Dieter Stiers, Marc Hooghe, & Ruth Dassonneville

Voting at 16: Does Lowering the Voting Age Lead to More Political Engagement?

Evidence from a quasi-experiment in the city of Ghent (Belgium)

Political Science Research and Methods, accepted

While youth suffrage is widely debated, the causal effects of being eligible to vote on adolescents' political attitudes are less well known. To gain insights into this question, we leverage data from a real-life quasi-experiment of voting at 16 in the city of Ghent (Belgium). We compare the attitudes of adolescents that were entitled to vote with their peers that just fell below the age cut-off. We also examine the effects of the enfranchisement at 18 years old. While we find an effect of youth enfranchisement on attention to politics, there is no evidence for an effect of enfranchisement on political engagement overall.

Keywords: Voting at 16; voting age; political interest; youth enfranchisement; youth suffrage; regression discontinuity design; Ghent

> *'Not interested in politics. I am only just 16 years old. Next elections I will study the political parties better. Currently I do not have the right to vote and therefore no interest.'* *(Respondent in the Ghent Study, autumn 2018)*

In several countries, it has been debated whether the voting age should be lowered from the current most often used age limit of 18, to the age of 16 (Zeglovits, 2013). Expectations regarding the beneficial effects of extending suffrage to 16-year-olds are based on two main causal mechanisms. First, it is thought that having the right to vote in itself, will lead to more political engagement in those who gain this right. Second, it is assumed that the age of 16 may be a particularly good choice when determining the minimal voting age because that phase of adolescence is a time of strong political socialisation. Engaging adolescents in the political process at this age would allow combining enfranchisement with citizenship education, which would have a stronger transformative impact than gaining the right to vote at an older age (Franklin, 2004; Hooghe, 2004). Empirical research that tests these assumptions, however, is rather scarce and sometimes contradictory (Bergh, 2013; Eichhorn, 2018; Rosenqvist, forthcoming; Zeglovits, 2013).

We contribute to this literature with a case study in Belgium, where local elections were held on 14 October 2018. The city of Ghent set up an experiment in which adolescents of 16 and 17 years old were granted the right to vote in a mock election. We designed a large-scale survey to investigate the effects of this enfranchisement on adolescents' political engagement using regression discontinuity designs. In addition, we estimate the effect of acquiring the right to vote in the actual election at 18 years old. The contrast between the two discontinuities in the data set makes for a unique research design that gives insights in the effects of acquiring the right to vote, and in how these effects differ depending on whether suffrage is gained in late adolescence (at 16) or in young adulthood (at 18).

Voting at 16: Previous empirical evidence

Previous work on the potential role of voting at 16 has mostly focused on comparing attitudes and levels of political "competence" between adolescents and young adults, with mixed results (Chan & Clayton, 2006; Hart & Atkins, 2011; McAllister, 2014). Others have moved beyond correlational analyses to gain insights in the causal effects of the right to vote in itself on attitudes. To do so, these studies typically include a relevant control group in their empirical design. Eichhorn (2018), for instance, compares the political attitudes of 16- and 17-year-old

Scots with those of their non-enfranchised peers in the rest of the United Kingdom. He finds that Scottish adolescents are more likely to indicate future participation, show higher levels of political efficacy, and engage with more information sources than their peers in the rest of the United Kingdom. Bergh (2013) investigates the 2011 Norwegian voting-age trial, where 16- and 17-year-olds were entitled to vote in some municipalities but not in others. He finds that 18-year-olds are somewhat more politically mature than 16- and 17-year-olds. However, these differences are not reduced in those municipalities in which the younger age group also had the right to vote.

In this study, we argue that, in order to test whether being entitled to vote makes adolescents more politically engaged, their engagement level should first of all be compared to their *younger* peers – i.e., those adolescents that just fell short of reaching the age on which they would be allowed to vote. Both groups share a very similar environment: they are in the same classrooms and have the same friends. This similarity allows for stronger inferences regarding the causal role of enfranchisement for explaining any differences in attitudes between these two groups

The debate about lowering the voting age to 16 is not only about enfranchisement, it is about *enfranchisement at 16* more specifically. It has been argued that 16 is a "better" age for gaining suffrage than 18, because adolescents at 16 are still more impressionable than they are at 18, which offers more effective opportunities for political socialisation (Franklin, 2004; Zeglovits and Aichholzer, 2014). Comparing the effects of enfranchisement at 16 with those of suffrage at 18 should give insights in the effects of suffrage at different age groups. Based on work on political socialisation, the expectation is that we will find more evidence of the positive effects of gaining suffrage at 16 than at 18.

The Case Study

The possibility to lower the voting age to 16 has been discussed for a long time in Belgium, but no proposals have made it into law. In the summer of 2018, the city of Ghent decided to spark a fresh debate over this issue, by inviting its 16- and 17-year-old citizens to cast a vote in the local elections of October 2018. Ghent is a major city in the Dutch-speaking part of the country, with some 260,000 inhabitants. Even though adolescents did not officially have the right to vote, the city municipality promoted the initiative as strongly as possible. First, all inhabitants in that age category received an official letter inviting them to vote. While fully enfranchised voters had to go to the voting booth to cast their vote, the invitation provided

every 16- and 17-year-old with a unique access code that allowed them to vote on a mobile device. The choice options (i.e., party lists) for adolescent voters were identical to those on the official ballot. Overall, the voting process for young voters was thus quite similar to that of fully enfranchised voters. Second, the majority of the city schools supported the initiative with various civic education efforts before the elections, thereby informing their pupils about the way local democracy works.

Clearly, the initiative of the city council was embedded in a real-life setting. Simultaneously, it has to be acknowledged that everyone involved was aware that the votes of the young people would not have any effect on the composition of the new city council, and this dampened the enthusiasm for the mock-elections. We hence consider this to be a hard test for evaluating the effects of voting at 16, and any effects we might find are most likely an underestimation of the likely effects of "real" eligibility.

Also in terms of the difference between the discontinuities at 16 and 18, our case study can be considered as a hard test. First, while 16-year-olds were invited to take part in a mock-election, those who had just turned 18 were officially granted the right to vote. Second, because voting is compulsory in Belgium, those who turned 18 were under the obligation to turn out to vote in the 2018 local elections. This context of compulsory voting is particularly relevant when studying political engagement, as some research find differences in information seeking between compulsory and non-compulsory contexts (Shineman, 2018). As a result, one might assume that the combined effects of enfranchisement and compulsory voting make for a particularly strong discontinuity at the age of 18.

