Tremors but no Youthquake

Measuring changes in the age and turnout gradients at the 2015 and 2017 British General Elections

Christopher Prosser, Edward Fieldhouse, Jane Green, Jonathan Mellon and Geoffrey Evans

Abstract

A common belief about the 2017 UK General Election is that Labour did unexpectedly well because of a surge in youth turnout. Constituency data appeared to support this claim: the increase in turnout from 2015 to 2017 is correlated with the number of young people in a constituency. Various polling estimates have been put forward for the size of this 'youthquake', ranging from a 12 point increase in turnout amongst the youngest age group to a 19 point increase. In this paper we show why these claims are wrong. We show why the aggregate relationship is likely to be spurious and is driven by the types of places young people live, not the behaviour of young voters. We also show how representative samples of voters and non-voters, properly weighted, reveal a very different conclusion. To examine the relationship between age and turnout in 2015 and 2017 we use the best available survey data, the British Election Study face-to-face surveys. We also provide methods to overcome three challenges in gaining accurate measures of turnout: 1) gathering accurate targets so as to accurately adjust the survey for demographic imbalances due to differential response rates 2) adjusting the survey to account for the fact that people who turn out to vote are more likely to take part in surveys; and 3) dealing with the fact that people over-report having voted in elections in surveys. Using these novel analyses we find no evidence of a substantial shift in the relationship between age and turnout between 2015 and 2017.

The result of the 2017 UK General Election confounded expectations. Having trailed the Conservatives by as much as 21 points in polls at the start of the campaign (YouGov 2017), Labour surged through the campaign and finished only 2.3 percentage points behind the Conservatives.[1] Although the Conservatives still finished well short of being able to form a government, the change in Labour's electoral fortunes was dramatic, and not just relative to the low expectations many voters had before the election.[2] The 2017 election saw Labour's highest vote share since 2001 and the largest single election increase in their share since 1945.

In the wake of this surprise outcome, political analysts and commentators began to look for an explanation for Labour's unexpectedly good performance. One explanation quickly became prominent: Jeremy Corbyn had mobilised previously disengaged young voters who had turned out in droves to vote Labour. Or, to use the term that soon gained traction, that there had been a 'youthquake'.

It is not hard to see why this explanation gained prominence. Increasing youth turnout was part of Corbyn's political strategy and anecdotal evidence seemed to support the argument: Corbyn appeared to be particularly popular amongst the young and was often photographed surrounded by young people. Chants of 'oh Jeremy Corbyn' echoed around the Glastonbury festival.[3] Corbyn and Labour were much more popular on social media and were widely cited as having a superior social media strategy (Booth and Hern 2017; Savage and Hacillo 2017; Cecil 2017).[4]

Initial evidence seemed to suggest that there was something to this claim. Curtice (2017) reported that Labour had been much more successful at winning support from those who did not vote in 2015. At the aggregate constituency level, turnout change between 2015 and 2017 was correlated with the number of young people in a constituency (Heath and Goodwin 2017; Fetzer 2017). Two post-election polls showed that turnout had risen dramatically amongst young respondents—one suggested that turnout amongst 18-24 year old went up by as much as 16 percentage points (Ipsos MORI 2017) and another suggested there had been an increase of 12 points (Britton 2017). Whiteley and Clarke (2017) reported that turnout among young people increased by 19 points in 2017, supporting the 'youthquake' explanation..

The Labour 'youthquake' explanation looks to become an assumed fact about the 2017 election. Oxford Dictionaries even went as far as to declared 'youthquake' their word of the year for 2017 (Oxford Dictionaries 2017). As we will show in this paper, however, people have been much too hasty in reaching this conclusion.

There are strong theoretical and methodological reasons to be sceptical about a sudden shift in the age/turnout gradient in a single election. Any such claims must be supported by robust evidence. Using the best available evidence—the British Election Study (BES) face-to-face surveys—we find no evidence of a surge in youth turnout at the 2017 election.

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[1] For a brief overview of the election, see Prosser (2018) and for an analysis of vote switching between the 2015 and 2017 elections, see Mellon et al. (2018a).

[2] For an analysis of expectations of Labour's electoral performance and its effects, see Mellon and Prosser (2017c).

[3] See https://www.youtube.com/watch?v=i1zLoG6YeA4

[4] At the time of writing, follows of Jeremy Corbyn's twitter account outnumbered Theresa May's 1.7 million to 460,000. Of course, Twitter is not representative of the general population and is younger, better education, and more left-wing and liberal than the British population as a whole (Mellon and Prosser 2017b) so the relative popularity of Corbyn and Labour of social media should not be unexpected.

What we do corroborate, however, is the increasing relationship between age and vote *choice* between the 2015 and 2017elections. The differences between the age and turnout relationship, and the age and vote choice relationship, have been conflated. They need to be clearly separated to avoid incorrect inferences and understanding of the 2017 general election, and the age and turnout relationship in British elections more generally.

The paper proceeds as follows: 1) We discuss previous research about the relationship between and age and turnout and why we should be sceptical about sudden changes in the age/turnout relationship. 2) We examine the aggregate level relationship between age and turnout and demonstrate why we should be extremely cautious in interpreting such aggregate relationships. 3) We discuss the BES face-to-face data and the innovations in weighting methods we use to address a number of challenges that bedevil turnout analysis in surveys. 4) We examine the age/turnout relationship in the BES 2015 and 2017 face-to-face surveys and show that there is no evidence of substantial changes to the age/turnout gradient between the elections. (5) We compare this to the age and vote *choice* gradients in the elections and discuss how these changes may have led people to incorrect inferences about turnout.

