On-line Appendix

Temporary Disenfranchisement: Negative Side-Effects of Lowering the Voting Age

Appendix: Temporary disenfranchisement

A.1 Temporary disenfranchisement due to voting age reductions

Temporary disenfranchisement occurs whenever there is an uncoordinated implementation of voting age reductions within federal systems. As voting age reductions tend to be implemented in lower-level elections such as municipal or state elections first, because seemingly less is at stake there, inconsistent voting age regulations within a country are increasingly frequent. However, in practice, such reforms entail that, if, for instance, a state election with voting age 16 takes place less than two years before a national election with voting age 18, some underage voters eligible for the former will have no right to vote in the latter. Thus, temporary disenfranchisement describes the situation in which young voters, after having been eligible to vote for the first time in their lives, will lose their right to vote in a subsequent election due to differing voting age thresholds.

However, temporary disenfranchisement is not limited to the above example and occurs in various countries and elections. Below, we provide an overview of cases of temporary disenfranchisement.

A.2 Cases of temporary disenfranchisement due to voting age reductions in Germany

In Germany, temporary disenfranchisement of underage voters has occurred in 46 elections since the first state lowered the voting age to 16 in 1996. Four of its 16 states have so far implemented a voting age of 16 years for both state and municipal elections. A further seven allow 16-to-17-year-olds to vote in municipal elections only. The voting age for national and European elections remains at 18. Hence, Germany's federal political system is an excellent example of how voting age reforms will likely play out in other countries: gradual implementation of a lower voting age at lower-level elections resulting in the temporary disenfranchisement of some young citizens.

In six states and 14 elections, young citizens were temporarily disenfranchised in a national election following a state or municipal election. In four states and 16 elections, young citizens were temporarily disenfranchised in a European election following a state or municipal election. And finally, in six states and 15 elections, young citizens were temporarily disenfranchised in a state election with voting age 18, which followed a municipal election with voting age 16.

Country	Election	Subsequent election	Cases
Germany	Municipal	National	14 elections
Germany	State	National	1 election
Germany	Municipal and/or State	European	16 elections
Germany	Municipal	State	15 elections

Table A.1. *Number of cases of temporary disenfranchisement in Germany. The first occurrence was the national election of 1998: six months earlier 16- and 17-year-olds had been eligible to vote in municipal elections in the state of Schleswig-Holstein for the first time ever. Many more occurrences of temporary disenfranchisement followed since then.*

A.3 Cases of temporary disenfranchisement due to voting age reductions worldwide

Internationally, the phenomenon of temporary disenfranchisement is increasingly widespread as well. In Estonia, 16- and 17-year-olds were allowed to vote for the first time in the 2017 municipal elections – held nationwide on the same date – and the youngest part of them were still barred from voting in the national election in 2019 with voting age 18. In Israel, where the voting age for local elections is 17, young citizens were temporarily disenfranchised in three national elections following a municipal election. In Malta, young citizens were temporarily disenfranchised in a referendum as well as a national election in 2015 following a municipal election. In 2018, Malta altered its constitution to lower the voting age to 16 for all elections, including European elections, thereby preventing any further occurrences of temporary disenfranchisement. In the United Kingdom, young citizens from Scotland were temporarily disenfranchised in three national elections following a referendum, state, or municipal election. Welsh 16- and 17-year-olds will soon face temporary disenfranchisement as well, as Wales has lowered the voting age for regional and local elections as well, but there is no political movement towards a lowering of the voting age for UK-wide elections to the House of Commons. Across the Atlantic, in 28 elections in four states of the US, young voters were temporarily disenfranchised in a national or state election following a municipal election in the past six years alone. And finally, Switzerland is a special case. Here, in the canton of Glarus, which reduced the voting age for municipal and regional elections to 16 in 2007, young citizens are virtually guaranteed to experience temporary disenfranchisement at least once in their life. This is because of national referendums, which in Switzerland take place on up to four dates per year, and very few years go by without a national referendum.

Unless the voting age for national elections is reduced, these countries will continue to produce cases of temporary disenfranchisement in the future. The same will happen in other countries if they reduce the voting age to 16 for some (lower-level) elections only. For instance, although no reforms seem imminent, a growing number of states and municipalities in the US are beginning to discuss lowering the voting age to 16 for state or municipal elections (for current information see the website maintained by Vote16USA.[1]

1. http://vote16usa.org/ (last accessed 01/29/2021)

Country	Election	Subsequent election	Cases
Estonia	Municipal	National	1
Israel	Municipal	National	3
Malta	Municipal	Referendum	1
Malta	Municipal	National	1
UK	State	National	1
UK	Municipal	National	1
UK	Referendum	National	1
USA	Municipal	National	18
USA	Municipal	State	10
Switzerland	State	National	1
Switzerland	State	Referendum	8
Switzerland	Municipal	National	3
Switzerland	Municipal	Referendum	6

Table A.2. *Number of cases of temporary disenfranchisement worldwide.*

A.4 Cases of temporary disenfranchisement due to residential mobility

Temporary disenfranchisement and its effect on affected citizens' political attitudes might not be limited to previously discussed cases but could potentially also apply to citizens losing their right to vote due to residential mobility (Squire, Wolfinger, and Glass 1987; Highton 2000; Hansen 2016). In many democracies, citizens who change their residency are required to re-register in the new area of residence. In the run-up to an election, it is common that countries limit re-registration up to a certain date before election day to minimize bureaucratic efforts. This results in many citizens being either partially restricted in where to cast the ballot or, in some cases, being temporarily disenfranchised. Citizens moving shortly before an election might in some – but certainly not all – cases be able to get registered in their new place of residence or vote at their old place of residence. However, immediately after moving, completing the often unnecessarily or even deliberately (e.g., in some states in the US) complicated administrative procedures is beyond many citizens' capacities, capabilities, or both.

While this phenomenon generally affects all eligible citizens moving during election times equally, many countries, such as the US, Germany, or Austria, usually hold elections in the fall. This timing coincides with first-year students, who are in most cases also first-time voters, moving to a new university and, as a consequence, being increasingly affected by restrictions due to voter re-registration. Given our findings, these cases of disenfranchisement after residential mobility, beyond mechanically posing a barrier to voting caused, might also further negatively affect (young) voters' political attitudes.

A.5 Cases of temporary disenfranchisement due to citizenship

Many states worldwide grant partial voting rights to non-citizen residents. For instance, all EU states (as legally mandated by the European Union) grant voting rights to non-citizen residents from other EU member states in European and local elections.[2] This implies, for

2. Several EU member states granted voting rights to resident aliens even before the EU mandated it.

instance, that a Danish citizen residing in Schleswig-Holstein, our case of study, will be able to elect his local council and German MEPs[3] but not the state or national parliament. Beyond the EU, many other countries have similar rules in place. For instance, in Switzerland, some but not all cantons grant voting rights to non-citizen residents on the cantonal and municipal levels. In other countries, such as South Korea, a national law extends voting rights to resident aliens for local but not national elections. A diverse set of countries from Colombia to Malawi to Wales has similar rules in place, granting voting rights to resident aliens in some but not all elections. While it is in principle possible for non-citizen residents to remedy their partial disenfranchisement themselves, it is still exceedingly difficult for them because most states impose strict requirements on naturalization, and the process takes a long time to complete.

Voting rights for non-citizen residents have been subject to both empirical – see, for instance, several articles by David C. Earnest (2006, 2015a, 2015b) on the determinants of these voting rights extensions – and normative research – see, for instance, Beckmann (2006).[4] However, these articles do not problematize temporary disenfranchisement as a side effect of partial franchise extensions. Hence, despite the size of foreign populations with partial voting rights (e.g., 5 million nationals of other EU countries residing in Germany, about 6% of the population, and even higher numbers in other countries), we, to the best of our knowledge, know nothing about the effects of partial eligibility on the political attitudes and behavior of this demographic.