Data and measures

We rely on data from the Ghent Study, which was conducted in the autumn of 2018. All citizens of Ghent between the ages of 15 and 20 received a questionnaire in the week after the elections. This broad age range allows us to investigate the group of newly enfranchised citizens, but also to compare them with their younger and older peers. The overall response rate was 21.62 per cent, and in total the Ghent Study includes information on 2,360 adolescents.[1] For the purpose of this analysis, we distinguish three different groups (Table 1), based on their age. We gained access to the official birth day of the respondents from the National Register of Belgium. As

[1] As this is a rather low response rate, we report analyses testing for selection effects in Appendix C. Even though these do not seem to indicate strong problems of selection bias, it needs to be noted that there can always be unobservable differences between participants and non-participants that we do not detect here.

elections were held on October 14, 2018, everyone born on or before October 14, 2000 had a legal right to vote. Those born between October 15, 2000 and October 14, 2002 (aged 16 and 17) were enrolled in the experiment, and those born on or after October 15, 2002 (aged 15 or younger) did not take part in the experiment.

Table 1. Overview of the Age Groups in the Ghent Study

Age	Born between	Eligible to vote in mock election?	Eligible to vote in real election?	Included in Ghent Study?	N	Response rate	Reported turnout rate
15	15.10.2002-14.10.2003	No	No	Yes	638	25.93%	0.00%
16-17	15.10.2000-14.10.2002	Yes	No	Yes	897	21.36%	32.76%
18-19	15.10.1998-14.10.2000	No	Yes	Yes	825	18.95%	98.90%

Our basic assumption is that the mere fact of being granted the right to vote leads to higher levels of political engagement. To capture "political engagement" we rely on a number of proxy indicators (Verba, Burns & Schlozman, 1997). More specifically, we use measures of attention to politics, discussing politics[2], political knowledge, internal and external political efficacy, and political trust. Detailed information about the question wording, coding of the variables, and descriptive statistics, are included in Appendix A. To allow for comparison between the different indicators, all variables have been rescaled ranging from 0 to 1.

Empirical strategy

The Ghent experience can be regarded as a quasi-experiment. Adolescents of 16 and 17 years old on or before Election Day were granted the right to vote. Their friends and peers that were born just after this date were not allowed to vote. Assuming that the date of birth in the weeks around 14 October 2002 is random,[3] we can use the age cut-off to divide our respondents in a treatment group that had the right to vote, and a comparison group that did not.[4] It is important to note that 15-year-olds and the 16-year-olds in the sample are enrolled in the same schools

[2] Note that this measure taps the frequency of in-person discussions with parents and friends. Especially for the younger age groups, it could be expected that their discussions take place on online forums and social media. To test for this, we also examined the effects on online political activity. The results do not reveal any significant effects (Appendix B).

[3] This assumption seems warranted, as a density test of observations by age does not show a discontinuity at the age cut-off (test 16-year-olds: 0.997; test 18-year-olds: 0.647).

[4] It has to be noted that the common (experimental) terminology here is to distinguish a 'treatment group' from a 'control group'. However, as we do not strictly have randomly assigned respondents to either group, we refer to the latter as the 'comparison group'.

and even in the same classes, and that therefore both of these groups were exposed to the same kind of political information.[5] Similarly, reaching the full legal right to vote in Belgian elections at 18 constitutes a quasi-experiment (Cepulani & Hidalgo, 2016). Comparing the attitudes of adolescents that were born just before and after these age cut-offs allows for a sharp regression discontinuity design to estimate the effect of being granted the right to vote (Lee & Lemieux, 2010).

We use these data to estimate series of sharp regression discontinuity (RD) models, using age on Election Day as cut-off. In our main analyses, we use the bandwidth selector as proposed by Calonico, Cattaneo, and Titiunik (2014a) and a triangular kernel function to construct the local polynomial estimators (Cepaluni & Hidalgo, 2016). Following Cepaluni and Hidalgo (2016), we report the bias-corrected RD estimates suggested by Calonico, Cattaneo, and Titiunik (2014b). However, to verify the robustness of our conclusions, we present several additional analyses.

Our running variable, citizens' date of birth, is discrete. The values that this variable can take are limited to dates of birth, implying days act as "mass points" that contain multiple observations. With such discrete running variable, traditional continuity-based RD models cannot be used if the number of mass points is too low (Cattaneo, Idrobo, & Titiunik, forthcoming). In our case, however, the 2,360 observations are spread over 1,282 unique values, which is a high number of mass points, allowing us to use traditional RD methods. Furthermore, as the number of observations at the values around the cut-offs are low (Appendix C), we cannot conduct a local randomisation analysis. Following the recommendation of Cattaneo et al. (forthcoming), we focus on the factual number of observations and present the results of analyses on a collapsed data set – in which each observation is the mean of the responses for that day. Analysing the raw data, however, leads to the same conclusions (see Appendix C). In Appendix C, we elaborate further on the model choice, and present falsification tests for the models presented here.

[5] We examined this assumption by testing whether the groups differed in political information they received at school. The results, reported in Appendix D, show that adolescents below the cut-off were aware of the experiment to the same extent as the adolescents above the cut-off, but that the latter reported learning about voting in local elections more than the former. Furthermore, it needs to be noted that, even if there are no strong differences in civic learning between the different age-groups, it is possible that the classes resonated more among those that were eligible to vote than those who were not.

Results

To investigate the effect of youth enfranchisement on adolescents' political engagement, we estimate regression discontinuity models, each time using another indicator of political engagement as dependent variable (Table 2).

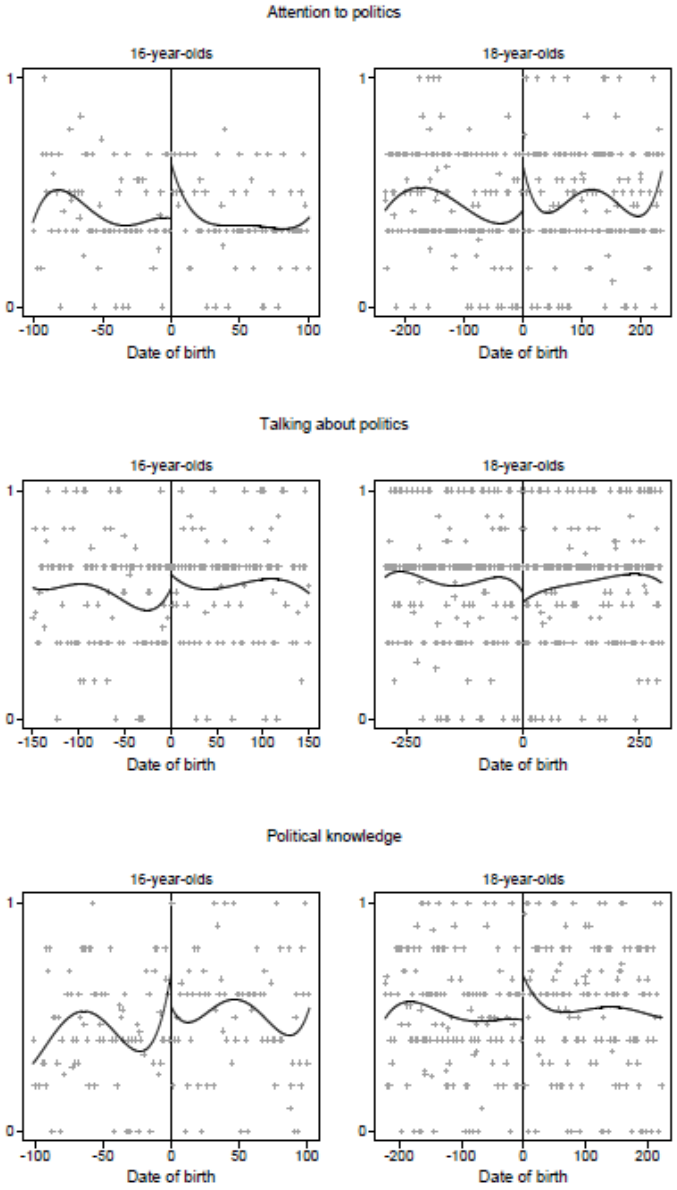
Table 2. Effects of youth enfranchisement on the political engagement of adolescents

