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Public Support for Votes at 16 in the UK: The Effects of Framing on Rights and Policy Change

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With votes at 16 implemented for local and devolved assembly elections in Scotland and Wales, the debate on the issue continues amongst politicians in England and Northern Ireland. Testing arguments that are often made in that debate, we analyse two survey experiments and show that framing on extending rights prompts higher support, whilst framing on policy change depresses support. These effects hold when priming on consistency of legal ages and are particularly strong amongst the very right-wing. A majority of the public remains opposed to votes at 16, but our results indicate the malleability of public opinion on the issue.

Keywords: Framing, priming, public opinion, survey experiment, votes at 16

1. Introduction

Much of the literature around the possible extension of the franchise to 16- and 17-year olds has focused on the arguments for and against the policy. These are neatly summarised in [Berry and Kippin's \(2014\)](#) report for *Democratic Audit*, which brought together comment by proponents and opponents and built on interventions from many others ([Cowley and Denver, 2004](#); [Folkes, 2004](#); [Tonge and Mycock, 2010](#); [Mycock and Tonge, 2012](#)). Empirical work on the topic has reached differing conclusions regarding the political maturity of young people themselves ([Chan and Clayton, 2006](#); [Wagner et al., 2012](#); [McAllister, 2014](#)), and has shown varying support amongst groups in the population for extending the franchise ([Birch et al., 2015](#)). Further, [Birch et al.'s \(2015\)](#) study observed that public opinion on the topic should not be considered solidified, and concluded that if 'franchise change occurs, it likely will be the result of an elite-driven project

that succeeds because of widespread public indifference' (2015, p. 291). That conclusion provides the starting point for this article in prompting the following question: if public opinion is not solidified on the topic of votes at 16, can it be moved by the frames that are likely to be deployed by elites?

Whilst public opinion is not solidified, political elites have taken clear stances on the issue, with the two major UK parties diverging; Labour tends to be supportive of the initiative (Labour Manifesto, p. 103), as do other progressive parties (i.e. the Liberal Democrats, SNP and Greens), whilst Conservatives are generally opposed.¹ Thus, whilst party adherents in the mass public are likely to mirror party stances in their own political attitudes (Zaller, 1992; Levendusky, 2009), because the votes at 16 issue is currently low on the political agenda it is unlikely that all but the most ardent party supporters have internalised their party's stance on the issue. The lack of strong views amongst the population means there is considerable room to shape public opinion (Zaller, 1992).

That capacity to shape public opinion is additionally relevant because, having been adopted in Scotland and Wales as well as included in the Labour Party's 2019 manifesto, the salience of votes at 16 is likely to grow in coming elections. Thus, the question of how public opinion responds to the debates surrounding the issue is one of importance. Notably, the *way* that the debate is framed is likely to have an impact on the content and dynamics of public opinion on extending the vote to younger citizens. In this study, we focus on how framing the issue in terms of extending rights versus changing policy affects public support for votes at 16. We also examine whether priming on consistency of legal ages lowers support or dampens framing effects. To further tease apart the dynamics of public opinion, we test the role of political ideology in moderating the framing effects. We find that public support for votes at 16 remains low, though left-wingers exhibit higher levels of support than right-wingers. More importantly, we find that public support is malleable: framing the issue in terms of rights tends to increase support, whilst the policy change frame depresses it. We do not find that priming on legal consistency affects support overall, but do find that the framing effects are moderated to some degree by ideology.

2. Framing on rights or policy change

There are different conceptualisations of framing found across the fields of political science, psychology, sociology and communication (see Cacciatore *et al.*, 2016 for a thorough discussion), so the concept is mired in a degree of conceptual

¹There is some debate within the party, with backbenchers such as Sir Peter Bottomley supporting lowering the voting age, but recent Conservative governments have been unwilling to bring the issue to a vote in Parliament.

murkiness. Framing first gained attention with Kahneman and Tversky's famous Prospect Theory experiments (Kahneman and Tversky, 1979; Tversky and Kahneman, 1981). In these studies, they manipulated the presentation of logically equivalent information, demonstrating that human decision-making does not adhere to strict rational calculations. Rather, decision-making can be influenced by the way that information is presented. Kahneman and Tversky's work demonstrates that when information is framed as a potential gain, humans exhibit a preference for certain over risky options (e.g. a program could save 200 people versus 1/3 probability that 600 people will be saved and 2/3 probability that no people will be saved). Conversely, when information is framed in terms of potential loss, the riskier option is preferred. This illustrates the basic idea behind 'equivalence framing'.