A.6 Cases of temporary disenfranchisement due to felony disenfranchisement

Citizens in many countries can also lose their right to vote due to imprisonment. The most prominent case of felony disenfranchisement is arguably that of the United States, which has received considerable attention in political science research. Previous literature identified a link between imprisonment and decreases in external efficacy (Shineman 2020; White 2019; Weaver and Lerman 2010; Gerber et al. 2017; Hjalmarsson and Lopez 2010) mainly building on two explanations: First, negative changes are explained through a felon's deteriorated perception of the state due to the punishment received by the latter. Second, a felon's decreased socioeconomic status after release from prison caused by the interruption and /or termination of employment, residence and social ties through imprisonment results in decreased political engagement.

Our results suggest that there may be a third mechanism leading to decreases in external efficacy due to imprisonment. Specifically, the loss of franchise by itself might have a negative effect on inmates' perceptions of the political system's responsiveness to them, which might extend beyond the time served in prison. While convicted felons, of course, have agency for the crimes they committed, they cannot do anything about having their voting rights taken away from them in addition to their freedom of movement. Although the latter is certainly the main punishment driving effects observed in the literature on felony disenfranchisement,

3. Members of the European Parliament

4. There exists also a comprehensive Wikipedia article (https://en.wikipedia.org/wiki/Right_of_foreigners_to_vote, last accessed 6 August 2021) that can serve as a starting point for those interested in reading more about the matter.

disenfranchisement constitutes an additional punishment[5] that may have an effect in itself, as our paper suggests. This third mechanism adds up to the already observed harmful effects of imprisonment on political attitudes and thereby contributes to felons being even more disintegrated from the political system after being released. As custodial sentences are accompanied by a partial or complete restriction on voting rights, this mechanism should have wide applicability. One way to counteract it would be to allow and encourage inmates to vote, which, for instance, prisons in Germany are obliged to do by law.

B The survey

B.1 Research ethics

The survey was conducted in full compliance with national and European data protection laws in place at the time. It did not pose any risks and/or harm to individuals or groups who participated. The survey was carried out anonymously, and contact details provided by respondents for re-contact were stored separately from the survey data. The survey did not entail deception, nor did it include any sensitive items. A unique feature of our study, in contrast to ordinary election studies or population surveys more generally, is that the target population consists predominantly of minors, specifically citizens who were between 15 and 18 years old at the time of the state election, i.e., the first wave of the panel survey. Consultations with various experts, including the project consulting of GESIS Leibniz Institute for the Social Sciences, revealed no fundamental objections to contacting minors for survey research. According to the Council of German Market and Social Research Institute's 'Guideline for Surveying Minors'[6], 14- to 17-year olds can be principally thought of as capable of informed consent when it comes to participation in a survey. In our invitation letter mailed to potential respondents, we specifically asked minors to discuss their possible participation in our survey with their parents.

In sum, we deem our survey and research based on it to be fully compliant with APSA's "Principles and Guidance for Human Subjects Research."[7]

B.2 Sampling

In total, 22,133 people received an invitation by mail to participate in our survey in the week following the state election. The letter contained the URL to our survey and a personalized access code. We deleted all names and addresses after completing the mailing and merged respondents' birth dates obtained from the population register to the survey data based on each respondent's unique access code. We sent out no reminders for the first wave of the

5. Note that the fairness of such a punishment is disputed. For instance, in the high-profile case *Hirst vs. the UK* in 2005, the European Court of Human Rights found the United Kingdom to be in breach of the European Convention on Human Rights because it disallowed prisoners from voting.

6. Arbeitskreis Deutscher Markt- und Sozialforschungsinstitute "Richtlinie für die Befragung von Minderjährigen" (https://www.adm-ev.de/wp-content/uploads/2021/01/RL-Minderjaehrigen-neu-2021.pdf, last accessed on 19 March 2021)

7. https://www.apsanet.org/Portals/54/diversity and inclusion prgms/Ethics/Final_Principleswith Guidance with intro.pdf?ver=2020-04-20-211740-153, last accessed 19 March 2021

survey as this would have required a new mailing. In the second and third waves, the initial invitation and reminders were sent to e-mail addresses and mobile phone numbers respondents had provided in the first wave. Table B.1 shows survey periods and participation figures for the three waves of the panel survey.

	1st wave	**2nd wave**	**3rd wave**
Election type	State election	National election	Municipal elections
Election date	May 7, 2017	September 24, 2017	May 6, 2018
Voting age	16	18	16
Invited	22,133	3,414	3,414
Field time	May 8, 2017 - June 6, 2017	September 25, 2017 - October 22, 2017	May 7, 2018 - May 24, 2018
Invitation via	Mail	E-Mail / SMS	E-Mail / SMS
Respondents	3,897 (18.4%)	1,900 (55.7%)	1,529 (44.8%)

Table B.1. *Overview of the three survey waves on the state elections, federal elections and local elections in Schleswig-Holstein.*

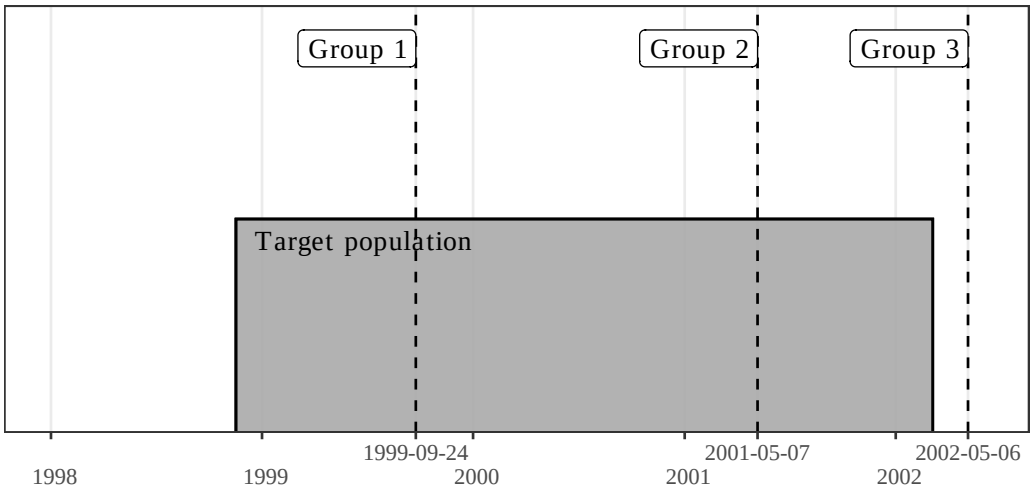

Figure B.1. *Periods of birth that define group membership. The vertical lines indicate the birth dates governing eligibility and thereby define the groups. The grey rectangle represents the population whose addresses we obtained from the registration offices (citizens born between 17/11/1998-07/03/2002). Young citizens belonging to group 1, born on or before 24/09/1999, are eligible for all three elections. Young citizens belonging to group 2, born after 24/09/1999 and on or before 07/05/2011, are eligible to vote in the state and municipal elections but not the federal election. Finally, young citizens belonging to group 3, born after 07/05/2001 and on or before 06/05/2002, are eligible to vote in the municipal elections only.*

Group	Birth dates	State election 2017	National election 2017	Municipal elections 2018	N
1	11/17/1998 - 09/24/1999	Eligible	Eligible	Eligible	581
2	09/25/1999 - 05/07/2001	Eligible	Ineligible	Eligible	916
3	05/08/2001 - 03/07/2002	Ineligible	Ineligible	Eligible	403

Table B.2. *Three groups can be formed on the basis of birthdays and the associated entitlement or lack thereof to participate in elections taking place during the observation period: (1) Persons entitled to vote in all three elections, (2) Persons entitled to vote in state and municipal elections and (3) Persons entitled to vote in municipal elections only.*

	Wave 1		Wave 2		Wave 3	
Group	N	%	N	%	N	%
1	1077	100	581	53.95	436	40.48
2	1939	100	916	47.24	752	38.78
3	881	100	403	45.74	341	38.71

Table B.3. *Number of respondents per group and wave. Column '%' indicates the percentage of respondents retained in each wave (relative to the first wave).*

In addition to the initial descriptive analysis, we further estimated the samples' representativeness by comparing the participants' overall proportions of characteristics to benchmark data from Schleswig-Holstein in 2017. As the governmental "Mikrozensus" (micro census) data for Schleswig-Holstein does not ask any political items, we focus on gender and migration status.