	16-year-olds	18-year-olds
Attention to politics	0.165	0.139
	[0.041;0.289]	[0.016;0.263]
	(0.009)	(0.027)
Talking about politics	0.108	-0.064
	[-0.008;0.225]	[-0.188;0.059]
	(0.069)	(0.306)
Political knowledge	0.038	0.130
	[-0.133;0.209]	[-0.016;0.275]
	(0.662)	(0.081)
Internal political efficacy	0.072	0.115
	[-0.031;0.174]	[0.025;0.206]
	(0.169)	(0.013)
External political efficacy	-0.034	-0.037
	[-0.120;0.052]	[-0.119;0.045]
	(0.440)	(0.374)
Political trust	-0.026	0.068
	[-0.113;0.061]	[-0.029;0.166]
	(0.563)	(0.171)

Note: coefficient is a Regression Discontinuity point estimator, 95% confidence intervals in brackets and p-value in parentheses. Data: Ghent Study.

The results suggest a positive impact of enfranchisement on adolescents' attention to politics. For 16-year-olds, the estimate shows that adolescents who turned 16 just before or on Election Day, score on average 0.165 points higher (on a 0-1 scale) on attention to politics than their peers that turned 16 just after the elections. Among young adults, the estimate shows a jump of 0.139 in political attention between adolescents that had the right to vote in the mock-election and voters that were obligated to turn out to vote. The difference in attention to politics between these age-groups, is in line with our expectations.

Figure 1. The effect of (compulsory) enfranchisement on three indicators of political engagement



Note: The line shows the local polynomial smooth below and above the cut-off respectively (Table 2).

The results for the other indicators show only feeble support for our expectations. The estimates in Table 2 suggest that internal political efficacy increases after gaining full enfranchisement. The fact that this effect on internal efficacy is limited to real elections could be taken to suggest that the experience of going through the voting process increases voters' belief in their own political capabilities. Apart from these results, there do not seem to be significant effects of enfranchisement. Two notes can be made about these results. First, with regard to political knowledge, our indicators capture "institutional" knowledge – identifying politicians, their respective parties, and institutional composition. As this is the kind of

information that is typically taught in school, its distribution might well be more equal over the age-groups than would be the case if we relied on measures of e.g., policy knowledge – which we cannot test here. Second, political efficacy and political trust are rather stable core political attitudes, and it can be assumed that just a one-time experiment does not provide a strong enough incentive to have any meaningful effect. Alternatively, it is possible that the fact that 16-year-olds' votes were not taken into account officially in the mock-election cancelled out the expected positive effect of enfranchisement.

To show our results graphically, Figure 1 displays the discontinuities for the indicators that we expected to be more variable (attention to politics, talking about politics, and political knowledge), while the results for the more stable core attitudes are displayed in Appendix E. We experimented with various possibilities in terms of cut-off points and bandwidths (see Appendix F), and these tests confirm a significant difference for political interest occurring at the cut-off point of 16-year-olds, while the findings regarding the 18-year-olds seem to be less robust.

It is possible that the results can partly be explained by post-rationalisation processes, in which those adolescents who casted a vote subsequently become more interested in the electoral result as they now have a stake in it (Dinas, 2014). If voters become more interested in the electoral result because they turned out to vote, the uncovered effects would be the result of voting itself rather than of enfranchisement. To test for this, we compared the results for voting and non-voting adolescents just below and above the cut-offs.[6] The results (Appendix G) show support for the finding presented here in both groups. This is important, as it suggests that the results are not driven by some post-rationalisation mechanism, but reflect the role of enfranchisement.

Overall, however, the effect of enfranchisement on adolescents' political engagement seem rather limited. We find significance for only one indicator of engagement – attention to politics. Given the large number of tests that we performed for different indicators of political engagement, a correction for multiple testing seems warranted. Using the Holm method[7] for doing so (Chen et al., 2017), the conclusion has to be that – when including all indicators for

[6] We cannot conduct a similar analysis comparing 17-year-olds with 18-year-old voters and non-voters respectively. As voting is compulsory in Belgium for the age of 18 onwards, and given that compliance is very high, our data set includes no 18-year-old non-voters that we could use for such a comparison.

[7] This method is based on a stepwise procedure in which the p-value of the estimate is compared to the threshold α_i', which is computed as follows: $\alpha_i' = \frac{\alpha}{m-i+1}$. The value of α_i' is subsequently compared to the p-value of the hypothesis test, and the result is declared non-significant when $p_i \geq \alpha_i'$. In our case, even the smallest p-value (i.e., for attention to politics) passes the threshold, leading to the conclusion of non-significance of our indicators of political engagement overall.

engagement – there is no support for a significant effect of enfranchisement on political engagement.

Conclusion

In several countries, there is an ongoing discussion about lowering the voting age to 16. An often used argument in this debate is that enfranchisement by itself has a transformative impact and motivates citizens to become more engaged in political life. Furthermore, it has been argued that the effects of gaining suffrage at 16 would have more beneficial effects than gaining suffrage at 18 as major changes in life occur at that age, implying 'the costs of learning to vote (…) will clearly be higher' (Franklin, 2004, p. 63).

In line with previous research (Bergh, 2013), our results do not offer evidence for a strong transformative effect of youth enfranchisement on political engagement overall. It is safe to state, therefore, that politics, for most adolescents, is not the most important element of their daily life, and the opportunity to take part in mock-elections clearly has not changed this. While the results seem to suggest an increasing interest as a result of eligibility, overall, the conclusion needs to be that adolescents' political engagement did not change as a result of the experiment. As we do not find evidence for effects on engagement, we can also not draw strong conclusions regarding the difference in effects on 16 years old and 18 years old respectively.