In our study, however, we focus on 'emphasis framing' (Cacciatore *et al.*, 2016), which differs in a fundamental way from equivalence framing. Instead of presenting logically equivalent information, emphasis framing focuses attention on 'one set of considerations over another' (Cacciatore *et al.*, 2016, p. 10). Emphasis framing provides a lens through which the public can define the parameters of a debate. In the sociological tradition, frames are seen as a means of symbolic issue construction (e.g. Gamson, 1992). They provide a storyline, or a way to help organise and structure the meaning of an issue (Nelson *et al.*, 1997). Like equivalence framing, emphasis framing can affect decision-making and influence public opinion.

Emphasis framing has been shown to have substantial effects on public opinion and policy attitudes across a variety of domains (Chong and Druckman, 2010), including racial attitudes (Kellstedt, 2000), tolerance for the KKK (Nelson *et al.*, 1997), gay rights (Brewer, 2002), economic attitudes (Marx and Schumacher, 2016) and general liberal policy (Lammers and Baldwin, 2018). What many of these studies have in common is that they discuss and/or employ frames as a way to reduce a complex debate down to one or two key elements. For example, Nelson *et al.* (1997) frame a KKK rally as either a free speech controversy or an issue of public order. The free speech frame increased public tolerance for the rally, whilst the public order frame led to more intolerance. Similarly, Kellstedt (2000) argues that long-term trends in American racial policy attitudes can be linked to media framing. Using time series data, he shows that when the media emphasises egalitarian values surrounding racial issues it leads to more racial policy liberalisation, whereas individualism frames decrease racial policy liberalism. Together, extant research on framing effects repeatedly demonstrates the malleability of public opinion and the role that framing plays in providing a lens through which the public comes to understand an issue. Thus, whilst there is currently little public support for extending the franchise to 16-

and 17-year olds, the likely malleability of the public opinion on this issue (Birch *et al.*, 2015) suggests a potentially important role in how the debate is framed.

It should be noted that whilst framing studies tend to focus on media effects, these effects are not confined to media communication (Chong and Druckman, 2010). Indeed, the media can be seen as the purveyor of elite communication, reinforcing the views of those who have a stake in the debate (Zaller, 1992; Nelson *et al.*, 1997). For our purposes, this is important because the way in which policymakers decide to frame the votes at 16 debates is likely to impact public support for extending the franchise to younger citizens. In other words, we expect elite opinion leadership to shape the contours of the debate and for the mass public to reflect these opinions (e.g. Zaller, 1992; Levendusky, 2009).

In our study, we narrow in on two key considerations that are likely to form part of the debate. We focus on the issue as one of either extending rights or changing policy from the status quo. To do so, we experimentally manipulate the way in which we ask respondents their degree of support for allowing 16- and 17-year olds to vote. We expect that framing votes at 16 as a rights issue will emphasise the applicability of liberal democratic values, increasing public support (Hypothesis 1). Alternatively, when framed as reducing the voting age, the debate becomes one of changing policy from the status quo, which is likely to garner less support (Hypothesis 2).

3. Priming on booze and marriage

We have described above the effects of issue framing on public opinion and our expectations that, *ceteris paribus*, we will see increased support for lowering the voting age when votes at 16 is framed in terms of extending rights rather than changing policy. But what happens if the public is reminded of other areas of social life in which 16- and 17-year olds are not granted rights, such as purchasing alcohol and marrying without parental consent? Whilst these issues do not focus on civic rights in the same way that voting does, we chose to focus on booze and marriage because they are frequently cited in the arguments relating to consistency of legal ages that are marshalled by opponents of votes at 16. We focus on the priming literature to understand how these legal consistency arguments may alter our framing effects.