Treatment and control groups are not only balanced but also represent the large population of young people in Schleswig-Holstein in 2017 quite accurately. The groups display only a minor deviation from the benchmark statistic in gender across waves, with the treatment group deviating at around 1.93 percentage points and the control group at an average of 1.01 percentage points. Concerning respondents' migration background, we find a slight deviation of around 4.24 percentage points from the benchmark statistic among the control group and an average 5.37 percentage point difference in the treatment group.

	Treatment			Control			Micro census
	1	2	3	1	2	3	
Female	51.51%	51.97%	55.32%	51.22%	52.44%	52.38%	51.00%
Male	48.49%	48.03%	44.68%	48.78%	47.56%	47.62%	49.00%
Migration background	21.62%	19.72%	19.31%	20.33%	18.80%	18.15%	14.85%
No migration background	78.38%	80.28%	80.69%	79.67%	81.20%	81.85%	85.15%

Table B.4. *Demographic statistics – proportions in gender and migration background compared to the German census data for all waves and respondent groups.*

We also compare our respondents' levels of political interest and external efficacy to other

relevant samples, namely that of the "Shell Jugendstudie 2019" [Shell Youth Study], which sampled young citizens between the ages of 12 and 25, and the German Longitudinal Election Study's post-election cross-section (GLES 2017), which covers the eligible population. Both surveys employ computer-assisted personal interviews (CAPI) – see Figure B.2. When comparing the share of respondents reporting to have high levels of external efficacy and political interest to the Shell Youth Study 2019, it becomes apparent that our sample proportions are by no means exceptional but rather the norm. Our young respondents apparently are very similar to young people across Germany, as sampled by the Shell Youth Study. However, it is important to highlight that respondents of the GLES have substantially higher efficacy and political interest, which is mainly a factor of older age groups covered in the GLES sample compared to our study and the Shell Youth Study.

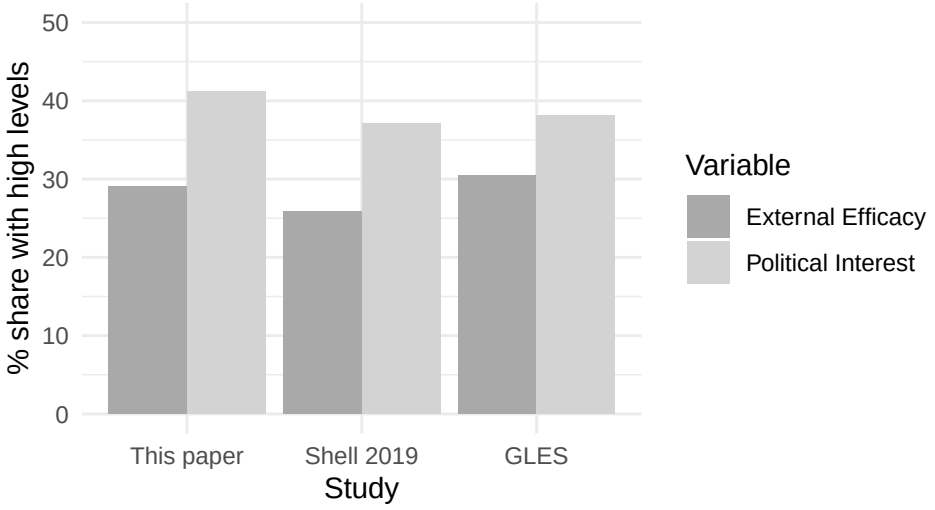

Figure B.2. *Comparison of external efficacy and political interest in our sample (15- to 18-year-olds) with samples from the Shell Youth Study (12- to 25-year-olds) and German Longitudinal Election Study (German population 16 and older).*

B.3 Variables and summary statistics

Here, we explain how we operationalized and measured our four independent variables and provide summary statistics across groups and survey waves. Firstly, internal efficacy – a respondent's evaluation of their own political abilities – is measured on a five-point Likert scale ranging from 1 to 5, with low values indicating low efficacy and high values indicating high efficacy. External efficacy is also measured on a five-point Likert scale. Both are based on the respective standard items used in many surveys (cf. Table B.6). Higher values indicate that respondents felt politicians cared about the interest of young voters, while low values indicate lower evaluations of the system's responsiveness. Political interest, also measured on a five-point Likert-scale item, captures how interested a respondent is in politics in general. High values indicate high levels of interest; low values indicate low interest. Finally, democratic satisfaction

– a respondent's level of satisfaction with democracy as it exists in Germany – is measured on a five-point Likert scale, with low values indicating low democratic satisfaction and high values indicating high democratic satisfaction.

	Wave 1			Wave 2			Wave 3		
	1	2	3	1	2	3	1	2	3
External Efficacy	2.97 (1.03)	3.05 (1.01)	3.07 (1.03)	3.04 (0.98)	2.88 (0.97)	3.00 (0.97)	2.88 (0.96)	2.80 (0.96)	2.79 (0.96)
Satisfaction with Democracy	3.63 (0.92)	3.68 (0.88	3.72 (0.87)	3.74 (0.82)	3.62 (0.90)	3.76 (0.89)	3.57 (0.92)	3.58 (0.91)	3.65 (0.89)
Internal Efficacy	3.08 (1.03)	3.18 (1.01)	3.17 (1.03)	3.29 (0.98)	3.44 (0.97)	3.43 (0.97)	3.19 (0.96)	3.34 (0.96)	3.24 (0.96)
Political Interest	3.25 (0.99)	3.28 (0.96)	3.17 (0.93)	3.43 (0.94)	3.53 (0.90)	3.46 (0.87)	3.33 (1.01)	3.37 (0.98)	3.27 (0.91)

Table B.5. *Summary statistics – mean and standard deviation (in parentheses) – of the dependent variables for all waves and groups of respondents as defined above.*

B.4 Questionnaire

Variable	German (original)	English (authors' translation)	Response Items (German original and English translation)
External Efficacy	Im Folgenden finden Sie einige Aussagen rund um Wahlen. Bitte geben Sie jeweils an, inwieweit Sie den einzelnen Aussagen zustimmen. [...] Politiker kümmern sich nicht darum, was junge Leute wie ich denken.	Below you will find a number of statements relating to elections. Please indicate the extent to which you agree with each statement. [...] Politicians do not care what young people like me think.	Stimme voll und ganz zu, Stimme eher zu, Teils/teils, Stimme eher nicht zuStimme überhaupt nicht zu Strongly agree, agree, neutral, disagree, strongly disagree
Satisfaction with Democracy	Wie zufrieden oder unzufrieden sind Sie – alles in allem – mit der Demokratie, so wie sie in Deutschland besteht?	How satisfied or dissatisfied are you - overall - with democracy as it exists in Germany?	Sehr zufrieden, eher zufrieden, Teils/teils, Eher unzufrieden, Unzufrieden Very satisfied, fairly satisfied, neutral, fairly unsatisfied, very unsatisfied (scale reversed in analyses)
Internal Efficacy	Im Folgenden finden Sie einige Aussagen rund um Wahlen. Bitte geben Sie jeweils an, inwieweit Sie den einzelnen Aussagen zustimmen. [...] Ich traue mir zu, in einer Gruppe, die sich mit politischen Fragen befasst, eine aktive Rolle zu übernehmen.	Below you will find a number of statements relating to elections. Please indicate the extent to which you agree with each statement. [...] I feel confident to take an active role in a group that discusses political issues.	Stimme voll und ganz zu, Stimme eher zu, Teils/teils, Stimme eher nicht zuStimme überhaupt nicht zu Strongly agree, agree, neutral, disagree, strongly disagree (scale reversed in analyses)
Political Interest	Wenn Sie jetzt einmal ganz allgemein an Politik denken: Wie stark interessieren Sie sich für Politik	If you think about politics in general: How much are you interested in politics?	Sehr stark, Stark, Mittelmäßig, Weniger stark, Überhaupt nicht Strongly interested, interested, moderately interested, weakly interested, not interested (scale reversed in analyses)