Importantly, these results should be interpreted keeping in mind that this was a mock election, and the adolescents were aware of the fact that their vote would not have an effect on the composition of the city council. On the other hand, it has to be noted, that the schools in Ghent offered various initiatives to make this kind of information available for their pupils. With regard to data quality, it needs to be noted that, even though we took many steps to ensure a representative sample, our analyses are based on a selected part of individuals that is most likely not fully representative for the whole population. While this limits the external validity of the findings, it also means that it is possible that there are effects of enfranchisement in the least interested part of the electorate – which we were less able to reach with our efforts. Future research could investigate more in-depth the heterogeneous effects of youth enfranchisement on political engagement.

References

Bergh, J. (2013). Does Voting Rights Affect the Political Maturity of 16- and 17-Year-Olds? Findings from the 2011 Norwegian Voting-Age Trial. *Electoral Studies, 32*(1), 90–100.

Calonico, S., Cattaneo, M., & Titiunik, R. (2014a). Robust Data-Driven Inference in the Regression-Discontinuity Design. *Stata Journal, 14*(4), 909-946.

Calonico, S., Cattaneo, M., & Titiunik, R. (2014b). Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs. *Econometrica, 82(6*), 2295-2326.

Cattaneo, M D., Idrobo, N, & Titiunik, R. (forthcoming). *A Practical Introduction to Regression Discontinuity Designs: Volume II*. Cambridge: Cambridge University Press.

Cepaluni, G., & Hidalgo, F. (2016). Compulsory Voting Can Increase Political Inequality: Evidence from Brazil. *Political Analysis*, *24*(2), 273-280.

Chan, T.W., & Clayton, M. (2006). Should the Voting Age be Lowered to Sixteen? Normative and Empirical Considerations. *Political Studies*, *54*(3), 533–558.

Chen, S.-F., Feng, Z., & Yi, X. (2017). A General Introduction to Adjustment for Multiple Comparisons. *Journal of Thoracic Disease, 9*(6), 1725-1729.

Dinas, E. (2014). Does Choice Bring Loyalty? Electoral Participation and the Development of Party Identification. *American Journal of Political Science, 58*(2), 449–465.

Eichhorn, J. (2018). Votes at 16: New Insights from Scotland on Enfranchisement. *Parliamentary Affairs*, *71*(2), 365–391.

Franklin, M. N. (2004). *Voter Turnout and the Dynamics of Electoral Competition in Established Democracies Since 1945*. Cambridge: Cambridge University Press.

Hart, D., & Atkins, R. (2011). American Sixteen- and Seventeen-Year-Olds are Ready to Vote. *Annals of the American Academy of Political and Social Science*, *633*(1), 201–222.

Hooghe, M. (2004). Political Socialization and the Future of Politics. *Acta Politica*, 39(4), 331-341.

McAllister, I. (2014). The Politics of Lowering the Voting Age in Australia: Evaluating the Evidence. *Australian Journal of Political Science, 49*(1), 68-83.

Rosenqvist, O. (forthcoming). Rising to the Occasion? Youth Political Knowledge and the Voting Age. *British Journal of Political Science*. DOI: 10.1017/S0007123417000515

Shineman, V.A. (2018). If You Mobilize Them, They Will Become Informed: Experimental Evidence that Information Acquisition is Endogenous to Costs and Incentives to Participate. *British Journal of Political Science, 48*(1), 189-211.

Verba, S., Burns, N. & Schlozman, K. (1997). Knowing and Caring about Politics: Gender and Political Engagement. *Journal of Politics*, *59*(4), 1051-1072.

Zeglovits, E. (2013). Voting at 16? Youth Suffrage is Up for Debate. *European View*, *12*(2), 249–254.

Zeglovits, E., & Aichholzer, J. (2014). Are People More Inclined to Vote at 16 than at 18. Evidence for the First-Time Voting Boost among 16- to 25-Year Olds in Austria. *Journal of Elections, Public Opinion and Parties, 24*(3): 351–361.

Appendices

Appendix A. Variables used in the Analyses.

-*Attention to politics*: Adolescents' level of attention for politics is included as a measure of political interest. Previous research has argued that political interest is a crucial attitude as it predicts attention to electoral campaigns, feelings of political empowerment, and political knowledge (Shehata & Amnå, forthcoming). We focus on a measure of attention to politics, which consisted of a single item: "How much attention do you pay to political topics on television, the radio, newspapers or the internet?" Respondents could answer on a four-point scale ranging from "no attention at all" to "a lot of attention".

-*Talking about politics*: Given the fact that adolescents are not always in a position to decide for themselves, e.g., with regard to subscribing to a newspaper or following the television news, talking about politics can be considered as an important proxy indicator for this age group (Dassonneville & Hooghe, 2011; Klofstad, 2007). Respondents were asked "How often do you talk about political and social topics with the following people" and were subsequently asked to indicate frequency of discussion with both their parents and their friends on a scale ranging from "never" to "often". As the correlation between both variables is 0.52, we create one index "talking about politics" by adding the answers of the two questions, and dividing by two.

-*Political knowledge*: Political knowledge can be regarded as a crucial political resource (Delli Carpini & Keeter, 1996). In the Ghent Study, political knowledge was measured with five questions about the basic functioning of the Belgian political system:

-For the first two questions, respondents were asked to link the pictures of two well-known politicians with their respective political parties;

-"Who was alderman for education in the city council of the city of Ghent before the elections of 14 October 2018?"

-"Which parties were part of the city council of the city of Ghent before the elections of 14 October 2018?"

-"The Federal Parliament consists of…"

For each question, four answer options were given, as well as an explicit "don't know" option (apart from the two pictures, for which respondents could choose from a list of five parties). For respondents indicating not to know the answer, the answers were coded as wrong.

-*Internal political efficacy*: The feeling of political efficacy is a crucial resource to enable meaningful political participation. Respondents were asked to indicate to which extent they agreed with four statements:

-"I think I am capable to take part in politics";

-"I think I would do as good of a job as most politicians we elect";

-"I think I am better informed about politics than most people";

-"I think I understand which are the problems our society is facing".

For each statement, respondents could indicate the extent to which they agree on a four-point scale ranging from "fully disagree" to "fully agree". As the answers to the different items load on one latent factor (eigenvalue 1.453, Cronbach's Alpha 0.72), we create one index by adding up the answers to the questions and dividing by four.

-*External political efficacy*: Respondents were asked to indicate to which extent they agreed with four statements:

-"At election time one party promises more than the others, but in the end not much happens" [reversed];

-"A normal citizen has an impact on what the government does";

-"It doesn't make much sense to turn out to vote, parties do whatever they want anyway" [reversed];

For each statement, respondents could indicate the extent to which they agree on a four-point scale ranging from "fully disagree" to "fully agree". The answers to the different items do not load very strongly on one latent factor (eigenvalue 0.715, Cronbach's Alpha 0.52). However, as these items have been well-established theoretically, we create one index by adding up the answers to the questions and dividing by three.