Whilst framing affects the *applicability* of certain considerations (e.g. rights vs. policy change) to political attitude formation, priming affects the *accessibility* of considerations (Iyengar *et al.*, 1984; Iyengar and Kinder, 1987; Nelson *et al.*, 1997; Cacciatore *et al.*, 2016). Priming is the process whereby mental constructs become activated in memory and unconsciously influence subsequent evaluations (Domke *et al.*, 1998). This process is theoretically based on 'models of memory as a network of interconnected cognitive structures or nodes that are used in the

storage, retrieval, and use of information' (Cacciatore *et al.*, 2016, p. 11). In long-term memory, conceptual objects, or mental constructs, are stored and connected to related conceptual objects (e.g. the 'young people' node would be connected to perhaps hundreds of other nodes such as 'energetic', 'irresponsible', 'my nephew' and so on; Lodge and Taber, 2013). These nodes and their networks of associated concepts can become activated through 'spreading activation' (Cacciatore *et al.*, 2016). Priming works by increasing the salience of the primed object, which then impacts subsequent information processing and evaluations. Theoretically, priming would impact all recipients to a similar degree by affecting the accessibility of the encountered information (Cacciatore *et al.*, 2016, p. 13). For example, if issues related to the legal age of adulthood are brought to the fore of the public discussion, it would increase the salience, and thus accessibility, of these considerations when the public considers extending the franchise to 16-year-olds.

Coupling the votes at 16 questions with legal consistency arguments may strengthen the association between the votes at 16 debate and other activities that young people are not allowed to do. It is also possible that legal consistency arguments activate related beliefs about the irresponsibility and inexperience of 16-year-olds. If this were the case, we would expect priming on legal consistency considerations to lead to reduced support for votes at 16 (Hypothesis 3). In a second experiment, we examine this possibility by asking respondents whether 16- and 17-year-olds should be able to buy alcoholic beverages and be allowed to marry without parental consent before asking them about extending the franchise.

4. Ideological asymmetries in framing effects

Not everyone forms the same opinions even when in the same circumstances. Individuals have their own values, political affiliations, identities and so on, all of which can affect how people respond to the political context (Lavine *et al.*, 2002). Thus, we do not expect our framing design to affect all people in the same way. Given that we focus the votes at 16 debate as either an issue of rights or of changing policy, we expect ideology to play an important moderating role (Hypothesis 4). More specifically, we expect those on the left to be more susceptible than those on the right to framing the issue in terms of extending rights, increasing their support for the initiative when framed in this way (Hypothesis 4a). Conversely, we expect the changing policy frame to impact those on the right more strongly than those on the left, leading to a reduction in support for votes at 16 amongst right-wingers (Hypothesis 4b). These expectations are drawn from a broad literature in political, social and cognitive psychology, which demonstrates pervasive psychological differences between left-wingers and right-wingers, and we believe some of the differences are relevant for understanding potential framing effects of

the votes at 16 debate. Our expectation that those on the political left and right will differentially respond to the frames is also in line with a growing literature that demonstrates differing responses to the political context across the ideological spectrum (e.g. [Lammers and Baldwin, 2018](#)).

Whilst it has long been lamented that the mass public lacks a coherently structured set of political opinions (e.g. Converse, 2006), there is ample evidence that enduring psychological differences structure the concerns and worldviews of liberals and conservatives, or left-wingers and right-wingers (e.g. [Jost et al., 2003](#)). As [Carney et al. \(2008\)](#) write: ‘ideology both reflects and reinforces individual differences in fundamental psychological needs, motives and orientations toward the world’ (808), and recent research even identifies biological underpinnings for ideological differences ([Amodio et al., 2007](#)). Political psychologists have also linked ideology to stable predispositions and personality (e.g. [Van Hiel et al., 2000](#); [Gerber et al., 2010](#); [Caprara and Vecchione, 2013](#)), epistemic and existential needs and motives ([Jost et al., 2003, 2008](#)), and values ([Inglehart, 1997](#); [Caprara et al., 2006](#)).

