Early Voting Experiences and Habit Formation

Elias Dinas[*]   Vicente Valentim[†]   Nikolaj Broberg[‡]   Mark N. Franklin[§]

March 5, 2021

Abstract

Research has shown that first-time voting experiences affect subsequent voting behavior, with salient elections boosting subsequent turnout and non-salient ones supressing it. We challenge this view. Following research on the context-dependent nature of habit formation, we argue that all elections should affect subsequent turnout in elections of the same type. Comparing individuals that differ only in how salient their first eligible election was (Presidential or Midterm), we find support for this expectation. Individuals are more likely to vote for, and be interested in, elections of the same type as their first voting experience. Leveraging voting age laws in the US, we also show that such laws affect subsequent participation by changing the type of election individuals are first eligible for.

Keywords: voting habits; midterm elections; turnout; presidential elections; eligibility

[*]European University Institute, Department of Political and Social Sciences, Villa Sanfelice, San Domenico di Fiesole, I-50014, Italy. Email: elias.dinas@eui.eu.

[†]European University Institute, Department of Political and Social Sciences, Badia Fiesolana, San Domenico di Fiesole, I-50014, Italy. Email: vicente.valentim@eui.eu.

[‡]European University Institute, Department of Economics, Villa La Fonte, San Domenico di Fiesole, I-50014, Italy. Email: nikolaj.broberg@eui.eu.

[§]Independent Scholar, Mark.Franklin@eui.eu

There is growing consensus among students of voting behavior that the electoral setting individuals encounter in their first eligible election is crucial in shaping future voting patterns (Meredith 2009; Dinas 2012; Kadt 2017; Coppock and Green 2016). The dominant view in this literature argues that this effect is due to the resources individuals gather from their initial voting experiences, which can make it easier for them to cast a vote later on (Gerber, Green, and Shachar 2003). An implication of this argument is that the direction of the effect should depend on the salience of the first election one is eligible to vote for. More salient elections are deemed to boost future turnout (Meredith 2009) by generating voting habits (Plutzer 2002; Franklin 2004). Less salient elections are hypothesized to either leave no imprint on future voting trajectories (Meredith 2009), or to build non-voting habits that undermine future electoral participation (Franklin and Hobolt 2011). This conclusion can fuel arguments against the existence of low-salience elections such as the European elections in EU member states, which could create abstentionist habits in voters.

There are, however, theoretical grounds to challenge this dominant view. Habits tend to be formed by individuals recognizing patterns in the co-occurrence of a given action and the contextual setting in which such action is performed (Hall 1969; Wood and Neal 2007). Once cued with the similarity between the present context and a previous one, individuals tend to respond to it with the same behavior (Wood and Neal 2007). For this reason, contextual continuity is crucial for the formation of habits.

A number of empirical studies have shown that the formation of human habits depends on the continuity of the context in which they take place. Individuals tend to perform a number of activities mostly under a similar context. For example, they tend watch the news on television on the same channel, at the sime time of the day; and they tend to purchase similar foods at the same time of the day (Ji and Wood 2007). Once created, these habits can last even if they are inefficient (Larcom, Rauch, and Willems 2017). Conversely, disruptions to contextual continuity are likely to lead individuals to break habits (Wood, Tam, and Witt 2005).

1

Similar to other types of behavior, voting behavior can also be contingent upon the context. If people develop regularities in their responses to familiar circumstances, the repeating context surrounding elections is likely to matter. Much like other instances of institutionalized collective action, similarity in the electoral context can provide individuals with the necessary cue to activate their habitual response (Aldrich, Montgomery, and Wood 2011).

Empirical evidence in political science has produced findings that mirror those obtained from social psychology when looking at different types of behavior. Continuity in the context of an election strengthens the effect of past vote on future elections. Conversely, disruptions to such continuity—such as those when individuals move to a different location—hamper that effect (Aldrich, Montgomery, and Wood 2011; Green and Shachar 2000; Denny and Doyle 2009).

We focus on another factor that should be expected to forge contextual continuity: the type of electoral battle. Different types of elections are associated with different types of campaigns and generate different electoral stimuli. As argued by Kadt (2017), if voting creates a positive emotional state, then future opportunities to enter that state will be attractive. Extending this line of argument, we expect same-type elections to present more evident opportunities to reach this state. Early voting experiences should thus have a higher effect on turnout in subsequent elections of the same type. Voting for the first time for a high-salience election should have a higher effect on turnout in subsequent high-salience elections; voting for the first time for a low-salience election should have a higher effect on turnout in subsequent low-salience election elections. This expectation comes in stark contrast with that of the salience argument, which would expect high-salience elections to have a higher effect on subsequent voting behavior in elections of any type.

Previous research enabling us to discriminate between these two mechanisms remains scarce. Existing evidence stems primarily from studies that leverage the fact that national elections in European countries are more salient than elections for the European Parlia-

2

ment (EP). Their results, however, remain ambiguous. Franklin and Hobolt (2011) find that eligibility to vote in an election for the European rather than the national parliament decreases turnout in subsequent elections, while Dinas and Riera (2018) show that EP vs. national eligibility increases future vote for small parties. Against these results, Schulte-Cloos (2019) finds that EP eligibility increases interest in politics without suppressing the vote for mainstream parties.

The most rigorous evidence in favour of the electoral saliency hypothesis comes from Meredith (2009). Looking at California voting records, this author finds consistent evidence that past participation in Presidential elections induces future electoral participation in the 2004 and 2008 Presidential elections. He also finds no effects of 2002 eligibility on vote in either the 2004 or the 2006 election. Yet, as Meredith points out, this finding might not be due to the different nature of the elections but rather due to the fact that turnout decisions among those who already vote in Midterm elections are less likely to be affected by past participation. Moreover, these findings are at odds with those of Coppock and Green (2016). Using eligibility discontinuities in 17 US states, these authors find that being eligible to vote in the upstream election significantly increases voting in downstream elections. Unlike Meredith (2009), they find that the effect holds both for Presidential and Midterm elections.

To provide a more authoritative answer to this question, we rely on a research design specifically conceived to test the salience hypothesis against the contextual-continuity hypohesis. We take advantage of the fact that, in the US, high salience Presidential elections happen intertwined with low salience Midterm elections. Using all publicly available American National Election Studies (ANES), we exploit as-good-as-random variation in the type of election that respondents were first eligible to vote in. Instead of comparing individuals on the basis of their eligiblity in a given election, our design compares them on the basis of the type of election for which they were first eligible. Doing so means that our design does not allow us to test the effect of eligibility per se (which previous literature has provided evidence on). Instead, our design is meant to

3

evaluate the conditions under which the effect of eligibility is stronger—is it stronger for individuals first eligible to vote in a highly salient election (electoral saliency hypothesis), or when the election type matches that of the first election respondents were eligible to vote in (contextual-continuity hypothesis)?

As a last step in the empirical analyses, we examine a policy implication of the theory. If individuals develop context-specific habits, policies that change the type of election individuals are first eligible for should have lasting consequences on their subsequent voting behavior. We test for this expectation by looking into changes to voting age legislation in the US. This law made some individuals who would otherwise be first eligible to vote for a Presidential election be first eligible for a Midterm election. Our analyses show that such change made them more likely to vote for subsequent Midterm elections, but less so for Presidential elections.

Data and Empirical strategy

We use all ANES data covering elections from 1956 to 2018.[1] Our outcome of interest is self-reported turnout in election s_n, predicted by a dummy that denotes whether election s_n is of same type (Presidential or Midterm) as election s_1—each respondents' first eligible election. We obtain this information by subtracting individuals' age from the year of the survey, which provides us with their year of birth. Knowing the year of birth allows us to predict with high accuracy individuals' first eligible election.[2] We use no information as to whether respondents actually voted in their first eligible election. Rather, we stick to the reduced form, employing type-of-election eligibility as a predictor of turnout in the year of the survey. Moreover, we omit from the analysis the elections used to classify respondents in terms of their treatment status, i.e. individuals' first pair of Presidential

---

[1]The only exceptions are the Midterm elections that took place in 2010 and 2014, for which ANES does not provide a survey in the Data Center section on their website.

[2]Uncertainty occurs only when the respondent becomes of age on the year of the election. Because elections happen in November, we assume that these individuals already had their birthday and thus we treat them as eligible to vote in that election. We show that results remain unchanged when we relax this assumption.

4

and Congressional elections. Additional information on the data can be found in the Online Appendix.
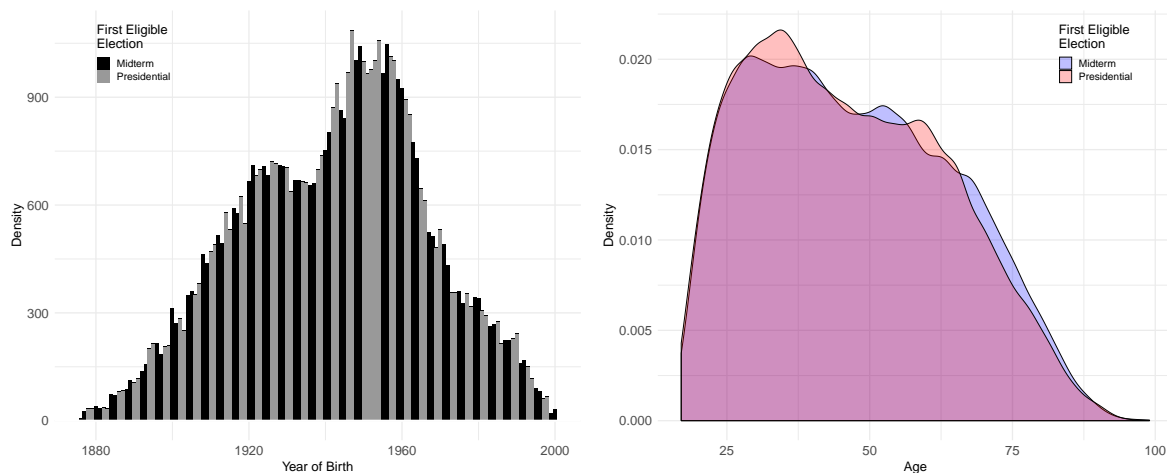

Figure 1: Visualizing the identification strategy: comparing groups according to the type of first elegible election.

Identification relies on two assumptions: that potential turnout rates in same-type elections would be the same for both groups had there been no difference in their type of first-eligible election (ignorability); and that type-of-first-election eligibility does not affect future turnout in same-type elections through other ways except via its effect on the type of election one encounters when coming of age (exclusion).[3] The first assumption, ignorability, would be violated if individuals (or their parents) could in some way select themselves (or their children) into the type of first-eligible election, a rather unlikely scenario.

Exclusion would be violated if type-of-election eligibility alone affected turnout patterns without this effect going through the experience of the election itself. This could be the case if for example Presidential-eligibles ended up being for some reason older or more educated than Midterm-eligibles. Figure 1 illustrates the logic upon which the design is based. The alternation of black and grey stripes in the left-hand panel suggests the absence of any monotone association between year-of-birth and type-of-election eli-

---

[3]Given that the estimand we use here is the Intent-To-Treat, exclusion is, strictly speaking, not needed. Yet, we choose to discuss it because we believe it matters for our interpretation of the effects, which rests on the idea that the effects are due to the election itself.

5

gibility. It is this alteration we aim to leverage for identification purposes. We believe that eventually belonging in one of the two groups is as-good-as-random. The right-hand side of the graph, which compares the age distribution between the two groups, does suggest a high degree of overlap. That said, we do find some imbalances in the age and level of education of respondents, which, as Table 1 shows, are statistically significant. To address this concern, we employ various specifications, including all or some of these variables as covariates. We also include a dummy for whether the current election is Presidential or Midterm. Finally, since survey respondents vary in age, they also vary in terms of how distant the outcome election, s_n, is from the treatment election, s_1. For some people their outcome election is the third or fourth Presidential or Midterm election in which they are of age to vote, while for others it might be their 10th eligible election. To account for such heterogeneity, we include election-counter fixed effects.

Table 1: **Difference in means between Presidential and Midterm eligibles.**

	Presidential Eligibles	Midterm Eligibles	Difference of Means	Standard Deviation	Number of Observations
Education: Grade school or less	0.112	0.127	0.014***	0.324	55376
Education: High school	0.432	0.442	0.011**	0.496	55376
Education: Some college	0.237	0.228	-0.009***	0.422	55376
Education: College or advanced degree	0.219	0.203	-0.015***	0.408	55376
White	0.770	0.770	0.000	0.421	58975
Age	46.333	47.085	0.752***	17.243	58975
Age Squared	2439.39	2519.23	79.84***	1729.16	58975
South	0.301	0.302	0.001	0.459	59800

Another way in which exclusion could be violated is if Presidential- or Midterm-year eligibility affects future turnout in Presidential or Midterm elections respectively via other routes than the first-eligible election itself. For example, if high-school curriculum also changes in years of Presidential or Midterm elections, it could be that effects are not due to the election itself but rather due to cohort-specific changes in school curriculum.[4] We cannot test this assumption, but we try to gauge whether it holds by looking into several post-treatment outcomes that are not election-specific. Our treatment is based on the

---

[4]To be sure, if these changes are produced by the type of election on the specific year, these effects could be still attributed, even if indirectly, to the election itself.