-*Political trust*: Political trust expresses most comprehensively a generally positive view on the legitimacy of, and the basic values underlying the political system. A large number of studies also suggest a connection between political trust and other indicators for political engagement (Uslaner, 2018). Respondents were asked to which extent they trust the national Parliament, the National Government, politicians, and political parties, on a scale ranging from 0 (no trust at all) to 10 (full trust). As we expect these different items to measure one latent general feeling of trust, we include them in a factor analysis:

Trust in…	Factor loading
National Parliament	0.889
National Government	0.883
Politicians	0.794
Political parties	0.790
Eigenvalue	2.827
Cronbach's Alpha	0.897

As expected, the different items load on one concept. Hence, we create the measure 'political trust' as an index by adding up the answers on the different items and diving by four.

Table A.1: Descriptive statistics of the variables in the analyses

	Mean	Std. Dev.	Minimum	Maximum
Attention politics	0.448	0.270	0	1
Talking politics	0.592	0.287	0	1
Political knowledge	0.531	0.316	0	1
Internal efficacy	0.458	0.204	0	1
External efficacy	0.498	0.187	0	1
Political Trust	0.524	0.176	0	1

Appendix B: Online (intended) discussion

Our operationalisation of political discussion in the main models is fairly traditional, as it is based on a measure of how often the adolescent talks about politics with their parents or friends. Among this age group, it is possible that increased involvement with politics mainly happens online, by posting/sharing/commenting political news through online forums and social media.

To test for this, we estimated the models again, this time asking respondents about their current online activities: 'How often do you do each of the following activities: (1) put a reaction or a picture about a political or social issue online; (2) react or share something about a political or social issue that someone else has posted.' Respondents could indicate their online engagement using a four-point scale: "never", "rarely", "sometimes", "often". As the answers to both questions are strongly correlated (Pearson correlation of 0.64), we combine them into a single indicator of "online political activity". We also asked about respondents' future intended participation: 'Will you take part in any of these activities in the future: (1) contribute to an online discussion board about political or social issues: (2) organise an online group to take a stance about a controversial political or social issue; (3) take part in an online campaign.' Respondents could answer these questions by means of a four-point scale with the following answer options: "I will definitely not do this", "I will probably not do this", "I will probably do this", "I will certainly do this". As the answers to these questions have correlations ranging from 0.54 to 0.62, we combined them to construct an indicator of "future online participation". To investigate whether there are differences in adolescents' (intended) online behaviour following enfranchisement, we estimate the same models as those reported in the main text. The results are included in Table B.1. Note that, for ease of interpretation, all variables are standardised to range from 0 to1.

Table B.1. Effects of youth enfranchisement on online political activity of adolescents

	16-year-olds	18-year-olds
Online activity	-0.003 [-0.100;0.094] (0.953)	0.049 [-0.036;0.133] (0.260)
Future online activity	0.042 [-0.092;0.177] (0.538)	-0.055 [-0.147;0.037] (0.239)

Note: coefficient is a Regression Discontinuity point estimator, 95% confidence intervals in brackets and p-value in parentheses. Data: Ghent Study.

The results reported in Table B.1 show no strong differences in (future) online activity between unfranchised and enfranchised groups. It seems that even when considering the effects on more youth-oriented measures of discussion and political activity, youth enfranchisement does not seem to a strong impact.

Appendix C: Model choice and falsification tests

Local polynomial vs. local randomisation methods

In this study, we use a discrete running variable: as date of birth can only take the value of specific days, respondents are clustered in days. With this kind of data, it is not always appropriate to use local polynomial methods, but rather local randomisation analysis (Cattaneo et al., forthcoming). Whether or not local polynomial methods are warranted, depends on the amount of mass points in the data. In the example used by Cattaneo et al. (forthcoming), the data set consists of 4,362 observations, but these only take 430 separate values – hence, the observations are clustered by averagely approximately 100 observations per value. This number is "a moderate value", and is on the edge of warranting using the running variable as continuous. In our study, we sent out surveys to a wide range of birth dates – more specifically, respondents being born between 15.10.1998, and 14.10.2000. This gives the potential for 1,460 different values, and in practice the birthdate variable has 1,282 unique values. As we have birthday data for 2,307 respondents, on average, we only have 1,8 respondents on every day of birth. Hence, we dispose of many mass points, which seems to warrant using traditional methods. This consideration also becomes clear when we look at the number of observations around the cut-off – which we report in Table C.1.

Table C.1. Observations at closest mass points

16-year-olds			18-year-olds		
Birth date	Days from cut-off	N	Birth date	Days from cut-off	N
19.10.2002	-5	1	19.10.2000	-5	1
18.10.2002	-4	1	18.10.2000	-4	0
17.10.2002	-3	3	17.10.2000	-3	2
16.10.2002	-2	1	16.10.2000	-2	0
15.10.2002	-1	1	15.10.2000	-1	1
14.10.2002	0	1	14.10.2000	0	0
13.10.2002	1	2	13.10.2000	1	4
12.10.2002	2	2	12.10.2000	2	0
11.10.2002	3	0	11.10.2000	3	1
10.10.2002	4	2	10.10.2000	4	1
09.10.2002	5	3	09.10.2000	5	0

The data in Table C.1 show that there are no values on which there are particularly high numbers of observations. The highest number of observations on the same day in the whole

data set is 8. Even though date of birth is a discrete value, given the low number of observations per value and in particular, we think that a local polynomial method is appropriate (Cattaneo et al., forthcoming, p. 58). Furthermore, as the number of observations per day around the cut-off is so low, using a local randomisation analysis, we would be comparing the answers of just one respondent to the left of the discontinuity with the answers of one other respondent to the right of the cut-off point. In the case of 18-year-olds, we do not have any observation from someone born on the exact day that would grant eligibility. We therefore have to increase the bandwidth to have a larger number of observations. Hence, the data at hand do not allow estimating this kind of model. Therefore, we use local polynomial methods. To take into account the clustering, we report the results of a collapsed data set. To verify whether this has an effect of our results, we present the results using the raw data in Table C.2. It is reassuring that the results of both sets of analyses are very similar. The main difference seems to be that, using the raw data, there seems to be a significant positive effect of enfranchisement at age 16 on talking about politics.

Table C.2. Effects of youth enfranchisement on the political engagement of adolescents – raw data

	16-year-olds	18-year-olds
Attention to politics	0.161	0.098
	[0.040;0.282]	[0.005;0.192]
	(0.009)	(0.040)
Talking about politics	0.136	-0.094
	[0.008;0.263]	[-0.201;0.013]
	(0.037)	(0.084)
Political knowledge	0.044	0.096
	[-0.106;0.194]	[-0.035;0.228]
	(0.564)	(0.152)
Internal political efficacy	0.066	0.071
	[-0.020;0.152]	[0.006;0.136]
	(0.134)	(0.032)
External political efficacy	-0.046	-0.008
	[-0.128;0.037]	[-0.084;0.069]
	(0.275)	(0.847)
Political trust	-0.012	0.053
	[-0.082;0.057]	[-0.034;0.141]
	(0.729)	(0.231)

Note: coefficient is a Regression Discontinuity point estimator, 95% confidence intervals in brackets and p-value in parentheses. Data: Ghent Study.