Amongst the many findings, research shows that those on the left are more open to change and experience ([Van Hiel et al., 2000](#); [Caprara et al., 2006](#); [Gerber et al., 2010](#)), and emphasise the importance of democratic participation and equality ([Van der Meer et al., 2009](#)). Those on the right tend to be more risk-averse ([Kam and Simas, 2010](#)), concerned with social order and security ([Feldman, 2003](#)), and emphasise tradition and resistance to change ([Jost et al., 2003](#); [Jost et al., 2008](#)), which leads them to exhibit a status quo bias. For these reasons, we expect those on the left and right of the political spectrum to respond to the frames that align with their underlying political predispositions: those on the left will be more supportive of lowering the voting age when the issue is framed in terms of rights, whereas support for the issue amongst those on the right will be particularly attenuated when the issue is framed in terms of changing the policy from the status quo.² We also expect to find higher levels of support on the left than right, regardless of framing.

5. Methodology

To test our hypotheses, two survey experiments were fielded to representative samples of the UK adult population drawn from YouGov’s online panel of respondents. Both surveys had four experimental treatments to which

²Here, we make a values-based argument, but acknowledge that partisan motivations could also be at play. We have several reasons to suspect, however, that our values-based approach is more appropriate in the context of the present study. We address this more thoroughly in Appendices I and J. All appendices are available via the link under the data availability statement.

respondents were randomly assigned. The first experiment, fielded to 3619 respondents from 7 to 9 November 2017, asked each respondent one of the following questions:

- Treatment 1 (rights frame): ‘To what extent, if at all, do you support or oppose giving 16 and 17 year olds the right to vote?’ ($n = 885$).
- Treatment 2 (policy change frame): ‘To what extent, if at all, do you support or oppose reducing the voting age from 18 to 16?’ ($n = 903$).
- Treatment 3 (rights then policy change frame): ‘To what extent, if at all, do you support or oppose giving 16 and 17 year olds the right to vote, which would mean reducing the voting age?’ ($n = 893$).
- Treatment 4 (policy change then rights frame): ‘To what extent, if at all, do you support or oppose reducing the voting age, so that 16 and 17 year olds have the right to vote?’ ($n = 938$).

The treatments were designed to reflect the likely framing of the debate around votes at 16, with those in favour emphasising rights and those opposed emphasising the need to change the voting age from the status quo. The treatments were also designed to be minimal interventions so that we can be more confident that any differences between groups do not result from presenting unduly strong arguments. As such, the treatments can be considered subtle forms of emphasis framing (Cacciatore *et al.*, 2016, p. 10). It was not possible to include a control group because the question wording must be formulated in some manner. Whilst this falls foul of one of Gaines *et al.*’s (2007) recommendations for the design of survey experiments, we expect that Treatments 3 and 4, which combine the two frames in different orders, will lead to levels of support in between the two singular frames.

The second survey experiment introduced another feature of the debate around votes at 16 by priming respondents to think about consistency of legal ages, with reference to two other activities denied to 16-year-olds: purchase of alcohol and marriage without parental consent. The survey was fielded to 3314 respondents (Treatment 1 $n = 824$, Treatment 2 $n = 846$, Treatment 3 $n = 813$ and Treatment 4 $n = 831$) from 20 to 23 November 2017 with the above questions preceded by questions on purchase of alcohol and marriage without parental consent.³ These treatments were designed to test whether legal age consistency arguments that are made by opponents of votes at 16 have the capacity to undermine any framing effects. This is why we did not ask about examples in which reducing the voting age would bring it into line with the current legal age, such as serving in the military. This also allowed us to keep the framing consistent across

³The full wording of the questions in the second survey experiment can be found in Appendix H.

the three questions (i.e. extending rights vs. policy change), though this repetition of the frames may have the consequence of strengthening their effects.