Table 2: **Differences between midterm and presidential eligibles on political outcomes.**

	Presidential Eligibles	Midterm Eligibles	Difference of Means	p−value	Number of Observations
Urbanism	2.120	2.110	-0.010	0.206	40014
Moved	0.916	0.913	0.003	0.238	55800
Approved President	0.570	0.568	-0.001	0.779	40568
Different btw parties	0.667	0.664	-0.003	0.589	32943
Parties only interested in votes	0.603	0.591	-0.012	0.191	12071
High Income	0.330	0.325	-0.005	0.220	50694
Region: Northwest	0.192	0.191	-0.001	0.894	55800
Region: North Central	0.262	0.265	0.002	0.531	55800
Region: South	0.355	0.355	-0.000	0.917	55800
Region: West	0.191	0.189	-0.001	0.658	55800
Voting Matters	0.103	0.104	0.001	0.815	14112
Politics Complicated	0.711	0.702	0.009	0.068	34720
Voters have a say	0.632	0.651	0.018	0.009	17968
Internal Efficacy	0.441	0.445	0.003	0.483	41517
External Efficacy	0.551	0.555	0.004	0.439	40767
Trust government	2.439	2.445	0.006	0.323	39324
Interest in which party wins	0.708	0.707	-0.001	0.863	31849

idea that similar contextual stimuli as those experienced in first eligible election should moderate behavioral responses. By extension, we expect that attitudinal outcomes not distinguishing between types of elections remain unaffected by our treatment. We use all such outcomes available in the cumulative ANES survey, as shown in Table 2. The means of the two groups are close to identical across all outcomes. Presidential- and Midterm-eligibles do not differ in terms of their attitudes towards government spending; whether they approve or not the President; whether they think that parties are only
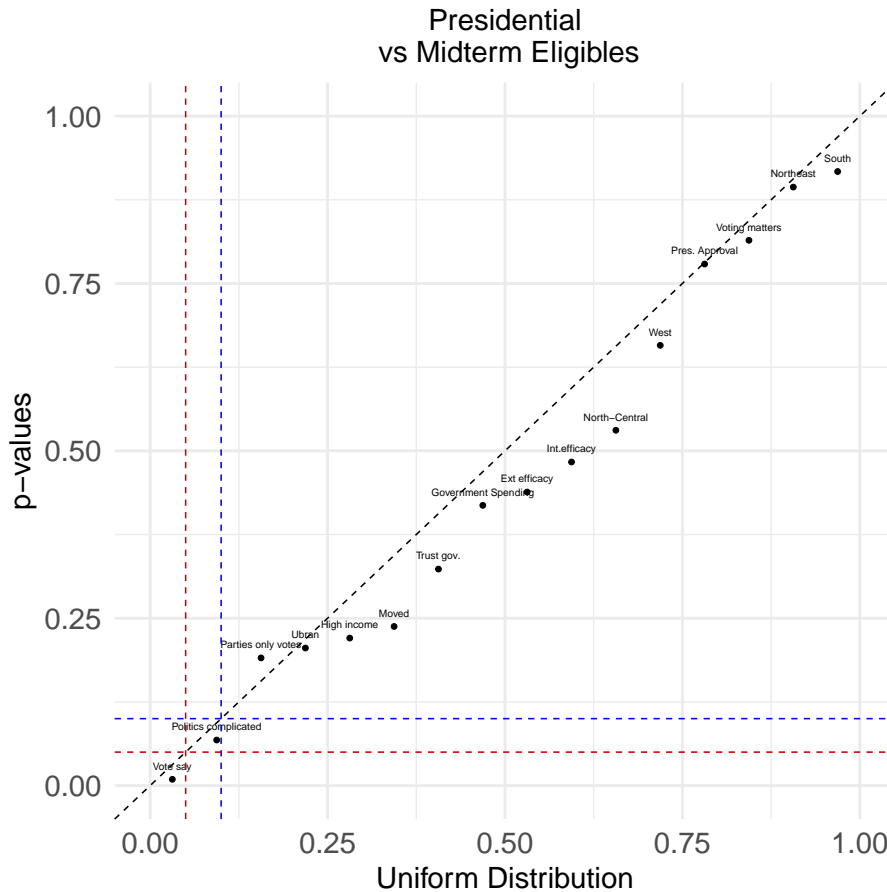
7

Figure 2: Balance on observables between Presidential and Midterm eligibles.
Note: The graph displays p-values corresponding to a difference-of-means test between Presidential and midterm eligibles. The dashed diagonal line denotes the 45-degree line, which is where the p-values would lie, if they came from a uniform distribution. The blue dashed lines denote the 5% cutoff point and the red dashed lines denote the 10% level.

interested in casting votes; or whether parties differ. They are also indistinguishable when it comes to regional residence as well as residential mobility. Finally, when looking also at the supplementary items included in the ANES (available for only a third of the total number respondents) we find that the two groups do not differ in terms of internal or external efficacy, trust in government, opinions about whether voters have a say in politics; whether voting matters; or whether politics is too complicated. Among these 17 items, only one indicates a difference which albeit very small is significant at the 5% level. Figure 2 shows that when arraying the *p*-values obtained from this exercise against a uniform distribution, we find only small variations from the 45 degree line, which corresponds to the level of balance expected under true randomization. Without being in

8

any way exhaustive, the evidence enhances our confidence that the effects reported here are due to type-of-election eligibility.

Thus, our baseline model takes the following form:

$$Vote_{i,s>1} = \sum_{s=2}^{15} \alpha_s E_s + \lambda_1 Same_{is} + \delta P_s + X\beta_2 + u_i \qquad (1)$$

where $Same_{is}$ is coded as one if the election in question is of same type as first-eligible election and zero otherwise. E_s indexes election-counter dummies, where $s = \{2, \ldots, 15\}$. Every s represents two elections, one Midterm and one Presidential, which together comprise a pair. Thus, $s = 2$ means that i is interviewed after the second election she was eligible to participate, either Presidential or Midterm, and so on, up until $s = 15$. On average, our respondents encounter their first eligible election when they are 20 years old. Thus, going up to $s = 15$ means that we trace a 60-year-long life trajectory, from 20 to approximately 80 years of age. P_s is a dummy for Presidential elections that controls for differences in turnout between these elections and midterm ones. X is a vector of control variables. In our preferred specification, we include all covariates included in Table 1.[5] In the Online Appendix we use two alternative specifications. The first includes only education and age, the only two covariates we deem necessary for identification.[6] The second is intended to assess the robustness of these estimates by including contemporaneous sociodemographics as proxies for socioeconomic background: income, region of residence

---

[5]Continuing into tertiary education is also, strictly speaking, post-treatment. We thus employ an additional specification, based on a recoding of education, collapsing in one category all respondents with at least a 12-grade diploma or equivalent. The results appear in Figures C8, C9, C10 and C11 and are similar to those presented in the main text.

[6]We consider age and level of education as necessary controls because they are both strong predictors of turnout, and their distribution in the quasi-experimental groups in our sample pushes the outcomes in the opposite direction of the treatment. For this reason, failing to account for them biases the coefficients toward zero. For example, individuals first eligible for Midterm elections whose outcome is measured after Presidential elections are older (47.37 years) than individuals first eligible for Midterm elections whose outcome is measured after Midterm elections (46.64 years). We would expect our treatment (similarity of election type between current and first eligible election) to increase turnout among the latter group. But this group is also younger, which should decrease their probability of voting—since we know that age is a strong predictor of turnout. A similar pattern happens with education. Individuals first eligible for Midterm elections whose outcome is measured after Presidential elections are more educated (2.58 if we assume education to be a continuous 1-4 measure) than individuals first eligible for Midterm elections whose outcome is measured after Midterm elections (2.38).

9

by the time of the ANES interview, and a trichotomy distinguishing respondents between those residing in central cities, suburban cities or rural towns (*Urbanism*).[7] Conditional on these observables, we assume as-good-as-random assignment of type of first eligible election. If this is the case, λ_1 recovers the effect of same-type election on probability of voting in election s_n. Since eligibility does not mean voting, equation 1 yields the intent-to-treat (ITT) effect of voting in the same type of election as the election in which one was first eligible to vote.

Equation 1 does not distinguish between types of elections. This is done in equation 2, which interacts P_s with $Same_{is}$:[8]

$$Vote_{i,s>1} = \sum_{s=2}^{15} \alpha_s E_s + \lambda_1 Same_{is} + \delta P_s + \lambda_2 Same_i P_s + v_i \tag{2}$$

Now, λ_1 yields the effect of same-type election for Midterm elections and $\lambda_1 + \lambda_2$ denote the same effect for Presidential elections. Finally, we decompose the same-type election effects across individuals' life trajectory. Equation 3 does this, by interacting $Same_i$ with each pair of elections in respondent's life span, up until election pair $s = 15$:

$$Vote_i = \sum_{s=2}^{15} \alpha_s E_s + \lambda_1 Same_{is} + \sum_{s=2}^{15} \delta_s Same_i E_s + \delta P_s + X\beta_2 + u_i \tag{3}$$

For each pair of election, $2 \leq s \leq 15$, $\lambda_1 + \delta_s$ yields the effect of same-type-eligibility on the probability of voting in election s. For all elections $s > 15$, the effect is summarized by λ_1. Equation 4, below, further distinguishes between each type of election:

$$V_i = \sum_{s=2}^{15} \alpha_s E_s + \lambda_1 Same_{is} + \delta P_s +$$
$$\sum_{s=2}^{15} \beta_s E_s Same_i + \sum_{s=2}^{15} \theta_s E_s P_s + \lambda_2 Same_i P_s + \sum_{s=2}^{15} \zeta_s E_s P_s Same_i + X\beta_2 + u_i \tag{4}$$

Now, for each $2 \leq s \leq 15$, $\lambda_1 + \beta_s$ yields the same-type-of-election effect for all Midterm elections while $\lambda_1 + \beta_s + \lambda_2 + \zeta_s$ yields the same-type-of-election effect on turnout in

---

[7]The results from these analyses appear in Figures C6, C7, C12 and C13 of the Online Appendix.

[8]We abuse notation between equations to retain simplicity.

10

Presidential elections.

Results

The left-hand panel on Figure 3 displays the main findings. Each entry denotes the effect of same-type-eligibility on future turnout. The middle entry comes from Equation 1, while the left and right ones come from Equation 2. We find a positive effect of almost three percentage point increase in the probability to vote in election s_n, if this election is of same type as election s_1. Our estimates are of the intent-to-treat, the effect of simply being eligible to vote in one election type rather than another. Since previous work indicates that turnout among first-time-eligible voters is around 60% in US Presidential elections (Dinas 2014), this means that the effect for compliers—i.e., individuals who actually vote in the first election for which they are eligible—would be around five percentage points ($3/0.6 = 5$). Importantly, as shown in the left- and right-most entries of the graph, this effect seems to be of similar magnitude either when the current election is Presidential or Midterm.

These results are not directly comparable with other studies looking at early-election effects (Meredith 2009; Kadt 2017; Franklin and Hobolt 2011), because such studies compare eligibles with non-eligibles—in other words, their treatment groups differ in terms of number of elections in which they were previously eligible. By contrast, because our main focus is on testing the contextual-continuity hypothesis against the salience hypothesis, we compare individuals in terms of type of first-eligible election. That said, our effect size is comparable to theirs. For example, Meredith (2009) estimates that previous voting experience increases participation by around five percentage points, an effect size that is very similar to ours.

While providing a summary of the average effect of same-type-eligiblity on future turnout, this analysis does not allow us to gauge how much these effects last along the life trajectory. Are these effects short-lived or do they persist across a long span of elec-
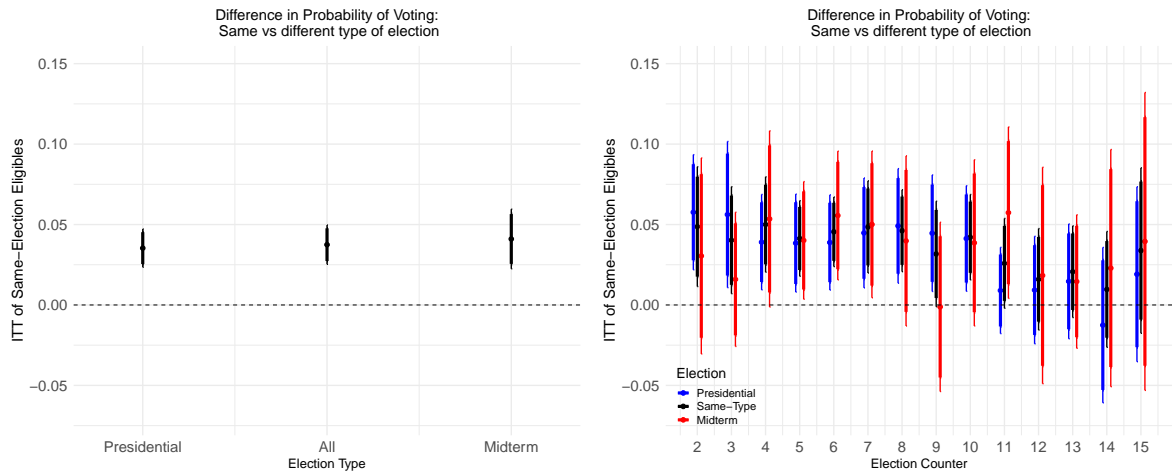
11

Figure 3: Effect of experiencing the same type of election as the first eligible election on the probability of voting.

Notes: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on voting in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on voting in subsequent Midterm elections. The analyses control for age, age squared, level of education, female respondents (dummy) and white respondents (dummy).

toral history? The right-hand panel of Figure 3 tries to answer this question, decomposing the effects across the first fifteen pairs of elections encountered in individuals' life span. The black entries denote the overall effect of same-type eligibility, while the blue and red entries decompose this effect into Presidential and Midterm elections respectively. Thus, the estimates denoted in black stem from Equation 3, while the blue and red entries stem from Equation 4. Three observations are worth making. First, as expected, all estimates carry now higher levels of uncertainty, especially when it comes to Midterm elections, which constitute one third of the total number of observations. Second and more importantly, despite some fluctuation, the effects seem to be remarkably persistent. Same-type eligibility increases turnout up until the tenth pair of elections individuals encounter. Although the effects decay afterwards, they seem to hold for approximately four decades, ranging between two and five percentage points. Third, the effects are once again symmetric across Presidential and Midterm elections. In other words, same-type-eligibility effects does not seem to be driven only by eligibility in Presidential elections, as the saliency hypothesis would lead us to expect. Rather, and consistent with the contextual continuity hypothesis, the effects are of same direction and comparable magnitude when

12

the same-type election is a Midterm election.

As discussed by Coppock and Green (2016, p. 1058), an alternative interpretation of these results is that instead of denoting the persistent effect of early electoral experiences, they may be driven by differential exposure to party targeting campaigns. If parties' voter files differentiate between types of elections, Midterm eligibles may be contacted more often than Presidential eligibles in Midterm elections, and vice versa. To see whether this is the case, we use as outcome a question available in most of the ANES surveys, asking whether respondents were contacted by the parties during the campaign. Figure C1 in the Online Appendix presents the results. We find no evidence that same-type eligibility significantly increases party targeting.

