

Research Note

Can Information Increase Turnout in European Parliament Elections? Evidence from a Quasi-experiment in Denmark*

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Abstract

We examine the effect of information on turnout at a European Parliament election in Denmark. We utilize a quasi-experimental design to sidestep the substantial problems related to causal inference associated with identifying the effect of information. Specifically, we look at a group of Danish first-time voters, some of whom were exogenously exposed to information in the run-up to the 2014 European Parliament election, by participating in a one-day workshop about EU (European Union) politics. We find that those who participated were more knowledgeable about and more likely to vote in the upcoming European Parliament election. This suggests that increasing political participation in the EU could, in part, be a matter of exposing the European public to more information about EU politics.

Keywords: European Parliament; experiment; turnout

Introduction

The right to vote is fundamental in any representative democracy, however, far from everyone exercises this right. This is especially true in European Parliamentary elections. In fact, since the 1999 election less than half of all eligible European voters have decided to vote in these elections – a particularly troubling fact in light of the increased importance played by the European Parliament in the legislative process (Kohler, 2014). Why do such a large proportion of voters decide to stay at home? In this article we try to answer at least part of this question, by examining the merit of one simple explanation; that an important reason voters do not go to the polls is that they need more information about the European Parliament elections. That is, if voters became more informed about the EU (European Union) election they would be more likely to turn out.

This explanation might not seem that controversial. After all, information and voting are intimately related. Theoretical models of voting suggest that without a minimum of information about a given election and informed expectations over various outcomes of this election, voters will have a hard time determining who they should vote for in order to realize their desired outcome (Matsusaka, 1995) and might therefore be better off delegating their decision to those who are more informed (Feddersen and Pesendorfer, 1996). Further, since voters are generally ill informed about European politics (Karp *et al.*, 2003), it is plausible that these theoretical arguments could be particularly powerful in the context

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of European Parliamentary elections. On the other hand, there are also reasons to believe that information should be a less important determinant of turnout in European Parliament elections. Specifically, these elections have long been viewed as second-order elections, and as such voters' motivation for showing up might be tied to national considerations rather than knowledge of the European issues at stake in the election (Flickinger and Studlar, 2007; Hix and Marsh, 2007; Corbett, 2014). In short, the extant literature does not make any uniform predictions about the effect of information, making it interesting to delve into whether there is such an effect.

Unfortunately, establishing a causal effect of information about the EU on turnout is complicated by the fact that the effect of being informed about the EU is hard to empirically sieve out from other important factors. Voters who decide to vote might very well self-select into information in order to make a more reasoned choice, and some factors which make it more likely for voters to turn out, such as education, might also make them more likely to self-select into or be exposed to information. Using the vocabulary adopted by the experimental tradition (Dunning, 2012; Gerber and Green, 2012), information can be said to be 'endogenous' to the calculus of voting. In this article we try to address this fundamental causal inference problem by applying a quasi-experimental approach (Shadish *et al.*, 2002) to the study of turnout in European Parliamentary elections. Specifically, we look at a group of Danish first-time voters who were as-if randomly exposed to information about the EU in the run-up to the 2014 European Parliament election.

In the spring of 2014, a number of high school students who were eligible to vote participated in an EU workshop ($n=270$) – a one-day event which featured lectures, debates and other types of interactive learning about the EU. Two weeks after the event, high school students who participated, as well as some students from the same high school who did not participate, filled out a survey, which amongst other things included an item about voting intentions. We estimate the causal effect of the EU workshop by comparing the participating students' intentions to vote with students who did not participate. We find that those who participated in the EU workshop were more knowledgeable and more likely to report an intention to vote. Further, we show that on important socio-demographic factors, the students who received information about the EU were no different than the students who did not. This suggests that receiving the information about the EU, not differences in the types of students who were given this information, had an effect on turnout.

The article thus contributes to the extant literature on political participation by presenting persuasive evidence that information has an effect on turnout, also at second-order elections like the European Parliament elections. Accordingly, the findings suggest that increasing political participation in the EU could, in part, be a matter of exposing the European public to more information about the EU. There is some hope then, that to the extent we succeed in informing citizens, and perhaps in particular younger citizens, about the EU, turnout in the European Parliament elections might substantially increase in the future. In addition to this, the article demonstrates that quasi-experimental methods can be fruitfully used to advance the study of the EU electorate.