Age and Turnout

It is long established in the literature that voter turnout varies with age (Milbrath 1965). The general consensus is that the relationship between turnout and age is curvilinear as rates increase through adulthood only to tail-off in older age due to frailty and ill health (Wolfinger and Rosenstone 1980). More recently, turnout has been referred to as a 'roller coaster ride' (Bhatti, Hansen, and Wass 2012) since a closer inspection of the youngest electors reveals that voting is initially quite high when voters are more likely to live at home with parents, only to drop off as young people leave home. This pattern has been widely attributed to life-cycle effects, in particular the accumulation of resources (Verba, Schlozman, and Brady 1995), and the acquisition of adult roles and experience (Strate et al. 1989). Other research suggests that life-stage factors provide only a very incomplete explanation (Highton and Wolfinger 2001). In Britain, Smets (2016) finds support for some aspects of the life-cycle model (home ownership, marriage, and cohabitation) but not others (leaving education, employment, and residential stability).

A competing explanation for why turnout is lower for young people focusses not on the material attributes of young voters but on the context in which they were politically socialized. Voting is thought to be habit forming (Plutzer 2002; Cutts, Fieldhouse, and John 2009) and young voters are yet to acquire these habits, making them more sensitive to the electoral context (Franklin 2004; Fieldhouse, Tranmer, and Russell 2007). From this perspective, patterns of turnout tend to vary generationally because each cohort of voters are socialized in a different political context, and there is no inherent reason why turnout of young people must be low. Currently low levels of turnout amongst younger voters may instead reflect the fact that they grew up in in a period of less engaging and competitive elections (Franklin 2004) or during a period less supportive of traditional civic values (Dalton 2007).

Whether persistent levels of low youth turnout are due to electoral context or value change (Blais and Rubenson 2013), the existing literature should not lead us to expect any great revival in youth turnout in 2017. If life-cycle effects or value change are the root cause, these are unlikely to change very much over the two years between the elections of 2015 and 2017. If political context is more important, the contest in 2017 was widely perceived to be less competitive in 2015 when a hung parliament was widely anticipated. There were features of the 2017 election that might have meant that more was at stake; Brexit, and the differences between May and Corbyn, but these effects would have to be unprecedented to alter the age/turnout relationship.

Aggregate evidence

Analysis of aggregate election results is commonplace in Britain and elsewhere (For recent examples, see: Goodwin and Heath 2016; Curtice, Fisher, and Ford 2016; Jennings and Stoker 2017; Heath and Goodwin 2017; Collingwood 2016; Becker, Fetzer, and Novy 2017). On the question of youth turnout in 2017, Heath and Goodwin (2017) show that the change in turnout between 2015 and 2017 is correlated with the number of people aged 18 to 29 in a constituency.

The appeal of aggregate analysis is obvious—aggregate data is available openly and considerably more quickly than survey data, presenting researchers with the opportunity to provide rapid analysis and commentary on election results. However, such analysis is not without risk of spurious inference because of the 'ecological fallacy'. The ecological fallacy arises when aggregate level correlations do not reflect individual level relationships but arise as a result of relationships with omitted variables and as a result of aggregation problems such as Simpson's paradox.

The perils of ecological inference have long been known (Robinson 1950). They are not just an abstract theoretical concern but something that regularly arises in social and political research. For example, Gelman (2008) demonstrates that the negative correlation between US state level income and Republican support (poorer states have higher levels of Republican support) is the reverse of the individual correlation between income and voting (poorer individuals are more likely to vote Democrat). Matsusaka and Palda (1993) show that the aggregate relationship between electoral closeness and turnout in Canadian electoral data arises from aggregation biases and is not present at the individual level. Tam Cho and Gaines (2004) show that inferences about levels of split-ticket voting from aggregate data are highly suspect. There are many such examples in the wider literature.

Even leaving the problems of ecological inference to one side, analysing aggregate level turnout data faces other problems. Turnout at the constituency level is measured as the proportion of the number of entries on the electoral register in each constituency. As Mellon et al (2018b) show, however, the number of entries on the electoral register includes large numbers of people who may be legitimately registered in more than one place (such as students and dual home owners) and a significant number of erroneous entries. Differences in aggregate turnout may reflect problems with the turnout denominator rather than changes in voting behaviour (Denver and Halfacree 1992; Denver, Carman, and Johns 2012; Mellon et al. 2018b). These problems may have been exacerbated when comparing turnout in 2015 and 2017 due to the removal of large number of (suspected to be erroneous) entries from the electoral register following the implementation of Individual Electoral Registration.

With regard to the 2017 constituency level 'relationship' between age and voter turnout, it is easy to show why we should be incredibly cautious before taking constituency-level evidence at face value. Table 1 shows the results from a series of regression models that analyse the relationship between the proportion of the population falling into particular age brackets and the change in constituency turnout levels between the 2015 and 2017 elections. At first glance the results appear to support the 'youthquake' argument: there is a positive relationship between the proportion of the population aged 18 to 24 and negative relationships between change in turnout levels and residents aged 45 and older.

Closer inspection raises a number of immediate concerns.

First, the relationship between turnout change and the 18 to 24 ('youth') bracket is actually the *weakest* of the five voter eligible age brackets. Increases in turnout are much more strongly related to adults aged 25 to 29 and 30 to 44—ages that can only be considered 'youth' by the most flattering definition. Second, the first column raises serious alarm bells about the exercise of inferring individual behaviour from aggregate data. It shows that the relationship between turnout change and the proportion of the population that are *children aged zero to four* is far stronger than the relationship between turnout change and any of the adult age groups. The relationship is not trivial—for every additional percentage point children aged zero to four in a constituency turnout increased by 0.9 percentage points between 2015 and 2017.

Table 1. Results from a series of linear regression models examining the relationship between the percentages of constituency population falling into each age bracket (according to the 2011 census) and change in turnout between the 2015 and 2017 elections.