We try to further assess the mechanism driving these effects by examining a complementary implication stemming from the contextual continuity hypothesis. If it is the case that voting habits are linked to the election type that generated them, we should find individuals to be more interested in elections of the type they were first eligible to vote in. We thus replicate the exercise above using interest in the election as the outcome variable. The results are shown in Figure 4. The pattern is quite similar to the one observed in Figure 3, especially when it comes to the overall effects. Respondents seem to be between two and five percentage points more likely to be interested in an election if this is of the same type as their first eligible election. Moreover, as with the findings shown in Figure 3, the effect seems to be unaffected by whether the first eligible election was a Presidential or a Midterm one. The only substantive difference between the two figures is the degree of persistence in the overall effects. Same-type-eligiblity effects appear to be considerably more short-lived when it comes to interest in the election than when it comes to actual voting. This divergence could be explained by the habitual nature of voting: electoral interest boosts turnout in first elections but is not much needed once such habits are formed.

The Online Appendix provides robustness checks for these analyses. In the first place, Figures C2 and C3 in the Online Appendix provide a placebo test that moves the treat-

13

ment group by one year, so that the resulting treatment and control groups retain their age differences while now having equal probability of encountering an election of same type as first eligible election. We find no discernible pattern, either when looking at turnout (Figure C2) or when looking at interest in the election (Figure C3). Figures C4 and C5 replicate the main analyses shown in Figures 3 and 4 respectively, this time adding state-fixed effects. The results remain substantively unchanged. Figures C6 and C7 replicate the analyses without additional control variables, while C12 and C13 extend the list of covariates to contemporaneous demographics. Both sets of results point to the same conclusions as those presented here. Figures C10 and C11 extend the analysis with no additional controls by further using a coding scheme for education that does not distinguish between high-school and above, to permit variation in education that has manifested itself only by the time the first eligible election takes place. The results are similar to those presented above. Moreover, since the election takes place in November, we treat all individuals coming of age in an election year as eligible to vote in that election. Figures C14 and C15 relax this assumption, presenting the results when excluding everyone becoming of age on in an election year. The results remain substantively very similar to those presented in the main analysis. Finally, Figure C16, C17 and Figure C18 replicate the main findings displayed in the left panel of Figure 3, after excluding one election year at a time. As expected, the point estimates are now less precisely estimated, but remain substantively very similar. Figures C19, C20 and C21 repeat the same exercise to replicate the findings for interest in elections (Figure 4). Again, the results remain very similar after excluding one survey at a time.

Policy implications

If the type of first eligible election matters, it should also help us understand better the effects of policies designed to reduce voting age. Reducing the age threshold of voting eligibility has become increasingly popular as a way of increasing youth political
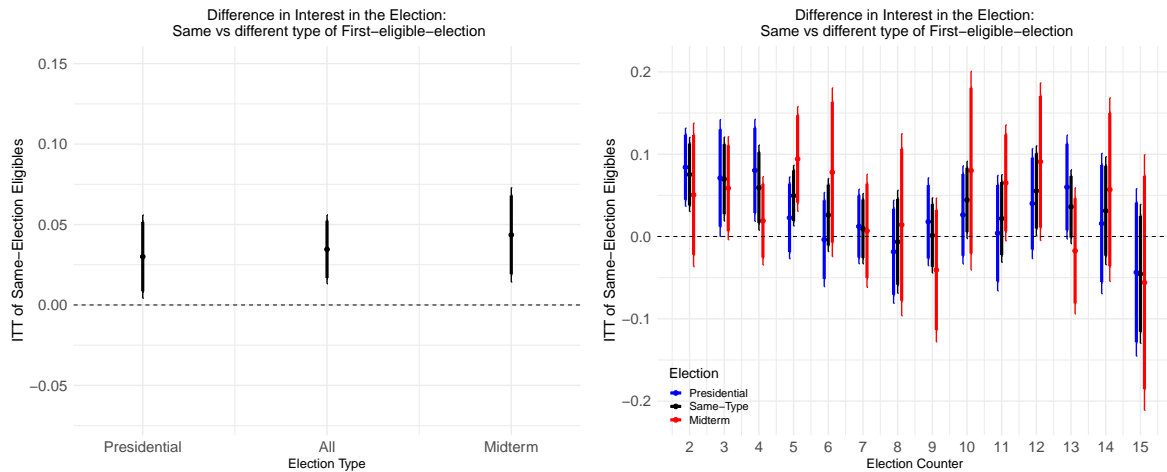
14

Figure 4: Effect of experiencing the same type of election as the first eligible election on the level of interest in the election.

Notes: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on interest in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on interest in subsequent Midterm elections. The analyses control for age, age squared, level of education, female respondents (dummy) and white respondents (dummy).

knowledge (Rosenqvist 2020) and locking young individuals into voting habits (Franklin 2020). If such policies change the type of first-eligible elections, they may also have hitherto neglected side-effects on the probability of voting for subsequent elections of different types. To shed light on these effects, we leverage the laws of 1972 reducing the voting age from 21 to 18 years old that affected most US States.

The 1972 law made voters born in years such as 1963, 1967 or 1971 first eligible to vote in midterm elections at age 19. In the absence of the reform, they would have been first eligible to vote in a Presidential election at age 21. On the other hand, people born a year before remain first eligible to vote in Midterm elections regardless of the reform. Comparing people aged 19 and 20 at the time of congressional elections before and after the reform, we can therefore test whether making one group to experience congressional elections first affects future turnout. Doing so also presents the advantage of accounting for potential cohort-specific fixed effects, which was impossible with the previous research design.

To test the effect of switching from a presidential to congressional election as a first eligible voting experience, we use the same data as in the previous analyses and estimate

15

the following equation:

$$Voted_{i,s>1} = \gamma_0 Change_i + \gamma_1 Presidential_i Change_i + \delta Presidential_i +$$
$$[\sum_{s=2}^{15} \alpha_{0s} E_s] + X_i \beta_0 + Presidential_i([\sum_{s=2}^{15} \alpha_{1s} E_s] + X_i \beta_1) + v_{is} \qquad (5)$$

In the equation, *Change* equals one if the individual is 19 years old in the year of congressional elections and is born after 1955—thereby experiencing a switch in their first eligible election from Presidential to Midterm. Alternatively, *Change* equals zero if the voter turns 19 in the year of congressional elections and is born before 1955 or turns 20 in the year of such elections and therefore always remains first eligible for presidential elections. We also include our *Presidential* dummy to control for turnout differences between these two elections as well as an interaction between *Presidential* and *Change*. This means that γ_0 represents the effect of switching to midterm elections on subsequent midterm election turnout while $\gamma_0 + \gamma_1$ represents the effect of switching to midterm on subsequent presidential elections' turnout. Finally, we include the same covariates and election-counter fixed effects as before in a fully interacted model to allow for these to depend on the type of election voters experience. To verify the robustness of our results, we also run the same regressions with state code fixed effects and their corresponding interactions with the presidential dummy. Results do not change, albeit becoming less precise.

Table 3 shows the findings.[9] Regardless of model specification, switching one's first election from Presidential to Midterm increases the likelihood of voting in subsequent Midterm elections by three to four percentage points and decreases the probability of voting in subsequent Presidential elections by between four and five percentage points.

Table C1 in the Online Appendix repeats the same exercise focusing on the group

---

[9]In these analyses, we remove women born 21 years before female voting rights were adopted and afro-american born more than 21 years before 1965. We also remove the states that reduced voting rights before 1972: Kentucky, Georgia, Alaska and Hawaii.

Table 3: Effect of changing one's first election type from Presidential to Midterm on voting for subsequent elections.

	(1)	(2)	(3)
Change	0.0446**	0.0388**	0.0307
	(0.0182)	(0.0186)	(0.0187)
Change × Presidential	-0.0935***	-0.0795***	-0.0682**
	(0.0248)	(0.0262)	(0.0263)
Observations	21,275	21,268	21,268
Necessary Controls	Yes	Yes	Yes
Necessary Controls interacted	Yes	Yes	Yes
Additional Controls	No	Yes	Yes
Additional Controls Interacted	No	Yes	Yes
State dummies	No	No	Yes
State dummies interacted	No	No	Yes
Election pair fixed effects	Yes	Yes	Yes
Election pair interacted	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

who experience a change from Midterm to Presidential elections after the reform. As expected the coefficients are of opposite sign to the ones shown in Table 3.

To make sure that we are not rather measuring the effect of one group now being eligible to vote in an election a year earlier than the other, Table C2 in the Online Appendix compares 18 and 19 year olds after the reform who are eligible for the same first election assigning treatment to 19 year olds (Models 1 and 2). We find no impact both for congressional and presidential elections. We repeat the exercise comparing 21 and 22 years who became first eligible for the same election pre-1972 (Models 3 and 4) and, again, find no effect.

Discussion

Drawing upon literature on habit formation, we have challenged the view that early elections exert more powerful effects on future turnout when they are of high salience. Given the contextual continuity between elections that share a type, we argued that one

17

should expect early elections to boost turnout in future elections of the same type. Our analyses show that, indeed, individuals first eligible to vote for a Presidential election are more likely to vote at subsequent Presidential elections; while individuals first eligible to vote for a Midterm election are more likely to vote at subsequent Midterm elections. These results have also non-negligible policy implications: policies that change the first election individuals are eligible for have a similar effect. This finding supports the suspicion put forward by Coppock and Green (2016, p. 1060) that downstream effects of voting experiences are more likely to happen in similar elections.

Our results underline the importance of the electoral context and question the ability of habits formed in previous elections to travel across elections of different types. The conventional view in the literature is that voting in previous elections—if these are sufficiently salient—can induce voting habits that travel to different contexts. Our results paint a more nuanced picture. They suggest that, while participatory habits are formed, such habits are rather constrained. Individuals seem to form election-specific habits that do not necessarily affect elections of different types.

Such conclusion qualifies the view that second-order elections can be damaging for electoral participation—an argument that has fueled criticism of the existence of low salience elections, such as elections for the European Parliament. As our findings suggest, early voting experiences can boost turnout in same-type elections. While saliency is indeed more important in predicting turnout across elections—hence the overall gap between Midterm and Presidential election turnout—, the long-term effects of early voting experiences seem to be affected more by the type than the salience of the first-eligible election.

References

Aldrich, John H., Jacob M. Montgomery, and Wendy Wood. 2011. "Turnout as a Habit". *Political Behavior* 33 (4): 535–563.

Coppock, Alexander, and Donald P. Green. 2016. "Is Voting Habit Forming? New Evidence from Experiments and Regression Discontinuities". *American Journal of Political Science* 60 (4): 1044–1062.

Denny, Kevin, and Orla Doyle. 2009. "Does voting history matter? Analysing persistence in turnout". *American Journal of Political Science* 53 (1): 17–35.

Dinas, Elias. 2014. "Does choice bring loyalty? Electoral participation and the development of party identification". *American Journal of Political Science* 58 (2): 449–465.

— . 2012. "The Formation of Voting Habits". *Journal of Elections, Public Opinion and Parties* 22 (4): 431–456.

Dinas, Elias, and Pedro Riera. 2018. "Do European Parliament Elections Impact National Party System Fragmentation?" *Comparative Political Studies* 51 (4): 447–476.

Franklin, Mark N. 2020. "Consequences of Lowering the Voting Age to 16: Lessons from Comparative Research". In *Lowering the Voting Age to 16: Learning from Real Experiences Worldwide*, ed. by Jan Eichhorn and Johannes Bergh, 13–41. Cham: Springer International Publishing.

Franklin, Mark N. 2004. *Voter turnout and the dynamics of electoral competition in established democracies since 1945*. Cambridge University Press.

Franklin, Mark N., and Sara B. Hobolt. 2011. "The legacy of lethargy: How elections to the European Parliament depress turnout". *Electoral Studies*, Special Symposium: Electoral Democracy in the European Union, 30 (1): 67–76.

Gerber, Alan S., Donald P. Green, and Ron Shachar. 2003. "Voting may be habit-forming: evidence from a randomized field experiment". *American Journal of Political Science* 47 (3): 540–550.

Green, Donald P., and Ron Shachar. 2000. "Habit formation and political behaviour: Evidence of consuetude in voter turnout". *British Journal of Political Science* 30 (4): 561–573.

Hall, Edward T. 1969. *Ecological Psychology: Concepts and Methods for Studying the Environment of Human Behavior*. JSTOR.

Ji, Mindy F., and Wendy Wood. 2007. "Purchase and consumption habits: Not necessarily what you intend". Publisher: Elsevier, *Journal of Consumer Psychology* 17 (4): 261–276.

Kadt, Daniel de. 2017. "Voting Then, Voting Now: The Long-Term Consequences of Participation in South Africa's First Democratic Election". *The Journal of Politics* 79 (2): 670–687.

Larcom, Shaun, Ferdinand Rauch, and Tim Willems. 2017. "The benefits of forced experimentation: striking evidence from the London underground network". Publisher: Oxford University Press, *The Quarterly Journal of Economics* 132 (4): 2019–2055.

Meredith, Marc. 2009. "Persistence in political participation". *Quarterly Journal of Political Science* 4 (3): 187–209.

Plutzer, Eric. 2002. "Becoming a habitual voter: Inertia, resources, and growth in young adulthood". *American political science review* 96 (1): 41–56.

Rosenqvist, Olof. 2020. "Rising to the occasion? Youth political knowledge and the voting age". *British Journal of Political Science* 50 (2): 781–792.

Schulte-Cloos, Julia. 2019. "The effect of European Parliament elections on political socialisation". *Journal of European Public Policy* 26 (7): 1094–1111.

Wood, Wendy, and David T. Neal. 2007. "A new look at habits and the habit-goal interface." Publisher: American Psychological Association, *Psychological review* 114 (4): 843.

Wood, Wendy, Leona Tam, and Melissa Guerrero Witt. 2005. "Changing circumstances, disrupting habits." Publisher: American Psychological Association, *Journal of personality and social psychology* 88 (6): 918.