I. Turnout in European Parliament Elections

When explaining turnout in European Parliament elections, the extant literature has primarily focused on country-level factors, such as the usual level of turnout in the country,

presence of compulsory voting, the level of subsidies the country receives from the EU or the timing of the elections (Franklin, 2001; Mattila, 2003; for an important exception, see Bartkowska and Tiemann, 2015). In the same vein, a recent study found that the changing generational composition is an important factor in explaining turnout in European elections (Bhatti and Hansen, 2012).

However, the effect of information on turnout in EU elections has received relatively little attention in the literature. Instead, being informed about the EU has been used to explain attitudes towards European integration (Clark and Hellwig, 2012), responsibility attribution (Hobolt *et al.*, 2013) and democratic quality (Karp *et al.*, 2003). In fact, while the previous literature has identified a positive correlation between political knowledge and turnout (Clark, 2010), no study has tried to grapple with the problems of identifying the effect of information on turnout at European Parliament elections. This is surprising for at least two reasons: first, EU officials, politicians as well as journalists covering EU politics often point to the different amount of attention which European and national politics receive when explaining low turnout rates (Gattermann, 2013). Second, information has been found to play an important role in the decision to vote in the general literature on turnout (Lassen, 2005; Larcinese, 2007). Accordingly, it seems interesting to complement existing perspectives on what drives turnout in European Parliament elections by studying the effect of information.

Related to this, it is important to note that one cannot simply infer that there is an effect of information on turnout in EU elections based on the fact that information has an effect at national elections. One of the most prominent models of voting behaviour at European elections is the second-order model, which suggests that for most voters, EU politics is solely a function of national political considerations and grievances (Reif and Schmitt, 1980; Hix and Marsh, 2007; Hobolt, 2007; Corbett, 2014). That is, voters use EU elections to send signals to national politicians. Based on this, one might also expect that being informed about the EU has no effect on turnout at European elections, since voters do not vote based on what they know about the EU, but what they know about national politics. Conversely, recent studies have found that in countries which supply more information about the EU, second-order voting is less prevalent (De Vries *et al.*, 2011a; Tillman, 2004), and that second-order voting is less prevalent amongst those who are more politically sophisticated (Hobolt, 2009; De Vries *et al.*, 2011b). These findings suggest that when voters decide whom to vote for in the European Parliamentary elections, it matters whether they know a little or a lot about the EU.

Similarly, information might very well matter for whether voters decide to show up at all. The critical point here is that an investigation of the effect of information on turnout is not merely a reaffirmation of what we already know; in the context of EU politics we do not know whether information affects vote choice, and the previous literature points in different directions.

Identifying the Effect of Information

Why might information matter for turnout? Theoretical models of the calculus of voting have often found that as voters' information about politics increase, so does their incentive to vote. For instance, Feddersen and Pesendorfer (1996) argue that when some citizens have little information, they 'delegate' their electoral decision to high information

voters by not turning out. Similar points have been made by Matsusaka (1995) and Ghirardato and Katz (2002), who show that low levels of information might increase the probability that the voters make the wrong decision if they decide to show up, and, consequently, decrease the expected utility of voting. These arguments seem to be very applicable to the context of European Parliament elections. For instance, if some voters know their national parties' position on domestic issues, but are uncertain about their position on EU-related issues, the outcome of their vote becomes more uncertain at European Parliament elections, and accordingly, they may be more comfortable leaving the decision to those who are more knowledgeable.

The empirical literature on the effect of information and turnout has primarily been based on the strong correlation between education and voting, arguing that such correlation can be explained in terms of information. Accordingly, empirical tests have usually centred on demonstrating a causal effect of education on political participation (see, for example, Wolfinger and Rosenstone, 1980; Milligan *et al.*, 2004; Kam and Palmer, 2008). While this is an important line of inquiry, an effect of education on turnout does not necessarily imply an effect of information on turnout. As such, one can easily imagine that education affects turnout in multiple ways, not just through information. Education does inform, but it also shapes one's preferences for public policy and moulds one's social network (Nie *et al.*, 1996; Brady *et al.*, 1995), factors which we know are important determinants of turnout. Hence, to be certain that information is the causal mechanism which underlies the effect of education, it needs to be isolated from the various other pathways through which education can plausibly be thought to affect turnout.

