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Is voting really habit-forming and transformative?

Long-run effects of earlier eligibility on turnout and political involvement from the UK*

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Abstract

Habit formation theory and the transformative voting hypothesis both imply that voting has downstream consequences for turnout and political involvement. Although several studies have applied causal research designs to study this question, the long-run evidence is extremely limited, especially for potentially transformative effects. We jointly examine the short- and long-term impact of earlier voting eligibility on subsequent turnout and political involvement using rich panel data from the UK. Exploiting the eligibility cut-off for national elections within a regression discontinuity design, our precise estimates document a short-run increase in voting—for those able to vote earlier—alongside a contemporaneous increase in several measures of political involvement. However, we show that these short-term effects fade away quickly and do not translate into permanent changes in turnout propensity or political involvement. Our results imply that, in a setting with low institutional barriers to vote, the transformative effects of voting are short-lived at most.

JEL: D01, D70, D72

Keywords: habit formation; transformative voting hypothesis; voter turnout; political involvement; regression discontinuity

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1 Introduction

The act of voting has long been portrayed as a habit-forming and transformative event in the political lives of citizens (Green and Shachar, 2000; Plutzer, 2002; Fowler, 2006; de Kadt, 2017). According to habit-formation theory, the act of voting itself has a direct effect on future turnout, leading citizens to develop a habit of voting. Several studies for the United States that rely on causal research designs have shown that past eligibility leads to increased participation in the subsequent election, providing supporting for this theory (e.g. Meredith, 2009; Dinas, 2012; Fujiwara et al., 2016; Coppock and Green, 2016). Relatedly, the transformative voting hypothesis posits that the act of voting has additional downstream effects on political involvement, so going out to vote increases political interest, knowledge, and non-electoral participation. This transformative effect could then be one mechanism underlying habit formation (for extensive reviews of this literature, see Holbein and Rangel, 2020; Holbein et al., 2021).

Recent research from other national contexts has, however, found little evidence of an impact of earlier voting or earlier eligibility on subsequent turnout and political involvement, casting doubt on the idea that voting is habit-forming or transformative (Bhatti et al., 2016; Bechtel et al., 2018; Hernæs, 2019; Holbein and Rangel, 2020). Yet, studies on the effects of earlier eligibility have one common and major shortcoming: with the exception of Meredith (2009) and Coppock and Green (2016), they focus on short-run outcomes as they largely examine the subsequent election for voting outcomes and political interest and involvement surveyed a few months after the first election (see Appendix Table A.1 for an overview of causal studies). This absence of long-run empirical evidence is particularly stark for the transformative effect of voting, even though it is a key theorised mechanism underlying the habit of voting (see Holbein et al., 2021).

The main purpose of our paper is to address this shortcoming by providing new causal evidence on the short- and long-run effects of earlier eligibility on political behaviour and involvement in a unified framework. To examine whether voting is habit-forming and/or transformative, we use survey data from the United Kingdom to examine the long-run effects of earlier voting eligibility on turnout and political involvement up to three general elections after the first vote, that is up to 15 years later. In a supplementary analysis, we also examine outcomes up to 30 years later, i.e., up to six subsequent elections. Moreover,

our survey data provides by far the largest sample size in this literature (up to 190,000 observations), allowing us to precisely estimate both the upstream (up to two years prior to the first election) as well as the downstream effects of voting. Our joint examination of the upstream and long-run downstream effects on voting and political involvement enables us to investigate whether past voting has a transformative effect on individuals and whether this is a potential mechanism underlying habit-forming effects of voting. The time span and number of individuals included by our data, together with the breadth of our outcomes considered, thus provide a unique window into the long-term habit-forming and transformative effect of voting.

Our paper makes two additional important contributions to existing research. First, we test for the effect of past voting eligibility on subsequent turnout and political preferences in a new context, the United Kingdom. Our case, described further in section 3, provides a useful comparison to the United States as the transformative effects of voting may be moderated by the institutional barriers to electoral participation. Unlike the US, the UK is characterised by comparatively low institutional barriers to voting, a feature that may reduce the habit-forming effects of voting (Bhatti et al., 2016). A further useful advantage of our setting is that our survey data is representative for the entire UK, covering both registered and unregistered individuals. This contrasts with the studies from the US that rely on administrative voter records, as this may induce a potential sample selection problem by focusing only on registered voters from selected states that make the data available.

Our data source and empirical design also substantially improve upon the only existing studies for the UK. Denny and Doyle (2009) examine turnout between two subsequent elections for individuals born in 1958 using a random effects model; this approach, however, relies on a small and selective sample and does not exploit exogenous variation in first-time eligibility. Cutts et al. (2009) conduct and analyse a field-experimental intervention to increase turnout in one parliamentary constituency during the 2005 general election; they find very large effects and concede that their study is impaired by a small sample and problems of attrition. Compared to both studies, our paper exploits exogenous variation in first-time eligibility and provides more general results by covering more elections, by using a representative sample, and by including birth cohorts spanning several decades.

Second, our data allow us to examine the effect of past eligibility for a large number of elections, covering seven UK general elections held between 1992 and 2017. These elections varied in the salience and competitiveness, features that may affect the impact of first-time voting (Franklin and Hobolt, 2011); for instance, the 1997 election, which led to Tony Blair’s premiership ending 18 years of rule by the Conservative Party, may have been a different first-time voting experience than the more humdrum 2001 election. Our results thus provide evidence for whether the characteristics of one specific election—and therefore first-time voting experience—moderate the habit-forming, transformative effects of elections (Bhatti et al., 2016).

To identify the causal effect of past voting eligibility, we use a regression discontinuity (RD) design (similar to, e.g., Meredith, 2009; Coppock and Green, 2016) on individual-level survey data (BHPS/UKHLS) from the UK. The RD approach is appealing because of its simplicity and transparency: we compare the long-run outcomes of individuals who turned 18 just before an election (and thus had the opportunity to vote) with those who turned 18 just after an election (who were thus first able to vote one electoral cycle later). Importantly, our estimates rely on comparing individuals at the eligibility cut-off and are thus not confounded by life cycle effects, which are often drawn on to explain low turnout of young voters. Our RD estimates thus cleanly identify the long-run effects of being able to vote at an earlier election.

Our study holds important normative and practical consequences for how efforts to increase turnout should be discussed. If participation in elections has long-lasting effects, then this raises the stakes of electoral turnout and the impact of exogenous events that may increase or decrease voting. If such long-lasting effects are absent, then it is less worrying that some elections, such as those for the European Parliament, see low levels of turnout (Franklin and Hobolt, 2011). In addition, potential habit-forming, transformative effects of voting also raise the stakes of lowering the voting age to 16. If these effects are absent, then the question of whether young voters are more or less likely to participate loses some of its urgency, and the voting age reform loses some of its instrumental justification.

To preview our results: we find little evidence that voting is habit-forming or transformative in the UK. Though we do estimate an increase in electoral participation and political involvement around the time of the first vote, these effects are short-lived and

disappear by the time of the second election. Our estimates are precisely estimated, robust to several specification checks, and do not suffer from potential non-response bias due to earlier eligibility. Our result that earlier eligibility does not affect subsequent voting implies that the habitual effects of voting found in previous work do not generalise to different contexts. In addition, our findings show little variation based on election salience and outcomes as moderators of the impact of past vote. We also go beyond existing studies by exploring the potential mechanisms of habit formation—political involvement, i.e., party support, political interest, voting as a civic duty and party membership—and show that there is at most a short-term positive effect of past eligibility on political involvement, consistent with a ‘first-time hype’ effect (Bhatti et al., 2016). However, we find no impact of early eligibility on political involvement in the long-run, casting further doubt on the hypothesised transformative effect of voting (see also Holbein and Rangel, 2020; Holbein et al., 2021).

The paper is structured as follows. In section 2 we discuss the previous literature and provide some additional background discussion. Section 3 presents our case, including institutional details, a description of the data source and the empirical methodology. In section 4, we present the empirical results, followed by several robustness checks. Finally, we summarise and discuss the implications of these findings in section 5.

2 Background and previous literature

2.1 Is voting habit-forming?

Turnout patterns persist over time: early correlational studies have shown that those who vote in one election are highly likely also to vote in later elections (Brody and Sniderman, 1977; Franklin, 2004). This stability in turnout behaviour may of course be due to the persistent influence of factors such as social background or partisanship that remain largely constant over time. However, Green and Shachar (2000) and Gerber et al. (2003) put forward an additional explanation: voting is habit-forming (see also Plutzer, 2002; Fowler, 2006). This means that citizens become used to voting, so the act of voting itself solidifies turnout propensity.

While the initial research on habitual voting relied on observational research designs (Holbein and Rangel, 2020), recent studies apply credible causal designs, for instance ex-

ploiting exogenous variation in eligibility—as pioneered by [Meredith \(2009\)](#)—and show that voting in past elections makes people more likely to vote in a subsequent election (for an overview of the literature, see Appendix Table [A.1](#)). [Meredith \(2009\)](#) relies on administrative voter records from California and compares average outcomes of individuals just eligible or ineligible for a presidential election. He shows that eligibility in the 2000 presidential election increased turnout at the next presidential election by 3 to 4.5 percent. In related work, [Dinas \(2012\)](#) uses US panel survey data for individuals born in 1947 or 1948 and examines how eligibility for the 1968 election affected turnout at the 1970 congressional elections; his intention-to-treat estimates indicate that prior eligibility increases turnout at the subsequent election by 12.3 percentage points (pp). Other recent research uses turnout decline due to rainfall to estimate a past voting effect: if turnout decreases by 1 pp, participation in the subsequent election is 0.6 to 1.0 pp lower ([Fujiwara et al., 2016](#)). Various field experiments (e.g., [Gerber et al., 2003](#); [Bedolla and Michelson, 2012](#); [Coppock and Green, 2016](#)) also show that randomized get-out-the-vote efforts aimed at increasing participation have effects over a year later (though see [Michelson, 2003](#), for contrasting findings). These findings, again mostly based on US data, support the notion that voting is habit-forming.

An additional question concerns how long-lasting effects of a habit of voting are. [Coppock and Green \(2016\)](#) argue that there are good reasons to expect an effect to decay, for instance as people decide to abstain from low-salience elections. On the other hand, habit effects might also accumulate over time, broadening gaps in participation. Most evidence on the habit of earlier voting eligibility examines only comparatively short-term effects, almost exclusively for the first subsequent election (see Appendix Table [A.1](#)). One notable exception is [Coppock and Green \(2016\)](#) who provide evidence from public voter files data from 17 US states using eligibility discontinuities. The empirical approach is in principle identical to our research design, though the authors have to additionally control for lagged votes cast by a birth date cohort as their data does not include individuals who are eligible, but not registered, to vote. Their results show persistence of habit effects of up to 20 years, though the estimates for 16 and 20 years rely on selected data from three to four states (Arkansas, Illinois, Florida and Missouri). [Meredith \(2009\)](#) also examines how turnout in California in the 2004 presidential election was affected by eligibility in presidential and congressional elections between 1988 and 1998; his results indicate zero

effects, though the results are not consistent across different elections and estimators.