Table B.6. *Dependent variables, original question wordings (in German) and authors' translations (to English)*

C Auxiliary results

In the first wave of our panel survey, we asked all respondents whether they would participate in the upcoming national election. Respondents could indicate their likelihood to turn out on a scale from 1 (certainly will) to 5 (certainly not) and were also given the option to respond that they will not be eligible to vote. Table C.1 reduces the variable to three categories and compares answers across respondents of differing eligibility status across the two elections. What is striking is that more than a third of 16- and 17-year-old respondents who voted in the state election said they would also vote in the national election, having not yet realized they would not be eligible. This descriptive finding already hints at the potentially frustrating effects of temporary disenfranchisement, which can also be seen in the following table.

State election eligibility status	**National election eligibility status**	**Will vote**	**Will not vote**	**Will not be eligible**	**Sum**
Voters	***Eligible***	785	6	3	794
		(98.9%)	(0.8%)	(0.4%)	(100%)
	Ineligible	571	8	889	1468
		(38.9%)	(0.5%)	(60.6%)	(100%)
Non-Voters	***Eligible***	149	42	3	194
		(76.8%)	(21.6%)	(1.5%)	(100%)
	Ineligible	172	49	94	315
		(54.6%)	(15.6%)	(29.8%)	(100%)
Ineligible	***Ineligible***	280	26	530	836
		(33.5%)	(3.1%)	(63.4%)	(100%)

Table C.1. *Intention to vote in the upcoming federal election among voters, non-voters, and ineligible respondents (rows) by eligibility status in the federal election (columns): absolute numbers and row percentages of those intending to vote per cell.*

To capture how respondents felt about not being eligible to vote, we asked respondents in the second wave of the panel survey conducted right after the national election whether they were angry (*verärgert*) that they were not able to vote.[8] Table C.2 compares answers from respondent eligible to vote in the state but not the national election (group 2) and respondents not eligible for both elections (group 3). Here, we can see that respondents who were able to vote before are significantly more angry about not being able to vote than those who were not, suggesting that there is something particularly frustrating about having taken the right vote away again (even if only temporarily).

8. Although the German wording translates directly to anger, its connotation is not as strong as the English word and can also imply being frustrated, annoyed, or upset.

Variable	Group 2	Group 3	Difference	N
Average Anger	3.98	3.55	0.42***	1238
Percentage Very Angry	0.39	0.23	0.16***	1238

*** p < .01; ** p < .05; * p < .1

Table C.2. *Average anger and percentage of respondents being very angry over not being eligible to vote in the federal election among group 2 (the treatment group being temporarily disenfranchised) and group 3 (respondents not eligible for state and national election). Data comes from wave 2 of the panel survey.*

Respondents who do not yet have the right to vote or who have lost it temporarily due to temporary disenfranchisement only have non-electoral forms of political participation available to them to make their voices heard. Table C.3 shows respondents' reported participation in various forms of non-electoral participation. The relevant item battery was, unfortunately, only fielded in wave 3 of the panel survey and asked whether respondents' participated in any of the listed activities at least once in the past 12 months. We can therefore not analyze respondents' non-electoral participation in our DiD framework. We instead, merely, provide some descriptive results in Table C.3. Interestingly, respondents belonging to group 2, the treatment group affected by temporary disenfranchisement, report the highest frequency of activity across almost all forms of participation. Of course, given the cross-sectional nature of the data, we cannot rule out a mere cohort effect. Nevertheless, these results are suggestive of an effect of temporary disenfranchisement on non-electoral political participation. The temporary loss of franchise, which angers affected young citizens (cf. Table C.2) but leaves them unswayed in their confidence about their political selves, seems to trigger other forms of political participation. In any case, this pattern is certainly not a linear age effect, as respondents in group 1 are older but less active than respondents of group 2.

Variable	Group 1	Group 2	Group 3
'Liked' political content on social media	39.9	42.7	37.0
Signed an online petition	29.4	27.1	20.2
Forwarded or shared other people's political posts on social media	23.2	23.9	23.5
Offline signature collection	22.0	22.9	16.4
Actively participated in discussions at public meetings	15.8	19.0	16.7
Protest	14.9	15.3	13.2
Posted political contributions on social media or mailing lists	10.8	12.1	9.1
Contacted a politician	5.0	9.3	5.0
Citizen participation platforms used by government agencies on the Internet, e.g. participatory budgeting, or liquid democracy forums	3.7	5.2	4.1
Citizen initiative	2.3	3.3	2.9

Table C.3. *Percentage of respondents engaging in non-electoral forms of political participation. All measurements are from wave 3 (after the municipal election) of the panel survey.*

D Research Design

We use a difference-in-differences design (DiD) to investigate the effect of temporary disenfranchisement on affected underage citizens. A DiD model is a commonly applied method of causal identification, which compares the differences in trends in the dependent variable between the treatment and control group. For a more extensive elaboration of the concept see, for instance, Angrist and Pischke (2008, Chapter 5).

We estimate the following model:

$$Y_i = \alpha + \beta T_i + \gamma t_i + \delta(T_i \times t_i) + \varepsilon_i$$

In this equation, an individual's political attitude is therefore modeled as the result of the constant term α (indicating average values in the control group in wave 1), initial differences in level between treatment and control group β, a secular time trend γ, as well as the difference in time trends between treatment and control group δ. For further interpretation, it is important to point out that while β accounts for average permanent differences between treatment and control group in the absence of the intervention, it is δ that measures the actual treatment effect of variable eligibility. In contrast, γ measures a change over time in the absence of treatment.

Thus, as demonstrated in Figure 1, one would assume that both groups would have followed the same trend in the dependent variable, in this case, external efficacy, if it were not for the national election, in which the treatment group experienced temporary disenfranchisement after already having voted in the previous state election. After this intervention, the treatment group potentially decreases in external efficacy.

E Main results

The following tables present the models generating the coefficient estimates presented in Figure 2 in the manuscript.

	External Efficacy	Satisfaction with Democracy	Internal Efficacy	Political Interest
Group 2	0.04	0.01	0.12*	0.08
	(0.05)	(0.05)	(0.06)	(0.05)
Wave 2	−0.02	0.02	0.12*	0.12**
	(0.06)	(0.05)	(0.07)	(0.06)
Group 2 × Wave 2	−0.19**	−0.14**	0.03	0.03
	(0.08)	(0.07)	(0.09)	(0.07)
(Intercept)	3.06***	3.73***	3.17***	3.31***
	(0.04)	(0.04)	(0.05)	(0.04)
R^2	0.01	0.00	0.01	0.01
N	2861	2873	2864	2885

***$p < 0.01$; **$p < 0.05$; *$p < 0.1$

Table E.1. *The effect of* losing eligibility. *Presented estimates capture the results from DiD-specifications comparing groups 1 and 2 across waves 1 and 2.*