Partly as a response to this, some empirical studies have looked more directly at the effect of exposure to political information on turnout. Larcinese (2007) looks at whether exposure to different media sources, such as subscription to a newspaper, affect turnout through political knowledge. Lassen (2005) shows that voters in city districts who were exposed to information about an upcoming referendum were also more likely to vote in the referendum (see also Chong *et al.*, 2015). We follow these studies in trying to investigate the effect of information, by looking at differences in direct exposure to information, rather than differences in educational attainment.

One advantage of using exposure to information rather than education to study the information–turnout relationship, is related to causal inference. Both education and information are spuriously related to turnout; people who plan to turn out plausibly expose themselves to more political information, and those who self-select into education are more likely to have a strong socio-economic background as well as other less observable characteristics, such as a 'civic' personality, which also plays a role in deciding whether to vote (Schlozman *et al.*, 1995). In the context of EU elections, studies have shown that people with a stronger 'European identity' are more likely to be educated and well informed, which might also increase their propensity to turnout (Agirdag *et al.*, 2012). Even so, it is easier to identify the causal effect of exposure to information than it is to identify the causal effect of education.

People are rarely exposed to an education without their expressed desire and consent. On the other hand, people are sometimes exposed to information without any expressed desire or consent. In these cases it is possible to identify the effect of information, by utilizing the fact that the information was 'assigned' to individuals without any specific regard to who received it, making it less likely that those who did receive it were different

from those who did not. In this article we utilize a case like this, in which information was quasi-randomly given to some voters but not others (see also Lassen, 2005). Specifically, we utilize the fact that some Danish high school students were exogenously exposed to a one-day seminar on EU politics just before the 2014 European Parliament election.

II. An Exogenous Shock to Information: A One-Day Seminar on EU Politics

In early April 2014, the EU movement hosted five full day workshops on the EU at five different Danish high schools. The workshops comprised various activities that all dealt with EU politics, including panel debates amongst politicians, writing of debate pieces, role playing, etc. The information was, amongst other subjects, on the organizational structure of the EU, its primary areas of responsibility and the most important political issues of the campaign.

In a quasi-experimental design, we utilize these workshops as a source of as-if random variation in exposure to information about the EU. That is, we take advantage of the fact that two otherwise similar sets of students were assigned to different levels of information about the EU. Via post-workshop surveys, we compare the participating high school students (treated) with statistically similar students from the same high schools, who did not participate in the workshop (control).

To identify a causal effect in a quasi-experimental setting, we need to fulfil three core assumptions: as-if random allocation of treatment, excludability of the treatment and non-interference between the units (Gerber and Green, 2012; Dunning, 2012). We discuss the extent to which the present quasi-experiment conforms to these assumptions below.

We did not have direct control over the allocation of students into the treatment group, and as such we could not ensure random allocation of the treatment (participating in the workshop). Instead, a coordinator at each high school allocated specific classes for whom it was mandatory to attend. Subsequently, we chose classes with similar characteristics for the control group (for instance, similar levels of social science training and same cohort). In spite of this non-random allocation procedure, we argue that the assignment of classes to either treatment or control was in fact independent of the classes' pre-treatment intention to vote in the election. First of all, in corresponding with the high school coordinators it became clear that there were many exogenous factors that contributed to whether or not a class could participate. For instance, one of the coordinators said that treatment assignment was due to 'whether the class had other obligations that day, whether it had module-surplus and whether the relevant teacher had made other arrangements', and accordingly that participation 'was unrelated to whether the class had or were going to be taught about the EU'. Further, participation was mandatory at class level, so students could not opt in or out of the treatment at their own discretion, providing another source of exogenous variation. Accordingly, this design works around one of the key sources of bias in studies of information effects, namely, self-selection into information. Finally, as we show below, the treatment and control group is balanced on a number of individual-level covariates, suggesting that as-if random assignment is not an unreasonable assumption.