	Age					
	0 to 4	**18 to 24**	**25 to 29**	**30 to 44**	**45 to 64**	**65 Plus**
% population in age range (2011 census)	0.942***	0.0852**	0.393***	0.296***	-0.316***	-0.184***
	(0.116)	(0.0344)	(0.0531)	(0.0435)	(0.0341)	(0.0295)
Constant	-3.381***	1.577***	-0.247	-3.644***	10.54***	5.452***
	(0.718)	(0.343)	(0.373)	(0.890)	(0.890)	(0.509)
N	632	632	632	632	632	632
R^2	0.095	0.010	0.080	0.069	0.120	0.058

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

Few people, it is probably safe to say, would argue that the increase in turnout in 2017 was due to a sudden surge in the number of British toddlers voting in elections. 2017 was not the 'toddlerquake' election. Yet if we think that aggregate constituency relationships are a proxy or indicator for individual behaviour, the statistical evidence for a surge in toddler voting is seemingly more compelling than the evidence of a 'youthquake': the coefficient for children aged zero to four is 11 times greater than the coefficient for adults aged 18 to 24.

What this trend is showing is not that turnout went up amongst toddlers but that turnout went up in *sorts of places* with lots of toddlers. The same is true of the relationship between the number of young adults and turnout. Turnout went up slightly in the sorts of places with lots of young adults. That does not necessarily mean it was those young adults doing the extra turning out.

The sorts of places with lots of young adults are, of course, cities. Table 3 shows that once we include population density as a control variable, the relationship between the number of young adults living in a constituency and change in turnout disappears. For all of three of the youngest adult age brackets the coefficient (which was previously positive and statistically significant) is now negative and not statistically significant. The same is also true for the oldest age bracket, which flips sign and is no longer statistically significant. Only the relationships between turnout change and 45 to 64 year and zero to four year olds are still significant and in the same direction (albeit substantially reduced in size).

Population density clearly does not explain *all* of the correlation between population composition and turnout. However, that the relationship between the number of young adults in a constituency and turnout change disappears with the inclusion of this single control variable is a strong indication that the apparent relationship between the number of young adults in a constituency and turnout change is spurious.[5]

Table 2. Results from a series of linear regression models examining the relationship between the percentages of constituency population falling into each age bracket (according to the 2011 census) and change in turnout between the 2015 and 2017 elections, controlling for population density.

	Age					
	0 to 4	**18 to 24**	**25 to 29**	**30 to 44**	**45 to 64**	**65 Plus**
% population in age range (2011 census)	0.529***	-0.0547	-0.140	-0.0198	-0.140**	0.0298
	(0.128)	(0.0353)	(0.0986)	(0.0626)	(0.0560)	(0.0404)
Population density (2011 census)	0.0341***	0.0473***	0.0546***	0.0457***	0.0293***	0.0476***
	(0.00506)	(0.00494)	(0.00860)	(0.00672)	(0.00744)	(0.00643)
Constant	-1.552**	1.920***	2.199***	1.845	5.392***	0.906
	(0.745)	(0.322)	(0.528)	(1.178)	(1.575)	(0.786)
N	632	632	632	632	632	632
R^2	0.156	0.136	0.135	0.133	0.141	0.133

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

Survey data, turnout, and weighting methodology

In order to properly examine the relationship between age and turnout in 2017 we need data on individual level electoral behaviour. In Britain, the only feasible data of this sort is survey data. Not all survey data is equally suited to the task, howeveras most political polling is conducted online and by phone. Both of these methods are prone to representativeness problems, and most relevant to our current purposes, tend to include too many voters in samples (Mellon and Prosser 2017a; Sturgis et al. 2017). Particularly problematic is the fact that this magnitude of this problem is highest in the youngest age groups (Sturgis et al. 2017), precisely the group we are interested in.

The best way of minimising these problems is to use a random probability sample, the ‘gold standard’ survey research methodology (Sturgis et al. 2017). Here we use two such surveys: the 2015 BES face-to-face survey (Fieldhouse et al. 2015) and the 2017 BES face-to-face survey (Fieldhouse et al. 2017). Both surveys are address-based random probability samples, which means that respondents have an equal

[5] The results in this section are robust to an alternative specification with an age 18 to 29 bracket. Likewise if we use the proportion of full time students in a constituency instead of age we see the same pattern.

chance of being selected to participate in the survey – in contrast telephone and online opt-in samples. Both surveys also benefit from having validated turnout, meaning that we can allow for over-reporting (Silver, Anderson, and Abramson 1986).

Even with high quality survey data, measuring turnout is not entirely straightforward. In particular we must overcome three challenges: 1) Gathering accurate targets so as to accurately adjust the survey for demographic imbalances due to differential response rates. 2) Further adjusting the survey to account for the fact that even with probability sampling and after accounting for demographic imbalances, people who turn out to vote are more likely to take part in surveys. 3) Dealing with the fact that people over-report having voted in elections in surveys. We outline how we deal with each of these challenges below.

Voting Eligible Population weighting targets

Even with the best sampling methodology and good response rates, demographic imbalances due to differential response rates are an inevitable part of survey research. Weighting survey samples to correct for these imbalances is required and when performed correctly, reduces the error in estimates of population values. The two main challenges for survey weighting are identifying the relevant weighting variables and gathering accurate targets to weight to.

Gathering weighting targets for election surveys is more complex than is generally acknowledged. Surveys generally use population level weighting targets derived from high quality official statistics such as the census and the Annual Population Survey (APS). These targets are appropriate when the target population and sampling frame are the whole population. For election surveys, however, the target population is not the whole population, but the sub-population who are eligible to vote in elections. The necessity to distinguish between these targets has expanded substantially since the rise in EU migration from 2004 onwards. EU citizens are not eligible to vote in UK General Elections and have substantially increased the gap between the Voting Age Population (VAP) and the Voting Eligible Population (VEP) (Mellon et al. 2018b). The demographic characteristics of migrants to the UK are not uniformly distributed across demographic characteristics—they are concentrated in younger age brackets. Nor are they uniformly distributed across the UK—they are particularly concentrated in London. Failure to account for these differences introduces systematic error into weighting targets, which will reduce the accuracy of population estimates.