Test for selection bias

It is possible that the "treatment" (i.e., being enfranchised) affected respondent's willingness to reply to the survey. We present several tests for this bias.

First, we compare observable characteristics of treated and control non-respondents. As we have the birthdate data of all citizens of the city of Ghent in the age categories of interest available, we can make this comparison based on the characteristics for which we have data: exact age on election day, and sex. We estimate t-tests to test whether the respondents of our survey differed in these characteristics from the population. These tests are reported in Table C.3.

Table C.3. T-tests comparing respondents to the population

	Non-respondents	Respondents	Difference
Age	16.982	16.877	-0.105**
Sex	0.469	0.546	0.077***

Significance levels: **: p<0.01; ***: p<0.001.

The results in Table C.3 show some differences between the sample of respondents and the population. First, it seems like the pool of respondents is somewhat younger than the population. This is most likely due to how we contacted our respondents. In our study, we each time contacted the youngest sibling of a family, asking them to pass on the questionnaires to their sisters and/or brothers. Most likely, not all of these respondents passed on the questionnaires, and this leads to a somewhat "young" sample. Second, there is a small difference in the gender-distribution (variable coded as 0=male; 1=female). More specifically, there seem to be more women in our sample than their proportion in the population. Adolescent girls seem to have been more likely to respond to our survey.

Second, we conducted a density test of the running variable. In this way, we can assess whether the number of observations left and right of the cut-off is similar. We conducted this test using the "rddensity" command. First, we conducted the test for only those respondents that answered our survey. Here we did not find any significant differences in density on the cut-off (p-value= 0.679 for 16-year-olds, and 0.494 for 18-year-olds). We also perform this test for the whole citizenry. These tests also revealed no significant differences in density (p-value=0.997 for 16-year-olds, and 0.647 for 18-year-olds).

As a more stringent test, we estimate RD models using participation in the survey as dependent variable. As we have the full data of birth dates of all adolescents in the age groups under investigation, we can create a binary indicator for every adolescent in these age groups in Ghent: participated in our study or not. Using this indicator, we estimate RD models at the 16- and the 18-cut-off. The results are presented in Table C.4

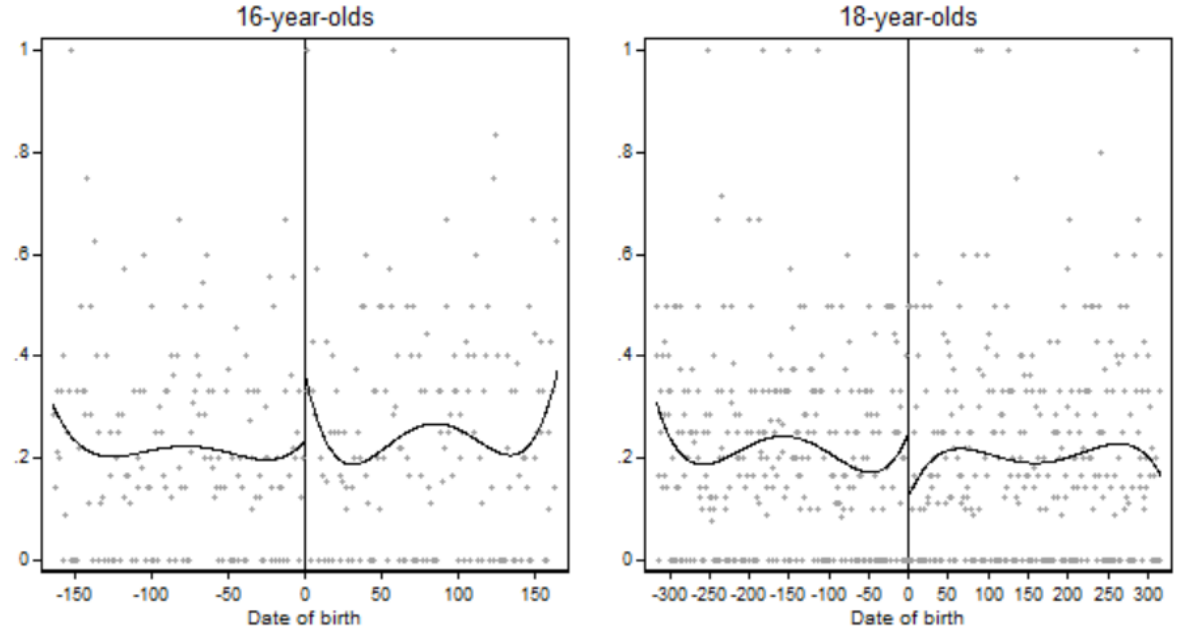
Table C.4. RD-analysis for participation in The Ghent Study

	16-year-olds	18-year-olds
Participation in study	0.038 [-0.059;0.135] (0.444)	-0.007 [-0.064;0.051] (0.814)

Note: coefficient is a Regression Discontinuity point estimator, 95% confidence intervals in brackets and p-value in parentheses. Data: Ghent Study.

The results in Table C.4 reveal no significant difference in participation in the survey study between the different age groups. These results are graphically displayed in Figure C.1.

Figure C.1. The effect of enfranchisement on taking part in the survey study



Note: The line shows the local polynomial smooth below and above the cut-off respectively (Table C.4).

It is clear from the results that enfranchisement itself does not seem to be related to participation in our study. Hence, we can rule out this alternative explanation for our findings.

Falsification tests

In line with the recommendations of Cattaneo et al. (forthcoming), we estimated RD-models including demographic covariates. More specifically, we include sex of the respondent, the self-reported amount of books at home (a measure of social class), and expected future education. We included these variables in models analogous to our main models – both at the 16- and 18-year-old cut-off. The results are reported in Table C.5.

Table C.5. Falsification analysis

	16-year-olds	18-year-olds
Sex	-0.200	-0.020
	[-0.085,0.485]	[-0.199;0.159]
	(0.169)	(0.823)
Number of books at home	-0.138	-0.735
	[-0.764;0.487]	[-1.353;-0.118]
	(0.664)	(0.019)
Expected education	-0.099	-0.358
	[-0.011;0.812]	[-1.034;0.319]
	(0.831)	(0.300)

Note: coefficient is a Regression Discontinuity point estimator, 95% confidence intervals in brackets and p-value in parentheses. Data: Ghent Study.