Even with as-if random allocation to the control and intervention groups, an unbiased estimate of the causal effect of information is dependent upon the excludability assumption. That is, the intervention group should not be affected by other factors than the

intervention itself, which in our case is exposure to information about the EU. Here it could be objected that the causal effect of the treatment is not driven by information as such, but rather by the fact that the workshop embedded certain norms of electoral participation in the participants through socialization. While it is admittedly challenging to tease out the exact mechanism, there are at least two reasons to think that any causal effect we unearth is due to information rather than the embedding of certain norms in the participants. First, as we show below, the workshop strongly affected participants' knowledge about the EU, and second, the workshop was only a one-day experience, and students participated in their familiar school environment with their classmates whom they already knew well prior to the event. If a one-day workshop was to embed new norms through socialization, it seems likely that the workshop would need to merge participants into new social settings and allow them to meet new peers. This workshop did not.

Finally, the validity of the design is predicated upon the assumption of non-interference. That is, the intention to vote for each student should be independent of the assignment of other students to either treatment or control. One can argue that the assumption of non-interference is violated because of potential spill-over effects from the intervention to the control group. As two weeks passed between the workshop and the survey, students had plenty of opportunities for cross-class interaction in which some of the effect could be transmitted to students in the control group. However, to the extent that such interference is present, it will produce a conservative bias, as it would likely increase the control group students' level of information, and in turn, their intention to vote.

On balance, the present design is likely to uncover the treatment effect of information on intention to vote in the European Parliament elections. This makes the study unique in the sense that it is the first study to leverage quasi-experimental methods, and therefore the first study which can claim to reliably estimate the effect of information in this setting.

III. Data

In order to assess the effect of the information provided during the workshop, surveys were distributed to both intervention and control groups approximately two weeks after the workshop. It was practically unfeasible to allow a longer time span between the workshop and the surveys, as classes were moving into exam periods, and it was not possible to identify students after the exam period, as most of them graduated from high school following their exams.¹ The surveys were handed out in paper format by the social science teacher and filled out in a classroom setting. This was done approximately one month prior to the European Parliament elections. Teachers were instructed to follow the same procedure and, to avoid bias, not explicitly mention that the survey was a follow-up on the EU workshop. Surveys took approximately ten minutes to complete, and there were no reports of problems in filling out the surveys.

¹ While a longer time span could be desirable to assess long-term effects, and in particular post-election surveys could be desirable to assess reported vote rather than intent to vote, the full two-week time span is nonetheless a substantial improvement from immediate post-workshop surveys, partly because the two-week span limits problems concerning social desirability bias and partly because two weeks should be enough time for the information provided to settle into the long-term memory of the students.

Dependent Variable

The primary dependent variable used in the survey was an item on voting intention for the European Parliament and the unified patent court referendum.² Translated directly from Danish, the question was expressed as follows:

On the 25th of May there is a European Parliament election as well as a referendum on the unified patent court. At the last election, a little more than half of all voters voted. How likely is it that you will vote?

Answers were placed on a 0–10 scale ranging from ‘Not at all likely’ to ‘Very likely’.

Respondents could also answer ‘Do not have the right to vote’, as some of the high school students were below the voting age of 18 at the time of the election. Evidently, the question relates to voting intentions rather than voting behaviour. Accordingly, it seems likely that there is an upward bias in the answers due to social desirability (Achen and Blais, 2015). However, we have no reason to believe that any potential bias would affect the treatment and control groups differently, and hence, it poses no threat to the identification of the effect of information. As such, the effect on voting intentions should be closely related to the effect on voting behaviour.

Besides voting intention, the survey included a secondary dependent variable used to corroborate our findings. The variable was a simple index indicating the proportion of correct answers to four factual questions.³

Covariates

Along with the dependent variable we measured a number of covariates at three different levels: (1) high school level, (2) class level and (3) individual level. At the high school level we simply included individual indicators for each high school as it seems likely that heterogeneity between high schools in different parts of the country could correlate with voting intentions.

At the class level we recorded two different parameters: social science level and cohort. When enrolling in their high school, students were able to pick classes with different levels of social science training; they could choose either level 1, 2 or 3. We also recorded which cohort the student was part of; that is, whether they were first-, second- or third-year students at their high school. Both can plausibly be assumed to correlate with voting intentions, the latter primarily through informational accumulation and the former through a whole host of factors, including information and, probably primarily, interest in politics and societal matters.

