Political Science Research and Methods (2021), **9**, 849–856
doi:10.1017/psrm.2020.8

RESEARCH NOTE

Voting at 16: Does lowering the voting age lead to more political engagement? Evidence from a quasi-experiment in the city of Ghent (Belgium)

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(Received 17 July 2019; revised 7 November 2019; accepted 16 December 2019; first published online 9 March 2020)

Abstract
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.

Key words: Ghent; political interest; regression discontinuity design; voting age; voting at 16; youth enfranchisement; youth suffrage

> "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 socialization. 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; Zeglovits, 2013; Eichhorn, 2018; Rosenqvist, forthcoming).

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 (RD) 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).

1. 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 and Clayton, 2006; Hart and 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, but 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 socialization (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 the work on political socialization, the expectation is that we will find more evidence of the positive effects of gaining suffrage at 16 than at 18.

2. 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.

3. 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 percent, and in total, the Ghent Study includes information on 2360 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 elections were held on 14 October 2018, everyone born on or before 14 October 2000 had a legal right to vote. Those born between 15 October 2000 and 14 October 2002 (aged 16 and 17) were enrolled in the experiment, and those born on or after 15 October 2002 (aged 15 or younger) did not take part in the experiment.

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 *et al.*, 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 is included in Appendix A. To allow for comparison between the different indicators, all variables have been rescaled ranging from 0 to 1.

4. 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

[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.

[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".

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

important to note that 15- and 16-year-olds in the sample are enrolled in the same schools 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 (Cepaluni and Hidalgo, 2016). Comparing the attitudes of adolescents that were born just before and after these age cut-offs allows for a sharp RD design to estimate the effect of being granted the right to vote (Lee and Lemieux, 2010).

We use these data to estimate a series of sharp RD models, using age on Election Day as a cut-off. In our main analyses, we use the bandwidth selector as proposed by Calonico *et al.* (2014*a*) and a triangular kernel function to construct the local polynomial estimators (Cepaluni and Hidalgo, 2016). Following Cepaluni and Hidalgo (2016), we report the bias-corrected RD estimates suggested by Calonico *et al.* (2014*b*). 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, and Titiunik, forthcoming). In our case, however, 2360 observations are spread over 1282 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 is low (Appendix C), we cannot conduct a local randomization 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. Analyzing 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. Results

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

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.

[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.

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

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

Note: Coefficient is a regression discontinuity point estimator, 95 percent confidence intervals in brackets and p-value in parentheses. Data: Ghent Study.

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-rationalization 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

[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.

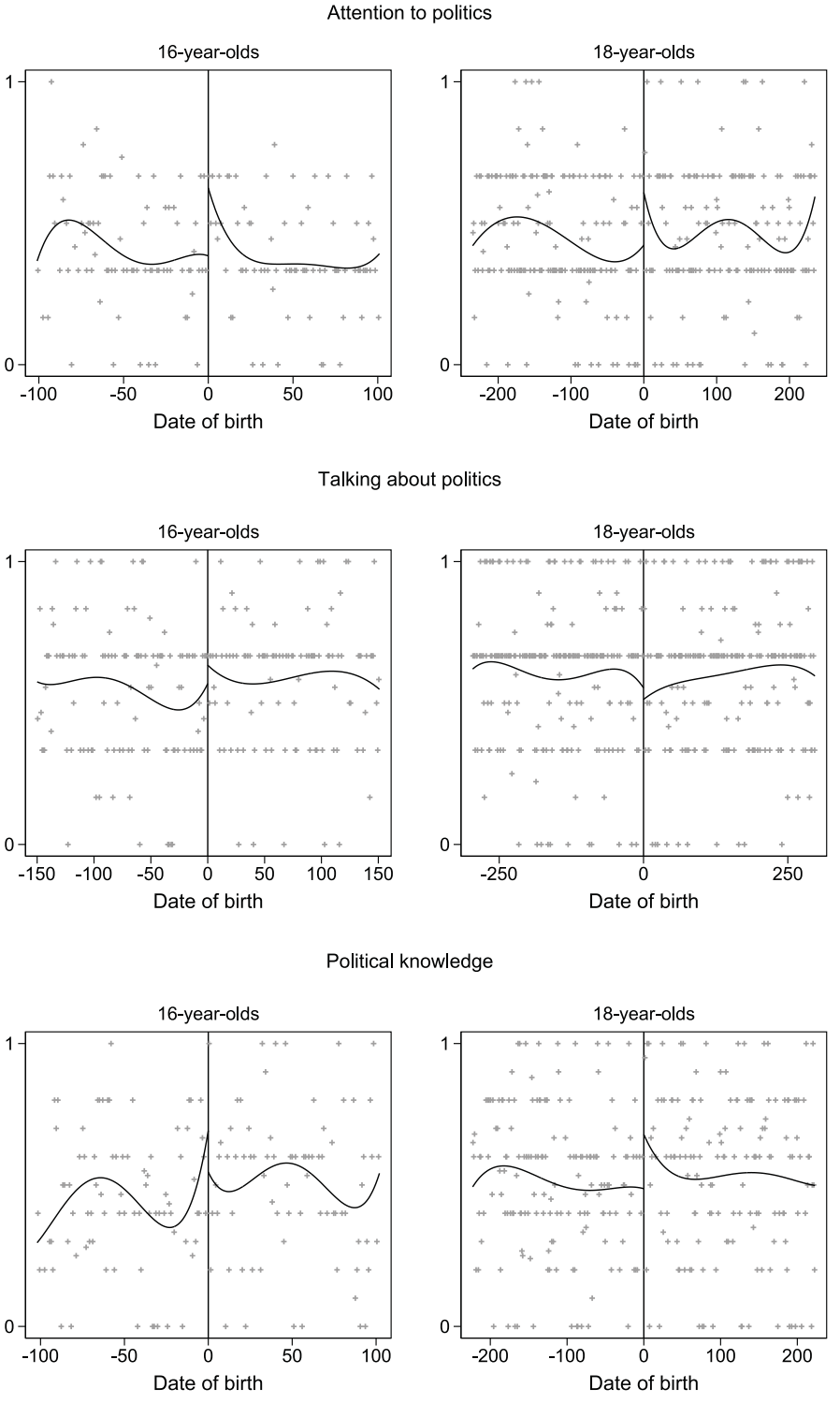

Figure 1. The effect of (compulsory) enfranchisement on three indicators of political engagement.
Note: The line shows the local polynomial smooth below and above the cut-off, respectively (Table 2).

finding presented here in both groups. This is important as it suggests that the results are not driven by some post-rationalization mechanism, but reflect the role of enfranchisement.

Overall, however, the effect of enfranchisement on adolescents' political engagement seems 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 engagement—there is no support for a significant effect of enfranchisement on political engagement.

6. 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: 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- 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 heterogeneous effects of youth enfranchisement on political engagement.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/psrm.2020.8.

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[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: $\alpha_i' = \alpha/(m - i + 1)$. 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.

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Cite this article: Stiers D, Hooghe M, Dassonneville R (2021). 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* **9**, 849–856. https://doi.org/10.1017/psrm.2020.8