us from seeing the chiefly indirect effect of cognitive ability on the acquisition of knowledge and it hinders our ability to specify interaction effects altogether. Such overestimations and misinterpretations of the impact of ability distorts our understanding of the origins of political literacy and contributes to a fatalistic view of the prospects of representative democracy (assuming that the functioning of democracy is contingent upon the political sophistication of its citizens); after all, cognitive ability is the one element of the triad that cannot be externally manipulated by any feasible means. In this regard, our results are mostly encouraging; cognitive ability has a rather weak direct contribution to the acquisition of political knowledge. However, we find that cognitive ability moderates most relationships between the elements of the triad and political knowledge.

Future research (possibly the future of this manuscript) should directly test moderation using an interaction model specification of the triad. The authors also hope to replicate the results with the American National Election Study data used by Luskin (1990). The reason this dataset appears appropriate for replication is because it did not use education as a proxy for ability. Rather it includes a direct, though sub-optimal, measure of cognitive ability through the ANES interviewer debriefing questionnaire where the interviewer was asked to rate the respondent’s intelligence. The availability of this, and also an education, measure makes the dataset suitable for cross-validation of the Dutch results.
D6. Issue Publics Revisited: a Brief Research Note Assessing the *Dimensionality of Political Knowledge*

Paul T. Weith, Central European University
Andre Krouwel, Free University of Amsterdam

**Introduction**

We note that under the methodological assumption of uni-dimensionality, any empirically multi-dimensional construct will fail at explaining most of the variance it was theoretically expected to. We have previously shown that the way we measure political knowledge is likely to influence the size and robustness of information effects through format-effects on the reliability and validity of knowledge scales. As mentioned in previous sections of the report, it is generally assumed that political knowledge is unidimensional, and a considerable part of the literature on political knowledge relies on theories the accuracy of which rests on the assumption of a one-dimensional construct.

**Research Question**

Is political knowledge truly one-dimensional?

**Kieskompas Data**

We use the Dutch online (opt-in) Vote Compass data, collected in March 2012 and described in the previous chapter of the report. Our sample of N=3400 respondents generated data on six knowledge items on economics and finance, six on migrational and international issues and five on welfare and pensions. The two batteries of items on political knowledge were also designed to test three “types” of knowledge: chronic knowledge, referring to political issues that have mostly remained unchanged over a significant period of time; name/office recognition, where respondents were asked to link the name of a politician to their respective offices and vice versa; and knowledge about current affairs. We randomized the format of the questions assigned in one of the two batteries of knowledge items.
Empirical Tests

The standard approach to inquiring about the dimensionality of constructs is confirmatory factor analysis. However, we chose a more iterative approach that allowed us to have an intimate understanding of the results at every intermediate step of the analysis.

Test I.

We first computed polychoric correlations between all possible four-item additive knowledge scales and the remaining item from their domain category (economy, international or welfare) or “type” (chronic, current, name recognition). The resulting coefficients reflect the extent to which type-specific or issue-specific scales are internally consistent; we call them in-correlations. We further computed polychoric correlations between these four-item scales and all the items pertaining to other issue-domains or types (ex: four-item scales drawn from the “economy” category were correlated with all items outside the “economy” category). These scales reflect to what extent issue-specific or type-specific scales are consistent with a notion of a general political knowledge scales; we call them out-correlations because they reflect scale consistency across issue-domains or types.

Finally, we compare in-correlations to out-correlations with an independent sample t-test. We expect the former to be systematically stronger than the latter if political knowledge is multi-dimensional AND we successfully predicted its underlying dimensionality. We expect no significant differences if political knowledge is one-dimensional; significant results that do not point in the direction we predicted point to a multi-dimensionality that we did not anticipate accurately.

Test II

The second step of our analysis looks directly at the impact of issue-domain specificity and type-specificity on the reliability of scales, measured with Cronbach's Alpha. We compute Alpha for additive scales of 5 randomly drawn items from the full set of 17 knowledge questions and record count of items drawn from each of the three issue-domains and types. We repeat the procedure 5,000 times and save the values of Alpha and the values of the count variables into a separate matrix. We try to predict Alpha using the count variables with separate bivariate linear regression models for “types” and “issue-domains”. We also
correlate Alpha with a measure of type-specificity and issue-domain specificity, operationalized as the maximum of the three count variables corresponding to “types”, respectively “issue-domains”.

Note that we generally expect a positive slope for all count variables, under the assumption that the “type” and “issue-domain” categories, designed to approximate the possible dimensions of political knowledge in Dutch politics, were accurately anticipated prior to data collection. If, however, some categories are more meaningful than others, we are likely to find some positive and some negative slopes. This would happen due to the comparative nature of our regression tests conferred by the fact that each count variable is a perfect linear combination of the other two. Statistically or substantively insignificant effects are expected either if political knowledge is indeed one-dimensional or if none of the categories we anticipated corresponds to real dimensions of knowledge.