In contrast to these findings from the US, recent research from other contexts has cast doubt on whether voting is generally habit-forming. For instance, studies from Norway, Sweden, Finland, Switzerland and Brazil, fail to find consistent evidence of the habit-forming effects of elections (Górecki, 2015; Bhatti et al., 2016; Bechtel et al., 2018; Hernæs, 2019; Holbein and Rangel, 2020). These studies employ a variety of empirical approaches. Bhatti et al. (2016) use a regression discontinuity design similar to that in Meredith (2009) and Coppock and Green (2016), while other research leverages past compulsory voting in Switzerland (Bechtel et al., 2018) and Brazil (Holbein and Rangel, 2020) or the late enfranchisement of women in Switzerland (Górecki, 2015).

What might explain these inconsistent results? First, research that provided evidence in favour of voting being habit-forming has largely examined the US. However, the US case differs from other settings due to the stronger institutional barriers to voting. For instance, in most countries voter registration is far easier—in some cases even automatic. The institutional setting may moderate the impact of first-time voting: the less effort is required to register to vote, the smaller the effect of past eligibility. Second, past eligibility need not only have positive effects, as the novelty of voting might wear off the second time around, creating a countervailing, ‘first-time hype’ effect (Bhatti et al., 2016). The second time around, voting will be less exciting and novel, leading to lower enthusiasm and thus lower turnout. Descriptively, turnout is often indeed lower for second-time voters (Bhatti et al., 2012; Konzelmann et al., 2012). Hence, it may well be that the habit-forming effects of voting are particular to the US context, which is most notably characterised by high institutional barriers to voting, particularly for socio-economically disadvantaged groups. Third, the ‘treatments’ examined in the US and European literature differ—whereas the US literature cited above relies mainly on eligibility discontinuities, only Bhatti et al. (2012) and our study also use eligibility discontinuities, whereas the rest of the European literature (Górecki, 2015; Bechtel et al., 2018) leverages other exogenous variations in voting that do not allow for a direct comparison.

Further research has argued that the downstream impact of past eligibility may vary depending not only on the national, but on the specific electoral context. For example, vote eligibility may have a particularly long-lasting effect when the electoral context creates positive affect, such as at a first democratic vote in South Africa or Chile (de Kadt,

2017; Kaplan et al., 2019). A more general role may be played by electoral competitiveness: if one’s first election is close and exciting, this might strengthen habituation effects more than if the first election is uneventful or low-salience (Franklin, 2004; Franklin and Hobolt, 2011).

2.2 Is voting transformative?

In addition to fostering a habit of voting, it has been argued that voting is a transformative act (for a review of this research, see Holbein and Rangel, 2020; Holbein et al., 2021). According to this hypothesis, voting strengthens individual attitudes such as political interest, the sense of civic duty or political efficacy (Verba et al., 1995; Gerber et al., 2003; Denny and Doyle, 2009; Bhatti et al., 2016; Coppock and Green, 2016). Voting then has an ‘educative effect’ (Mansbridge, 1999), increasing civic involvement more broadly (Braconnier et al., 2017; Holbein and Rangel, 2020). Going out to vote would therefore not just be important as a way of contributing to political decision-making, it would also make people better citizens in the long-run (Mansbridge, 1999). Such downstream effects may be particularly likely when the target group is at a malleable age (Plutzer, 2002).

The transformative voting hypothesis is also a key mechanism leading to habit formation (Coppock and Green, 2016; Holbein and Rangel, 2020). If the act of voting durably increases political involvement, then this will increase the likelihood that voting will turn into a long-term pattern of behaviour. Other potential mechanisms underlying the habit to vote—not examined in this paper—are less clearly related to cognitive changes among voters. First, voting may lead individuals to reflexively associate elections with going out to vote (Bhatti et al., 2016). Second, the habit-forming effect may be spurious to the extent that it emerges mainly because parties target previous voters more strongly when campaigning (Gerber et al., 2003) or because institutional barriers mean that, once registered, the costs of voting decline significantly (Bhatti et al., 2016).

Despite the strong face validity of the transformative voting hypothesis, the evidence for downstream effects of voting is decidedly mixed. Among the sparse research able to infer causality (as summarised in Holbein et al., 2021), some studies show clear short-term effects of voting, for instance on political interest and knowledge (Braconnier et al., 2017; Shineman, 2018; Bruce and Lima, 2019), partisanship (Meredith, 2009), and on non-electoral political participation (Khoban, 2019). Mullainathan and Washington

(2009) also find that past vote eligibility leads to a polarization of political opinions that persists two years later, at the next election. However, other work presents null results on outcomes such as political knowledge, information consumption, political memberships and social awareness (Loewen et al., 2008; de Leon and Rizzi, 2014; Rosenqvist, 2017; Holbein and Rangel, 2020). For example, de Leon and Rizzi (2014) and Rosenqvist (2017) find that earlier eligibility does not increase political knowledge of individuals in Brazil and Sweden, respectively. A meta-analysis of existing studies that apply causal research designs ($N = 9$) concludes that “average voting experiences (which come with being made eligible to vote) have small to non-existent downstream transformative causal effects” (Holbein et al., 2021).

However, all these causally-identified research designs only examine short-term effects that immediately follow having voted; partial exceptions are Holbein and Rangel (2020) and Mullainathan and Washington (2009), who consider effects up to two years later. This short-term perspective is particularly problematic given the vague expectations of the transformative voting hypothesis. The following patterns could be consistent with the theory: “effects on adjacent attitudes and behaviours could arise immediately, then fade away; or, they could appear initially, and then remain; or, they could not appear initially, but then appear later” (Holbein et al., 2021). Such patterns cannot be uncovered if analyses focus only on the short term. In addition, to be truly transformative, the effect of voting should be more than ephemeral. This motivates our long-term study of the downstream impact of voting.

3 Case description, data and empirical approach

Case The Parliament of the United Kingdom represents the legislative body in the UK and is made up of two chambers - the House of Commons and the House of Lords. Whereas citizens can vote for the lower house, i.e., the House of Commons, the House of Lords was historically a largely hereditary aristocratic chamber that is currently undergoing extensive reforms due to the House of Lords Act 1999. Besides the national parliament, subnational parliaments exist in Scotland, Wales, and Northern Ireland, each endowed with varying degrees of legislative independence. The national government, led by the Prime Minister, is drawn from and answers to the House of Commons. In this paper, we focus on the general elections to the House of Commons as these are the most

important and salient national elections for citizens.

Up until 2011, the Prime Minister of the UK was obliged to call a general election within five years, but was also able to call elections earlier by asking the monarch to dissolve parliament. Since the passing of the *Fixed-term Parliaments Act 2011*—intended to remove the potential advantage for the party in power to call an early election at a favourable moment—the House of Commons is automatically dissolved and a new election is held after a fixed period of five years.

Turnout at general elections was between 70 and 80% from the 1950s to 1997 and has hovered around 65% since the early 2000s. To be eligible to vote in a general election, individuals need to have British citizenship, be resident at an address in the UK, have turned 18 on polling day,¹ and be on the electoral register. People can get on the register in two ways. First, voters can register either online or fill in a paper form. Second, during a process called the annual canvass, the local electoral registration officers are legally required to contact all households each year to check if the details on the electoral register are correct. If individuals move and do not register with the new address, they will be contacted by the local authority—seeking to update their list—in the lead-up to an election. Thus, our focus is on a country with comparatively low institutional barriers to voting.

Data and empirical approach We use British Household Panel Survey / Understanding Society: the UK Household Longitudinal Study (BHPS/UKHLS) data for the empirical analysis ([University of Essex, 2019](#)). The surveys provide information on political variables such as voting behaviour, political interest, party preferences, party membership and whether voting is seen as a social norm. We use the survey waves covering 1991-2018, thus we can examine voting at the general elections of 1992, 1997, 2001, 2005, 2010, 2015 and 2017. A special licence version of the data contains information on the birth month and year, allowing us to determine voting eligibility at each election.

We use a regression discontinuity (RD) design to estimate the long-run effects of past eligibility. Our empirical approach exploits the institutional rule that eligibility is determined by an individual's age on polling day. Intuitively, we compare political

¹The voting age was lowered from 21 to 18 in 1970, but all individuals in our sample were exposed to the lower voting age of 18.

outcomes of individuals who turned 18 just before an election with those of individuals turning 18 just after the same election. The age cut-off generates exogenous variation in the age at which individuals receive the first opportunity to vote. The idea underlying this approach is that individuals around the eligibility cut-off should be identical apart from the difference in the first eligibility to vote.

The key identifying assumption is that individuals cannot manipulate the ‘running variable’, i.e., their age on election day (Imbens and Lemieux, 2008). Given the strict enforcement rules and that eligibility is the product of two independent events 18 years apart—date of birth and the date of election—manipulation is highly unlikely. We provide supplementary evidence that supports this uncontroversial assumption. In Appendix Table A.2 we show the RD estimates of a set of exogenous characteristics around the cut-off. Overall, the variables show a smooth distribution around the cut-off with precisely estimated zero coefficients for 12 of the 13 variables shown—and only one imbalance. In a robustness check, we control for these pre-determined characteristics and show that including them does not affect our conclusions.² We additionally show that there is no bunching of observations on either side of the cut-off (McCrary, 2008, density test in Appendix Figure A.1).

We estimate the following regression model for voting:

$$y_{i,t_0+k} = \beta_0 + \beta_1 I(Z_{i,t_0} > 0) + \beta_2 Z_{i,t_0} + \beta_3 Z_{i,t_0} \cdot I(Z_{i,t_0} > 0) + \gamma_{t_0+k} + \epsilon_{i,t_0+k} \quad (1)$$

where y_{i,t_0+k} denotes the voting outcome of individual i at election t_{0+k} , where $k \in \{0, 1, 2, 3\}$ indicates the election relative to the treatment-defining election around the age cut-off. Thus, as our main voting outcome we examine the effect of earlier eligibility on voting for the initial and three subsequent elections. Z_{i,t_0} denotes the running variable, i.e., the relative age at the initial election t_0 . $I(Z_{i,t_0} > 0)$ takes the value of 1 for individuals eligible to vote at election t_0 , the initial eligibility-determining election, 0 otherwise. To maximise precision of the estimates, we use a symmetric 48-month observation window on either side of the cut-off date (we explore the sensitivity of our estimates to

²Employment, respondent’s education, marital status, and region of residence are not fully pre-determined, but they are unlikely to be endogenous with regard to the treatment assignment. Excluding those characteristics from the estimation yields similar results.

the bandwidth). As we have no information the exact day of birth (only month of birth), we exclude individuals who turn 18 in the month of an election as we cannot determine their eligibility on election day.³ In our baseline specification, we control linearly for the running variable, which captures age-related time trends in the outcomes, and allow for different slopes on both sides of the cut-off. Our coefficient of interest, β_1 , captures any discontinuous jumps in voting outcomes between just-eligibles and just-ineligibles. We include fixed effects for the election for which we analyse voting, γ_{t_0+k} , to capture differences between elections with respect to average turnout, salience, and media attention. For estimation, we use the *rdrobust* package for Stata by [Calonico et al. \(2017\)](#) with bias-corrected standard errors. Several specification checks confirm the robustness of our results.