On the individual level, we employ three different control variables: age, gender and parents’ educational level. Despite the control for cohort, there may still be up to two years of difference in age within the same cohorts, as there is some variation in when children start in school, and additionally, the Danish school system operates with an optional tenth grade immediately before high school. As age is well known to correlate with voting

² In the Danish 2014 European election, voters were also asked about whether Denmark should participate in the unified patent court. The measure was supported by 60 per cent of the Danish voters.

³ The four questions were: ‘How many countries are members of the European Union?’, ‘Who is the president of the European Council?’, ‘Which of the following policy areas takes up the largest part of the EU budget?’ and ‘In which of the following areas does Denmark not have an opt-out?’

behaviour (Bhatti and Hansen, 2012), we have included it as a control. Similarly, to make sure the estimate is not driven by gender differences, gender is controlled for as well. Lastly, as Kam and Palmer (2008) have shown, the educational level of parents is a significant factor in determining voting behaviour. We include parents' educational level as two separate variables, one for mother's and one for father's education.

Descriptive statistics on all covariates can be found in the Appendix. All variables are rescaled to go from zero to one.

IV. The Effect of Information

As discussed above, one of the necessary conditions for identifying the effect of information on intention to vote in the present study is that the assignment of classes to the treatment and control group was as-if random. While we argued above that this was the case, it is impossible to know for sure. However, one can examine the plausibility of this claim by looking at whether those classes and students assigned to treatment differed on some pre-treatment variables which might influence intent to vote – a so-called balance test. We conduct such a test in Table 1. As can be seen there is only one statistically significant difference across the covariates, which is that slightly more students in the control group

Table 1: Were Treatment and Control Classes Balanced?

	<i>Treatment</i>	<i>Control</i>	<i>Std. difference</i>	<i>P-value</i>	<i>n</i>
First year	0.17	0.29	−0.27	0.45	270
Second year	0.41	0.51	−0.19	0.69	270
Third year	0.42	0.20	0.55	0.37	270
Social science level	0.81	0.59	0.71	0.07	270
Gender	0.71	0.63	0.15	0.29	270
18 years old	0.27	0.37	−0.21	0.48	270
19 years old	0.45	0.48	−0.06	0.77	270
20 years old	0.28	0.15	0.36	0.33	270
<i>Mother's education</i>					
Elementary school	0.05	0.06	−0.02	0.85	270
High school	0.09	0.12	−0.09	0.52	270
Vocational degree	0.08	0.13	−0.16	0.12	270
Shorter tertiary	0.44	0.44	−0.00	0.97	270
Longer tertiary	0.27	0.17	0.24	0.12	270
Other	0.01	0.03	−0.07	0.41	270
Don't know	0.06	0.05	0.05	0.69	270
<i>Father's education</i>					
Elementary school	0.09	0.06	0.12	0.44	270
High school	0.05	0.10	−0.18	0.09	270
Vocational degree	0.23	0.26	−0.06	0.73	270
Shorter tertiary	0.28	0.22	0.15	0.26	270
Longer tertiary	0.28	0.27	0.03	0.87	270
Other	0.01	0.05	−0.20	0.03	270
Don't know	0.07	0.05	0.08	0.61	270

Notes: *p*-value obtained from difference-in-means estimator with robust standard errors clustered at the class level. Standardized difference computed as difference in means divided by standard deviation of control group.

reported that their father had ‘other’ education. This seems likely to be due to the fact that we are making multiple comparisons which increases the possibility of false positives. There also seems to be a slight imbalance in the social science level, specifically those in the treatment group seem to have chosen a slightly higher level of social science training. While the difference falls just short of statistical significance, it is potentially problematic as level of social science training is likely correlated with intent to vote. However, as will become apparent below, whether or not one corrects for this imbalance, or any other imbalances, the results do not change.