The experiments were run on separate omnibus surveys, so the preceding questions varied between them, which introduce the possibility of context effects. The first experiment was preceded by questions on respondents' priorities when choosing a place to live, and whether they would prefer to live in rural or urban areas. The second experiment was run on two surveys with different questions preceding it; in the first case they were on animal welfare priorities, and in the second case there was a survey experiment presenting scenarios that could constitute sexual harassment. Given that some of those scenarios described older and younger parties, it is possible that this experiment influenced respondents' answers regarding votes at 16. However, this only affected half of the sample, and it is possible that the randomisation of the older and younger elements of the preceding experiment minimised their effects on our second experiment. Finally, the samples are remarkably similar because YouGov used the same sampling frame for both experiments. There are, however, some substantively small but statistically significant differences between the samples on standard demographic and political variables, making it additionally important to control for them in our analyses.⁴

6. Main effects

Levels of support for votes at 16 in each sample are presented in [Figure 1](#) (Panel A = Experiment 1, Panel B = Experiment 2).⁵ Approaching half of the respondents are opposed whilst approximately three in ten are supportive, and the distribution of responses is remarkably similar across the two experiments (Experiment 1: *Median* = 2.00; *M* = 2.66; *SD* = 1.40. Experiment 2: *Median* = 2.00; *M* = 2.67; *SD* = 1.42). This may reflect both the similarity of the two samples and a limited impact of the additional priming considerations in the second experiment.

The mean levels of support in each treatment group are also remarkably similar across the two samples, as shown in [Figure 2](#). The rights frame (Experiment 1: *M* = 2.77, *SD* = 1.40; Experiment 2: *M* = 2.77, *SD* = 1.42) and the rights then policy change frame (Experiment 1: *M* = 2.8, *SD* = 1.42; Experiment 2: *M* = 2.76, *SD* = 1.42) have the highest mean support for votes at 16 in both experiments.

⁴Full tables are available in Appendix A.

⁵All analyses exclude respondents who answered 'Don't know' to the question on votes at 16.

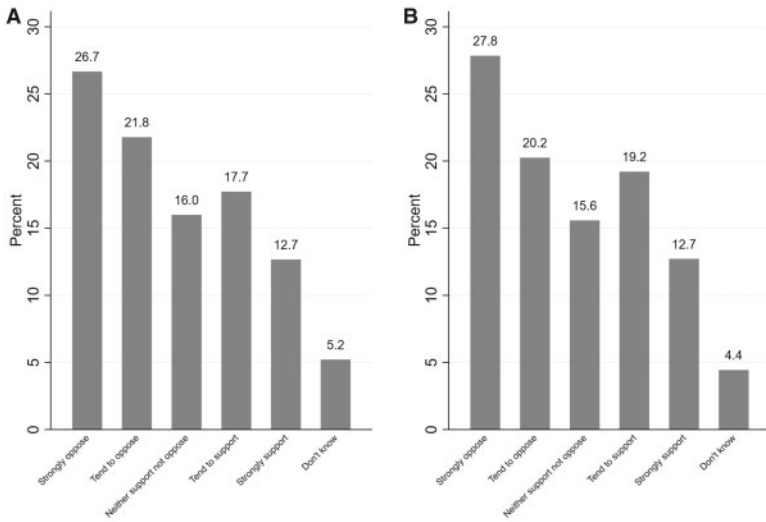


Figure 1. Support for votes at 16 by experiment. Panel A: Experiment 1 (framing on rights or policy change without prior priming on other legal ages) Panel B: Experiment 2 (framing on rights or policy change with prior priming on other legal ages).

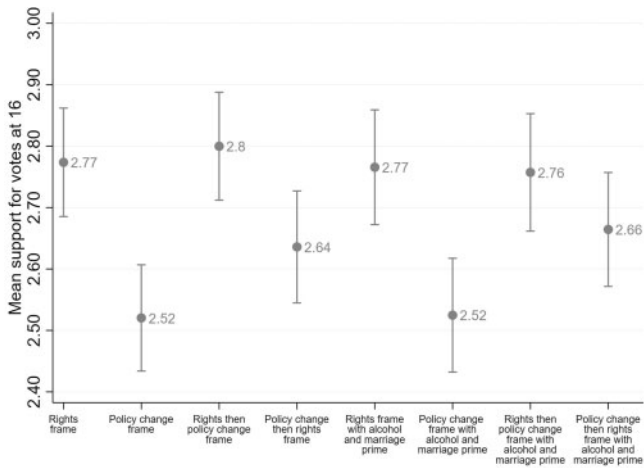


Figure 2. Mean support for votes at 16 by treatment.