Finally, the relationship between the specificity variables (maximum of the count variables for “types” and “issue-domains”, respectively) and Alpha is highly sensitive to the empirical accuracy of our preselection of knowledge dimensions. A null result is highly inconclusive for these tests, because it can be interpreted as evidence for one-dimensionality, or for a poor preselection of issue- and type- dimensions. A significant positive relationship indicates correctly anticipated multi-dimensionality; a significant negative relationship indicates that at least one of the anticipated dimensions is very inconsistent, or even multi-dimensional. However, the slopes we find with our bivariate regression models with the count variables as predictors can be used to diagnose such results.

Results

We find that in-correlations are systematically stronger than out-correlations for the categories of knowledge “type”, both separately for types A,B,C and for the three of them pooled together. The pattern is less clear in the case of issue-domains; overall, in-correlations are not significantly stronger than out-correlations. Knowledge about economic or financial issues correlates better with items on non-economic issues than it does with other economic knowledge items. This leads to the failure to reach statistical significance for our t-test on in-correlations and out-correlations for the pooled issue-domains.
Table 1. In-correlations and Out-correlations for Issue-domains and Knowledge Types

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<tr>
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<th>In-correlations vs. out-correlations for issue-domains</th>
<th>In-correlations vs. out-correlations for type of knowledge</th>
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\textsuperscript{a}=p<0.05; \textsuperscript{b}=p<0.1; a=economics and finance; b= welfare pensions; c=international and migrational issues; A=chronic knowledge; B=current issues; C=name/office recognition.

Using two out of three count variables for both “types” and “issue-dimensions” (to avoid computational redundancy) we managed to predict 10 percent of the variance in alphas for “issue-domains” and 30 percent for “type”. The bivariate regression coefficients are significantly different from 0, with the exception of that of issue-domain specificity and that of chronic knowledge. The former is likely due to the economic issue-domain that stands in sharp contrast to the other two issue-domains. The more items are drawn from the “economy and finance” category, the lower the reliability of the resulting scale. We intuitively interpret this result as evidence for the heterogeneity of political-economic interests that respondents may have: few people are interested in all things related to economic or financial matters; however, many people are attentive to political information that is relevant for their particular field of activity. Knowledge of economic policies related to agricultural subsidies does not necessarily entail knowledge about the taxation of automotive imports.

The same explanation is likely to hold for the surprisingly strong negative effect of type-specificity on Alpha. We find that the more items we draw from the “current affairs” category, the lower the reliability of the scale; respondents are not likely to be interested in all the political topics covered in an over-saturated news media environment, and it appears wrong to assume that whoever is familiar with the issues discussed in the prime-minister's latest meeting with the German chancellor should also know something about possible changes to the pension system. The negative effect of the “current issues” category on Alpha appears to be strong enough to contaminate that of type-specificity, overcompensating not only the strong positive effect of “name recognition” but also the sizeable errors in the relationship between “chronic knowledge” and reliability.
Conclusions and Discussion

The reliability of political knowledge scales is partially dependent upon their type- and domain-consistency. Composite scales are often less reliable than single issue ones. We found support for the existence of issue publics (for “welfare” and “migration”, but NOT for “economy”), and found divergent yet very robust evidence for the effect of “type” (chronic knowledge, knowledge of current affairs, name-recognition) on reliability. These results call for a rethinking of both theoretical and empirical approaches to studies involving political cognition. Issue-specific scales may enable us to find empirical support for theories that were previously wrongly discarded due to suboptimal measurements resting on unsubstantiated assumptions. Political literacy may be more important for the functioning of democracy than we think. Nationally representative data is needed for further generalizations of the results.

Finally, it is possible that our results only reflect the differential quality of the questions we used for measuring the various types of knowledge, or the knowledge about the various issue-domains. However plausible such criticism may be, our 3-parameter logistic IRT model reveals no major imbalances at the level of difficulty or discrimination between the items within the separate categories we considered.
References


## Appendix

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Raw scores for item difficulty (d_v1; d_v2 and d_v3), Cronbach's alpha (alpha), Loevinger's H (H), single item impact on alpha and H (a_v1; a_v2; a_v3; H_v1; H_v2 and H_v3)
Correlations between political knowledge items and education (c1), gender (c2, female=1) and age(c3); standard errors in brackets. The 37 rows of data represent the 37 elections in our sample
Item 1. Correct answer: 5
Item 2. Correct answer: 5
Item 3. Correct answer: 7

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1.  2.  3.  4.  
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[Box Diagrams]
Item 4. Correct answer: 8

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Item 5. Correct answer: 7

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Item 6. Correct answer: 8

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Item 7. Correct answer: 6