Despite these problems, because demographic data is more readily available for the population as a whole political surveys in Britain are weighted to VAP targets. In contrast we develop at set of VEP weighting targets—the first time that this has been done, to our knowledge. To gather the targets we use APS microdata on nationality to distinguish between voting eligible and ineligible respondents, including respondents only if they state either a nationality or country of birth that is eligible to vote in UK parliamentary elections.[6] We then derive interlocked targets for age and education (degree level/below), and separate targets for gender and region.

An additional complication is that the APS does not ask respondents older than 69 who are not in the labour force their qualification levels. To provide this data we supplement the APS with an education

[6] Unfortunately the APS does not report all nationalities and countries of birth in the microdata, so we have to assume that respondents from low-frequency “other” countries are ineligible to vote.

target for this oldest age group derived from Understanding Society data (University of Essex et al. 2017). Understanding Society does not have a nationality variable, so we derive the proportion of 65-74 and 75+ year olds who have degrees from Understanding Society and multiply this proportion by the number of eligible people in each age group to get the relative size of the degree and non-degree groups. This percentage will still be relatively accurate, because the eligible proportion of the oldest age groups is very high.

To illustrate the differences between VAP and VEP, we show differences between each of these targets (both gathered using the same data sources) for 2017 in Table 3. Compared to the VAP targets, the VEP targets have lower numbers of younger and educated respondents than the VAP targets and have a lower share of respondents in London compared to other regions.

Table 3. Percentage point difference in VEP target compared to VAP target (positive numbers indicate the group is more highly represented within VEP targets) for 2017.

Weighting target	**VEP - VAP %**	**Weighting target**	**VEP - VAP %**
Age/Education		*Gender*	
18-24 Degree	-0.1	Male	0.1
18-24 No degree	-0.1	Female	-0.1
25-34 Degree	-0.9	*Region*	
25-34 No degree	-0.8	East Midlands	0.1
35-44 Degree	-0.6	Eastern	0.1
35-44 No degree	-0.4	London	-2.1
45-54 Degree	0	North East	0.2
45-54 No degree	0.4	North West	0.4
55-64 Degree	0.2	Scotland	0.2
55-64 No degree	0.6	South East	0.2
65-74 No degree	0.7	South West	0.2
65-74 Degree	0.2	Wales	0.2
75+ No degree	0.6	West Midlands	0.1
75+ Degree	0.2	Yorkshire & Humber	0.3

Weighting to the result

Even with high quality probability survey data and after demographic weighting, the proportion of people who report voting in both BES face-to-face surveys is higher than the proportion of the population who actually voted. This is not a new problem and is true of every instance of the British Election Study and its predecessors from the first post-election survey in 1964 onwards (Prosser and Mellon 2018) and also affects national election studies in other countries such as the US (Burden 2000). The reason for this gap are twofold: 1) response bias that is correlated with turnout, and 2) people misreport having voted. We deal with each of these problems in turn.

The over-representation of voters is indicative of non-response bias to the survey—non-voters are more likely to decline the invitation to take part in the survey. It is unlikely that it is turnout itself that is correlated with non-response but rather some other factor that is correlated with both turnout and non-response (e.g. political engagement or a general disposition to take part in things). Additionally, this non-response bias might change in size between years and if we do not account for it we run the risk of drawing spurious inferences about turnout that are artefacts of different levels of bias. The ideal solution would be to weight directly on this factor but this is impossible because it is either unmeasured, or where it is measured (i.e. political attention) we lack a suitable population target to weight to.[7]

We can, however, weight to the turnout result itself. Doing so has three advantages: 1) It ensures that the survey has the correct level of overall turnout. 2) Previous research has shown that turnout weighting can reduce the bias in other variables of interest, such as vote share (Mellon and Prosser 2017a; Mellon and Prosser 2016). Since turnout is correlated with whatever factor influences non-response, the overall non-response bias in other variables due to this factor will be substantially reduced. 3) Since turnout and non-response is not uniformly distributed between or within demographic groups, by weighting to age and turnout simultaneously, the weighting process will increase the accuracy of relative turnout rates between different demographic cells as well as pegging the overall turnout rate to the correct result.

To demonstrate that this is the case, we simulate this non-response and weighting process on a simplified scenario that mimics the turnout and non-response problem. In this scenario we have an observed uniformly distributed 'demographic' variable x between 0 and 100 (mimicking a continuously distributed demographic variable such as age) and a correlated variable u (mimicking factors that influence turnout and response bias, such as political engagement). The correlation between x and u is a uniformly distributed random parameter that is restricted to plausible values of the correlation between demographics and turnout/response bias: 0.1 and 0.6.[8]

As with continuous demographic variables such as age, x is divided into quintiles for weighting purposes.

The 'turnout' variable y is determined by a logistic model of the form:

$$y* = \beta_x x + u$$

And whether an observation is included the final 'survey' sample is determined by a logistic model of the form:

$$sample* = \beta_u u$$

The parameters β_x and β_u are set to approximate a plausible scenario where turnout is between 58% and 78% (depending on the correlation between x and u) and the 'response rate' is 50%: $\beta_x = 0.05$ and $\beta_u = 0$.

Each round of the simulation begins with 5000 cases (the approximate size of the original sampling frame in the 2017 BES face-to-face) and the x, u, y, and $sample$ variables are generated as described above. Two sets of weights are then generated for the sample: 1) x 'demographic' weights that weight the

[7] Indeed the BES face-to-face surveys are themselves used to provide weighting targets of this sort of other surveys.
[8] The higher the correlation between demographics and turnout/response bias, the more effective demographic weighting will be at correcting for these biases because the bias lies largely between weighting cells. The turnout/response bias problem is greatest at low correlations, which increase the bias within cells.

population quintiles of x so they are evenly distributed in the sample. 2) $x + y$ 'turnout' weights that add an additional weighting factor that weights y ('turnout') to the correct population proportion.