Having examined the balance between the treatment and control group, we now turn to estimating the effect of participation in the workshop on turnout. To do this we use an IV (instrumental variables) approach, treating self-reported participation as an endogenous regressor which is instrumented by whether the student’s class was assigned to participate. This estimation strategy recovers the causal effect of participating, by only using the variation in self-reported participation which was caused by being in a treated class, taking care of any potential problems with non-compliance (for details, see Gerber and Green, 2012, pp. 131–61). By doing this we take into account that certain types of students in the treatment group might have decided to shirk and stay at home. As a consequence, our effect estimate will only be generalizable to those students who complied with the treatment, which, however, was 93 per cent of the students (cf. the Appendix).

In column 1 of Table 2 we estimate the effect of participating using this IV approach. The estimated effect of participating is statistically significant, with an effect on voting intentions in the area of 0.1 on a scale from 0 to 1. Specifically, the model predicts that intent to turn out goes up from 0.67 to 0.78 if you go from control to treatment. In light

Table 2: IV (Instrumental Variables) Regression of Treatment on Intention to Vote

	(1)	(2)	(3)
Participated	0.11* (0.05)	0.09* (0.03)	0.09* (0.04)
Social science level		0.19* (0.06)	0.22* (0.07)
High school by cohort fixed effects		X	X
Female (ref: male)			0.04 (0.03)
19 years old (ref: 18)			0.11* (0.05)
20 years old			0.06 (0.08)
Dummies for mother’s educational level			X
Dummies for father’s educational level			X
Constant	0.67* (0.04)	0.51* (0.11)	0.46* (0.13)
<i>N</i>	270	270	270
<i>R</i> ²	0.02	0.10	0.15
RMSE	0.31	0.30	0.29
Classes (control/treatment)	(12/14)	(12/14)	(12/14)

Notes: Standard errors clustered on class level in parentheses. All variables recoded to go between zero and one. Variable participated instrumented in the first stage using a dummy indicating respondent was treated. * $p < 0.05$.

of the treatment being only a one-day workshop and that the surveys were distributed a full two weeks after the treatment, this seems to be a quite substantial effect.

As described above there were some small imbalances across the treatment and control group. To investigate whether this is what is driving the results we introduce controls in columns 2 and 3 of Table 2. In column 2 we introduce only high school dummies and class-level controls and in the third model we use the full set of controls including the ones at the individual level.

The estimated effect of participating does not change after introducing the class and individual-level controls. Accordingly, moving from model (1) to (2), we see that the effect is not merely a result of high school and class-level differences between the intervention and control group, and further, when moving from model (2) to (3), we see that results are not driven by internal differences in age, gender and parents' educational level between the treatment and control group. This increases our confidence that the identified effect is unbiased. If the effect was driven in part by unobservable differences between the treatment and control groups, it seems likely that these unobservable differences would have manifested themselves in the observable covariates, and accordingly reduced the estimate substantially as more controls were added. In sum, we have strong reasons to believe that our models identify the causal effect of information on intent to turn out.

Next, we briefly look at the effect participation had on a related variable; the proportion of correct answers to four factual questions about the EU. Results are reported in Table 3. This variable works as a proxy for knowledge and information, and can accordingly be understood as a manipulation check. That is, a check on whether participation in the workshop actually affected how informed students were about the EU.

Table 3: IV (Instrumental Variables) Regression of Treatment on Knowledge

	(1)	(2)	(3)
Participated	0.27*	0.16*	0.17*
	(0.08)	(0.07)	(0.07)
Social science level		0.19	0.12
		(0.13)	(0.09)
High school by cohort fixed effects		X	X
Female (ref: male)			-0.02
			(0.02)
19 years old (ref: 18)			-0.07
			(0.04)
20 years old			-0.11*
			(0.05)
Dummies for mother's educational level			X
Dummies for father's educational level			X
Constant	0.25*	0.19*	0.37*
	(0.05)	(0.07)	(0.08)
<i>N</i>	270	270	270
<i>R</i> ²	0.02	0.10	0.15
RMSE	0.31	0.30	0.29
Classes (control/treatment)	(12/14)	(12/14)	(12/14)

Notes: Standard errors clustered on class level in parentheses. All variables recoded to go between zero and one. Variable participated instrumented in the first stage using a dummy indicating respondent was treated. * $p < 0.05$.

We use a similar estimation strategy as we used in Table 2, simply changing the dependent variable from turnout to the knowledge index. As can be seen from Table 3, the workshop had a significant effect on knowledge in the expected direction.