Online Appendix

A   Additional details on the data

Our data comes from the American National Election Study (ANES), which is run after each election in the US—be they midterm or Presidential. These studies often include a pre-election and a post-election wave. In this paper, since we are mostly interested in whether respondents turned out to vote, we focus solely on the post-election waves, where the turnout question is asked. Our final dataset pools all datasets available in the ANES website. Each respondent is included in the sample only once, after the election where their outcome (turnout) was registered. This also means that we only have one outcome per respondent: whether or not they voted in the most recent election, which preceded their ANES interview. We do not have data on whether respondents voted in the most recent midterm and presidential election—just on whether they voted in the most recent election, whatever its type.

We use data on the birth year of each respondent to know which election they were first eligible for. Respondents first eligible for a given election were often interviewed in different waves of the ANES. By calculating how many elections had gone by since a respondent's first eligible election and the election for which we have evidence on their turnout, we can infer how the persistent the effect is across the lifetime—or, to put it differently, how the effect changes as we increase the distance between the election that provides the treatment and the election where the outcome is measured.

B   Tables of the main findings

	(1)	(2)	(3)	(4)
Same	0.0375***	0.0410***	-0.0184	0.0386
	(0.00607)	(0.00923)	(0.0210)	(0.0447)
Presidential	0.168***	0.171***		0.189***
	(0.00781)	(0.00829)		(0.0446)
Same x Pres. Election		-0.00568		-0.0713
		(0.00854)		(0.0589)
2nd Election	0.959***	0.958***	0.367***	0.775***
	(0.139)	(0.139)	(0.129)	(0.137)
3rd Election	0.875***	0.874***	0.313**	0.710***
	(0.125)	(0.125)	(0.119)	(0.132)
4th Election	0.783***	0.782***	0.242**	0.583***
	(0.114)	(0.114)	(0.109)	(0.118)
5th Election	0.684***	0.683***	0.188*	0.490***
	(0.102)	(0.101)	(0.1000)	(0.103)

1

6th Election	0.604***	0.603***	0.133	0.434***
	(0.0934)	(0.0930)	(0.0911)	(0.0958)
7th Election	0.513***	0.512***	0.0788	0.333***
	(0.0879)	(0.0875)	(0.0878)	(0.0909)
8th Election	0.436***	0.435***	0.0397	0.290***
	(0.0793)	(0.0790)	(0.0818)	(0.0891)
9th Election	0.370***	0.369***	0.0260	0.265***
	(0.0691)	(0.0688)	(0.0789)	(0.0783)
10th Election	0.303***	0.302***	-0.00864	0.204***
	(0.0629)	(0.0626)	(0.0667)	(0.0657)
9th Election	0.236***	0.235***	-0.0206	0.141**
	(0.0565)	(0.0563)	(0.0612)	(0.0657)
12th Election	0.202***	0.201***	-0.00710	0.151***
	(0.0445)	(0.0443)	(0.0496)	(0.0472)
13th Election	0.159***	0.158***	-0.0149	0.0927*
	(0.0411)	(0.0410)	(0.0470)	(0.0525)
14th Election	0.0991***	0.0985***	-0.0244	0.0566
	(0.0302)	(0.0301)	(0.0334)	(0.0533)
15th Election	0.0795***	0.0793***	-0.0170	0.0749
	(0.0252)	(0.0252)	(0.0376)	(0.0507)
Presidential x 2nd Election				0.0575
				(0.0503)
Presidential x 3rd Election				0.0105
				(0.0444)
Presidential x 4th Election				0.0485
				(0.0506)
Presidential x 5th Election				0.0311
				(0.0447)
Presidential x 6th Election				-0.0197
				(0.0494)
Presidential x 7th Election				-0.00446
				(0.0518)
Presidential x 8th Election				-0.0538
				(0.0541)
Presidential x 9th Election				-0.0922**
				(0.0451)
Presidential x 10th Election				-0.0962**
				(0.0447)
Presidential x 11th Election				-0.0665

2

		(0.0484)
Presidential x 12th Election		-0.100*
		(0.0522)
Presidential x 13th Election		-0.0544
		(0.0476)
Presidential x 14th Election		-0.0492
		(0.0615)
Presidential x 15th Election		-0.0929*
		(0.0496)
Same x 2nd Election	0.0596**	-0.00812
	(0.0258)	(0.0435)
Same x 3rd Election	0.0516**	-0.0227
	(0.0235)	(0.0435)
Same x 4th Election	0.0689***	0.0149
	(0.0257)	(0.0514)
Same x 5th Election	0.0508*	0.00153
	(0.0267)	(0.0480)
Same x 6th Election	0.0561**	0.0170
	(0.0222)	(0.0452)
Same x 7th Election	0.0556**	0.0115
	(0.0240)	(0.0519)
Same x 8th Election	0.0550**	0.00123
	(0.0228)	(0.0501)
Same x 9th Election	0.0322	-0.0398
	(0.0303)	(0.0455)
Same x 10th Election	0.0465*	0.0000271
	(0.0242)	(0.0495)
Same x 11th Election	0.0297	0.0188
	(0.0246)	(0.0594)
Same x 12th Election	0.0178	-0.0203
	(0.0215)	(0.0570)
Same x 13th Election	0.0203	-0.0240
	(0.0222)	(0.0468)
Same x 14th Election	0.00227	-0.0157
	(0.0221)	(0.0644)
Same x 15th Election	0.0360	0.000899
	(0.0375)	(0.0669)
Pres. x Same x 2nd Election		0.0984
		(0.0608)

3

Pres. x Same x 3rd Election				0.112
				(0.0674)
Pres. x Same x 4th Election				0.0568
				(0.0709)
Pres. x Same x 5th Election				0.0696
				(0.0612)
Pres. x Same x 6th Election				0.0545
				(0.0628)
Pres. x Same x 7th Election				0.0660
				(0.0745)
Pres. x Same x 8th Election				0.0806
				(0.0712)
Pres. x Same x 9th Election				0.117*
				(0.0608)
Pres. x Same x 10th Election				0.0740
				(0.0581)
Pres. x Same x 11th Election				0.0229
				(0.0697)
Pres. x Same x 12th Election				0.0622
				(0.0766)
Pres. x Same x 13th Election				0.0714
				(0.0585)
Pres. x Same x 14th Election				0.0358
				(0.0800)
Pres. x Same x 15th Election				0.0509
				(0.0696)
Age	0.0544***	0.0544***	0.0503***	0.0623***
	(0.00568)	(0.00569)	(0.00736)	(0.00682)
Age squared	-0.000316***	-0.000317***	-0.000362***	-0.000407***
	(0.0000412)	(0.0000413)	(0.0000586)	(0.0000528)
High school	0.149***	0.149***	0.159***	0.154***
	(0.0154)	(0.0154)	(0.0157)	(0.0155)
Some college	0.283***	0.283***	0.302***	0.289***
	(0.0169)	(0.0168)	(0.0178)	(0.0171)
College or advance degree	0.360***	0.360***	0.381***	0.365***
	(0.0181)	(0.0181)	(0.0188)	(0.0182)
Dummy female	-0.0265***	-0.0265***	-0.0272***	-0.0271***
	(0.00633)	(0.00633)	(0.00623)	(0.00641)
Dummy white	0.0497***	0.0497***	0.0364***	0.0484***

4

	(0.00860)	(0.00860)	(0.00901)	(0.00870)
Constant	-1.951***	-1.954***	-1.138***	-1.964***
	(0.234)	(0.235)	(0.246)	(0.254)
N	47987	47987	47987	47987

Standard errors in parentheses
Standard errors are clustered by state
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B1: Effect of experiencing the same type of election as the first eligible election on the probability of voting.

	(1)	(2)	(3)	(4)
Same	0.0346***	0.0436***	-0.0345	0.0550
	(0.0106)	(0.0146)	(0.0494)	(0.0678)
Presidential	0.171***	0.178***		0.281***
	(0.0147)	(0.0131)		(0.0783)
Same x Pres. Election		-0.0135		-0.104
		(0.0176)		(0.0891)
2nd Election	0.713***	0.712***	0.155	0.619***
	(0.157)	(0.157)	(0.147)	(0.158)
3rd Election	0.589***	0.588***	0.0450	0.480***
	(0.150)	(0.150)	(0.146)	(0.154)
4th Election	0.444***	0.443***	-0.0809	0.371**
	(0.138)	(0.139)	(0.137)	(0.141)
5th Election	0.359***	0.358***	-0.132	0.241*
	(0.130)	(0.130)	(0.135)	(0.137)
6th Election	0.274**	0.273**	-0.186	0.187
	(0.122)	(0.122)	(0.129)	(0.138)
7th Election	0.183	0.181	-0.237*	0.142
	(0.114)	(0.115)	(0.123)	(0.126)
8th Election	0.148	0.146	-0.234*	0.0827
	(0.107)	(0.107)	(0.121)	(0.124)
9th Election	0.0789	0.0770	-0.265**	0.0539
	(0.0949)	(0.0950)	(0.113)	(0.115)
10th Election	0.0911	0.0893	-0.243**	-0.00266
	(0.0895)	(0.0894)	(0.112)	(0.112)
9th Election	0.0341	0.0325	-0.242**	-0.0259
	(0.0799)	(0.0801)	(0.0989)	(0.0981)
12th Election	0.0805	0.0791	-0.173*	0.0322
	(0.0652)	(0.0651)	(0.0924)	(0.0944)
13th Election	0.0559	0.0547	-0.150*	0.0631

5

	(0.0538)	(0.0538)	(0.0788)	(0.0818)
14th Election	-0.0101	-0.0111	-0.166**	-0.0357
	(0.0440)	(0.0439)	(0.0675)	(0.0794)
15th Election	0.00292	0.00229	-0.0681	0.0713
	(0.0358)	(0.0359)	(0.0597)	(0.0873)
Presidential x 2nd Election				-0.0796
				(0.0861)
Presidential x 3rd Election				-0.0697
				(0.0773)
Presidential x 4th Election				-0.134
				(0.0815)
Presidential x 5th Election				-0.0616
				(0.0785)
Presidential x 6th Election				-0.0968
				(0.0951)
Presidential x 7th Election				-0.158*
				(0.0792)
Presidential x 8th Election				-0.103
				(0.0869)
Presidential x 9th Election				-0.160*
				(0.0838)
Presidential x 10th Election				-0.0814
				(0.0912)
Presidential x 11th Election				-0.0980
				(0.0868)
Presidential x 12th Election				-0.121
				(0.0832)
Presidential x 13th Election				-0.165*
				(0.0836)
Presidential x 14th Election				-0.0859
				(0.0967)
Presidential x 15th Election				-0.132
				(0.100)
Same x 2nd Election			0.106*	-0.00437
			(0.0534)	(0.0815)
Same x 3rd Election			0.101	0.00373
			(0.0656)	(0.0751)
Same x 4th Election			0.0978*	-0.0358
			(0.0571)	(0.0714)

Same x 5th Election	0.0793	0.0392
	(0.0528)	(0.0635)
Same x 6th Election	0.0568	0.0231
	(0.0553)	(0.0937)
Same x 7th Election	0.0372	-0.0481
	(0.0547)	(0.0766)
Same x 8th Election	0.0222	-0.0407
	(0.0510)	(0.0745)
Same x 9th Election	0.0210	-0.0956
	(0.0546)	(0.0723)
Same x 10th Election	0.0672	0.0251
	(0.0588)	(0.0883)
Same x 11th Election	0.0436	0.0102
	(0.0640)	(0.0829)
Same x 12th Election	0.0758	0.0359
	(0.0644)	(0.0949)
Same x 13th Election	0.0549	-0.0724
	(0.0560)	(0.0753)
Same x 14th Election	0.0424	0.00203
	(0.0608)	(0.0875)
Same x 15th Election	-0.0291	-0.111
	(0.0677)	(0.110)
Pres. x Same x 2nd Election		0.138
		(0.0997)
Pres. x Same x 3rd Election		0.117
		(0.0996)
Pres. x Same x 4th Election		0.165
		(0.0994)
Pres. x Same x 5th Election		0.0327
		(0.0937)
Pres. x Same x 6th Election		0.0224
		(0.120)
Pres. x Same x 7th Election		0.110
		(0.0910)
Pres. x Same x 8th Election		0.0714
		(0.111)
Pres. x Same x 9th Election		0.163*
		(0.0965)
Pres. x Same x 10th Election		0.0505

				(0.110)
Pres. x Same x 11th Election				0.0431
				(0.114)
Pres. x Same x 12th Election				0.0533
				(0.114)
Pres. x Same x 13th Election				0.182*
				(0.102)
Pres. x Same x 14th Election				0.0630
				(0.115)
Pres. x Same x 15th Election				0.116
				(0.132)
Age	0.0660***	0.0661***	0.0669***	0.0729***
	(0.00653)	(0.00649)	(0.00823)	(0.00780)
Age squared	-0.000442***	-0.000444***	-0.000525***	-0.000521***
	(0.0000560)	(0.0000557)	(0.0000736)	(0.0000675)
High school	0.275***	0.275***	0.283***	0.276***
	(0.0191)	(0.0191)	(0.0199)	(0.0192)
Some college	0.502***	0.502***	0.519***	0.504***
	(0.0215)	(0.0214)	(0.0231)	(0.0214)
College or advance degree	0.625***	0.625***	0.643***	0.627***
	(0.0219)	(0.0218)	(0.0228)	(0.0219)
Dummy female	-0.104***	-0.104***	-0.104***	-0.104***
	(0.00919)	(0.00919)	(0.00921)	(0.00922)
Dummy white	-0.0491***	-0.0491***	-0.0613***	-0.0498***
	(0.0139)	(0.0139)	(0.0138)	(0.0138)
Constant	-0.578**	-0.585**	0.0967	-0.646**
	(0.257)	(0.255)	(0.248)	(0.268)
N	49393	49393	49393	49393

Standard errors in parentheses

Standard errors are clustered by state

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table B2: Effect of experiencing the same type of election as the first eligible election on the level of interest in the election.