We are primarily interested in how these weights affect the estimation of the levels of y within quintiles of x (i.e. turnout within age groups). For each simulation we calculate the error between the population level of y in each quintile of x and the estimated level of level of y in each quintile of x in the sample calculated with each set of weights.

Figure 1 displays the distribution of these errors from 10,000 simulations. It clearly shows that the 'demographic' x weights do not recover the accurate relative levels of turnout between quintiles, and that because x and u are correlated, the level of error is correlated with x: it is highest in the lower quintiles (i.e. the younger age groups). In contrast, the $x + y$ 'turnout' weights perform a much better job of recovering the correct level of turnout within quintiles of x: the peak of the error distribution is close to zero for all quintiles.

The advantages to weighting to the correct level of turnout are hopefully clear. However, implementing turnout weighting in reality is more complicated than it would first appear. The turnout figures reported after each election are calculated as the proportion of total votes to total entries on the electoral register. This is not the same thing as the proportion of total votes to the total number of registered *voters*.

The total number of entries on the electoral register is greater than the total number of registered voters due to a combination of legitimate multiple registrations (i.e. students registering at home and at university, and home owners registering in two locations) and inaccuracies in the register. The actual turnout amongst registered voters is substantially higher than the official turnout statistics would suggest (Mellon et al. 2018b). Even accounting for these problems with the turnout denominator, registered voter turnout ignores those who are eligible to vote but who are not registered.

To properly weight a VEP sample, we need a VEP turnout estimate. Mellon et al. (2018b) estimate VEP turnout for the UK. This VEP turnout target is also calculated using the APS and estimating the number of over 18 year olds resident in the UK who are eligible to vote in UK elections based on their nationality. The VEP turnout targets for 2015 and 2017 are 65% and 68%, respectively.[9]

For similar reasons to those outlined above, in addition to weighting to the correct turnout figures, we also weight to the correct party vote shares amongst those that cast a vote in the election. Inflated turnout figures are indicative of turnout related response bias, and weighting to turnout reduces the impact of this response bias. Similarly, incorrect party shares are suggestive of other forms of response bias, and weighting to the result reduces the impact of these biases.

[9] As a robustness check we calculate alternate weights using turnout targets that are 10 points different in both directions (see appendix). Our conclusion of no change in the age-turnout relationship between 2015 and 2017 remains the same.

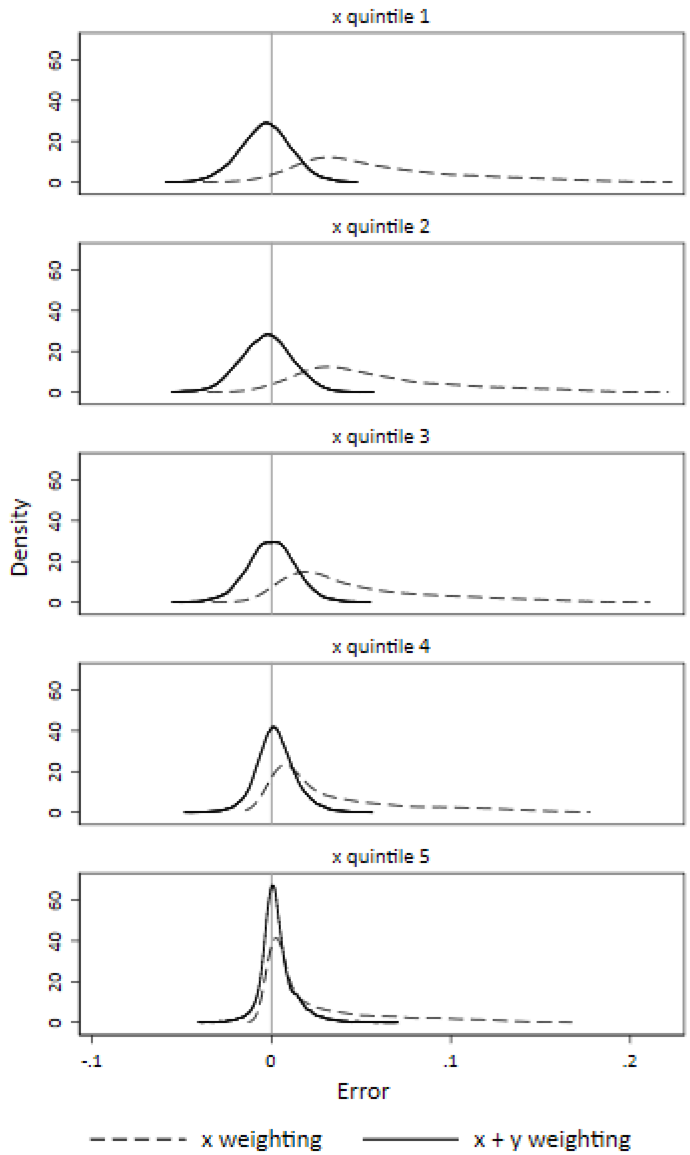

Figure 1. Kernel density estimation of the distribution of error in level of y in each quintile of x from 10,000 simulations of different weighting methods.

Vote Validation

The final problem we must overcome is the tendency of some respondents to over-report having voted after elections (Bernstein, Chadha, and Montjoy 2001; Silver, Anderson, and Abramson 1986). In order to do so after each election we conducted a vote validation exercise. This process involved gathering the marked electoral registers for the wards covered by our sampling frame and checking to see whether the BES respondents were registered, and if so, whether they were marked has having voted in the election.

Where respondents were found to have been marked as having voted in the electoral register, we can be confident that they did in fact vote in the election. Where respondents were either found on the register and not marked, or not found on the register at the address they were surveyed, we can be reasonably confident that they did not vote. The exception to this rule is the small number of respondents who told us they were registered at another address (and thus potentially voted in an area outside our sample for which we did not have the registers). For these respondents we use their reported turnout to be true. Our conclusions are robust to the exclusion of these respondents.