	External Efficacy	Satisfaction with Democracy	Internal Efficacy	Political Interest
Group 2	-0.15^{**}	-0.09	0.06	0.06
	(0.06)	(0.06)	(0.08)	(0.06)
Wave 3	-0.20^{***}	-0.15^{**}	-0.25^{***}	-0.17^{**}
	(0.07)	(0.07)	(0.09)	(0.07)
Group 2 × Wave 3	0.09	0.07	0.13	0.01
	(0.09)	(0.08)	(0.12)	(0.09)
(Intercept)	3.07^{***}	3.76^{***}	3.42^{***}	3.51^{***}
	(0.05)	(0.05)	(0.06)	(0.05)
R^2	0.01	0.01	0.01	0.01
N	1840	1856	1841	1860

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table E.2. *The effect of* regaining eligibility*. Presented estimates capture the results from DiD-specifications comparing groups 1 and 2 across waves 2 and 3.*

	External Efficacy	Satisfaction with Democracy	Internal Efficacy	Political Interest
Group 2	0.07	0.05	0.13^{*}	0.11^{*}
	(0.06)	(0.05)	(0.07)	(0.06)
Wave 3	-0.20^{***}	-0.15^{**}	-0.05	-0.02
	(0.07)	(0.06)	(0.09)	(0.07)
Group 2 × Wave 3	-0.14^{*}	-0.04	0.03	-0.07
	(0.09)	(0.08)	(0.11)	(0.09)
(Intercept)	3.08^{***}	3.72^{***}	3.23^{***}	3.36^{***}
	(0.05)	(0.04)	(0.06)	(0.05)
R^2	0.02	0.01	0.00	0.00
N	2245	2256	2243	2268

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table E.3. *The net effect of* temporary disenfranchisement*, i.e., of losing and re-gaining eligibility. Presented estimates capture the results from DiD-specifications comparing groups 1 and 2 across waves 1 and 3.*

F Additional results

F.1 Placebo treatment group

Our data, unfortunately, do not allow us to test for parallel trends before the treatment (sometimes referred to as placebo treatment timing) to plausibilize this assumption. However, we can construct a placebo treatment group to plausibilize the parallel trends assumption. To do so, we can compare groups 1 – those who were 18 at the time of the national election – and 3 – those who were 15 at the time of the state election. For respondents belonging to these two groups, eligibility status did not change from wave 1 to 2 of our survey. Respondents belonging to group 1 were eligible for both elections, while respondents belonging to group 3 were ineligible for both elections. If our argument, that absent any major shocks, such as temporary disenfranchisement, attitudes among adolescents and young adults develop similarly over time is correct, we should see no difference in trends between these two groups.

	External Efficacy	Satisfaction with Democracy	Internal Efficacy	Political Interest
Group 3	0.09	0.06	0.12	0.01
	(0.06)	(0.06)	(0.08)	(0.06)
Wave 2	−0.02	0.02	0.12*	0.12**
	(0.06)	(0.05)	(0.07)	(0.06)
Group 3 × Wave 2	−0.13	−0.04	0.02	0.02
	(0.09)	(0.08)	(0.11)	(0.09)
(Intercept)	3.06***	3.73***	3.17***	3.31***
	(0.04)	(0.04)	(0.05)	(0.04)
R^2	0.00	0.00	0.01	0.00
N	1891	1899	1889	1903

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table F.1. *The "effect" of* losing eligibility*. Presented estimates capture the results from DiD-specifications comparing groups 1 (original control group) and 3 (placebo treatment group) across waves 1 and 2. Both groups do not actually experience a change in eligibility status between the two waves.*

F.2 Comparison of treatment group with alternative control group

Table F.2 presents models, which mirror the specifications of the main models (cf. Table E.1), but the sample includes groups 2 and 3, i.e., drops group 1, and compares them across waves 1 and 2 of the panel survey. Here, group 3 (instead of group 1) serves as an alternative albeit imperfect control group. Respondents in group 3 can serve as a control group because their eligibility status remains constant across the state election in May 2017 and the national election in September 2017: they are not eligible to vote in both. However, this group is much smaller than the other two groups, thereby, providing less power for statistical tests. Also, we can compare groups 2 and 3 across waves 1 and 2 only because group 3's eligibility status also changes with wave 3: respondents belonging to this group are all 16 at the time of municipal elections and, hence, are eligible to vote. Notwithstanding these difficulties, we do obtain results broadly in line with our hypotheses H1a and H1b: the coefficients on the treatment variable ($Group\ 2 \times Wave\ 2$) carry the expected sign but are estimated with greater imprecision.

	External Efficacy	Satisfaction with Democracy	Internal Efficacy	Political Interest
Group 2	−0.06	−0.05	−0.00	0.07
	(0.06)	(0.05)	(0.07)	(0.06)
Wave 2	−0.15**	−0.02	0.14	0.14**
	(0.07)	(0.06)	(0.09)	(0.07)
Group 2 × Wave 2	−0.06	−0.10	0.01	0.00
	(0.09)	(0.08)	(0.10)	(0.08)
(Intercept)	3.15***	3.78***	3.29***	3.32***
	(0.05)	(0.04)	(0.06)	(0.05)
R^2	0.01	0.01	0.00	0.01
N	2524	2536	2525	2550

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table F.2. *The effect of* losing eligibility*. Presented estimates capture the results from DiD-specifications comparing groups 2 and 3 across waves 1 and 2*

F.3 Results for respondents who voted in the state election only

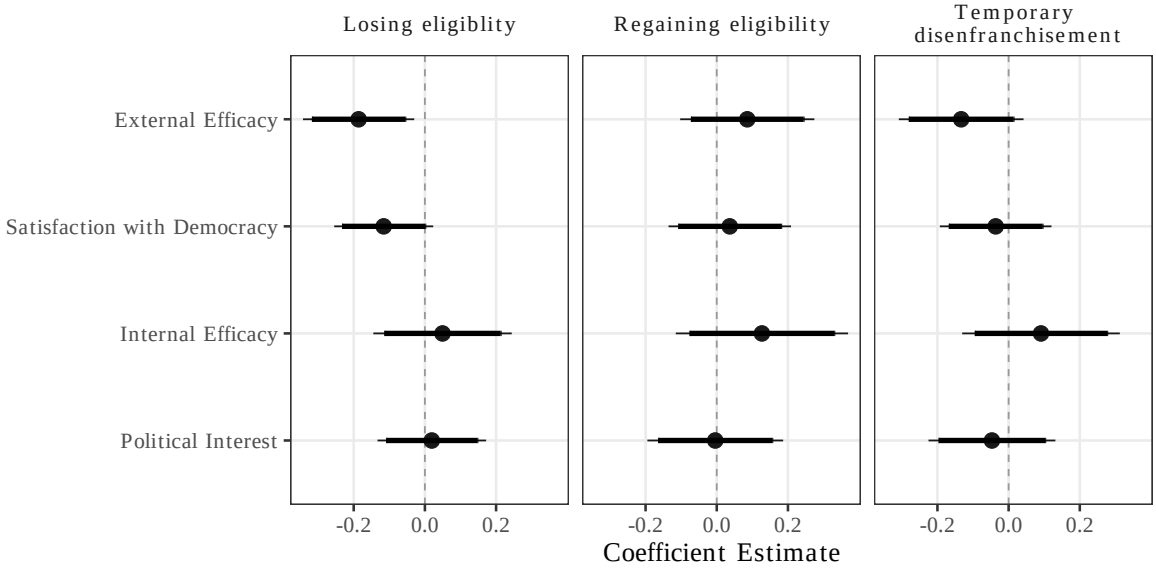

Figure F.1. *Estimates of the effect of* losing eligibility *(left panel),* regaining eligibility *(mid panel) and both taken together, i.e.,* temporary disenfranchisement*, (right panel) on external political efficacy, democratic satisfaction, political interest, and internal efficacy. The coefficient plot shows results from DiD-specifications comparing groups 1 and 2 across waves 1 and 2 (left panel), waves 2 and 3 (middle panel), and 1 and 3 (right panel). We estimate the DiD-specification on* a sample of respondents who voted in the state election only*. Horizontal bars indicate 90% and 95% confidence intervals.*

The following figure and tables present the models mirroring those presented in section E but estimated on a sample of voters in the federal election only. Treatment effects estimated from these models are visualized in Figure F.1. We do not present separate models for non-voters as those make up only 11% of our sample.[9] The results presented in the main text took both non-voters and voters into account. They can be considered conservative estimates of the effect of temporary disenfranchisement because non-voters, those who did not make use of their voting rights in the state election, should not be affected as strongly by disenfranchisement. And indeed, the results presented here indicate an even stronger effect of disenfranchisement on actual voters in the state election.