The policy change frame has the lowest mean support (Experiment 1: $M = 2.52$, $SD = 1.37$; Experiment 2: $M = 2.52$, $SD = 1.44$) whilst the policy change then rights frame falls in the middle (Experiment 1: $M = 2.64$, $SD = 1.47$; Experiment 2: $M = 2.66$, $SD = 1.41$). There are clear differences between the treatment groups, in particular between the rights frame and the policy change frame. Nevertheless, the differences we observe are modest in terms of the 5-point Likert scale that

support was expressed on: the difference between highest and lowest means in the first experiment is 0.28, whilst in the second experiment it is 0.25.⁶

When regressing support for votes at 16 onto the treatment groups in the first experiment, we took the policy change then right frame as the reference category because of the middling support in that group, which provides a more conservative test of statistical significance between the treatments.⁷ Table 1 presents the unstandardised coefficients from the OLS regression models that we specified.⁸ Model 1 is a simple OLS regression with only the experimental conditions, whilst Model 2 includes age, gender, education, social grade, 2016 EU Referendum vote, 2017 general election vote, political attention and a Scottish residence binary due to that country's partial adoption of votes at 16 at the time of the fieldwork. These models employ design weights to adjust for the slight differences in numbers of respondents between the treatment groups. As a robustness check, we also treat the dependent variable as categorical and run equivalent ordered probit models, which produce similar results in terms of the direction and significance of the coefficients.⁹

The direction, magnitude and significance of the OLS coefficients are largely robust to model specification, though the addition of demographic and political variables renders the difference between Treatments 2 and 4 significant ($p < 0.05$). Given the previously observed significant relationships between treatment group allocation and some of the control variables, we favour the models including those variables, which show significant differences in support for votes at 16 between the treatment groups. Focusing on Model 2, the results indicate that, compared to framing on policy change then rights, framing in terms of rights raises support for votes at 16 by approximately 0.13 points on a 5-point Likert scale running from 'Strongly oppose' (1) to 'Strongly support' (5) ($b = 0.125$, $p < 0.05$).¹⁰ Equally in line with our expectations, framing in terms of a policy change reduces support by approximately 0.13 points ($b = -0.129$, $p < 0.05$). The difference between the rights frame and the policy change frame is also significant ($p < 0.001$), and the results provide support for Hypotheses 1 and 2. More unexpectedly, when participants are presented with both frames, the

⁶Figures showing the distribution of support for votes at 16 within treatments are available in Appendix C.

⁷The analyses were also run with different reference categories with no substantive change to the results.

⁸The full main effects OLS regression results, including the coefficients for demographic and political variables and models run with different weights, are available in Appendix D.

⁹Full ordered probit results tables are available in Appendix D.

¹⁰The original questions had 'Strongly oppose' coded as 5 and 'Strongly support' as 1, but these were reversed for the analysis so that higher values equate to stronger support for votes at 16.

Table 1 Rights versus policy change framing effects on support for votes at 16

	Experiment 1 Framing only		Experiment 2 Framing + Priming	
	Model 1 Simple OLS, design weights	Model 2 OLS with controls, design weights	Model 3 Simple OLS, design weights	Model 4 OLS with controls, design weights
Treatment 1 Rights	0.146** (0.067)	0.125** (0.059)	0.117 (0.071)	0.123* (0.063)
Treatment 2 Policy change	-0.125* (0.068)	-0.129** (0.059)	-0.148** (0.071)	-0.123** (0.063)
Treatment 3 Rights then policy change	0.175*** (0.067)	0.150** (0.059)	0.081 (0.071)	0.127** (0.063)
Treatment 4 Policy change then rights		Reference category		
<i>N</i>	3,429	3,287	3,166	3,051
Adjusted <i>R</i> ²	0.007	0.275	0.004	0.254

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

order in which they appear affects support. Compared to the policy change then rights frame, the rights then policy change frame raises support by approximately 0.15 points ($b = 0.150$, $p < 0.05$), suggesting that the rights frame may carry a primacy effect.