V. Discussion and Conclusion

In this article we showed that exposure to information about EU politics increases the propensity to vote in European Parliament elections. We did so by taking advantage of five one-day workshops on European politics which groups of Danish high school students were as-if randomly exposed to in the run-up to the 2014 European Parliament election. This quasi-experimental design allowed us to arrive at a plausible causal estimate of the effect of information. Accordingly, this study takes a step forward from the existing literature, which has chiefly looked at how knowledge and education correlates with turnout at European Parliament elections.

Our study focused on a particular group of Danish first-time voters, and as such, the group distinguished itself from average European voters particularly in respect to their age and belonging to a particular nation-state which normally has higher than average turnout rates at EU elections. Thus, one of the chief concerns relating to this article pertains to the generalizability of the findings to the broader European public. For instance, young people are in general less likely to vote than the average citizen (Bhatti and Hansen, 2012). Hence, one could worry that a potential interaction effect between information and prior propensity to vote would limit the generalizability of the sample to the European public. However, this low propensity to turnout by young people, is in part cancelled out by the fact that turnout rates in Denmark are higher than average European turnout rates. Thus, the turnout rate of 18–21 year olds in the 2014 election to the European parliament was approximately 43 per cent – almost exactly the same as the average turnout rate for Europe as a whole. A fact which should increase our confidence in the generalizability of the findings.

In spite of this ameliorating fact, a study of high school students in Denmark might seem less relevant in the larger context of a diverse European electorate. However, one needs to keep in mind that it is hard to find cases where the independent variable of interest, information, is assigned in a quasi-experimental way, so that we can identify causal effects. As a consequence, when we want to do causal inference, we sometimes need to settle for smaller samples in specific contexts. Put differently, the skewed sample is the price we pay in the present study for high internal validity. Accordingly, it is important that quasi-experimental studies like this one are supplemented by observational studies of several European countries, which have lower internal validity, but which can reveal more general patterns (see, for example, Clark, 2010).

What are the implications of the present study? That depends, first and foremost, on the exact nature of the mechanism involved. If, as we have argued is plausible, information is the driving force, it provides additional evidence to the claim that information about the EU has an effect on voters' propensity to turn out. What makes our findings particularly relevant to the debate on how to increase turnout at the country level is the magnitude of the intervention; if a relatively small intervention in the total stream of information that citizens are exposed to can significantly increase voting intentions two weeks after the intervention, one can easily imagine quite substantial effects as a result of small structural changes in the public and private coverage of EU-related matters. If news coverage and

public debates on EU policy were increased, it would provide an informational flow of a magnitude that far exceeds what was provided in the present study, and accordingly it seems likely that such changes in the informational landscape would similarly produce even more substantial effects on turnout. A highly desirable effect in the light of the low turnout rate at European Parliament elections.

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Appendix: Description Statistics

	<i>M</i>	<i>SD</i>	<i>n</i>
Participated	0.52	0.50	283
Intervention	0.55	0.50	283
Complier	0.93	0.26	283
Intent to vote	0.73	0.31	283
Knowledge	0.39	0.30	283
Female	0.67	0.47	280
Social science level 1	0.11	0.31	277
Social science level 2	0.44	0.50	277
Social science level 3	0.45	0.50	277
18 years old	0.33	0.47	283
19 years old	0.45	0.50	283
20 years old	0.22	0.41	283
First year	0.23	0.42	283
Second year	0.46	0.50	283
Third year	0.32	0.47	283
<i>Mother's education</i>			
Elementary school	0.05	0.23	276
High school	0.11	0.31	276
Vocational degree	0.11	0.31	276
Shorter tertiary	0.43	0.50	276
Longer tertiary	0.22	0.42	276
Other	0.02	0.15	276
Don't know	0.05	0.23	276
<i>Father's education</i>			
Elementary school	0.05	0.23	276
High school	0.11	0.31	276
Vocational degree	0.11	0.31	276
Shorter tertiary	0.43	0.50	276
Longer tertiary	0.22	0.42	276
Other	0.02	0.15	276
Don't know	0.05	0.23	276

Note: All variables recoded to go from zero to one.

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