9. Only 209 (11%) out of 1900 respondents who participated in the first and second wave said that they did not participate in the election. 65.8% said that they did, and the remaining 23.2% were not eligible to vote in the state election because of their age.

	External Efficacy	Satisfaction with Democracy	Internal Efficacy	Political Interest
Group 2	0.06	0.00	0.13*	0.10*
	(0.06)	(0.05)	(0.07)	(0.05)
Wave 2	−0.03	0.01	0.10	0.09
	(0.06)	(0.06)	(0.08)	(0.06)
Group 2 × Wave 2	−0.19**	−0.12	0.05	0.02
	(0.08)	(0.07)	(0.10)	(0.08)
(Intercept)	3.09***	3.77***	3.28***	3.39***
	(0.04)	(0.04)	(0.05)	(0.04)
R^2	0.01	0.00	0.01	0.01
N	2403	2412	2404	2421

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table F.3. *The effect of* losing eligibility*. Presented estimates capture the results from DiD-specifications on* a sample of respondents who voted in the state election *comparing groups 1 and 2 across waves 1 and 2.*

	External Efficacy	Satisfaction with Democracy	Internal Efficacy	Political Interest
Group 2	−0.15**	−0.06	0.09	0.07
	(0.07)	(0.06)	(0.09)	(0.07)
Wave 3	−0.19**	−0.14**	−0.25**	−0.15*
	(0.08)	(0.07)	(0.10)	(0.08)
Group 2 × Wave 3	0.09	0.04	0.13	−0.00
	(0.10)	(0.09)	(0.12)	(0.10)
(Intercept)	3.09***	3.80***	3.47***	3.56***
	(0.05)	(0.05)	(0.07)	(0.05)
R^2	0.01	0.01	0.01	0.01
N	1637	1650	1639	1654

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table F.4. *The effect of* regaining eligibility*. Presented estimates capture the results from DiD-specifications on* a sample of respondents who voted in the state election *comparing groups 1 and 2 across waves 2 and 3.*

	External Efficacy	Satisfaction with Democracy	Internal Efficacy	Political Interest
Group 2	0.06	0.06	0.11	0.10
	(0.06)	(0.06)	(0.08)	(0.06)
Wave 3	−0.20***	−0.14**	−0.08	−0.05
	(0.07)	(0.06)	(0.09)	(0.07)
Group 2 × Wave 3	−0.13	−0.04	0.09	−0.05
	(0.09)	(0.08)	(0.11)	(0.09)
(Intercept)	3.11***	3.75***	3.32***	3.45***
	(0.05)	(0.04)	(0.06)	(0.05)
R^2	0.02	0.01	0.00	0.00
N	1961	1972	1960	1981

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table F.5. *Main results of the net effect of losing and regaining one's right to vote, i.e.,* temporary disenfranchisement*. Presented estimates capture the results from DiD-specifications on* a sample of respondents who voted in the state election *comparing groups 1 and 2 across waves 1 and 3.*

F.4 Turnout after temporary disenfranchisement

Although turnout is undoubtedly a highly important outcome variable, it is complicated to investigate the effect of temporary disenfranchisement on turnout. Here, we are severely limited by the fact that group 2 is eligible for only two out of three elections and group 3 for only one election. Hence, the only DiD specification that we are able to estimate is a comparison of groups 1 and 2 across waves 1 and 3 – Columns (1) and (2) in Table F.6. However, running a regression on a dichotomous dependent variable via OLS and imposing a parallel trends assumption can be regarded as questionable. Our analysis is further complicated by the fact that, unlike attitudes, turnout has a strong mechanical component. What we mean by this is that independent of their attitudes, young citizens who, for instance, still live at home with their parents will be more likely to vote than those who do not simply because their parents will take them along to the polling station. This complicates the comparison because the share of respondents leaving the parental home, school, or both differ between the two groups. We try to control for this by including respective covariates and by estimating the model on a sample of respondents whose status does not change. The results we obtain can be characterized as inconclusive. While model 1 seems to suggest a positive effect if at all, significant only at the 5%-level, model 2 indicates a similar but insignificant effect.

	Turnout			
	(1)	(2)	(3)	(4)
Group 2	0.00	0.05	−0.02	−0.10
	(0.02)	(0.03)	(0.03)	(0.18)
Wave 3	−0.17***	−0.18***		
	(0.03)	(0.04)		
Group 2 × Wave 3	0.07*	0.07		
	(0.03)	(0.04)		
Lives in family home	0.08**		0.12*	0.62*
	(0.03)		(0.06)	(0.33)
Attends school	0.03		0.06	0.34
	(0.03)		(0.06)	(0.31)
(Intercept)	0.79***	0.86***	0.62***	0.48
	(0.04)	(0.03)	(0.08)	(0.40)
R^2	0.04	0.04	0.01	
N	2158	1545	926	926
Log Likelihood				−476.13

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table F.6. *The effect of* temporary disenfranchisement *on turnout. Columns (1) and (2) capture results from DiD-specifications comparing groups 1 and 2 across waves 1 and 3. The model in column 1 controls for differential levels and trends in living at home and attending school between the two groups by including respective covariates. The model in column 2 controls for these factors by being estimated on a sample of voters who experience no change in values for these covariates from waves 1 to 3. Columns (3) and (4) capture results from cross-sectional specifications comparing groups 2 and 3 in wave 3. The first model is a linear model estimated by OLS, while the second model is a logistic model estimated by maximum likelihood.*

An alternative approach is a cross-sectional comparison of groups 2 and 3 in wave 3 –

Columns (3) and (4) in Table F.6. Here, a DiD is not possible because group 3 is only eligible in the local elections. Still, we conduct this comparison because groups 2 and 3 are more similar in living circumstances than are groups 1 and 2 because respondents in the former two groups are still young enough to mostly live with their parents and go to school. Here, we find slightly lower turnout among temporarily disenfranchised citizens in their second-ever election compared to turnout in group 3's first-ever election. However, estimates are again insignificant both for a linear (model 3) and logistic (4) specification. Even if they were significant, interpretation of the results would be difficult: Does the difference between groups 2 and 3 in wave 3 represent demobilizing effect of temporary disenfranchisement on group 2 or a boost-effect of the first-ever election for group 3? In summary, robustly estimating the effect of temporary disenfranchisement on turnout with our data is very complicated, if not impossible, and the results we are able to obtain are inconclusive. As a more detailed analysis is beyond the scope of this paper, the impact of temporary disenfranchisement on turnout constitutes an important topic for future research.

G Creating treatment and control groups that resemble each other more closely

To further test the robustness of our results, we created samples with more equal and, therefore, comparable groups by using nearest neighbor matching and by reducing the sample to respondents born close to the cutoff date governing (in)eligibility in the federal election.

G.1 Matching

Applying nearest neighbor matching, using the MatchIt package (D. E. Ho et al. 2007; D. Ho et al. 2011), we matched treatment and control group on the variables of gender, school type, and whether a respondent lives in a larger city. Figure G.1 shows covariate balance between treatment and control group before and after matching.