C   Additional figures and tables
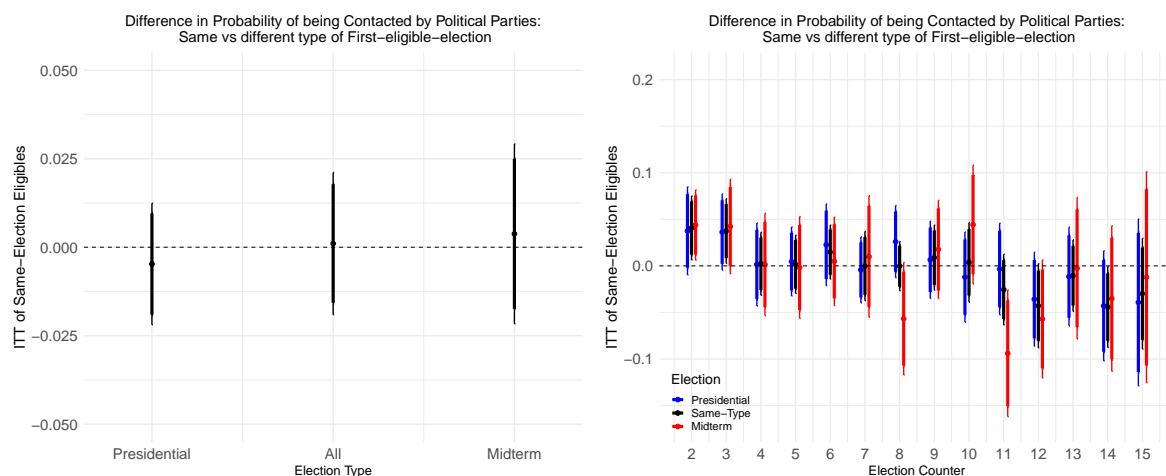
Figure C1: Effect of experiencing the same type of election as the first eligible election on the probability of being contacted by party canvassers.
Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on contacts to vote in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on contacts to vote in subsequent Midterm elections.

Figures C2 and C3 show the results from a placebo test. We recode type-of-election eligibility by simply shifting both groups by one year. For example, in the actual treatment variable, cohorts born in 1930, 1931, 1934 and 1935 are coded as Presidential eligibles while cohorts born in 1932, 1933, 1936 and 1937 are coded as midterm eligibles. We change the coding by shifting each group by one year. In this way the placebo eligibles become those born in 1931 and 1932, while the midterm eligibles become those born in 1933 and 1934. This exercise randomizes the initial election experience, half of each group now experiencing a presidential election before a midterm election and half having the opposite experience. The new groups retain the same zig-zagging pattern we observed in the first panel Figure 1 as well as the average age differences, without now being systematically different in terms of type-of-election eligibility. We replicate our analyses using these new placebo groups as comparison groups, employing as outcomes both turnout (Figure C2) and interest in the election (C2). Both sets of results point to the same conclusion. We find no significant same-type eligibility effects, either when it comes to Presidential or Midterm elections. Looking at the way the effects span across the life trajectory yields a pattern that is far from consistent, with the effects changing sign and magnitude across elections. The overall pattern strengthens our confidence that the observed differences found in the previous analyses are indeed due to the type of first-election eligibility.

Electronic copy available at: https://ssrn.com/abstract=3466515
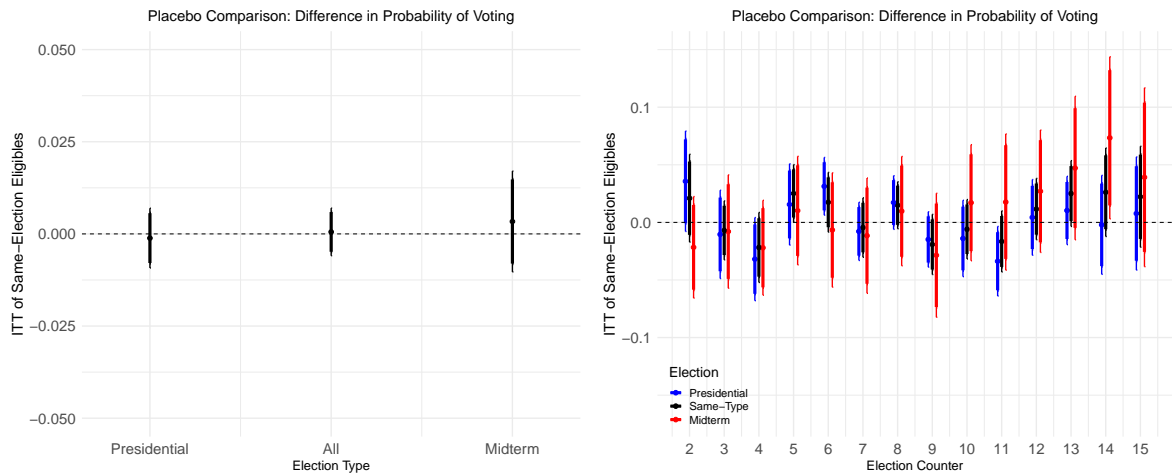
Figure C2: Placebo Effect of being first eligible to vote in a same-type election on probability of voting in future elections.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on voting in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on voting in subsequent Midterm elections.
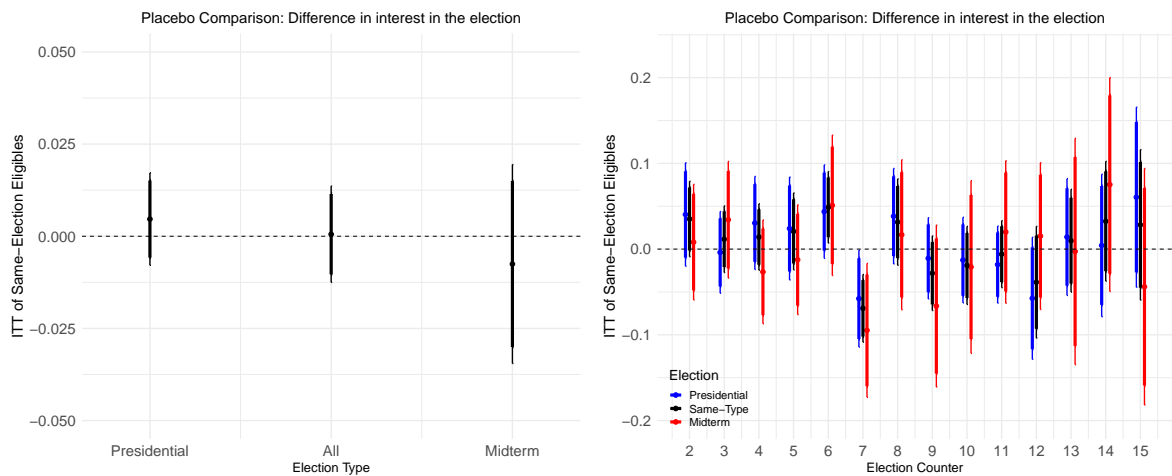
Figure C3: Placebo Effect of being first eligible to vote in a same-type election on interest in the current election.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on interest in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on interest in subsequent Midterm elections.

10
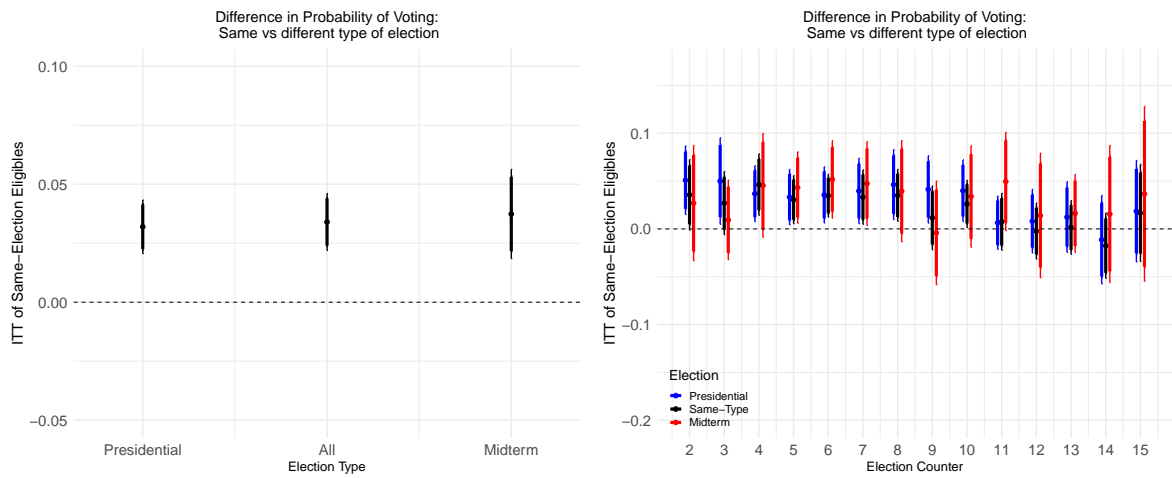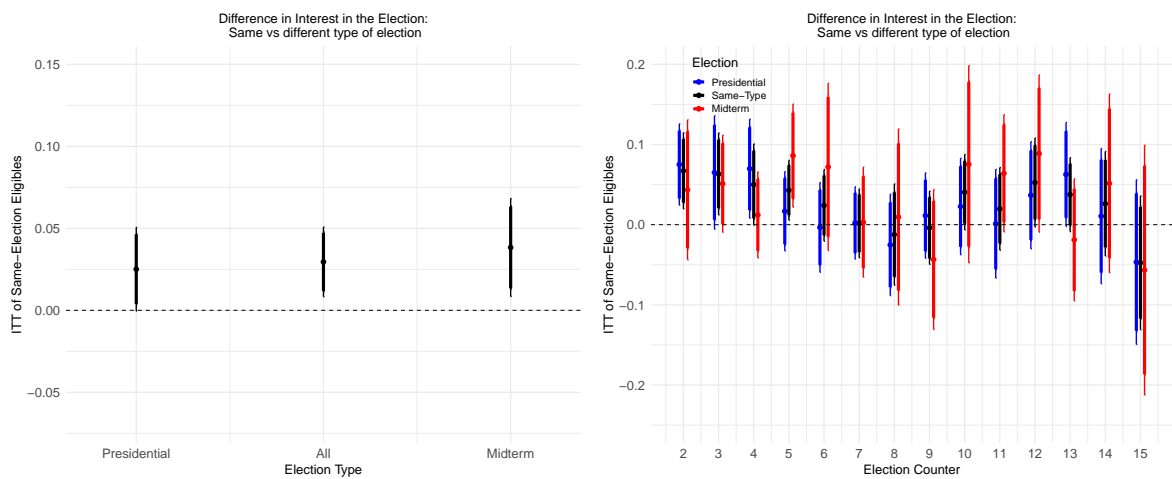
Figure C4: Replicating the main analysis shown in Figure 3, adding state-fixed effects. *Note:* The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on voting in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on voting in subsequent Midterm elections.
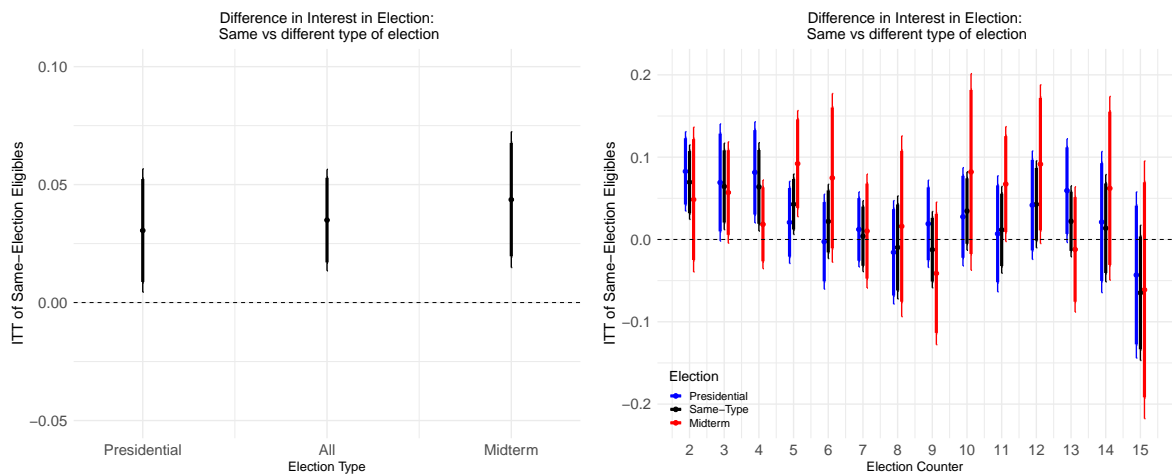
Figure C5: Replicating the main analysis shown in Figure 4, adding state-fixed effects. *Note:* The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on interest in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on interest in subsequent Midterm elections.

11
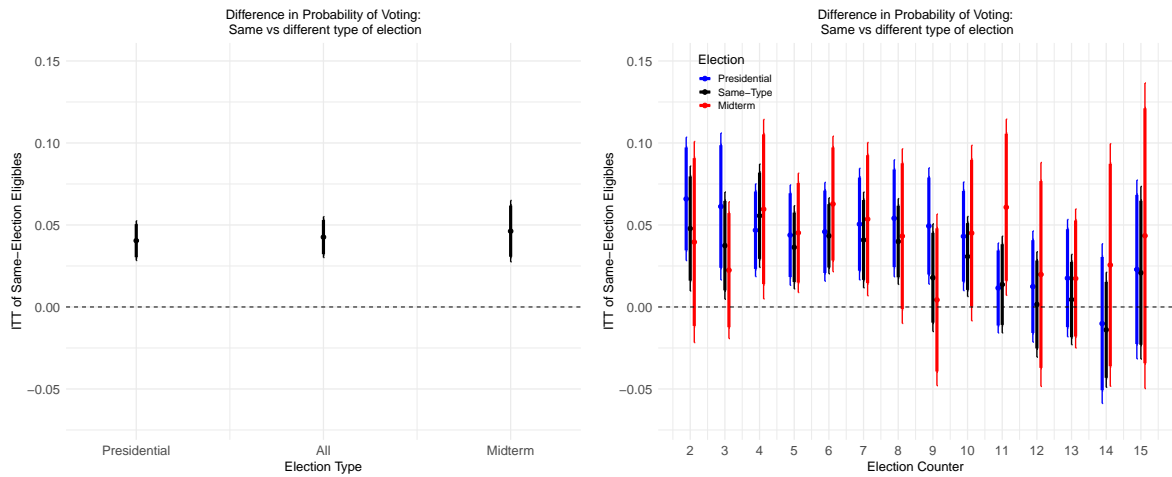
Figure C6: Replicating the main analysis shown in Figure 3 without additional covariates.
Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects
are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote
for a Presidential election on voting in subsequent Presidential elections; and the effect of being first
eligible to vote for a Midterm election on voting in subsequent Midterm elections. The analysis differs
from 3 in that it does not include race and gender as covariates.