The results in Table C.5 show that there are only small differences in these demographic covariates at the age cut-offs. The only difference that reaches significance is the number of books at home, which seems to increase when young adults turn 18. One possible explanation of this effect might be that 18-year-olds might not live with their parents anymore (i.e., they live alone, or in a student room), and the reference to their "home" might be different than for adolescents. However, it is reassuring that in five out of six cases, there is no significant difference.

Appendix D. Learning about politics at the discontinuity

We tested whether 15-year-olds were exposed to civic classes in school as were their older peers. To examine this, we look at the extent to which they indicate having learned about the following topics: (1) voting in local elections; (2) voting in national elections; (3) how laws are made and implemented in Belgium; (4) how civil rights are protected in Belgium. For each of these topics, respondents could indicate whether they learned about it in the current school year "never" (code 0), "few", "a little", or "a lot" (code 3). Furthermore, to test whether one group heard about the experiment specifically to a larger extent than the other, we asked through which means they heard about the initiative of the city of Ghent to allow 16- and 17-year-olds to cast a vote. We combine the respondents who indicate having heard about it at school or having had a series of classes about democracy, and test whether there is a difference in the proportion of respondents indicating this answer.

As can be seen in Table D.1, there is no difference in the extent to which adolescents just below and just above the cut-off date had heard about the experiment at school.[8] In terms of the content of learning at school, while there is a small difference between both groups for the question about learning about voting in local elections, there are no significant differences with regard to the other topics. Overall, it has to be noted that despite the efforts of the city, few respondents reported having learned extensively about local elections at school.[9] The general conclusion therefore is that, most likely, the 15- and the 16-year-olds were exposed to roughly the same kind of political information in the period leading up to the elections of October 2018. However, given the small and significant difference for learning about voting in local elections, when interpreting the results one should take into account the possibility that this imbalance has increased the political information and attention in this age group somewhat. Furthermore, it needs to be noted that, even if there are no strong differences in civic learning between the different age-groups, it is possible that the classes resonated more among those that were eligible to vote than those who were not.

[8]. To make for a hard test, we make the comparison for a limited group of respondents around the cut-off. To identify the two groups of comparison, we rely on the bandwidth used in the RD model. In the model investigating the difference in political attention (see Table 2), the bandwidth is 90. Hence, we look at the mean scores for adolescents born 90 days before and 90 days after 14 October 2002 respectively.

[9]. An important side-note is that the school year traditionally starts on the first day of September in Belgium, and hence there was not much time for schools to provide students with civic education before the local elections of 14 October.

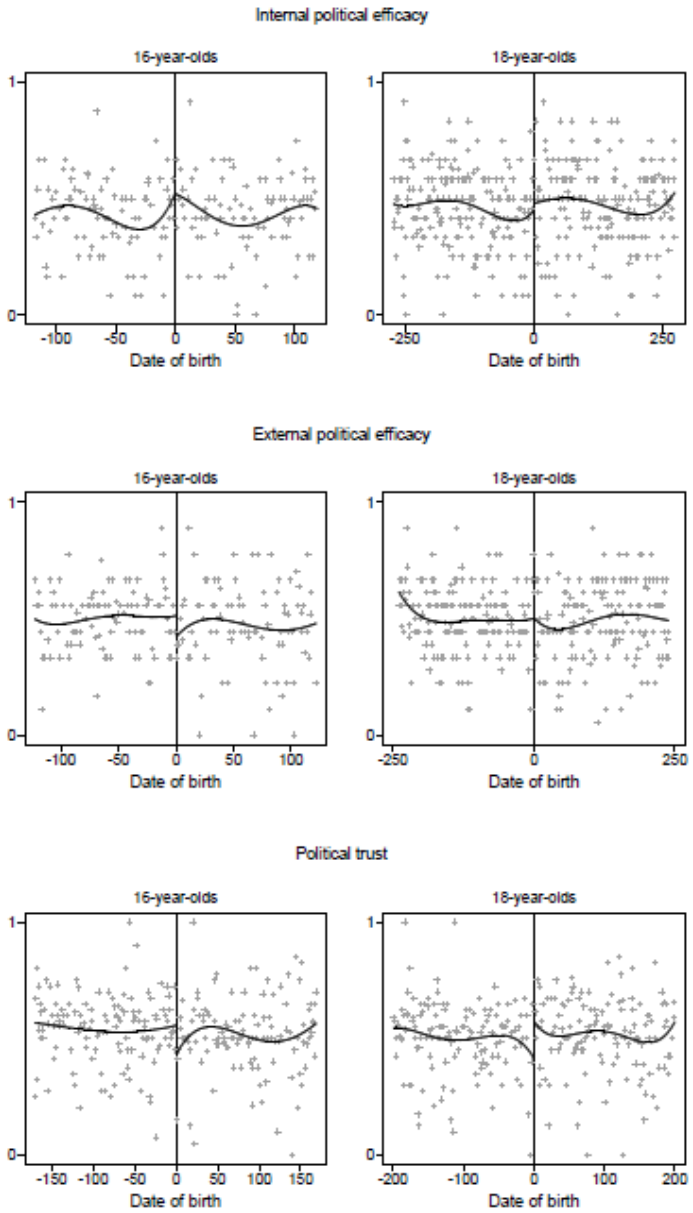
Table D.1. Learning in school and hearing about experiment in school for adolescents just below and just above cut-off.

Learned about…	Below cut-off	Above cut-off	Difference
Voting in local elections	1.063	1.357	0.295*
Voting in national elections	0.785	0.947	0.162
How laws are made and implemented in Belgium	1.035	0.951	-0.083
How civil rights are protected in Belgium	0.895	1.044	0.149
Heard about experiment through school or classes in school	0.430	0.368	-0.062

Note: Entries are the mean score of learning about different subjects on school on a 0-3-scale and the proportions of for adolescents having heard about the experiment through the school for adolescents born in the 90 days after and the 90 days before the cut-off point respectively. *: $p<0.05$.

Appendix E. Regression Discontinuity figures

Figure E.1. The effect of (compulsory) enfranchisement political engagement



Note: The line shows the local polynomial smooth below and above the cut-off respectively.