The results from the second experiment, which include priming on legal consistency, largely replicate the findings from the first experiment, with some caveats (see right-hand side of Figure 2 and Table 1, Models 3 and 4).¹¹ Despite the identical specification of the models and the similarity of the samples, the significance of the difference between the rights frame and policy change then rights frame does not hold in the second experiment. Mean support in the policy change then rights frame (the reference category) increased by 0.05 points, which may explain the loss of significance between Treatments 1 and 4. By contrast, the difference between the rights frame and the policy change frame retains significance ($p < 0.001$). Thus, the difference between framing on rights and framing on policy change persists across the two experiments, whilst the difference between

¹¹As an additional robustness check, we randomly removed cases to equalise the size of the treatment groups and the samples (which were reweighted) for the two experiments, before running the main effects models again. The full regression tables for these robustness checks are available in Appendix E.

framing on rights and framing on policy change then rights is no longer significant when preceded by priming on other legal ages. When compared to framing on policy change before rights, framing on policy change continues to have a significant negative effect and lowers support by approximately 0.12 points ($b = -0.123$, $p < 0.05$). The positive effect of framing on rights then policy change, which raises support by 0.13 points ($b = 0.127$, $p < 0.05$), also retains significance when controls are included. We again favour the models including controls, given that some of them relate significantly to treatment group allocation. Thus, priming on consistency of legal ages in the second experiment does not appear to have an impact on support for votes at 16 or, greatly, on the framing effects. The direction and magnitude of coefficients remain similar in most cases and the rights frame continues to prompt higher support than the policy change frame.

Overall, we observe consistent framing effects across both of our experiments. Throughout our analyses, our reference category framed votes at 16 in terms of policy change then rights. Compared to this, framing on policy change alone consistently reduces support for votes at 16, regardless of the inclusion of demographic and political controls or the particular weights applied. Framing in terms of rights then policy change significantly raises support across all models in the first experiment, and models including controls in the second experiment. Finally, framing in terms of rights alone significantly raises support for votes at 16 across all models in the first experiment but not in the second experiment. Nevertheless, the direction and magnitude of the coefficients are consistent across the experiments, as is the significance of the difference between the rights frame and the policy change frame. These results are supportive of Hypotheses 1 and 2 and demonstrate that the public are willing to move their position on votes at 16, even when interventions on the topic are relatively weak. However, the consistency of the results across the two experiments indicates that priming on consistency of legal ages does not impact on support for votes at 16, or the observed framing effects, counter to Hypothesis 3. Legal consistency arguments prime respondents regarding the responsibility, and protection, of young people, which we might expect to have a strong effect. This makes the lack of difference in levels of support for votes at 16 across the two experiments all the more surprising.

7. Interaction effects

In line with applicability effects (Cacciatore *et al.*, 2016), we expect different frames to be meaningful for different respondents. As noted previously, given the observed differential effects of other experimental treatments on conservatives and liberals (e.g. Marx and Schumacher, 2016; Lammers and Baldwin, 2018), we expect ideology to moderate the framing effects (Hypothesis 4), with left-wingers more receptive to the rights frame (Hypothesis 4a) and right-wingers more

receptive to the policy change frame (Hypothesis 4b). Our data include a 7-point self-placement scale running from ‘Very left-wing’ (0) to ‘Very right-wing’ (6), which we use as our main indicator of ideology despite incomplete coverage. In terms of baseline support amongst ideological groups, if we code the variable into a binary (excluding those in the centre) then we see that, as expected, left-wingers are more supportive (Experiment 1: $M = 3.42$, $SD = 1.34$; Experiment 2: $M = 3.33$, $SD = 1.40$) than right-wingers (Experiment 1: $M = 1.88$, $SD = 1.14$; Experiment 2: $M = 1.92$, $SD = 1.19$) and this difference is significant (Experiment 1: $t(1,885) = 26.98$, $p < 0.001$; Experiment 2: $t(1,727) = 22.82$, $p < 0.001$).¹² We also test for moderation effects using respondents’ votes in the 2017 UK general election and in the 2016 referendum on the UK’s membership of the European Union, with similar results.¹³