Of course, the vote validation process is not perfect. Similar exercises in the US have been criticised on the grounds that errors in the matching process exceed any benefits from validation and artificially reduce the turnout rate (Berent, Krosnick, and Lupia 2016). In the different election administration environment of the UK, many of these criticisms do not apply, however, and the match rate is substantially higher than those reported in the US—90% of 2017 BES face-to-face respondents were matched to the electoral register, compared to 78.5% reported by Berent et al. (2016) using the least strict matching criteria. This 90% figure is slightly higher than the Electoral Commission's estimate of the completeness of the electoral registers in Britain of 85% (Electoral Commission 2017).

Our vote validation process cannot be perfect and errors of incorrectly classifying a respondent as not having voted might arise as a result of several factors, for example: having moved between the election and taking part in the survey, being legitimately registered in more than one location and having voted in a location different to where the respondent was sampled, and errors made polling station officials in marking off the electoral register.

Given these concerns, we present our turnout analysis using both self-reported and validated vote measures. The likely errors in these approaches work in the opposite direction: self-reported vote will overestimate turnout and validated vote might underestimate it. Where these two measures agree on the age/turnout relationship—and they generally do—we can be confident in our findings. To facilitate this analysis we calculate separate sets of turnout weights for the self-reported and validated samples.

Age and turnout in 2015 and 2017

Now that we have described our data and weighting methodology we turn to our analysis of turnout and age at the 2015 and 2017 elections. To ensure our findings are not simply an artefact of a particular method we do so in three ways: 1) A nonparametric smoothed local mean of turnout rates by age 2) a series of pairwise comparisons of turnout rates in different age groups 3) a series of pooled logistic regression models of turnout testing the interaction of age and election.

Nonparametric analysis

Figure 2 shows the nonparametric smoothed local mean analysis of turnout by age using self-reported and validated turnout. Both analyses suggest there was a very similar age/turnout gradient in 2015 and 2017 and that a substantial rise in turnout amongst young voters is very unlikely to have occurred.

The two plots differ slightly in where they suggest turnout did rise at the margins in 2017. Self-reported turnout suggests that it rose slightly amongst voters younger than about 40 and also amongst voters aged

50 to 60. The validated turnout analysis suggests that turnout remained the same amongst the youngest voters in both elections but rose more substantially amongst 30 to 40 year olds, and possibly slightly amongst voters aged around 60. In both cases, with the exception of the 30 to 40 year olds in the validated analysis, the confidence intervals are overlapping between years and therefore we cannot be certain about where turnout did change slightly between years. We can be more confident, however, that the overall age/turnout gradient did not change substantially between 2015 and 2017.

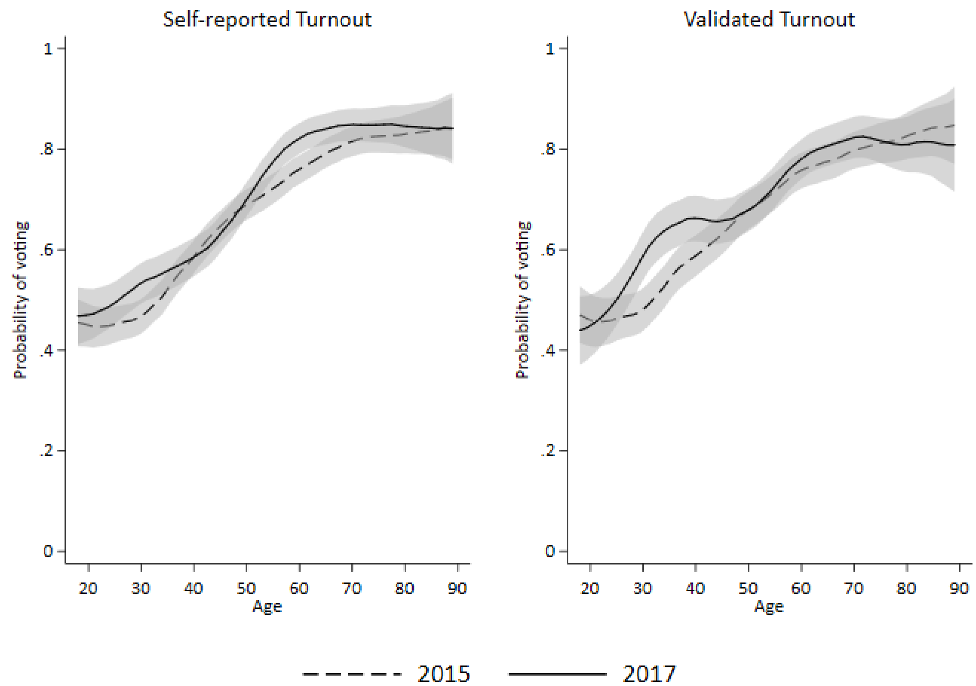

Figure 2. Self-reported and validated nonparametric smoothed local mean turnout probability over age, bandwidth = 5. The shaded areas show 95% confidence intervals.

Pairwise comparison of age groups

Next we analyse turnout changes between 2015 and 2017 by grouping respondents into age groups and performing Wald tests of the null hypothesis that turnout level in each age group remained the same at both elections. These comparisons are shown in Table 4.

Table 4. Comparison of turnout in 2015 and 2017 by age group.