Appendix F. Robustness tests

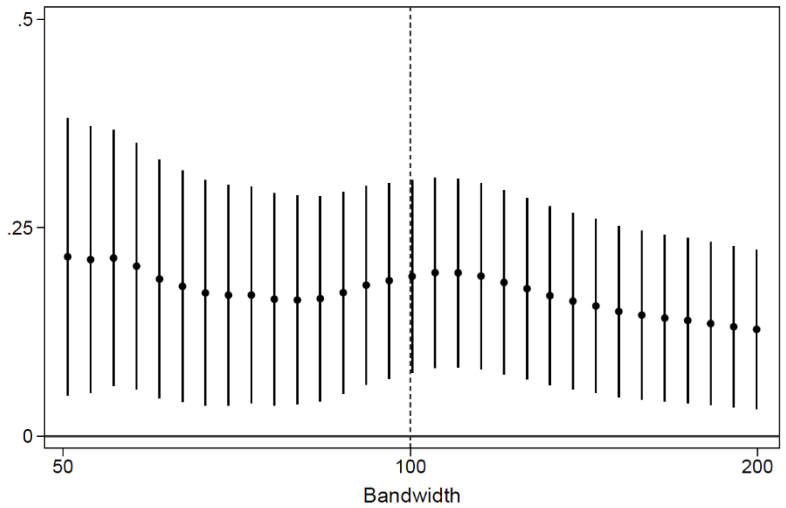
We report several robustness tests for the significant findings presented in the main text. More specifically, we each time include two additional tests:

1) We report the results of the conventional RD estimates with conventional variance estimator and bias-corrected RD estimates with robust variance estimator (rather than the bias-corrected RD estimate with conventional variance estimator reported in the text). See Calonico, Cattaneo, and Titiunik (2014) for more details.
2) We report the RD estimate using different bandwidths. Starting from the badwidth calculated using the approach by Calonico, Cattaneo, and Titiunik (2014), we report the results ranging from half to double the suggested bandwidth.

Attention to politics 16-year-olds

Estimator	
Bias-corrected, conventional variance (reported in main text)	0.165 [0.041;0.289] (0.009)
Conventional, conventional variance estimator	0.137 [0.013;0.261] (0.031)
Bias-corrected, robust variance estimator	0.165 [0.026;0.304] (0.020)

Note: RD results using different estimators. Data: Ghent Study. Significance levels: *: p<0.05; **: p<0.01; ***: p<0.001.

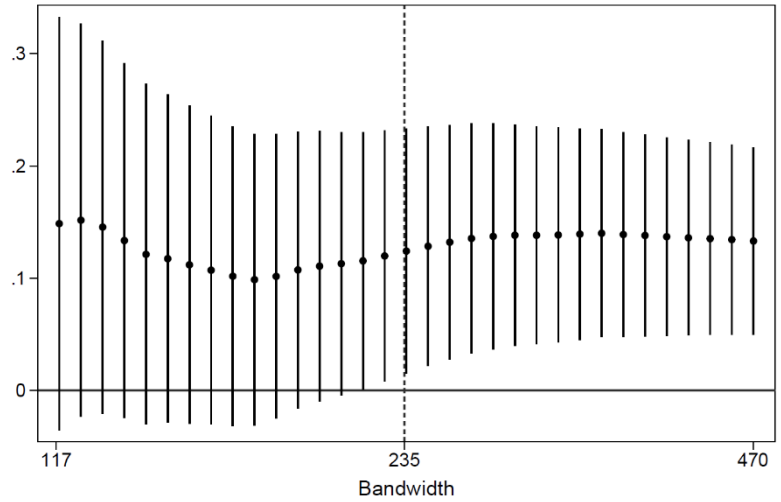


Note: Figure shows RD estimates using different bandwidths. Bars present 95% confidence intervals. Data: Ghent Study.

Attention to politics 18-year-olds

Estimator	
Bias-corrected, conventional variance (reported in main text)	0.139 [0.016;0.263] (0.027)
Conventional, conventional variance estimator	0.118 [-0.006;0.242] (0.062)
Bias-corrected, robust variance estimator	0.139 [-0.003;0.282] (0.055)

Note: RD results using different estimators. Data: Ghent Study. Significance levels: *: p<0.05; **: p<0.01; ***: p<0.001.

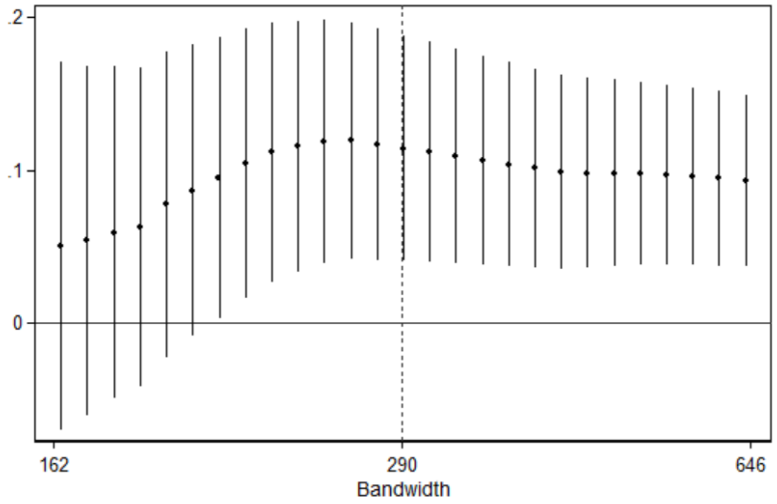


Note: Figure shows RD estimates using different bandwidths. Bars present 95% confidence intervals. Data: Ghent Study.

Internal political efficacy 18-year-olds

Estimator	
Bias-corrected, conventional variance (reported in main text)	0.115 [0.025;0.206] (0.013)
Conventional, conventional variance estimator	0.101 [0.011;0.192] (0.029)
Bias-corrected, robust variance estimator	0.115 [0.009;0.221] (0.054)

Note: RD results using different estimators. Data: Ghent Study. Significance levels: *: p<0.05; **: p<0.01; ***: p<0.001.



Note: Figure shows RD estimates using different bandwidths. Bars present 95% confidence intervals. Data: Ghent Study.

Appendix G: results comparing with 16 year old voters and 16 year old non-voters respectively

15 year olds versus…	16-year-old voters	16-year-old non-voters
Attention to politics	0.244 [0.099;0.390] (0.001)	0.139 [0.044;0.234] (0.004)

References

Calonico, S., Cattaneo, M., & Titiunik, R. (2014). Robust Data-Driven Inference in the Regression-Discontinuity Design. *Stata Journal, 14*(4), 909-946.

Cattaneo, M D., Idrobo, N, & Titiunik, R. (forthcoming). A Practical Introduction to Regression Discontinuity Designs: Volume II. *Cambridge University Press*, forthcoming.

Dassonneville, R. & Hooghe, M. (2011). The Effects of Civic Education on political knowledge. A two year panel survey among Belgian adolescents. *Educational Assessment, Evaluation and Accountability, 23*(4), 321-339.

Delli Carpini, M. & Keeter, S. (1996). *What Americans Know about Politics and Why it Matters.* New Haven: Yale University Press.

Klofstad, C. (2007). Talk Leads to Recruitment: How Discussions about Politics and Current Events Increase Civic Participation. *Political Research Quarterly, 60*(2), 180-191.

Shehata, A. & Amnå, E. (forthcoming). The Development of Political Interest Among Adolescents: A Communication Mediation Approach Using Five Waves of Panel Data. *Communication Research*, in press.

Uslaner, E. (ed., 2018). *The Oxford Handbook of Social and Political Trust.* Oxford: Oxford University Press.