Figure C7: Replicating the main analysis shown in Figure 4 without additional covariates.
Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects
are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote
for a Presidential election on voting in subsequent Presidential elections; and the effect of being first
eligible to vote for a Midterm election on voting in subsequent Midterm elections. The analysis differs
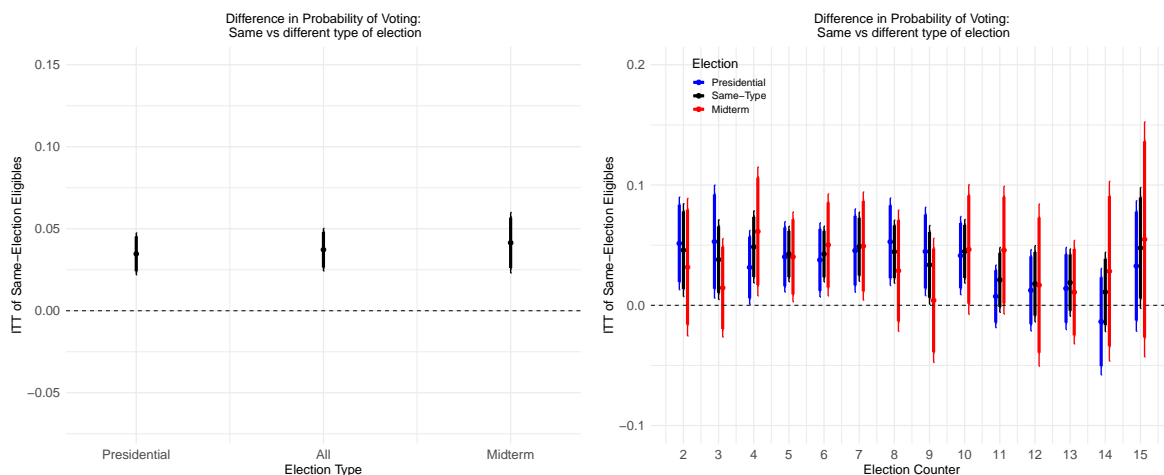from 3 in that it does not include race and gender as covariates.

12
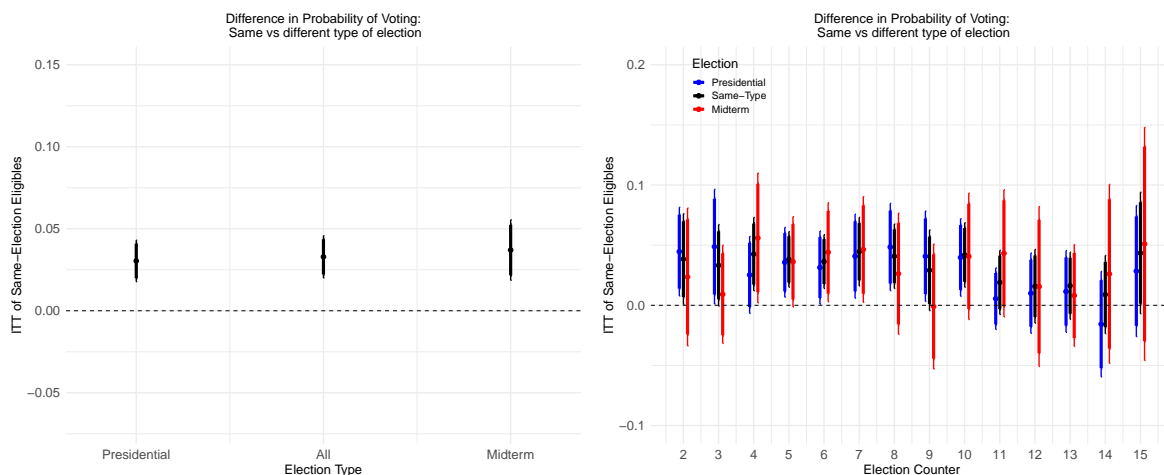
Figure C8: Replicating the main analysis shown in Figure 3 collapsing all post-secondary-school categories together.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on voting in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on voting in subsequent Midterm elections. Education is recoded into the following categories: 8 grades or less ('grade school'); 9-12 grades ('high school'); 12 grades, diploma or equivalency or above.
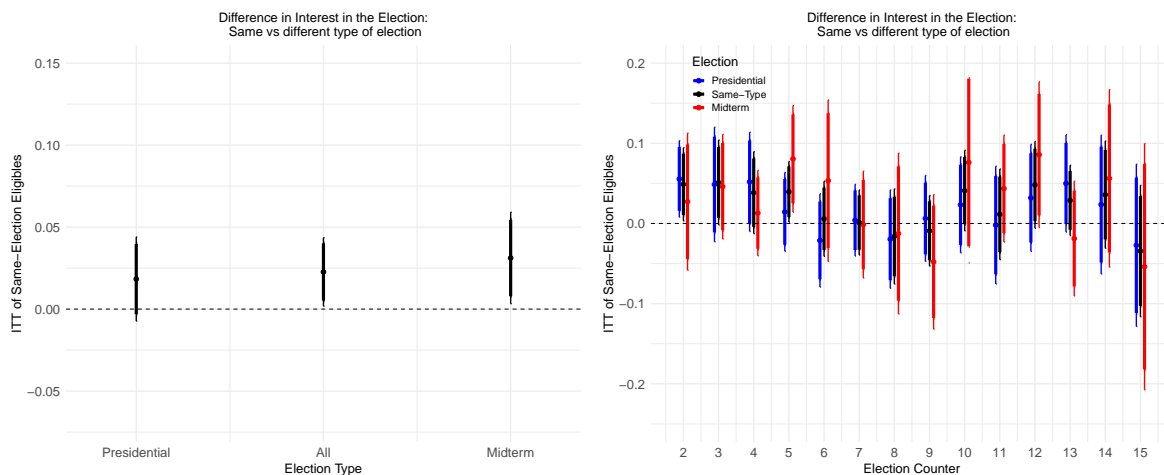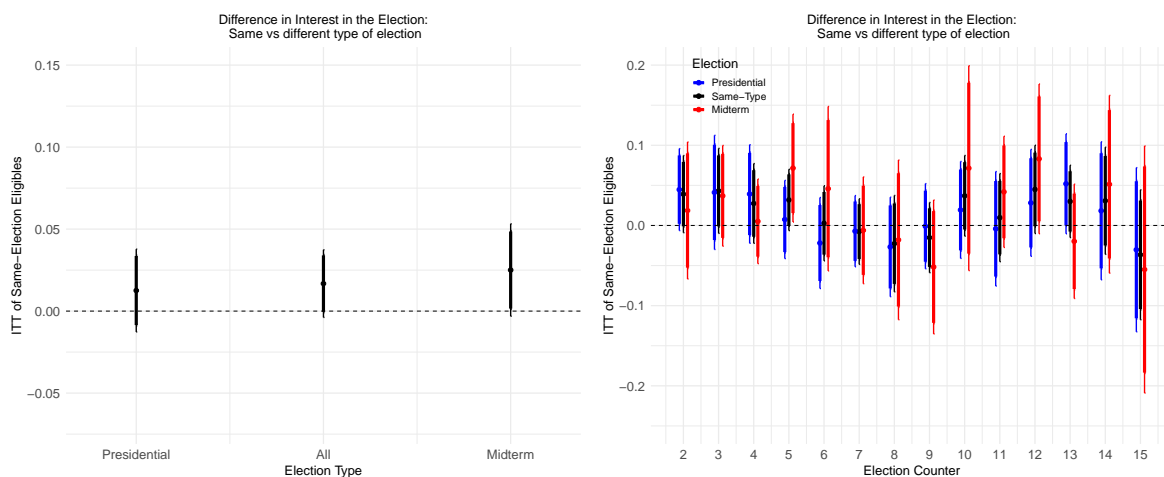

Figure C9: Replicating the main analysis shown in Figure 4 collapsing all post-secondary-school categories together.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on voting in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on voting in subsequent Midterm elections. Education is recoded into the following categories: 8 grades or less ('grade school'); 9-12 grades ('high school'); 12 grades, diploma or equivalency or above.
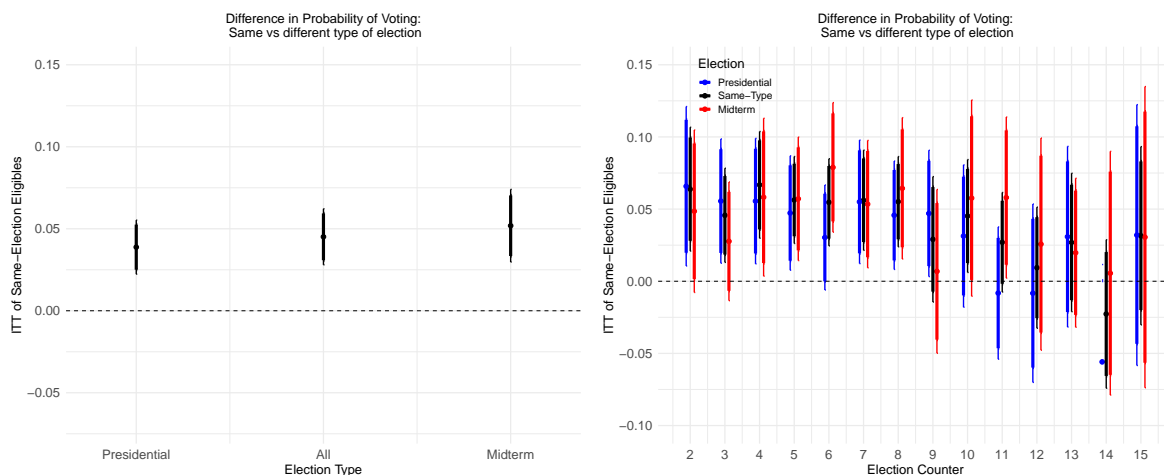
Figure C10: Replicating the main analysis shown in Figure C6 collapsing all post-secondary-school categories together.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on voting in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on voting in subsequent Midterm elections. Education is recoded into the following categories: 8 grades or less ('grade school'); 9-12 grades ('high school'); 12 grades, diploma or equivalency or above.

Figure C11: Replicating the main analysis shown in Figure C7 collapsing all post-secondary-school categories together.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on voting in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on voting in subsequent Midterm elections. Education is recoded into the following categories: 8 grades or less ('grade school'); 9-12 grades ('high school'); 12 grades, diploma or equivalency or above.
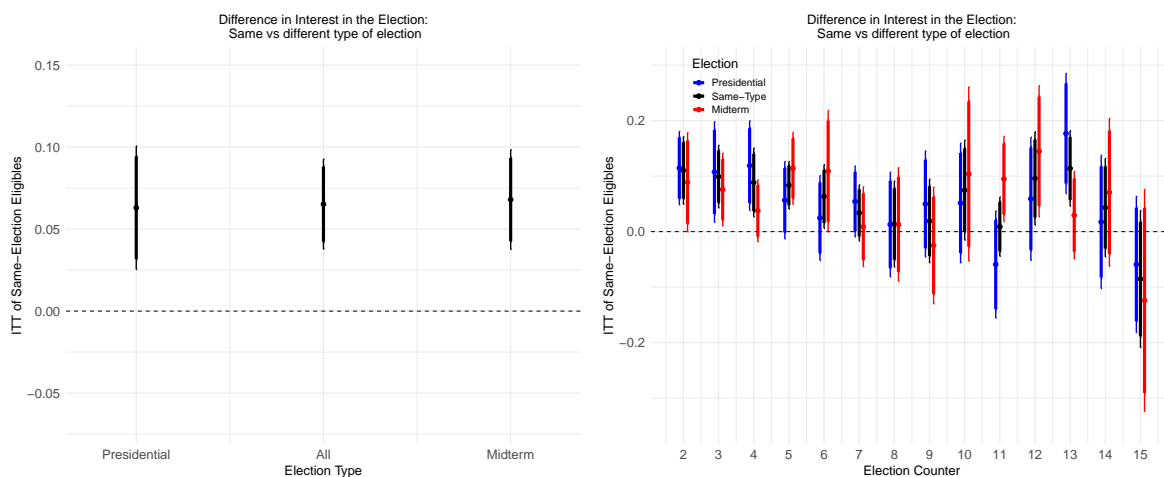
Figure C12: Replicating the main analysis shown in Figure 3 extending the list of control variables to contemporaneous demographics.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on voting in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on voting in subsequent Midterm elections. Apart from education, age, race and gender, we also include region of residence; *Urbanism*; and a dummy for high income. All variables are included fully factorized.
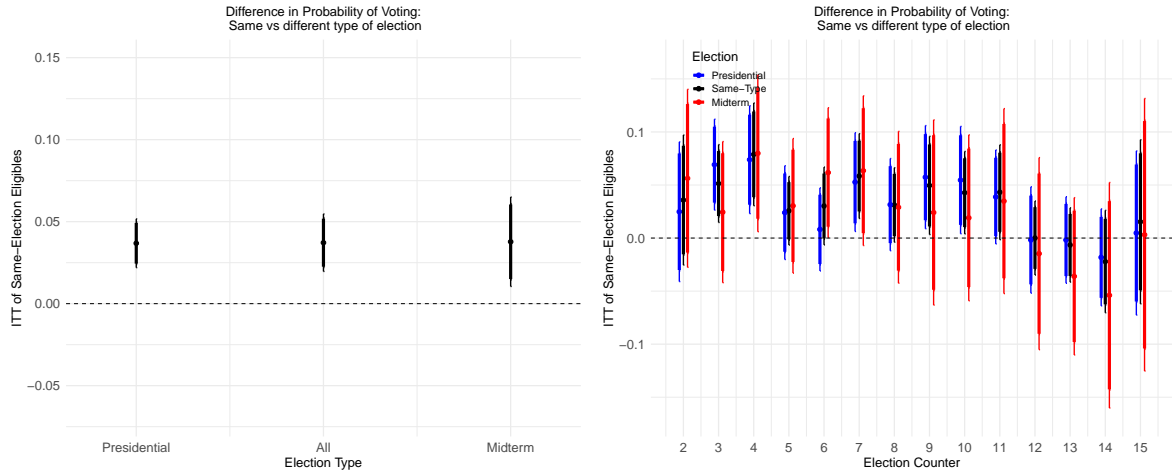

Figure C13: Replicating the main analysis shown in Figure 4 extending the list of control variables to contemporaneous demographics.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on voting in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on voting in subsequent Midterm elections.Apart from education, age, race and gender, we also include region of residence; *Urbanism*; and a dummy for high income. All variables are included fully factorized.