	Self-reported Turnout %			Validated Turnout %		
Age Group	**2015**	**2017**	**Difference**	**2015**	**2017**	**Difference**
18-24	47.4	48.2	0.9	48.7	43.1	-5.6
n	*209*	*151*		*157*	*109*	
25-34	44.6	50.2	5.6	47	56.4	9.5*
n	*374*	*292*		*260*	*189*	
35-44	57.1	59.1	2	58.3	69.1	10.8**
n	*445*	*309*		*288*	*193*	
45-54	69.7	70.1	0.4	67.2	66.4	-0.8
n	*515*	*340*		*328*	*229*	
55-64	75.2	81.8	6.6*	75.4	77.2	1.8
n	*464*	*365*		*305*	*250*	
65-74	82.6	85	2.4	79.5	82.8	3.4
n	*448*	*347*		*306*	*250*	
75+	81.3	83.5	2.3	82.5	81.3	-1.1
n	*308*	*263*		*201*	*179*	

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

In the self-reported sample only one comparison suggests a statistically significant difference between years, and then only at the p<0.1 level: turnout increased amongst 55 to 64 year olds. There are small increases in turnout for all of the other age groups but none of these differences are statistically significant. In the validated sample two differences are statistically significant: an increase in the 25-34 year old turnout (only significant at the p<0.1 level)and a similar increase in the proportion of 35-44 year olds voting (at the p<0.05 level). The validated data also show a *decrease* in the turnout in the youngest age bracket, and small changes in both directions for the older age brackets. None of these changes are statistically significant.[10]

We should be cautious about over interpreting these results. Most of the differences are not statistically significant, and even those that are would not be if we made multiple comparison adjustments to our p values. Additionally, once the sample is broken down into the age groups the sample sizes for each comparison are quite small—the size of each age group in each year ranges from 109 (18 to 24 year olds in the 2017 validated turnout sample) to 515 (45-54 year olds in the 2015 self-reported turnout sample). Small samples are more likely to throw up differences in the wrong direction and of an exaggerated magnitude (Gelman and Carlin 2014). Where we can be confident is that these results show no evidence of a substantial change in the age/turnout gradient between 2015 and 2017, and in particular no evidence of a dramatic rise in youth turnout in 2017.

[10] The substantive result of no statistically significant change in turnout for the youngest age group is robust to an alternative age grouping of 18-29.

Logistic regression model of age and turnout

Finally we test whether the relationship between age and turnout changed between 2015 and 2017 with a series of logistic regression models of turnout on the pooled data from both years. Specifically, for each of the self-reported and validated samples we estimate three logistic models of the form:

(1) $turnout* = \beta_{age}Age + \beta_{election}Election + \beta_{interaction}Age.Election$
(2) $turnout* = \beta_{age}Age + \beta_{age^2}Age^2 + \beta_{election}Election + \beta_{interaction}Age.Election$
(3) $turnout* = \beta_{age}Age + \beta_{age^2}Age^2 + \beta_{election}Election + \beta_{interaction}Age.Election + \beta_{interaction^2}Age^2.Election$

Where Age is an interval variable recording respondent ages, Age^2 is its squared term and $Election$ is a binary variable that indicates whether the data comes from the 2015 or 2017 elections (2017 = 1). We are primarily interested in whether the age/turnout gradient has changed between elections, which is measured by the $\beta_{interaction}$ and $\beta_{interaction^2}$ terms. The results from these models are shown in Table 5. Again, these results show no evidence of a change in the relationship between age and turnout between 2015 and 2017: the interaction terms have different signs in the self-reported and validated turnout data and in none of the models are the interactions statistically significant.

Table 5. Results from logistic models of turnout by age on pooled 2015 and 2017 data.

	Self-reported turnout			**Validated turnout**		
	1	**2**	**3**	**1**	**2**	**3**
Age	0.0342***	0.0539***	0.0546***	0.0311***	0.0550***	0.0439**
	(0.00306)	(0.0126)	(0.0163)	(0.00345)	(0.0144)	(0.0192)
Age²		-0.000203	-0.000210		-0.000246*	-0.000132
		(0.000125)	(0.000163)		(0.000139)	(0.000190)
Election = 2017	-0.00553	-0.00199	0.0326	0.178	0.176	-0.373
	(0.234)	(0.231)	(0.586)	(0.270)	(0.266)	(0.670)
Election = 2017 x Age	0.00326	0.00315	0.00150	-0.000795	-0.000768	0.0253
	(0.00470)	(0.00455)	(0.0254)	(0.00523)	(0.00506)	(0.0285)
Election = 2017 x Age²			0.0000170			-0.000266
			(0.000254)			(0.000279)
Constant	-0.995***	-1.407***	-1.421***	-0.853***	-1.354***	-1.121**
	(0.151)	(0.304)	(0.376)	(0.177)	(0.356)	(0.449)
N	4830	4830	4830	3244	3244	3244

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

Age and vote choice in 2015 and 2017

It is clear that the BES face-to-face surveys show no evidence to suggest a substantial rise in youth turnout. There was, in short, no 'youthquake'. Why then, did the idea of youthquake take such a strong and fast hold?

As we have discussed above, this was due in part to misleading aggregate level relationships and biased polling data. This is not the full story, however. The suggestion that there had been a surge in youth turnout emerged within hours of polls closing—well before any polls and aggregate analysis had been released, indeed before most votes had been counted. One claim that quickly gained prominence on social media during election night was that turnout amongst 18-24 year olds was 72%, a figure that was apparently not based on any actual data at all and seemed to be plucked out of thin air (Dahir 2017). The early suggestion of a surge in youth turnout may have acted as an anchoring heuristic (Tversky and Kahneman 1974) and when later data appeared to confirm the rise in youth turnout, it tapped into an already existing belief.

The misperception that there was a rise in youth turnout seems to be a clear case of people being misled by the 'availability heuristic' (Tversky and Kahneman 1974). The availability heuristic is the tendency to rely on readily available information in making decisions and judgements. In political analysis this can manifest itself in the mistaken belief that the characteristics of the most distinctive and visible groups of a party's supporters are typical of a party's supporters as a whole.[11]

Young voters *were* distinctly Labour supporting. Figure 3 shows the relationship between age and Labour and Conservative support, and the relationships between age and vote choice between the 2015 and 2017 general elections. It is true that Labour were more popular among young voters and that the Conservative were more popular with older voters, and that these relationships increased between 2015 and 2017. The increase in the age and vote choice relationship is primarily due UKIP voters defecting to the Conservatives and Green voters defecting to Labour (Mellon et al. 2018a). Young voters are the most distinctively Labour voting group of voters. It would be a mistake, however, to assume that young voters are typical of Labour voters as a whole—in both 2015 and 2017 voters under the age of 25 made up only 12-13% of all Labour voters. Young people are also the most distinctively non-voting. Again, though young people are not typical of non-voters, people under the age of 25 make up only 16-20% of non-voters.