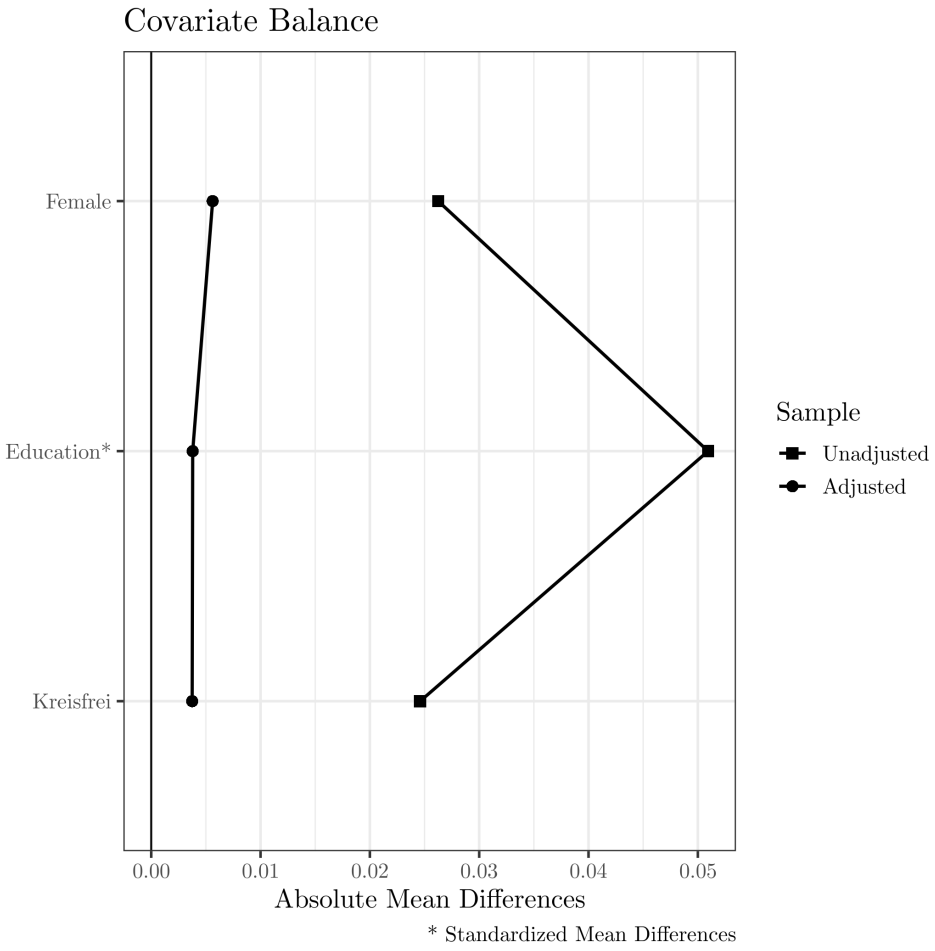

Figure G.1. *Balance in covariates (gender, school type, size of city) before and after matching on the variables. Adjusting for the variables decreases standardized mean differences between treatment and control group.*

G.2 Reduced samples defined by respondents' birth dates

We created six different samples based on how many days a respondent's birthday was away from the cutoff date defining eligibility. In steps of 50 days, we restricted the sample to respondents born within 50 to 300 days before or after the cutoff date defining eligibility. This means we are comparing respondents who, in terms of age, differ by less than a year on average. This implies that both eligible and ineligible respondents still live in relatively similar contexts, e.g., they attend the same grade in school. Table G.1 compares the covariate balance between treatment and control groups across the different subsamples. As can be seen there, differences in observables between treatment and control groups are very small across all samples. However, reducing the difference in age between treatment and control groups should reduce the difference in, for instance, cognitive development and other unobservables possibly linked to our dependent variables.

	Full sample	50	100	150	200	250	300
Female	-0.03	0.01	0.01	-0.04	-0.04	-0.03	-0.04
Education	-0.02	-0.03	-0.06	-0.06	-0.03	-0.02	-0.02
Lives in bigger city	0.03	-0.03	-0.04	-0.03	-0.02	-0.03	0.01

Table G.1. *Balance in covariates (gender, school type, respondent lives in a bigger city) in treatment and control groups.*

H Results based on alternative samples

We test the robustness of our findings in the following section by re-estimating the models on different subsamples, which contain treatment and control groups that resemble each other more closely. One limitation of our data source is that we cannot explicitly test the parallel trends assumption as we do not observe respondents for multiple periods before the treatment. Although a DiD does not require us to assume balanced groups, we would be more confident about respondents developing in similar ways over the five months between two elections if they were even more similar in terms of age and other characteristics than they already are.

H.1 Matching

Results of this robustness test suggest an even more substantial effect of temporary disenfranchisement. Losing one's eligibility is associated with a 0.25 point decrease in external efficacy, meaning that the average respondent decreases by around 8.2%. All other dependent variables are not affected by the treatment. As with the previously presented analyses, regaining the right to vote might have a slightly positive effect. However, this effect is not significant as intervals of all coefficients cross the dashed line and are therefore not significantly different from zero – see Figure H.1. Turning to the net effect of temporary disenfranchisement, we can again confirm previously reported results. Losing and regaining the right to vote is associated with a 0.17 point decrease in a young voter's perception of the system's responsiveness. Thus, solely by experiencing temporary disenfranchisement, an average respondent of the treatment group decreased by nearly 6% in their external efficacy.

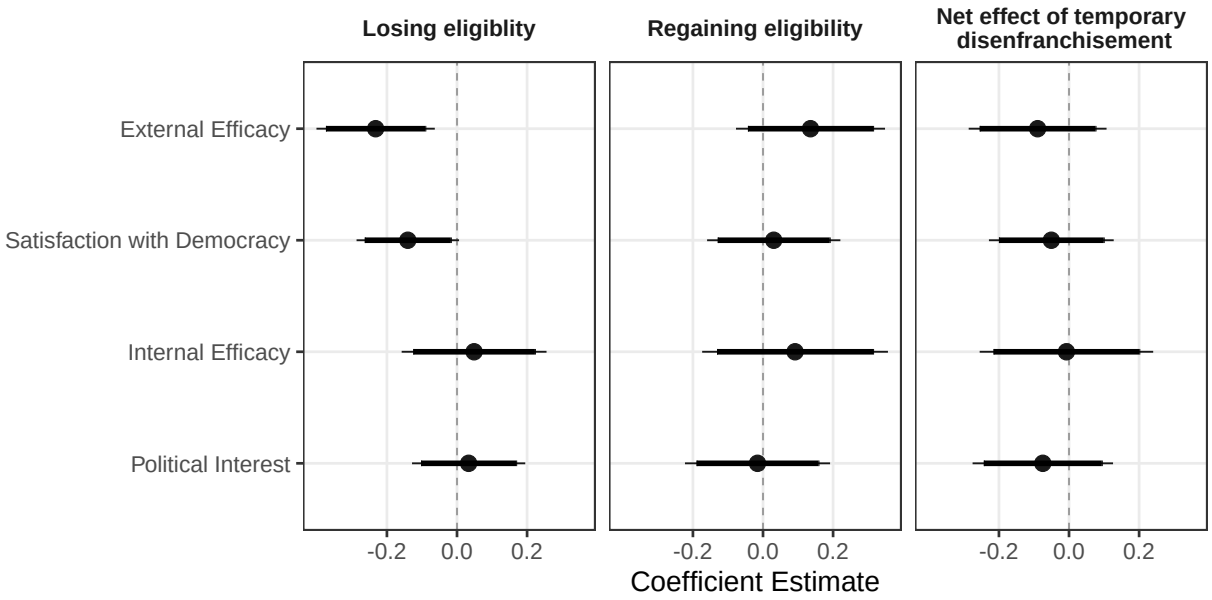

Figure H.1. *The effect of* losing eligibility *(left panel),* regaining eligibility *(mid panel) and both taken together, i.e.,* temporary disenfranchisement*, (right panel) on external political efficacy, democratic satisfaction, political interest, and internal efficacy. The coefficient plot shows results (for coefficient δ) from DiD-specifications comparing groups 1 and 2 across waves 1 and 2 (Panel 1), waves 2 and 3 (Panel 2), and 1 and 3 (Panel 3). We estimate the DiD-specification on the* matched sample*.*