15

Figure C14: Replicating the main analysis shown in Figure 3, excluding respondents coming of age in an election year.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on voting in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on voting in subsequent Midterm elections.
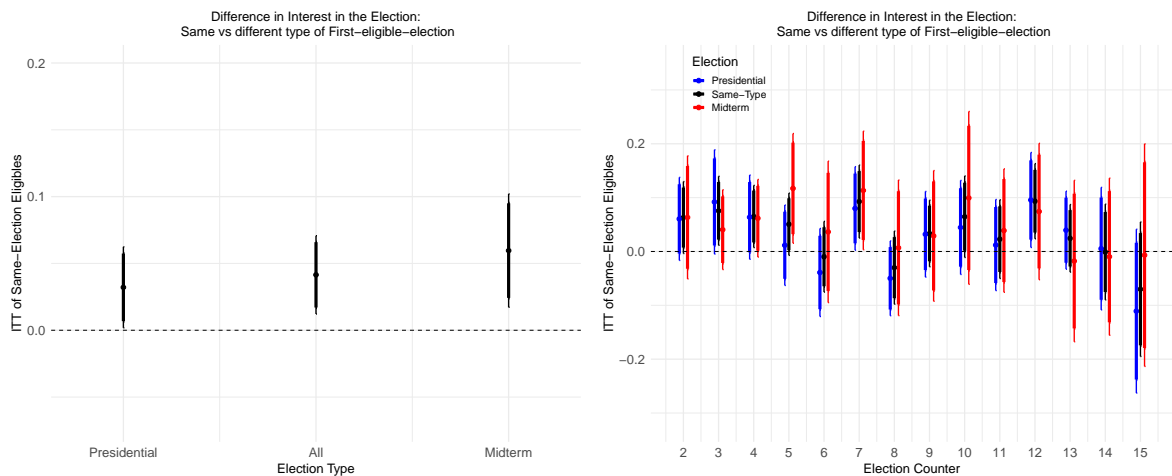

Figure C15: Replicating the main analysis shown in Figure 4, excluding respondents coming of age in an election year.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals. Effects are broken down into the overall effect of same-type eligibility, the effect of being first eligible to vote for a Presidential election on voting in subsequent Presidential elections; and the effect of being first eligible to vote for a Midterm election on voting in subsequent Midterm elections.

16
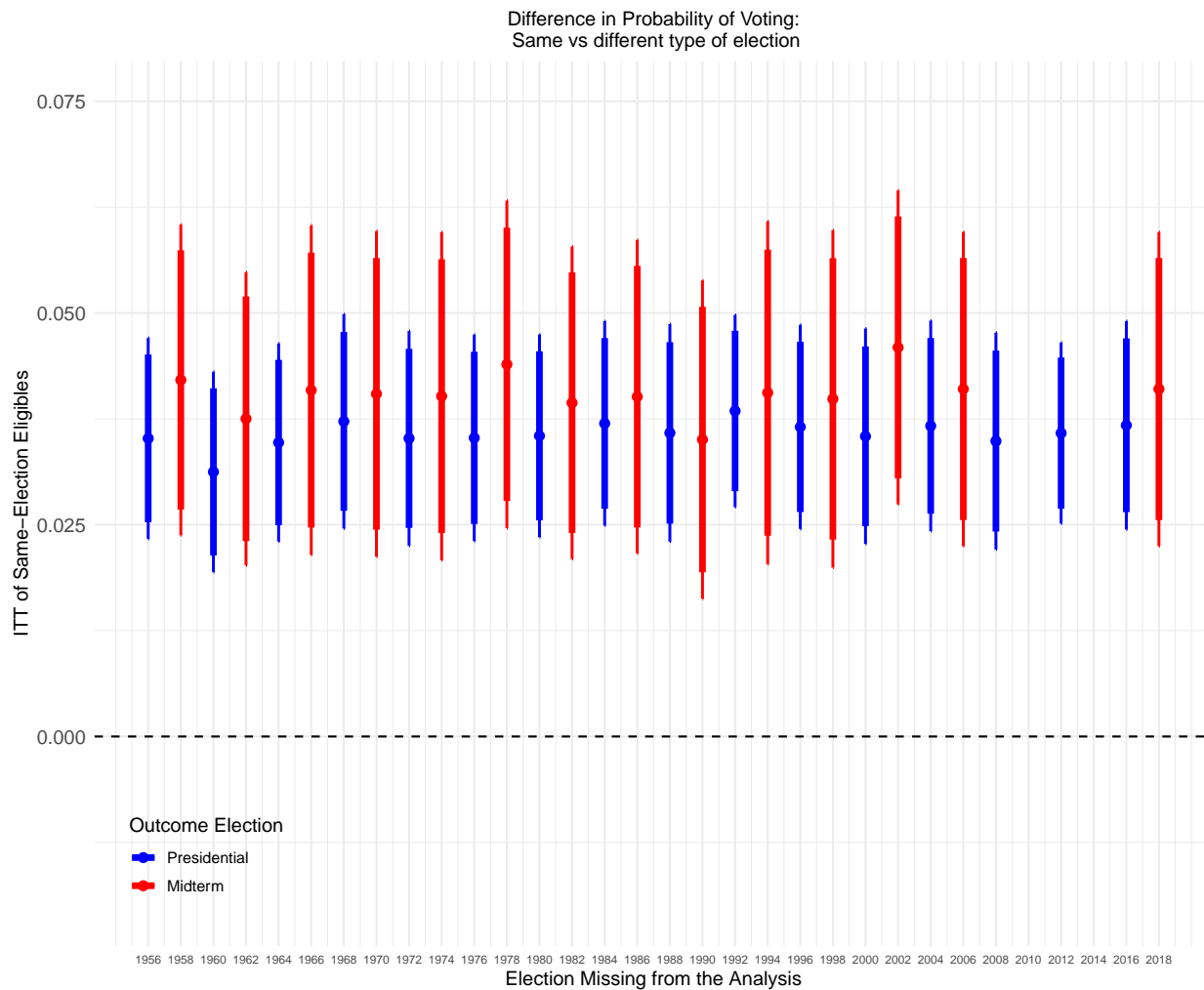
Figure C16: Same-type eligibility effects on future turnout, excluding sequentially one survey from the estimation.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals.
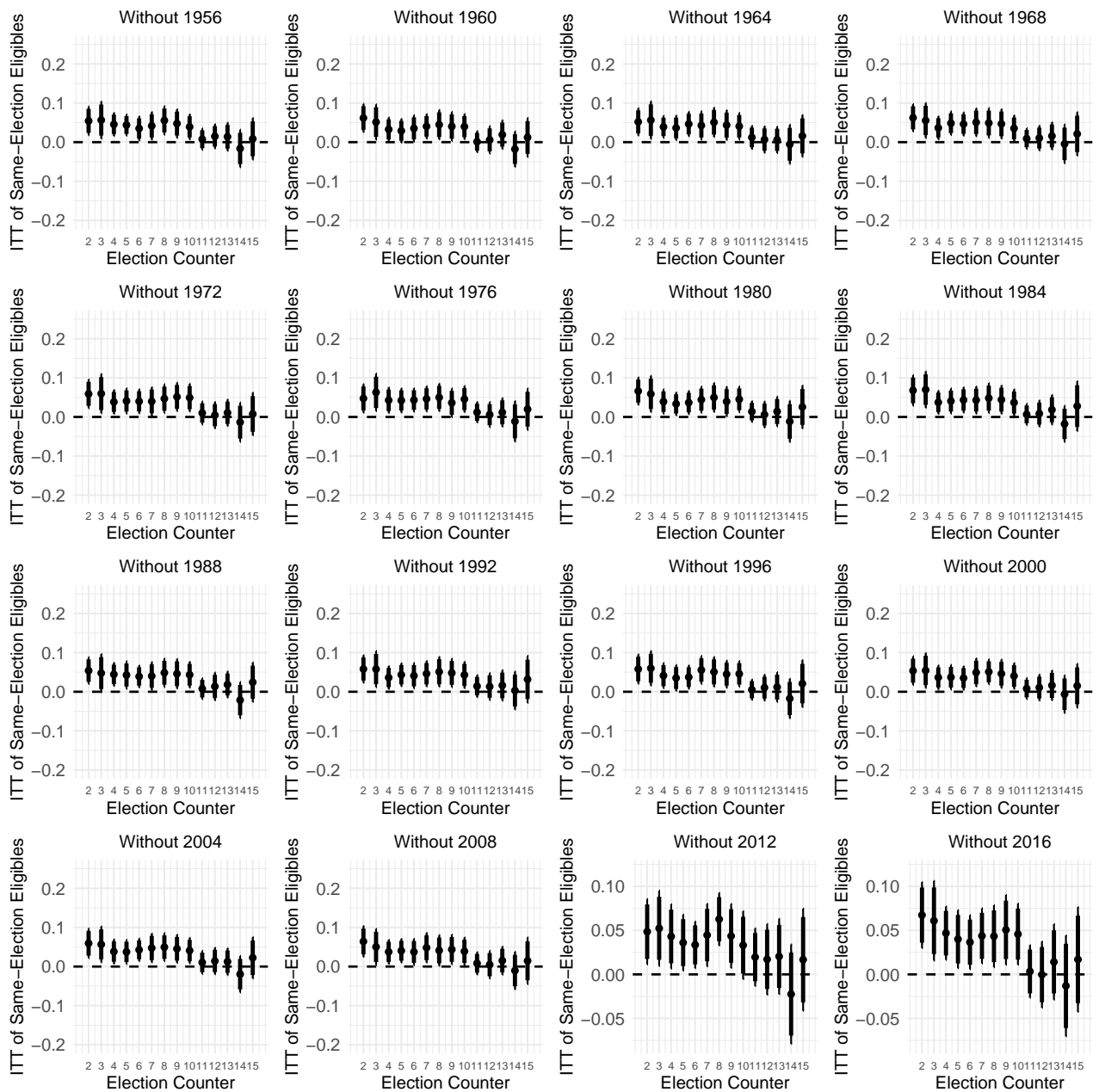
17

Figure C17: Same-type eligibility effects on future turnout across the life-span, excluding sequentially one survey from the estimation, Presidential elections.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals.
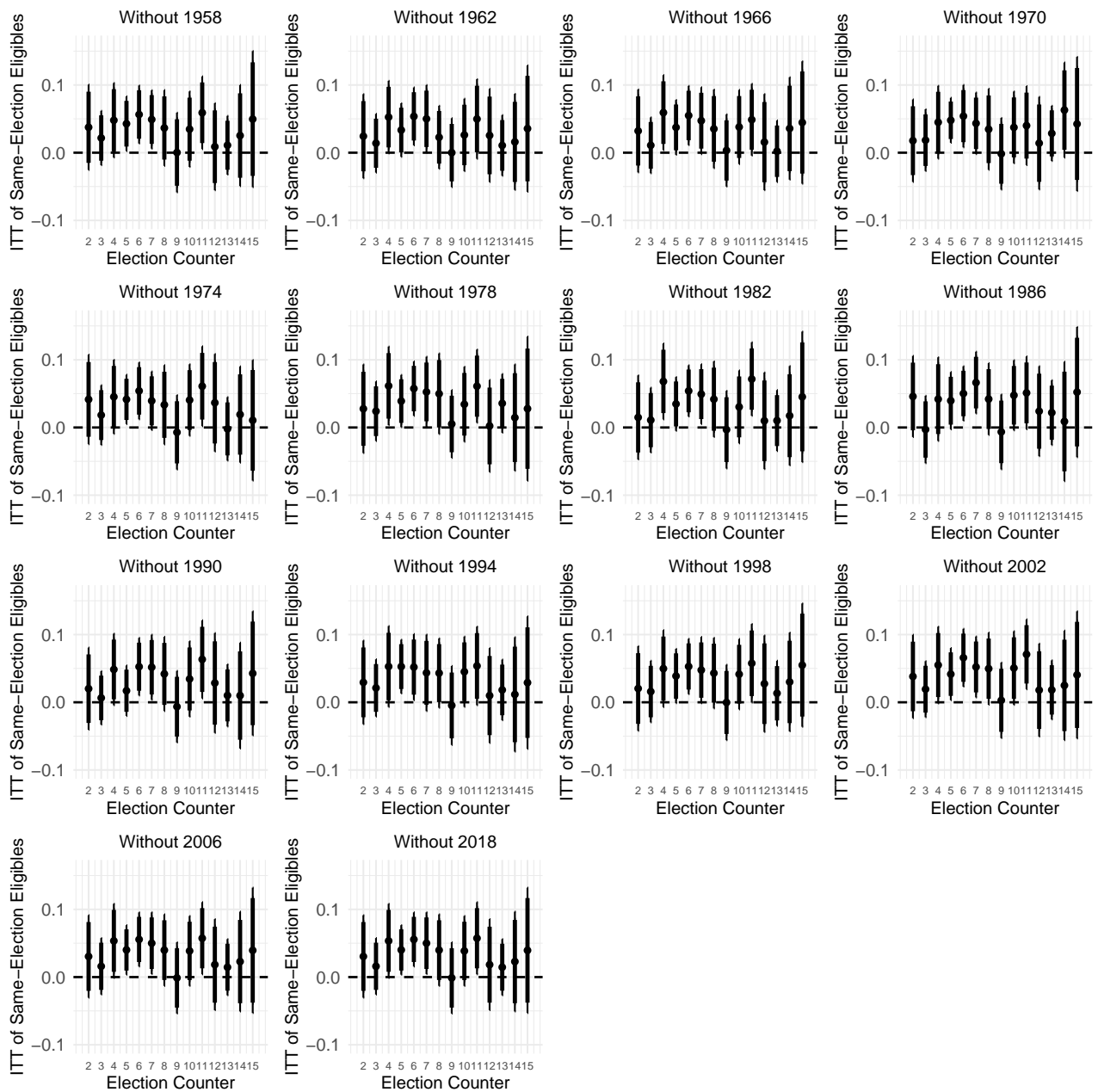
18