As questions on political involvement are asked annually, we slightly modify the regression specification and include survey year fixed effects instead of election fixed effects to absorb time-specific differences in these outcomes. In contrast to voting, for political involvement we can also consider upstream effects and $k \in \{-1, 0, 1, 2, 3\}$.

As we use panel survey data, which carry the advantage of being able to examine a broad set of voting and non-voting outcomes for the same individual over time, this data source also generates possible limitations that we address carefully in our analysis. For instance, turnout may be misreported in survey data; though this is a potential issue, random misreporting at the eligibility cut-off would not impose any bias on our estimated treatment effects and only affect precision of the estimates. Moreover, we observe increasing voting propensities over the life cycle, indicating that our voting measures do validly capture previous turnout. Second, our empirical approach assumes that people do not selectively decide whether to answer the survey and—conditional on answering—to respond to all or only part of the questions. This could be an issue if individuals who turned 18 just before the election were more likely than those who turned 18 just after to answer the questions. We can test directly for this and find that this is not an issue in our setting, as non-response is found to be distributed smoothly around the cut-off.

In the period we study, UK voters could also vote in subnational, European, and local elections. To ensure that our results are not confounded by European or subnational

³As month of birth approximately follows a uniform distribution, in our preferred 48-months window we thus exclude around 1% of observations through this restriction.

elections, in a robustness check we exclude individuals within a six-months window on each side of the eligibility cut-off date for European elections and also restrict the sample to individuals residing in England. We find no evidence that subnational and EP elections affect our results (see Appendix Figure A.2).

4 Results

4.1 Voting

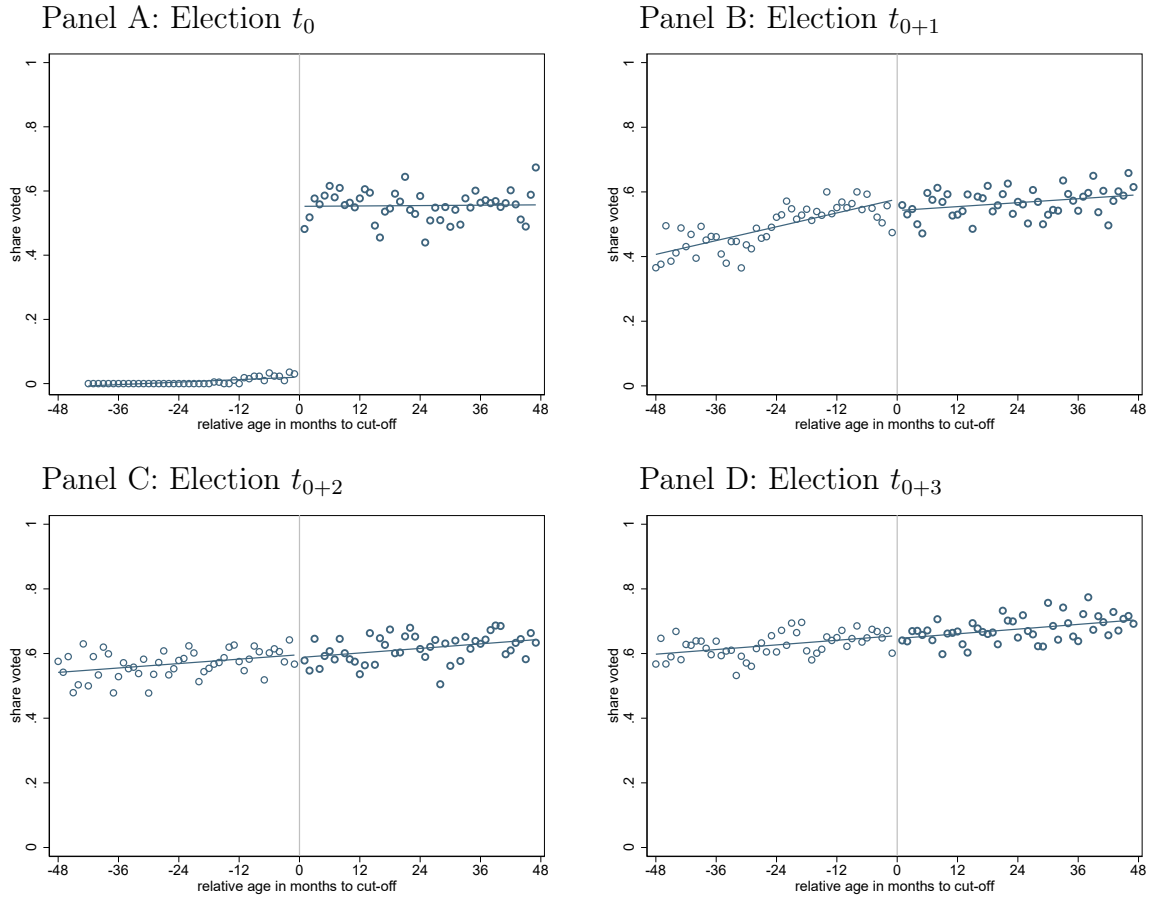
We begin our empirical analysis by examining visually how just becoming eligible to vote affects first time voting, and how past eligibility affects average turnout for three subsequent elections. Figure 1 presents RD plots, with each panel showing the reduced form relationship between the running variable, relative age at election t_0 , and turnout at these different elections. We plot the average voting shares for each monthly bin of relative age and fit linear trends separately for each side of the cut-off. Table 1 contains the corresponding RD estimates and bias-corrected standard errors in the figure for our main specification with linear age trends.

Panel A clearly shows that having passed the age threshold of 18 affects turnout for the newly eligibles: We observe a discontinuous jump in the probability to vote of around 53.4 percentage points (pp) between individuals who turned 18 just before the election date (to the right of the vertical axis) and individuals who turned 18 just after the election. To the left of the cut-off, 1.1% of just-ineligibles report having voted in this election; this small share is likely due to reporting errors as these individuals were legally not eligible to vote. When we manually set the voting outcomes to 0 for the ineligible, we estimate a jump in turnout of 56.4 pp.⁴

Does past voting eligibility then have an effect on subsequent turnout? Panels B to D in Figure 1 show the RD plots for three subsequent elections (columns (2)-(4) in Table 1). If voting was habit-forming, we would expect that individuals who had the opportunity to vote earlier would exhibit higher turnout at subsequent elections. However, our results provide precisely estimated evidence that past eligibility does not generate any long-run effects on voting. The outcomes trend smoothly on each side of the cut-off and, consistent

⁴Appendix Figure A.3 plots the first *observed* voting age against birth months. Reassuringly, we observe a pronounced discontinuity in the first observed voting age at each election cut-off date.

Figure 1: Relationship between relative age at election t_0 and turnout at subsequent elections



Note: Dots represent average voting shares for individuals born in a certain month relative to the cut-off date; at election t_0 , individuals born to the right of the cut-off have the right to vote. We pool information on all available elections. See Table 1 for RD coefficients and sample sizes.

Source: BHPS / UKHLS, own calculations.

with prior studies, the Figure also indicate an initial increasing propensity to vote as individuals get older (e.g., see Phelps, 2004; Bhatti et al., 2012). The point estimates presented in columns (2)-(4) Table 1 are all close to zero and statistically insignificant. The largest negative point estimate is for t_{0+1} , which would be consistent with a ‘first-time hype’ of previous ineligibles.

In column (5) we pool all subsequent elections, i.e., t_{0+1} to t_{0+3} , to maximise precision and to provide a summary effect of earlier eligibility on subsequent voting. In the pooled estimation we identify a statistically significant *negative* effect of 2.1 pp on future turnout, but we show in subsection 4.3 that this coefficient is sensitive to the functional form and choice of bandwidth. More importantly, we can strongly refute a positive effect on voting

Table 1: Relative age at election t_0 and turnout

Election relative to age cut-off	t_0	t_{0+1}	t_{0+2}	t_{0+3}	t_{0+1} - t_{0+3} pooled
	(1)	(2)	(3)	(4)	(5)
RD estimate	0.534*** (0.012)	-0.025 (0.017)	-0.016 (0.016)	-0.011 (0.015)	-0.021** (0.009)
N	15,598	18,017	18,525	20,034	56,411

Note: Table shows the relationship between relative age at election t_0 and voting at the UK general election. Election cycle t_0 denotes the period between the first election where individuals are around the eligibility cut-off and the next election, t_{0+1} the election cycle thereafter etc. All estimates include election fixed effects. We use a symmetric 48 month bandwidth and the running variable is controlled for linearly. No individual characteristics are included. See Figure 2 for other specifications. Estimation following Calónico et al. (2017) with bias corrected standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: BHPS and UKHLS, own calculations.

through habit formation in our setting; past eligibility does not affect aggregate long-run voting behaviour. In a supplementary analysis, we show that these results persist when considering up to six subsequent elections (see Appendix Figure A.4 which contains the RD plots and estimated coefficients).⁵

One concern that arises in survey data—in contrast to register data—is that selective non-response from individuals could bias the estimates, as, e.g., individuals who have just cleared the threshold are more willing to answer questions on voting behaviour. However, only 0.93% of survey participants refuse to provide an answer for voting and we find no evidence that this share differs discontinuously at the age threshold at election t_0 (p-value of 0.981) or at any of the three subsequent elections (p-values of 0.938, 0.358 and 0.886, for t_{0+1} , t_{0+2} and t_{0+3} , respectively, and 0.619 for the pooled estimation). Thus, selective non-response is not an issue in our context.

⁵Though we generally find null effects for all subsequent elections, we do not include these results in our main analysis as we estimate a small positive and statistically significant coefficient for the election t_{0+5} . Upon further investigation, we found out that this estimate is entirely driven by the election in 2005, i.e., for voters who were at the eligibility threshold in 1983. When dropping only the 2005 election, the long-run effects at t_{0+5} are zero. Further, regardless of including the 2005 elections or not, the summary effects for the periods t_{0+1} to t_{0+6} are a precisely estimated zero.

As elections differ in their salience, which may affect both election turnout and long-run effects, we also examine the downstream voting effects of each individual election in Appendix Table A.3. The table shows the RD estimates from Table 1 *by* treatment-defining election; for instance, the third row refers to the effect of being just eligible to vote in the 1992 election. The first estimate is the effect on turnout in that election (t_0), column (4) considers turnout in the 2005 election (t_{0+3}). Three key findings emerge. First, being newly eligible to vote always affects turnout at the first election, with the effect ranging from 37 pp (2005) to 69 pp (1992 and 1997). Second, we find no consistent evidence for a first-time hype effect, as evidenced by the opposite signs for t_{0+1} for the elections in 1987 and 1992 and the zero coefficients for the remaining elections. Third, we find no long-run effect of past eligibility, in particular not for elections with larger first-time turnout.