	External Efficacy	Satisfaction with Democracy	Internal Efficacy	Political Interest
Group 2	0.06	0.05	0.10	0.05
	(0.06)	(0.05)	(0.07)	(0.06)
Wave 2	0.01	0.01	0.09	0.09
	(0.06)	(0.05)	(0.07)	(0.06)
Group 2 × Wave 2	-0.23^{***}	-0.14^{*}	0.05	0.03
	(0.09)	(0.07)	(0.11)	(0.08)
(Intercept)	3.05^{***}	3.74^{***}	3.21^{***}	3.34^{***}
	(0.04)	(0.04)	(0.05)	(0.04)
R^2	0.01	0.00	0.00	0.00
N	2091	2102	2092	2109

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table H.1. *The effect of* losing eligibility*. Presented estimates capture the results from DiD-specifications comparing groups 1 and 2 across waves 1 and 2. Based on samples created through matching.*

	External Efficacy	Satisfaction with Democracy	Internal Efficacy	Political Interest
Group 2	-0.17^{**}	-0.05	0.07	0.10
	(0.08)	(0.07)	(0.10)	(0.07)
Wave 3	-0.24^{***}	-0.14^{**}	-0.27^{***}	-0.17^{**}
	(0.08)	(0.07)	(0.10)	(0.07)
Group 2 × Wave 3	0.14	0.03	0.09	-0.02
	(0.11)	(0.10)	(0.14)	(0.11)
(Intercept)	3.10^{***}	3.78^{***}	3.46^{***}	3.53^{***}
	(0.05)	(0.05)	(0.07)	(0.05)
R^2	0.01	0.01	0.01	0.01
N	1300	1308	1300	1309

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table H.2. *The effect of* regaining eligibility*. Presented estimates capture the results from DiD-specifications comparing groups 1 and 2 across waves 2 and 3. Based on samples created through matching.*

	External Efficacy	Satisfaction with Democracy	Internal Efficacy	Political Interest
Group 2	0.01	0.06	0.15^{*}	0.13^{*}
	(0.07)	(0.06)	(0.09)	(0.07)
Wave 3	-0.22^{***}	-0.13^{**}	-0.05	-0.01
	(0.07)	(0.06)	(0.09)	(0.07)
Group 2 × Wave 3	-0.09	-0.05	-0.01	-0.07
	(0.10)	(0.09)	(0.13)	(0.10)
(Intercept)	3.09^{***}	3.72^{***}	3.26^{***}	3.36^{***}
	(0.05)	(0.05)	(0.06)	(0.05)
R^2	0.02	0.01	0.00	0.00
N	1532	1539	1531	1543

$^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$

Table H.3. *Main results of the net effect of* temporary disenfranchisement*. Presented estimates capture the results from DiD-specifications comparing groups 1 and 2 across waves 1 and 3. Based on samples created through matching.*

H.2 Birth date samples

We further tested the robustness of our results using different bandwidths. While, of course, confidence intervals widen as the sample size decreases, as can be seen in Figure H.2, the effect remains relatively stable across various birth date samples, making us more confident in our results.

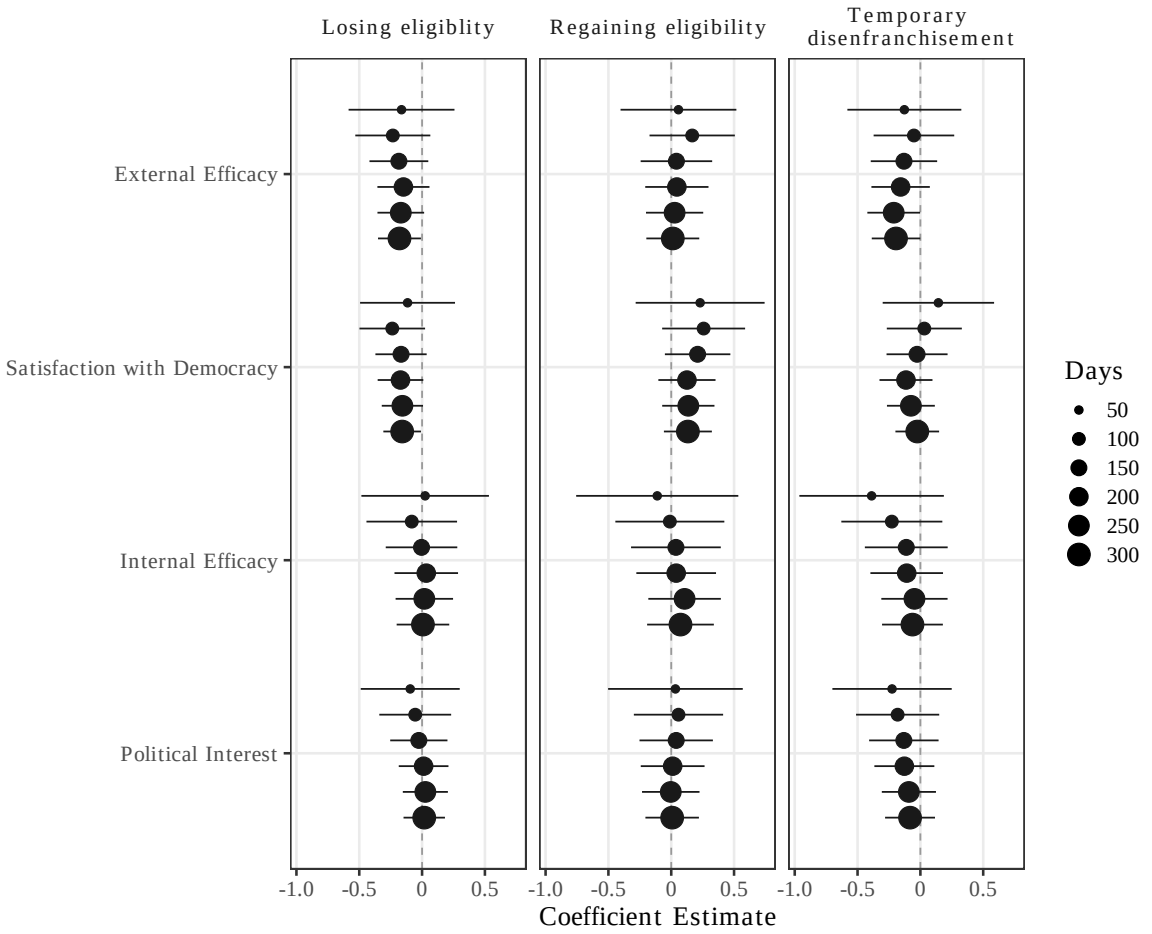

Figure H.2. *Estimates of the effect of* losing eligibility *(left panel),* regaining eligibility *(mid panel) and both taken together, i.e.,* temporary disenfranchisement*, (right panel) on external political efficacy, democratic satisfaction, political interest, and internal efficacy based on different samples. We estimate the DiD-specification on various samples of respondents defined by their birthdate: up to 50, 100, 150, 200, 250, or 300 days before or after the cutoff date defining eligibility in the national election. The coefficient plot shows results from DiD-specifications comparing groups 1 and 2 across waves 1 and 2 (left panel), waves 2 and 3 (middle panel), and 1 and 3 (right panel). Horizontal bars indicate 95% confidence intervals. We report the models behind these results in section H of main.Rmd / main.pdf in the replication materials, which can be found at https://doi.org/10.7910/DVN/T5LYWS*

I Replication materials

Replication materials to reproduce all findings presented in the paper and this appendix are provided at Harvard Dataverse: https://doi.org/10.7910/DVN/T5LYWS.

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