Figure C18: Same-type eligibility effects on future turnout across the life-span, excluding sequentially one survey from the estimation, Midterm elections.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals.
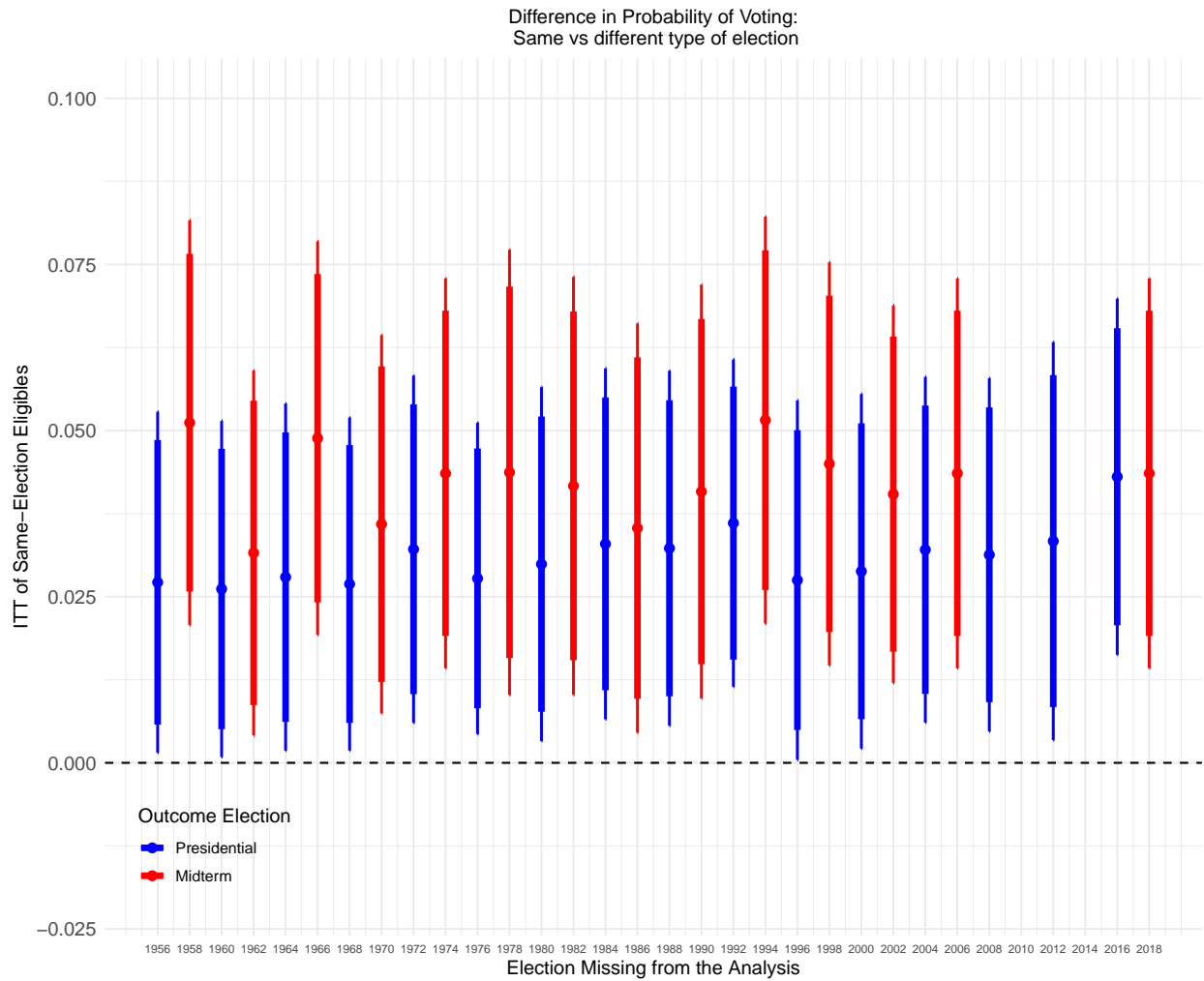
19

Figure C19: Same-type eligibility effects on future interest in election, excluding sequentially one survey from the estimation.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals.
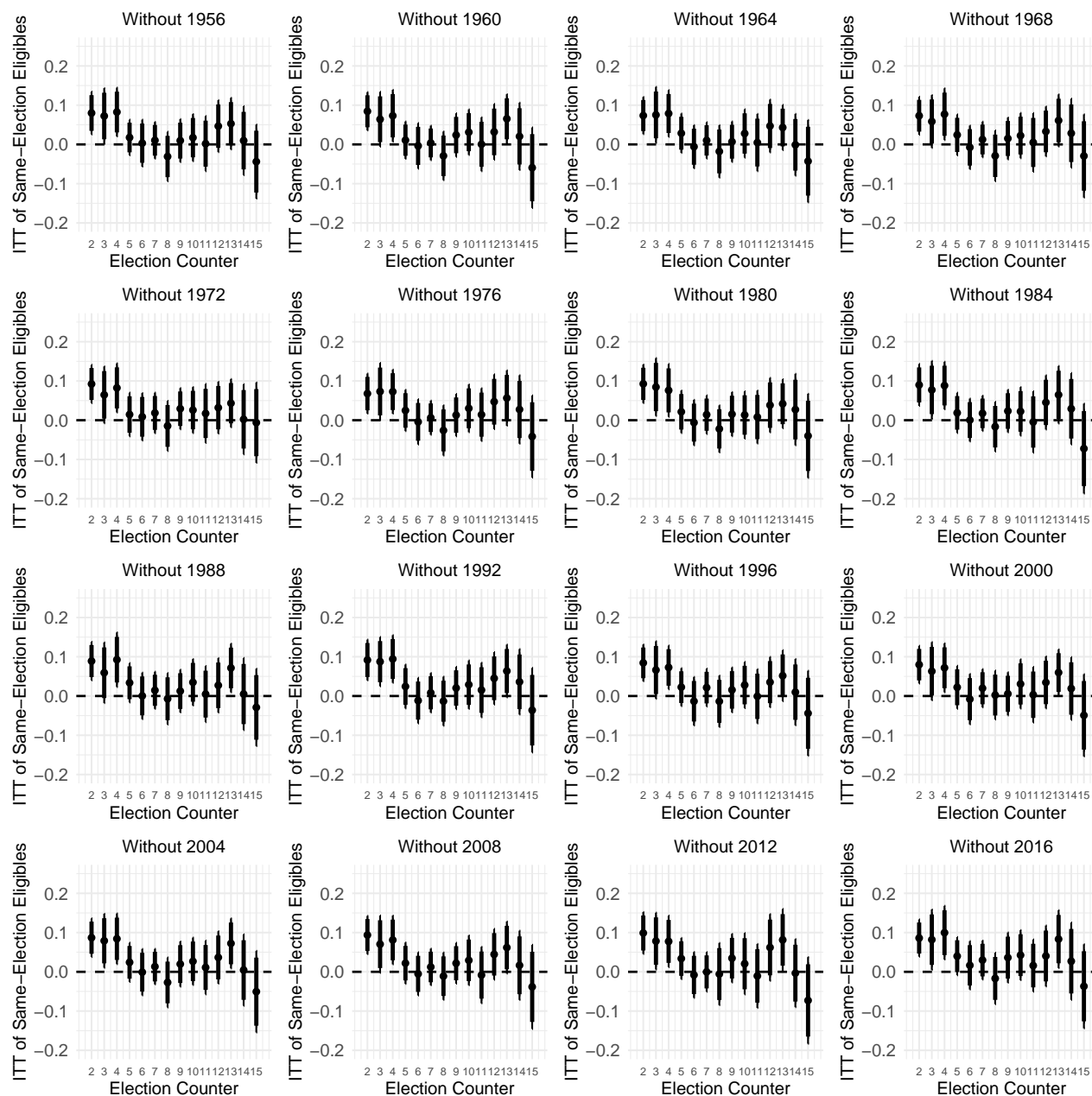
20

Figure C20: Same-type eligibility effects on future interest in election across the life-span, excluding sequentially one survey from the estimation, Presidential elections.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals.
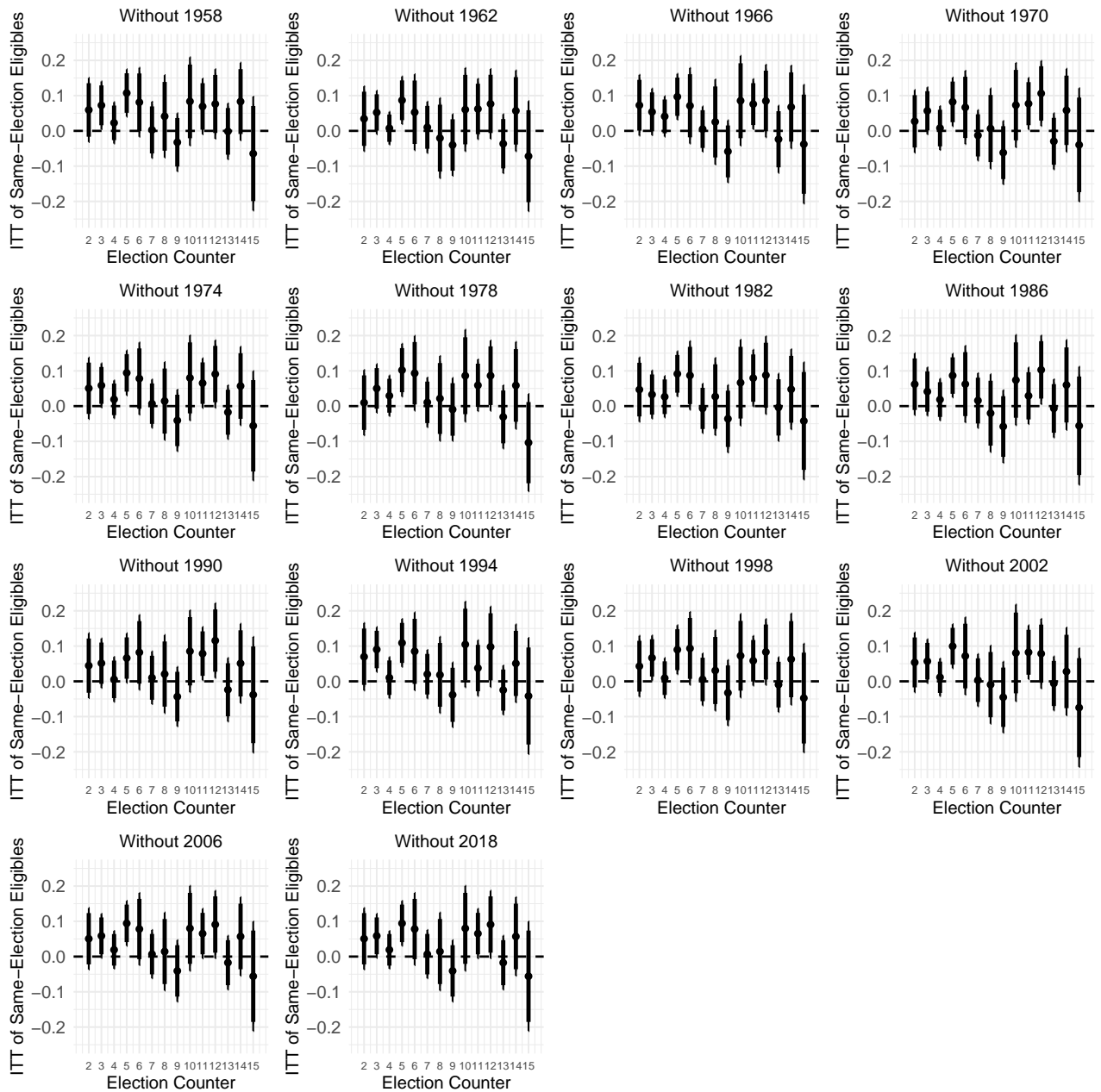
21

Figure C21: Same-type eligibility effects on future interest in election across the life-span, excluding sequentially one survey from the estimation, Midterm elections.

Note: The vertical spikes denote the 90% (thick line) and 95% (thin line) confidence intervals.

22

	(1)	(2)	(3)
Change	-0.0841***	-0.0740***	-0.0675***
	(0.0214)	(0.0225)	(0.0235)
Change * Midterm	0.101***	0.0896***	0.0804***
	(0.0259)	(0.0267)	(0.0271)
Observations	18,280	18,268	18,268
Necessary Controls	Yes	Yes	Yes
Necessary Controls interacted	Yes	Yes	Yes
Additional Controls	No	Yes	Yes
Additional Controls Interacted	No	Yes	Yes
State dummies	No	No	Yes
State dummies interacted	No	No	Yes
Election pair fixed effects	Yes	Yes	Yes
Election pair interacted	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C1: Effect of changing one's first election type from Midterm to Presidential on voting for subsequent elections.

	(1)	(2)	(3)	(4)
Placebo change	0.0184	-0.125	-0.0458	-0.0450
	(0.116)	(0.130)	(0.0659)	(0.0291)
Placebo change * Presidential	-0.0185	0.120	0.0718	0.0153
	(0.111)	(0.133)	(0.0683)	(0.0353)
Observations	6,756	7,694	10,225	13,941
Necessary Controls	Yes	Yes	Yes	Yes
Necessary Controls Interacted	Yes	Yes	Yes	Yes
Additional Controls	Yes	Yes	Yes	Yes
Additional Controls Interacted	Yes	Yes	Yes	Yes
State dummies	Yes	Yes	Yes	Yes
State dummies interacted	Yes	Yes	Yes	Yes
Election pair fixed effects	Yes	Yes	Yes	Yes
Election pair interacted	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C2: Placebo for changing one's first election type from Presidential to Midterm on subsequent voting behavior.