4.2 Political involvement

Next, we examine whether earlier voting eligibility affects political involvement over the next three election cycles, i.e., up to 15 years after the treatment defining election. Similar to previous studies examining political interest (Holbein et al., 2021), we focus on the following outcomes: party identification, defined as supporting or feeling closer to one political party; political interest, coded as whether individuals have high or very high interest in politics; whether voting is seen as a social norm—a proxy for civic duty (Blais, 2000; Gerber and Rogers, 2009); party preference, measured as whether an individual states a party she will vote for; and membership of a political party as a stronger indicator for political participation and interest.

To avoid issues associated with multiple hypothesis testing, we create an index of political involvement and use this index as our main outcome. We proceed as follows to construct the index: we first standardise ($Z \sim \mathcal{N}(0, 1)$) each outcome and then sum up the individual scores. As the variables are not always covered jointly, we divide the sum by the number of variables contributing to the index; in a final step, we standardise again. Besides the joint index, we also show results for the individual measures, which we code as indicator variables for ease of interpretation. For the estimation of the effect in the election cycle t_0 (after the election where individuals are around the cut-off) we only use observations from the election year if the interview date took place after the election.

In contrast to voting, where those younger than 18 are strictly ineligible, for political preferences we can also estimate ‘upstream effects’ of future eligibility; in anticipation of being able to vote in the upcoming election, political involvement may increase beforehand. For these estimates, i.e., in t_{0-1} , we thus consider individuals observed before the treatment defining election.

We present the RD coefficients of eligibility on political preferences in Table 2 for each outcome over time.⁶ We do not find any evidence for upstream effects at t_{0-1} , neither for the average index nor for the individual indicators of political involvement. A different picture emerges once some individuals become eligible to vote for the first time: we now find some substantial and significant effects in the short-run at t_0 . Not only did many use the chance to vote (as shown in the previous subsection), but we also observe a simultaneous and pronounced increase in political involvement. For just-eligibles, the index of political preferences increases by 0.11 of a standard deviation. We also estimate higher levels of party identification, political interest, perceptions of voting as a social norm, and party preference.⁷ Only for the most stringent measure of political involvement, party membership, do we see no effect.

These effects on political involvement could be consistent with the notion that voting is habit-forming or even transformative—but only if these effects persist in the long-run. We present the results for the long run in columns (3) to (6). These, however, document that the short-term effects fade away quickly as we observe small and mostly statistically insignificant coefficients in future election cycles. To maximise precision, we pool all downstream periods in column (6) and estimate a precisely estimated zero for all outcomes.

Taken together, our results show that past eligibility indeed increases individuals’ initial connection to and interest in politics, but that these effects are altogether short-lived. Thus, we find no evidence that voting leads to long-term habit formation or constitutes a transformative event.

⁶For the corresponding reduced form graphs, see Appendix Figures A.5-A.10.

⁷Looking at effects of favouring a specific party, we find slightly less support for the Conservative Party in the long-run, see Appendix Table A.4.

Table 2: Relationship between relative age at election t_0 and political involvement

Election cycle	Upstream		Downstream			
	t_{0-1}	t_0	t_{0+1}	t_{0+2}	t_{0+3}	$t_{0+1}-t_{0+3}$ pooled
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Index of political involvement</u>						
RD estimate	0.025 (0.038)	0.110*** (0.022)	0.019 (0.018)	0.020 (0.017)	-0.012 (0.016)	0.015 (0.010)
N	14,162	45,070	58,626	64,587	67,071	189,623
<u>Supporting a political party or close to one party</u>						
RD estimate	0.002 (0.016)	0.038*** (0.010)	0.002 (0.008)	0.005 (0.008)	-0.006 (0.008)	0.003 (0.005)
N	14,085	44,950	58,465	64,408	66,911	189,123
<u>Interested in politics</u>						
RD estimate	0.010 (0.020)	0.033*** (0.010)	0.008 (0.009)	0.017* (0.009)	-0.007 (0.008)	0.008 (0.005)
N	11,262	38,249	49,857	54,230	54,624	158,093
<u>Agree that voting is a social norm</u>						
RD estimate	0.000 (0.047)	0.060** (0.025)	0.011 (0.021)	-0.001 (0.021)	-0.027 (0.020)	-0.005 (0.012)
N	1,971	7,663	9,230	10,051	10,500	29,687
<u>States a party to vote for</u>						
RD estimate	0.017 (0.028)	0.025* (0.014)	0.020* (0.011)	-0.017 (0.011)	-0.020* (0.011)	-0.005 (0.006)
N	4,984	22,768	29,799	31,133	31,173	91,814
<u>Member of a political party</u>						
RD estimate	-0.004 (0.008)	-0.006 (0.005)	-0.004 (0.004)	-0.006 (0.004)	-0.000 (0.004)	-0.003 (0.002)
N	2,753	15,207	19,407	21,345	22,358	62,893

Note: Table shows the relationship between relative age at election t_0 and a range of political preferences. See text for details on the construction of the index. Election cycle t_0 denotes the period between the first election where individuals are around the eligibility cut-off and the next election, t_{0+1} the election cycle thereafter etc. Election t_{0-1} indicates the period before the treatment defining election. All estimates include survey year fixed effects. Dependent variables are covered over the period 1991-2019 with the exception of voting as a social norm which has only been included since 2010. Bias corrected standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: BHPS and UKHLS, own calculations.

4.3 Robustness

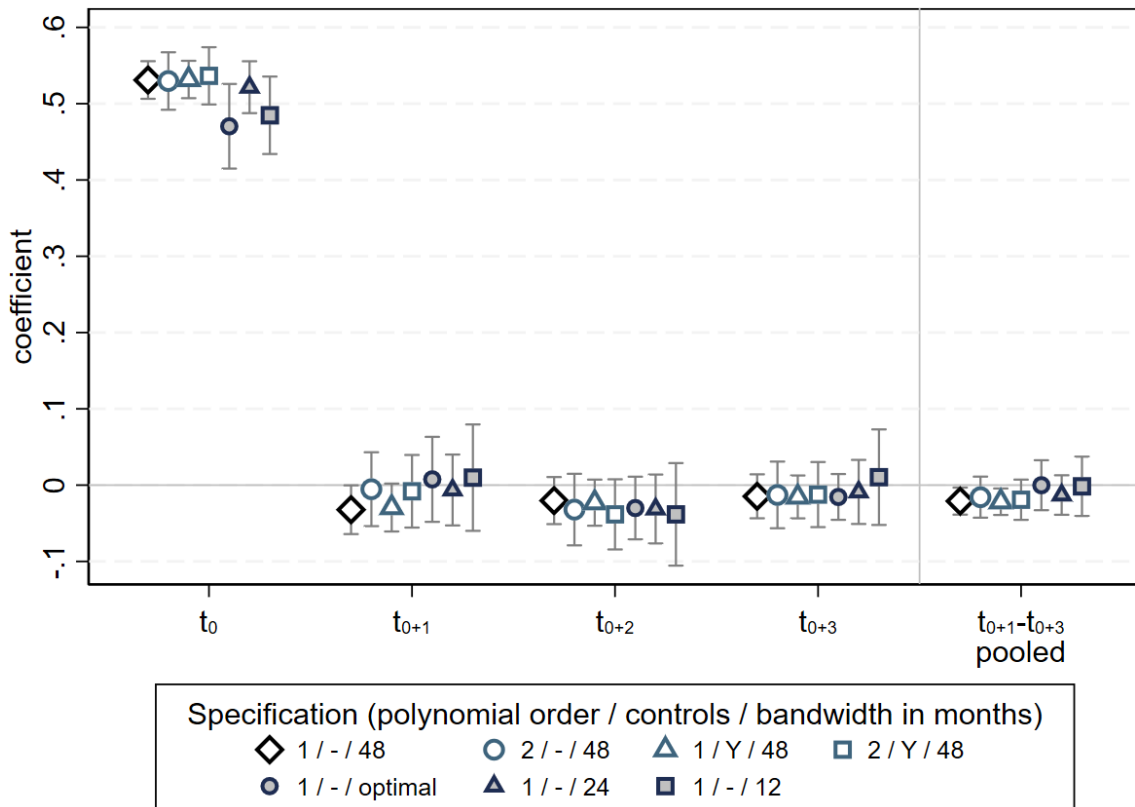
RD estimates often rely heavily on their functional form or choice of bandwidth, making it crucial to assess robustness of the coefficients (Lee and Lemieux, 2010). In this subsection, we provide a range of different specifications to alleviate those concerns. Specifically, we contrast our main estimates to six other specifications. First, we specify the running variable quadratically to identify whether potential non-linearities could be driving our effects.⁸ Second, we add exogenous control variables to our estimation. While the inclusion of control variables in our setting is not theoretically well-grounded, this could reduce the variance of estimates (Frölich and Huber, 2019) and also allows controlling for small idiosyncratic imbalances in these variables. Finally, we vary the bandwidth. We first show estimates using the ‘optimal’ bandwidth from Calonico et al. (2020), and then narrower symmetric bandwidths of 24 and 12 months, respectively. By nature, the size of the bandwidth is a trade-off between precision and reliance on the functional form.

Figure 2 shows RD estimates for voting. Overall, the estimated coefficients are stable across specification. Though we do observe that the estimates at t_0 decline when using the optimal bandwidth, the confidence intervals always include the main estimates. In addition, all confidence intervals for the estimates in t_{0+1} to t_{0+3} include zero. Thus, these robustness checks do not confirm a ‘first-time hype’ effect, and overall we can firmly refute the hypothesis that voting in the UK is habit-forming. The pooled estimates also suggest that effects are likely to be very close to zero.

For political involvement, we focus on estimates in t_0 and the pooled estimates over the election cycles t_{0+1} to t_{0+3} . Panel A of Figure 3 shows coefficients for the period after the first election. The main finding of an increase in political involvement, defined either through the joint index or the individual measures, is confirmed; effects are neither driven by the choice of functional form, individual characteristics, nor the choice of bandwidth. Some estimates are—as expected—less precise, but the point estimates remain stable overall. Panel B shows the pooled estimates and similarly confirms our main finding that earlier eligibility does not lead to a long-term increase in political involvement.

⁸Following Gelman and Imbens (2019) we do not consider higher-order polynomials.

Figure 2: RD specifications - effect of earlier eligibility on subsequent elections



Note: Figure shows coefficients and 95% confidence intervals for different specifications. The first specification (linear, no controls, 48 months bandwidth) corresponds to the RD estimate shown in the Table 1. Control variables are all variables shown in Appendix Table A.2. The optimal bandwidth (fifth estimate) is obtained following Calonico et al. (2020), in all other specifications bandwidths are symmetric. All estimates include election fixed effects.

Source: BHPS and UKHLS, own calculations.