These two pieces of information—that young people do not vote but when they do they vote Labour—are readily available stereotypes. Combined they seem to suggest that the most obvious source of Labour recruits are young non-voters and it is not hard to see how someone could reach the conclusion that Labour did well in 2017 because there was a change in turnout amongst young people. That this is what Corbyn said he was planning to do only made this seem more plausible. As we have shown, however, it is a conclusion that is not borne out by the evidence.

Of course the BES data could be wrong. Even if this were the case and there really was a substantial rise in young people voting in 2017, the youthquake argument—in the sense that Labour increased its vote so dramatically in 2017 *because* of a sharp increase in youth turnout—would still be implausible. People under the age of 25 make up 11% of the electorate, about 5.2 million people in total. Labour won 3.5 million more votes in 2017 than in 2015. If we are wrong about the level of turnout amongst young people and it was in fact as high as 72% (which seems almost as unlikely as toddlers voting in elections) this would be 1.2 million more young voters than 2015. Even if *every single one* of these extra voters voted Labour (which again, seems absurd) this would only account for about one third of Labour's vote gains in 2017. This represents the upper bound of possible youthquake effects. Even if turnout was higher than the level in the BES data, it is unlikely to have been as high as 72% and fewer than 100% of the newly mobilised

[11] For example, see Gelman (2008) for an examination of the effect of the availability heuristic and misapprehensions about the relationship between income and voting in the US.

young voters would have voted Labour. And this does not account for countervailing vote flows away from Labour from 2015 to 2017, which took place in large numbers (Mellon et al. 2018a) and were counteracted by other flows towards Labour. The youthquake argument simply does not hold water.

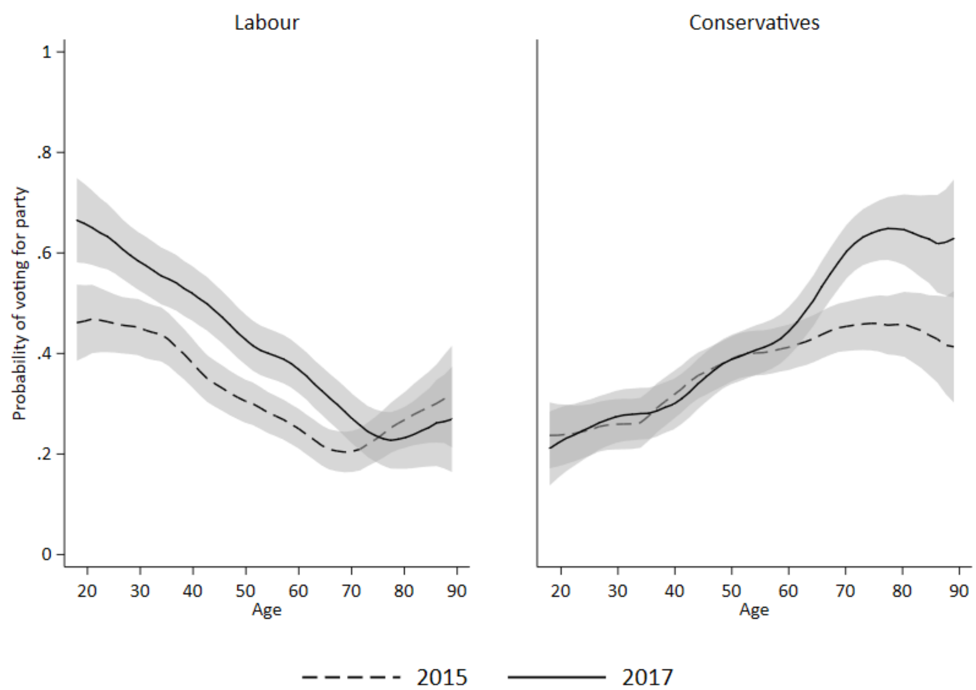

Figure 3. Nonparametric smoothed local mean probability of voting Labour and Conservative over age using the validated vote sample.[12] Bandwidth = 5. The shaded areas show 95% confidence intervals.

Conclusions

It is perhaps unsurprising that following the dramatic and unexpected result of the 2017 election, a similarly dramatic explanation was put forward to explain what happened. As we have shown in this paper, however, there is no evidence to suggest the relationship between age and turnout changed substantially between the 2015 and 2017 elections. Instead, the explanation for the large increase in Labour's vote share is more prosaic—Labour increased its share of the vote across a wide spectrum of the electorate.

The idea of a 'yYouthquake' does not tell us much about turnout at the 2017 election, as it turns out. It does reveal, however, a great deal about the perils of making inferences about electoral turnout. Analysing electoral turnout is hard. Careful analysis takes time. Even measuring the overall turnout level

[12] The same graph drawn using the self-reported turnout sample is omitted for reasons of space and repetition. It shows essentially the same relationship between age and vote choice.

amongst those that are eligible to vote is not straightforward. We have shown here that aggregate level analysis of turnout can easily be misleading and that individual data is necessary. However, analysing turnout in surveys is fraught with difficulties. We have detailed our approach to dealing with the problems due to non-response bias and turnout misreporting. Having done so we approached the question of the relationship between age and turnout at the 2015 and 2017 elections in three different ways. All three of these analyses across two measures of turnout—self-reported and validated—concur: there was no substantial change in the relationship between age and turnout between the 2015 and 2017 elections.

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