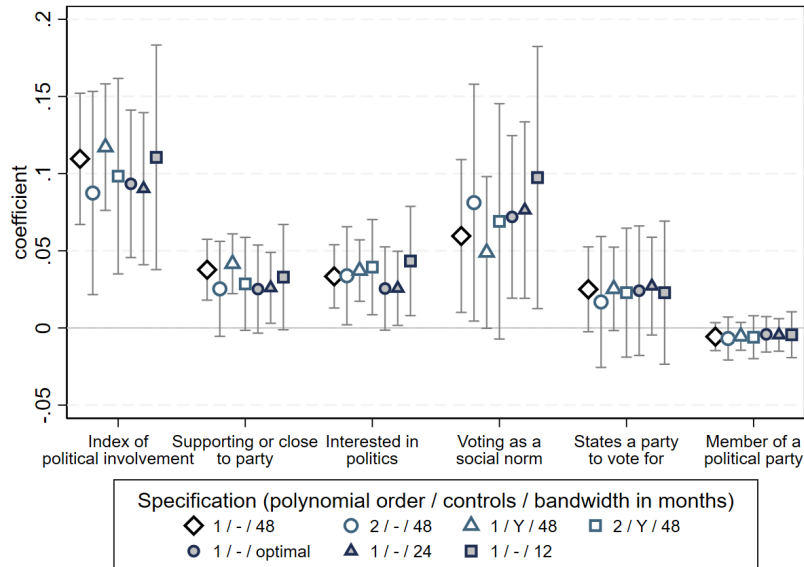
5 Conclusion

Past research, particularly for the US, has shown that voting is habit-forming and transformative, in the sense that earlier voting affects future turnout and political involvement. In contrast, recent studies applying causal research designs from other contexts do not confirm the US findings, instead reporting null effects. However, one common and major shortcoming of these studies is the focus on short-run outcomes, both for voting and political involvement, thus providing little evidence on the potentially ‘transformative’ effect of voting.

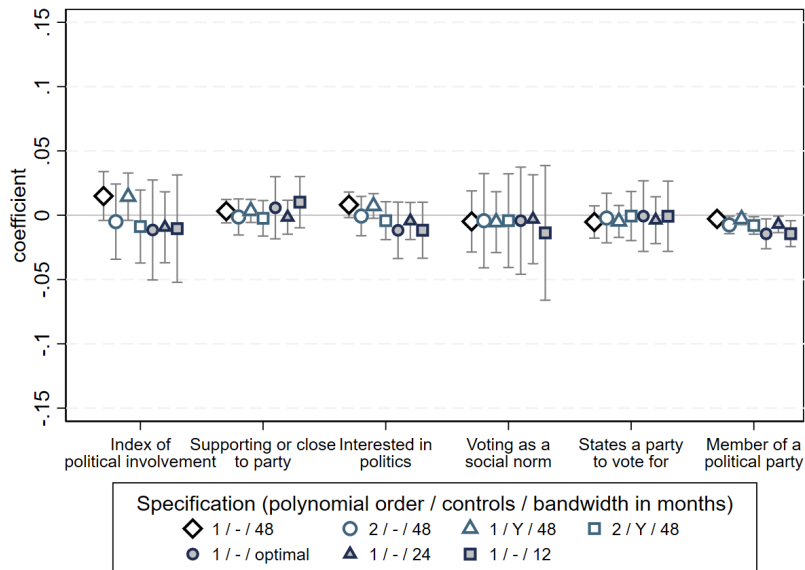
Our paper adds to this important and long-standing debate by providing new causal

Figure 3: RD specifications - effect of earlier eligibility on political attitudes

Panel A: Election cycle t_0



Panel B: Election cycles $t_{0+1}-t_{0+3}$ pooled



Note: Figure shows coefficients and 95% confidence intervals for different specifications. The first specification (linear, no controls, 48 months bandwidth) corresponds to the RD estimate shown in Table 2. Control variables are all variables shown in Appendix Table A.2. The optimal bandwidth (fifth estimate) is obtained following Calonico et al. (2020), in all other specifications bandwidths are symmetric. All estimates include survey year fixed effects.

Source: BHPS and UKHLS, own calculations.

evidence on the short- and long-run effects of earlier voting on turnout and involvement within a unified framework. We jointly examine the upstream and long-run downstream

effects of earlier voting eligibility on voting and political involvement, up to three elections after the first vote. This allows us to investigate the potential mechanisms through which past voting may have a truly transformative, lasting effect on individuals.

We make two additional important contributions to the literature. First, our data covers seven UK general elections, allowing us to examine whether specific election characteristics moderate the habit-forming, transformative effects of voting. Second, we provide evidence for a new context, the UK, which has low institutional barriers to voting compared to the US.

Methodologically, we apply an RD which exploits the institutional rule that voting eligibility is determined by an individual’s age on polling day. We use individual-level survey data from the UK, which is representative for the entire UK, covers both registered and unregistered individuals, and provides a large sample size, particularly for the non-voting outcomes.

We first show that earlier eligibility has an effect on turnout at the first election. We next document a lack of long-run effects of earlier eligibility on voting, leading us to conclude that voting is not habit-forming in the UK. This null effect largely holds across six elections from 1992 to 2017. As such, our results extend recent work ([Bhatti et al., 2016](#); [Bechtel et al., 2018](#); [Hernæs, 2019](#)) on the non-existence of a habit of voting outside the US in the short-term, and provide strong evidence that this null effect holds across different types of first elections. Since we apply a similar research design as [Coppock and Green \(2016\)](#), the most likely explanation for the differences between the US and our findings are institutional differences. In the UK, the process of voter registration and voting itself is less restrictive than in the US, where some states follow restrictive policies with respect to state voter ID laws, automatic voter registration, and early and mail-in voting (see, e.g., [Schraufnagel et al., 2020](#)).

In the second part of our paper, we provide evidence that voting only has a temporary, short-term impact on several measures of political involvement. Specifically, we examine how earlier eligibility affects an index of political involvements (based on political interest, party identification, party membership and seeing voting as a social norm) in the two years preceding the first election, the year of the election, and for the three subsequent election cycles. We find no evidence for upstream effects, a short-term increase in the first election cycle, and precisely estimated zero coefficients for the subsequent cycles. Our

study complements and extends [Holbein and Rangel \(2020\)](#) and [Holbein et al. \(2021\)](#), who focus on short-term outcomes. Like theirs, our results also question whether voting is transformative, making people better citizens in the long-run ([Mansbridge, 1999](#)). Our findings are consistent with the interpretation that voting generates a first-time hype effect ([Bhatti et al., 2016](#)), but this effect wears off by the time of the next election.

Finally, the null findings we report in this paper are normatively relevant. In general, the lack of persistence in turnout behaviour and the lack of negative downstream effects of abstention are reassuring: the long-term effects of low-turnout elections are less severe than often feared. Our findings also mean that discussions about lowering the voting age should not be framed instrumentally around the long-term impact of higher initial turnout rates. While voters under 18 may indeed turn out at higher rates than those over 18 ([Aichholzer and Kritzinger, 2020](#)), this may not establish habits of voting among this cohort. Since instilling a habit of turnout may not be as straightforward as expected, lowering the voting age needs to be justified using other arguments.

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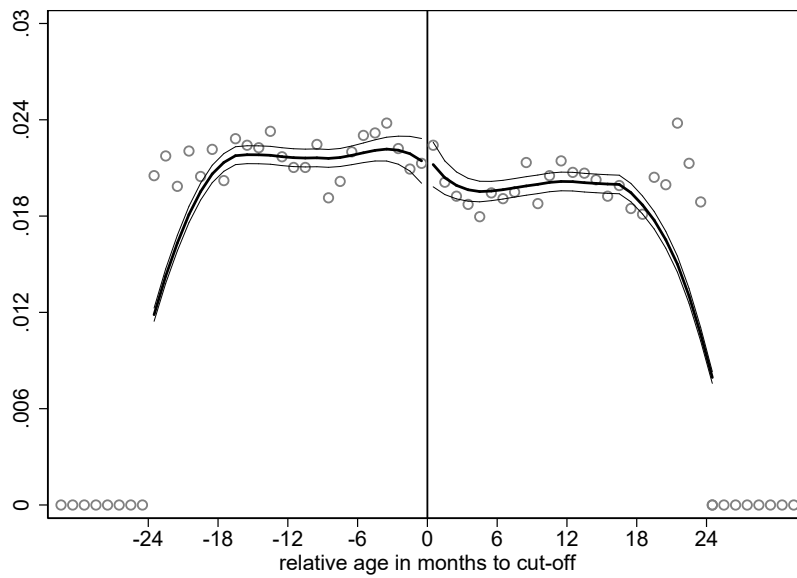
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Online Appendix A: Supplementary material

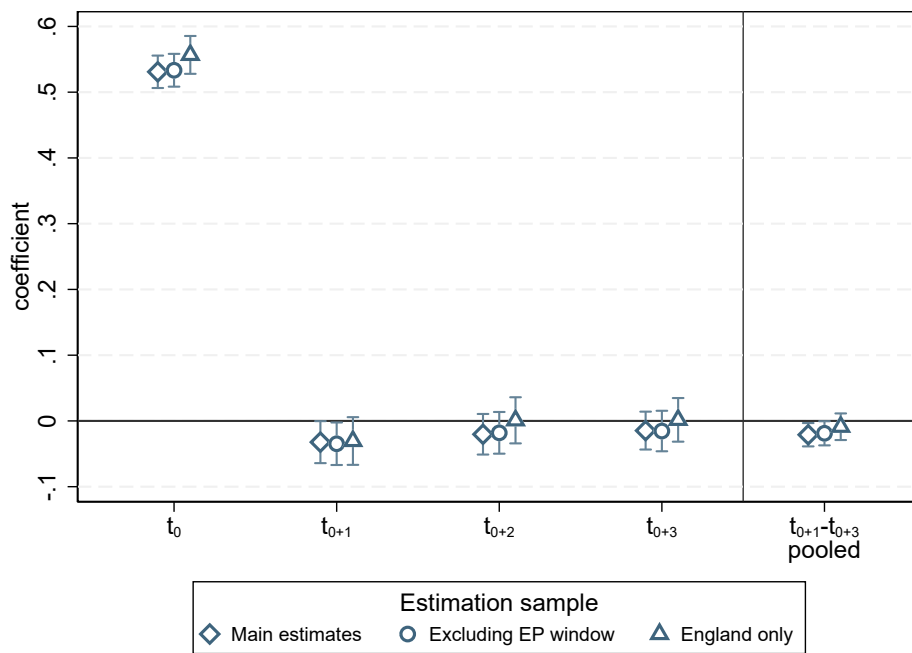
Figures

Figure A.1: Manipulation test of running variable



Note: Figure shows a [McCrary \(2008\)](#) density test of observations around the cut-off of the running variable (relative age in months). T-statistic of density test is 0.423. *Source:* BHPS and UKHLS, own calculations.

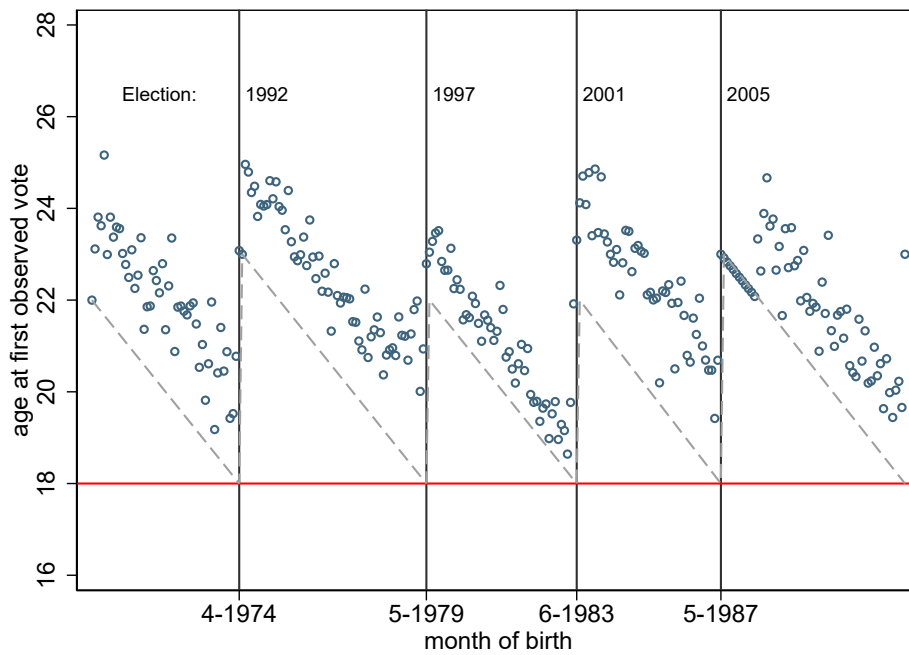
Figure A.2: Robustness of voting coefficients to European and subnational elections



Note: Figure shows robustness checks for voting at the 'initial' and subsequent elections. Main estimates are shown in Figure 2. The first robustness check excludes all individuals who were within six months of the age cut-off relative to an election for the European Parliament (EP). The second set of estimates is restricted to individuals residing in England to avoid subnational elections in Wales, Scotland and Northern Ireland biasing the results.

Source: BHPS and UKHLS, own calculations.

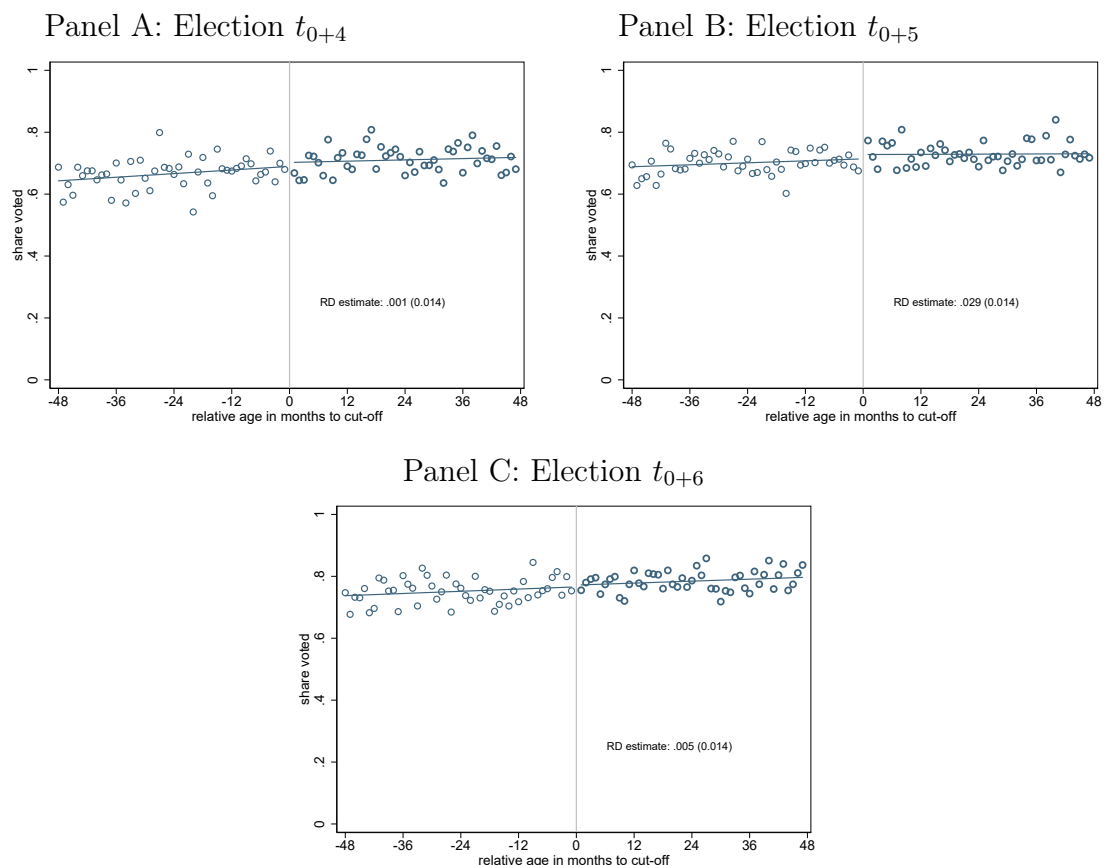
Figure A.3: Month of birth and age at first observed vote



Note: Figure shows age at first observed vote by month of birth. For this analysis, make two additional sampling restrictions to calculate the first observed voting age more reliably. We reduce the observation window to two years around each cut-off to avoid using the same individual twice (once on each side of the cut-off). Second, we restrict the sample for each election to individuals aged up to 27 years, which corresponds to two electoral cycles (4+5 years). This restriction reduces the problem of not observing the first vote in the data if individuals join the sample after their first vote. $N = 4912$.

Source: BHPS and UKHLS, own calculations.

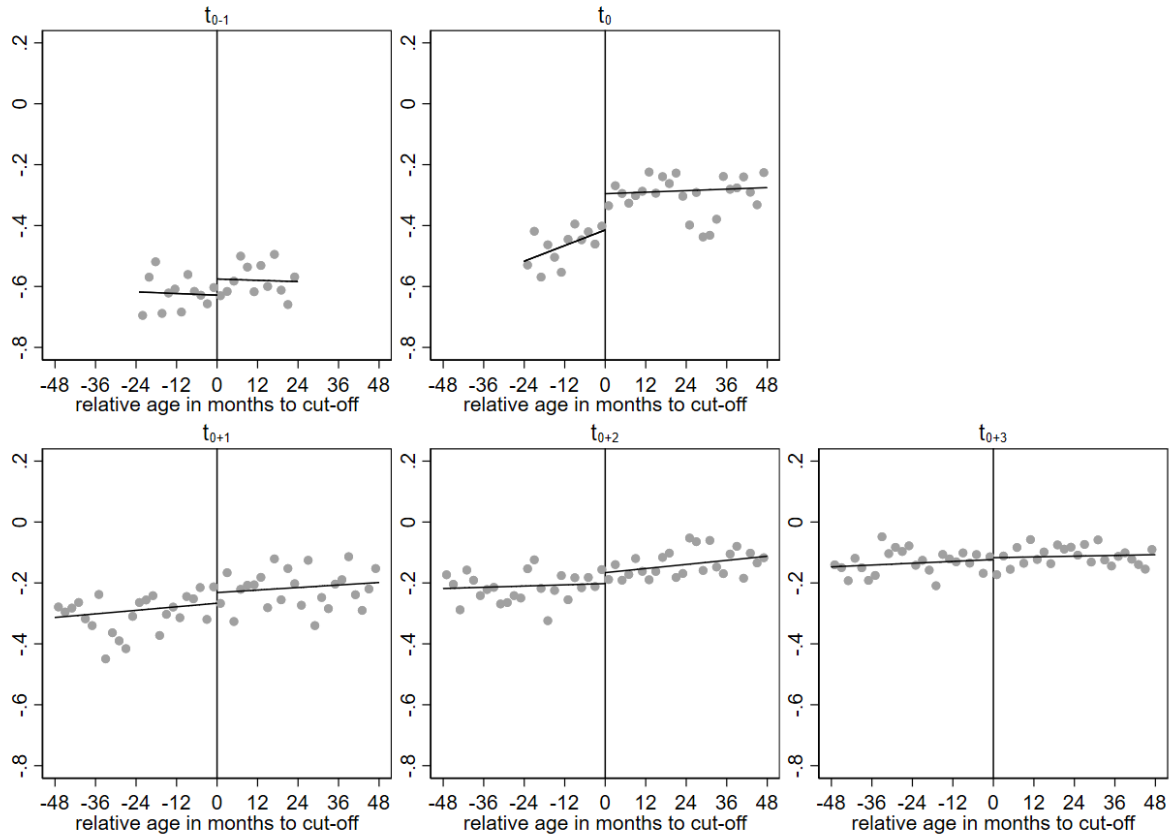
Figure A.4: Relationship between relative age at election t_0 and turnout at subsequent elections



Note: This figure shows the relationship between the running variable, relative age at election t_0 , and average turnout at different subsequent elections. The dots represent the average voting shares for individuals born in a certain month relative to the cut-off date; at election t_0 , individuals born to the right of the cut-off have the right to vote. We exclude observations who turn 18 in the month of each election as we cannot assign eligibility for those individuals cleanly. We pool information on all available elections (1992, 1997, 2001, 2005, 2010, 2015 and 2017). As two elections took place in 1974, the cut-off date cannot be cleanly assigned in our framework, thus we exclude elections for which t_0 is 1974; for voting in t_{0+4} this is the 1992 election, for t_{0+5} 1997 and for t_{0+6} the 2001 election. RD estimates include election fixed effects. Standard errors shown in parentheses. Respective $N = 19862, 18405, 16126$.

Source: BHPS and UKHLS, own calculations.

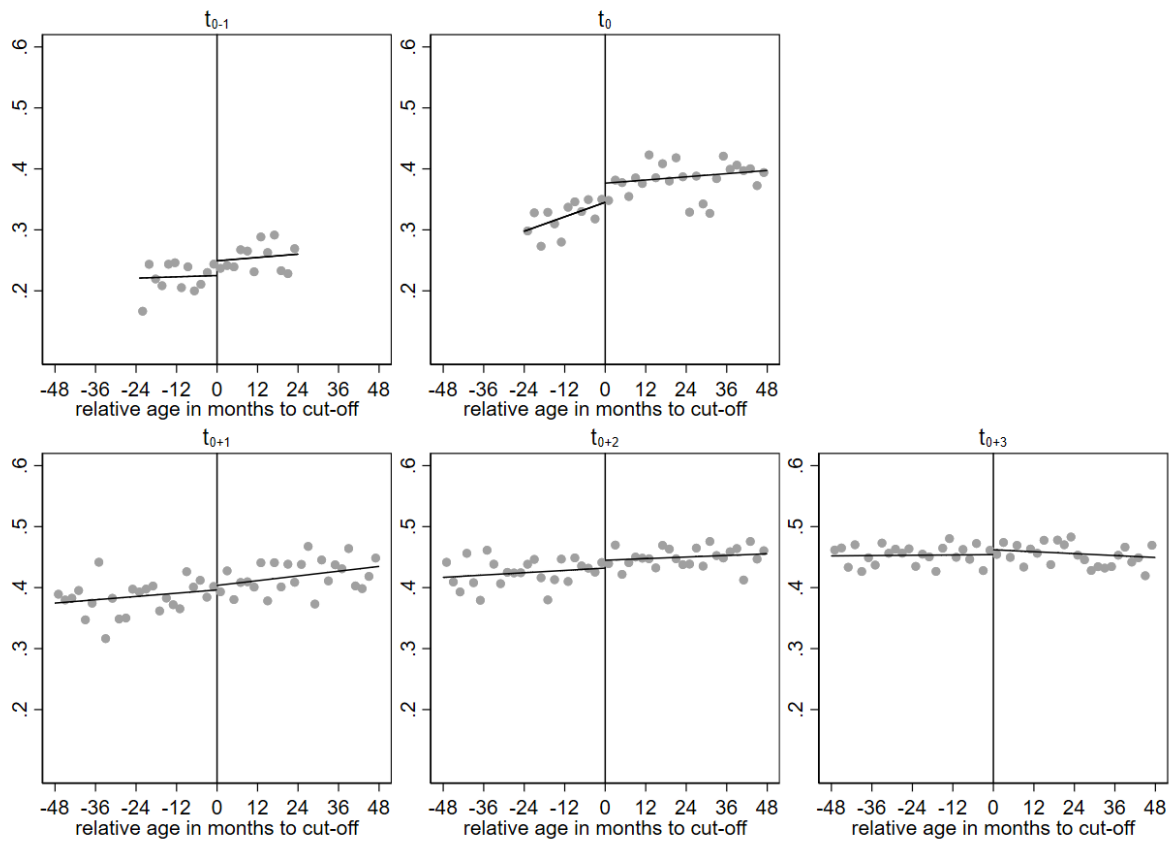
Figure A.5: Index of political involvement



Note: Figure shows the relationship between relative age at election t_0 and index of political involvement (see text). Election cycle t_0 denotes the period between the first election where individuals are around the eligibility cut-off and the next election, t_{0+1} the election cycle thereafter etc. Election t_{0-1} indicates the period before the treatment defining election. Figure shows raw data, i.e., survey year fixed effects as in Table 2 are not partialled out.

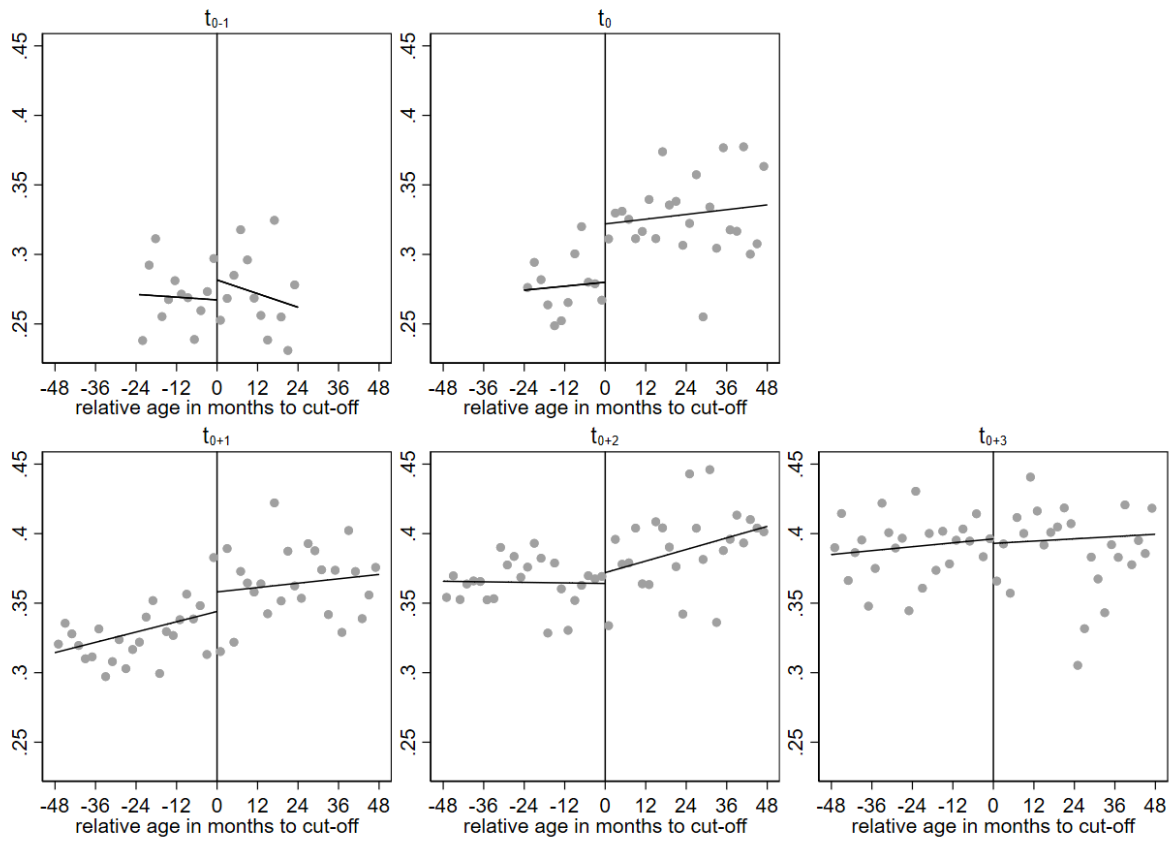
Source: BHPS and UKHLS, own calculations.

Figure A.6: Supporting a political party or close to one party



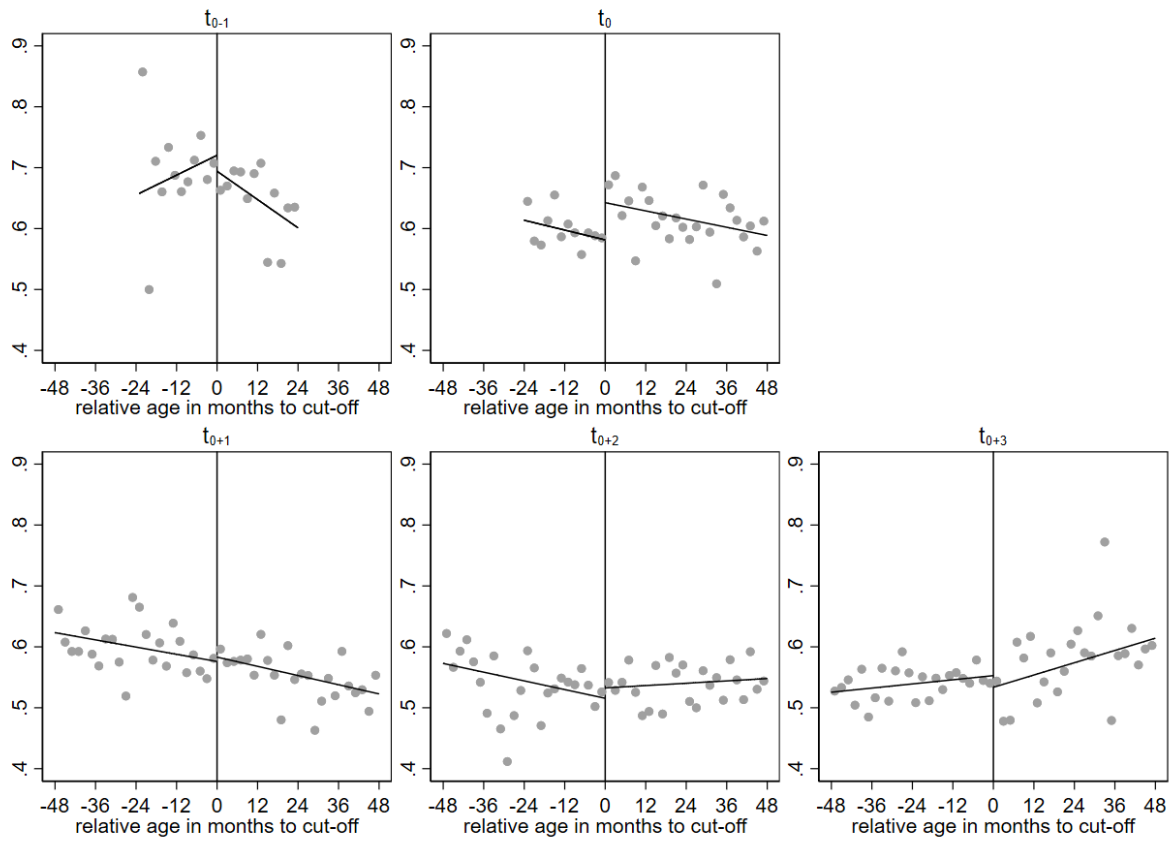
See notes for Figure A.5.

Figure A.7: Interested in politics



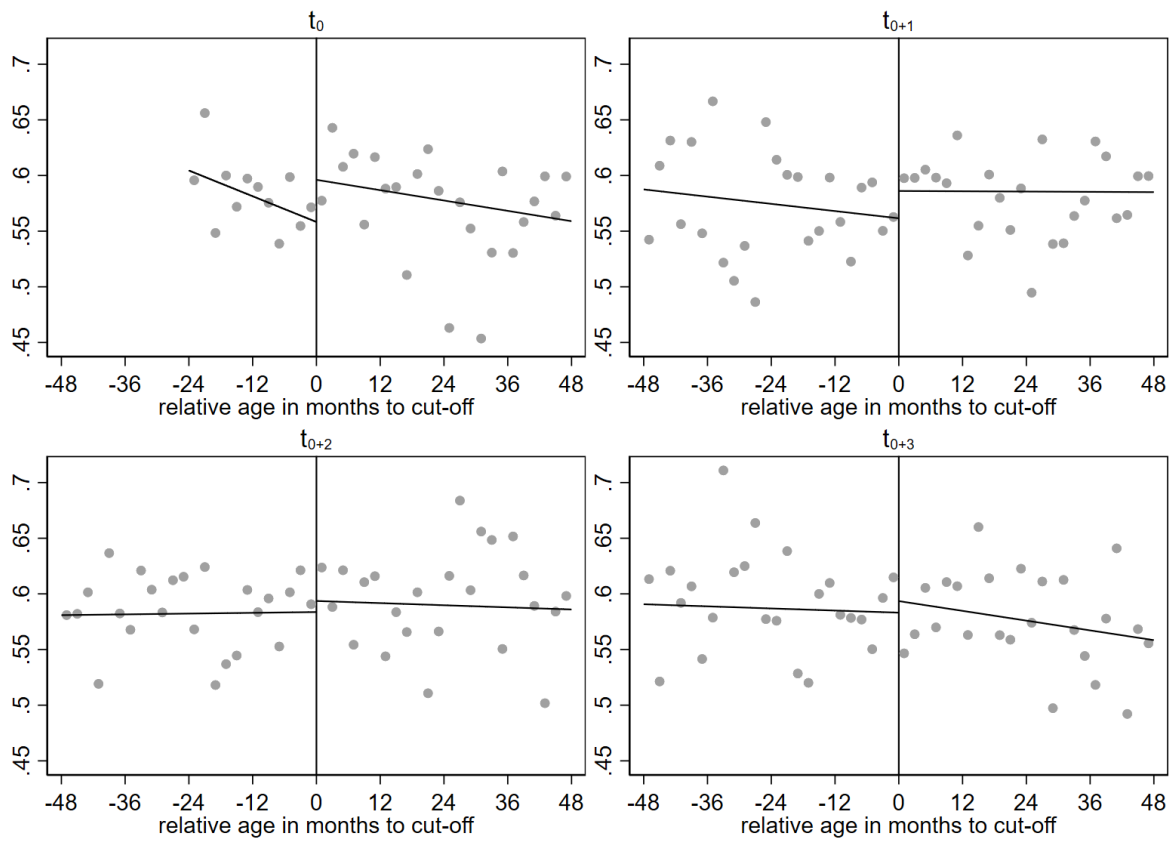
See notes for Figure A.5.

Figure A.8: Agree that voting is a social norm



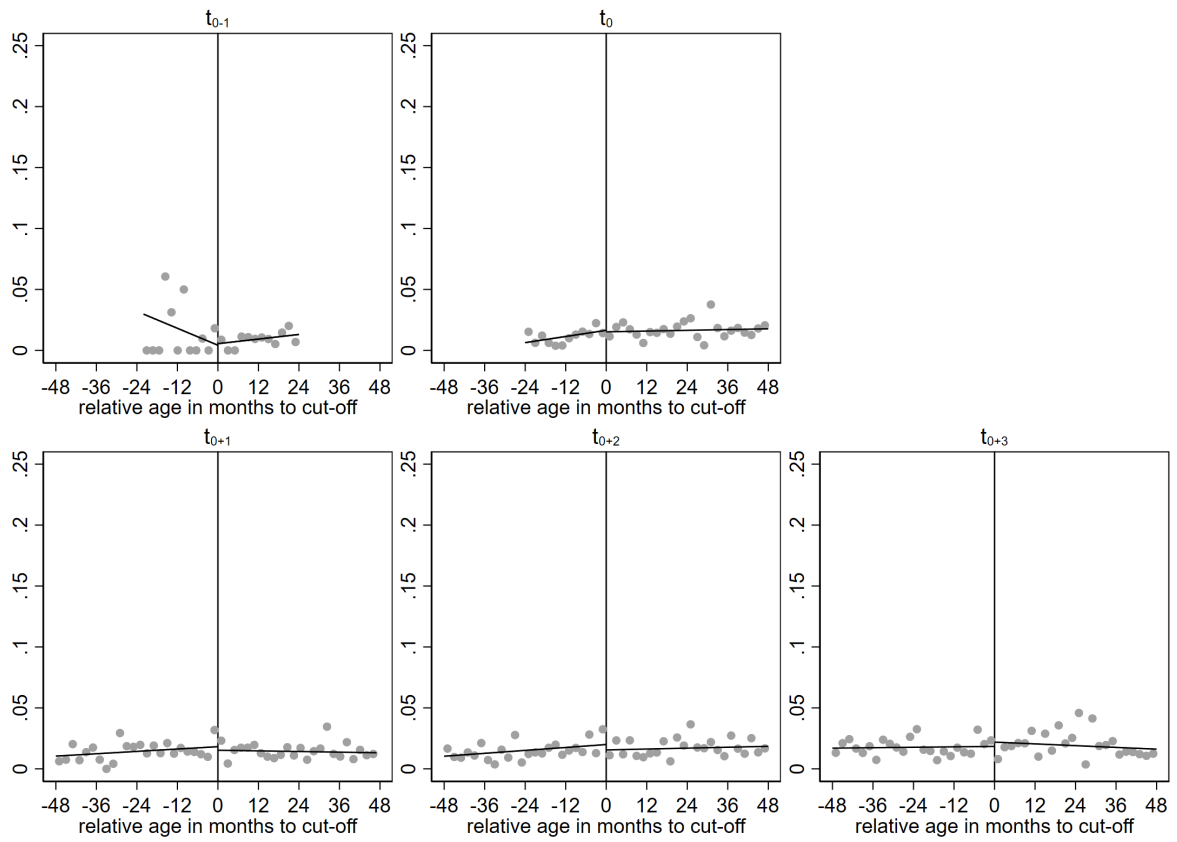
See notes for Figure A.5.

Figure A.9: States a party to vote for



See notes for Figure A.5.

Figure A.10: Member of a political party



See notes for Figure A.5.

Tables

Table A.1: Literature summary - causal effects of earlier voting eligibility on subsequent turnout

Study	Treatment	Empirical Strategy	Country	Sample and Data Source	Analytic N	Findings	Outcome window
Meredith (2009)	Eligibility discontinuity	IV/Difference-in-Differences	USA	California Statewide Voter File (CSVF)	Up to 65,412 (N not shown for individual regressions)	Positive effect for 2000/2004, but long-run effects mostly insignificant	Main analysis subsequent election, but up to 1988 to 2004 (4 elections later)
Dinas (2012)	Eligibility discontinuity	RDD	USA	Youth-Parent Socialization Panel Study	790	Positive effect	Subsequent election
Bhatti et al. (2016)	Eligibility discontinuity	RDD	Denmark and Finland	Voting records	2,336,760	Negative or zero effect	Subsequent election
Coppock and Green (2016)	Eligibility discontinuities from 17 U.S. States	RDD	USA	Voter files in 17 U.S. states	not reported (Tables 4-6)	Positive effect on turnout	Up to 5 presidential elections later (1992-2012)
de Kadt (2017)	Eligibility discontinuity	RDD	South Africa	Human Sciences Research Council (HSRC)	43,948	Positive effect	15 years later (4 elections)
Kaplan et al. (2019)	Plebiscite eligibility	RDD	Chile	Individual-level voting data (SERVEL)	13,393,246	Positive effects	5 elections later (30 years)

Continued on next page

Table A.1 – continued from previous page

Study	Treatment	Empirical Strategy	Country	Sample and Data Source	Analytic N	Findings	Outcome window
Khoban (2019)	Eligibility discontinuity	RDD	Chile, Czech Republic, Denmark, Estonia, Latvia, Poland, Portugal, Slovenia, Sweden, and Switzerland	Civic education study (CivED) covering upper-secondary students aged 17-19	14,000	Positive effect on extensive (non-electoral) forms of political participation	Shortly after first election

Source: Own compilation.

Table A.2: Balancing of covariates

	RD coefficient (1)	SE (2)	Observations (3)
Female	-0.006	0.012	37037
Born abroad	-0.001	0.007	33183
Mother worked when respondent was 14	-0.008	0.015	21792
Mother with low education	-0.036***	0.013	22251
Ethnicity: British or Irish White	-0.004	0.009	36758
Employed	0.002	0.011	36995
Respondent with low education	0.011	0.011	36060
Married or in civil union	0.007	0.010	36866
Urban	0.012	0.013	19604
Region: England	0.005	0.009	37037
Region: Scotland	-0.002	0.006	37037
Region: Wales	0.001	0.006	37037
Region: Northern Ireland	-0.005	0.006	37037

Note: Table shows balancing of exogenous covariates around the relative age cut-off at election t_0 . All variables are coded as binary indicators. RD estimates are obtained using a linear polynomial for the running variable and a symmetric 24 months bandwidth. Standard errors in parentheses. Bias corrected standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: BHPS and UKHLS, own calculations.

Table A.3: Relationship between relative age at election t_0 and turnout by election

Election of vote:	t_0	t_{0+1}	t_{0+2}	t_{0+3}
	(1)	(2)	(3)	(4)
Election at t_0				
RD estimate 1983			0.028 (0.049)	0.059* (0.034)
N			1,514	3,299
RD estimate 1987		-0.122** (0.056)	0.055 (0.037)	-0.037 (0.032)
N		1,376	3,108	4,993
RD estimate 1992	0.694*** (0.041)	0.098** (0.041)	-0.002 (0.034)	-0.045 (0.035)
N	964	2,895	4,409	3,726
RD estimate 1997	0.692*** (0.031)	-0.031 (0.035)	-0.141*** (0.037)	-0.038 (0.044)
N	2,082	4,063	3,394	2,330
RD estimate 2001	0.490*** (0.026)	-0.060 (0.037)	0.000 (0.047)	0.047 (0.054)
N	3,162	3,503	2,222	1,566
RD estimate 2005	0.371*** (0.027)	0.014 (0.049)	-0.088 (0.057)	-0.044 (0.040)
N	2,871	2,068	1,423	2,546
RD estimate 2010	0.505*** (0.035)	-0.005 (0.057)	0.017 (0.041)	
N	1,692	1,439	2,455	
RD estimate 2015	0.562*** (0.042)	0.014 (0.049)		
N	1,175	2,068		
RD estimate 2017	0.593*** (0.033)			
N	2,041			

Note: Table show RD estimates shown in Figure 1 by election.

Source: BHPS and UKHLS, own calculations.

Table A.4: Relationship between relative age at election t_0 and party preferences

Election cycle	t_{0-1}	t_0	t_{0+1}	t_{0+2}	t_{0+3}	$t_{0+1}-t_{0+3}$ pooled
	(1)	(2)	(3)	(4)	(5)	(6)
<u>Would vote for Labour</u>						
RD estimate	0.002 (0.036)	0.019 (0.013)	0.001 (0.011)	0.002 (0.011)	-0.005 (0.010)	0.000 (0.006)
N	3,715	19,368	25,122	26,273	25,894	77,054
<u>Would vote for Conservatives</u>						
RD estimate	0.017 (0.025)	-0.013 (0.009)	-0.007 (0.008)	-0.014* (0.008)	-0.023*** (0.008)	-0.014*** (0.005)
N	3,715	19,368	25,122	26,273	25,894	77,054
<u>Would vote for Liberal Democrats</u>						
RD estimate	-0.040** (0.017)	0.011 (0.008)	0.009 (0.007)	-0.014** (0.007)	-0.001 (0.007)	-0.002 (0.004)
N	3,715	19,368	25,122	26,273	25,894	77,054
<u>Would vote for other party</u>						
RD estimate	0.051* (0.028)	0.011 (0.011)	0.020** (0.009)	0.004 (0.009)	0.006 (0.009)	0.009* (0.005)
N	3,715	19,368	25,122	26,273	25,894	77,054

Note: Table shows the relationship between relative age at election t_0 and party preference. Election cycle t_0 denotes the period between the first election where individuals are around the eligibility cut-off and the next election, t_{t+1} the election cycle thereafter etc. Election t_{0-1} indicates the period before the treatment defining election. All estimates include survey year fixed effects. Dependent variables are covered over the period 1991-2019. Bias corrected standard errors in parentheses, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Source: BHPS and UKHLS, own calculations.