Table 2 reports the results of models including ideology and its interaction with the treatments across both experiments.¹⁴ The Ideology Models add the left-right self-placement scale without interactions, whilst the Interaction Models include the interactions between the treatments and left-right self-placement.¹⁵ First, across both experiments, we find that left-wingers are more supportive than right-wingers of votes at 16 (Models 5 and 7). However, we can no longer simply interpret the main effects of the variables included in the interaction terms because the coefficients represent the effect when the other interacting variables have values of zero (Brambor et al., 2006, pp. 71–73). In this case, taking the policy change then rights frame as the reference category, the coefficient for the rights frame ($b = -0.927$, $p < 0.01$) indicates that it has negative effect amongst ‘Very left-wing’ respondents (coded as 0). However, the coefficient for the associated interaction term ($b = 0.264$, $p < 0.01$) indicates that the negative effect declines as the value of left-right self-placement increases (moving from left to right). As the Panel A of Figure 3 shows, this effect is such that right-wing respondents actually increase their support for votes at 16 when exposed to the

¹²Support in different ideological groups is graphed in Appendix F.

¹³Both 2016 EU referendum vote and 2017 general election vote are likely to encompass non-ideological effects such as those stemming from partisan and strategic considerations, as well as ideological considerations that may not be captured by the left-right self-placement scale. The likely opposition of 16- and 17-year-olds to the Conservative Party and Brexit suggests that supporters of those causes will be more likely to oppose extending the franchise on partisan and strategic grounds. Such considerations should provide no countervailing force to ideology, though we cannot disentangle these effects. The results of the interaction models with the vote variables are available in Appendix D.

¹⁴The full OLS regression results tables for the interaction models can be found in Appendix D.

¹⁵Again, we replicated the interaction results on our two samples with a random selection of cases removed to equalise the size of the treatment groups and the two samples (both reweighted). Full results tables for these robustness checks are available in Appendix E.

Table 2 Rights versus policy change framing effects on support for votes at 16 across ideology

	Experiment 1 Framing only		Experiment 2 Framing + Priming	
	Model 5 Ideology model	Model 6 Interaction model	Model 7 Ideology model	Model 8 Interaction model
Treatment 1	0.136**	-0.457***	0.103	0.023
Rights	(0.066)	(0.153)	(0.071)	(0.160)
Treatment 2	-0.128*	-0.602***	-0.140**	-0.096
Policy change	(0.067)	(0.159)	(0.071)	(0.159)
Treatment 3	0.136**	-0.059	0.113	0.102
Rights then policy change	(0.067)	(0.153)	(0.071)	(0.158)
Treatment 4		Reference category		
Policy change then rights				
Left-Right	-0.260***	-0.364***	-0.240***	-0.242***
	(0.022)	(0.037)	(0.024)	(0.038)
L-R ⁺ Treat1		0.199***		0.030
		(0.046)		(0.051)
L-R ⁺ Treat2		0.160***		-0.016
		(0.049)		(0.050)
L-R ⁺ Treat3		0.058		0.003
		(0.047)		(0.049)
Conservative Vote 2017	-0.435***	-0.426***	-0.085	-0.090
	(0.089)	(0.089)	(0.096)	(0.097)
Labour Vote 2017	0.103	0.121	0.342***	0.342***
	(0.087)	(0.087)	(0.094)	(0.095)
Leave Vote 2016	-0.257***	-0.241***	-0.383***	-0.383***
	(0.060)	(0.060)	(0.065)	(0.065)
<i>N</i>	2,439	2,396	2,291	2,247
Adjusted <i>R</i> ²	0.314	0.319	0.283	0.283

Note: Entries are OLS coefficients with standard errors in parentheses; estimates based on design-weighted data.

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

rights frame. Indeed, each move right along the ideological scale is associated with lower support for votes at 16 and also with a less negative, and eventually more positive, effect of the rights frame.

Still compared to the policy change then rights frame, the picture with regard to the policy change frame is similar. The negative effect of the treatment amongst 'Very left-wing' respondents ($b = -1.040$, $p < 0.05$) reduces as respondents move from left to right (interaction $b = 0.228$, $p < 0.01$). Panel B of Figure 3 shows that there is a clear negative effect of the treatment amongst the most left-wing respondents, which flattens to essentially no effect amongst the most right-

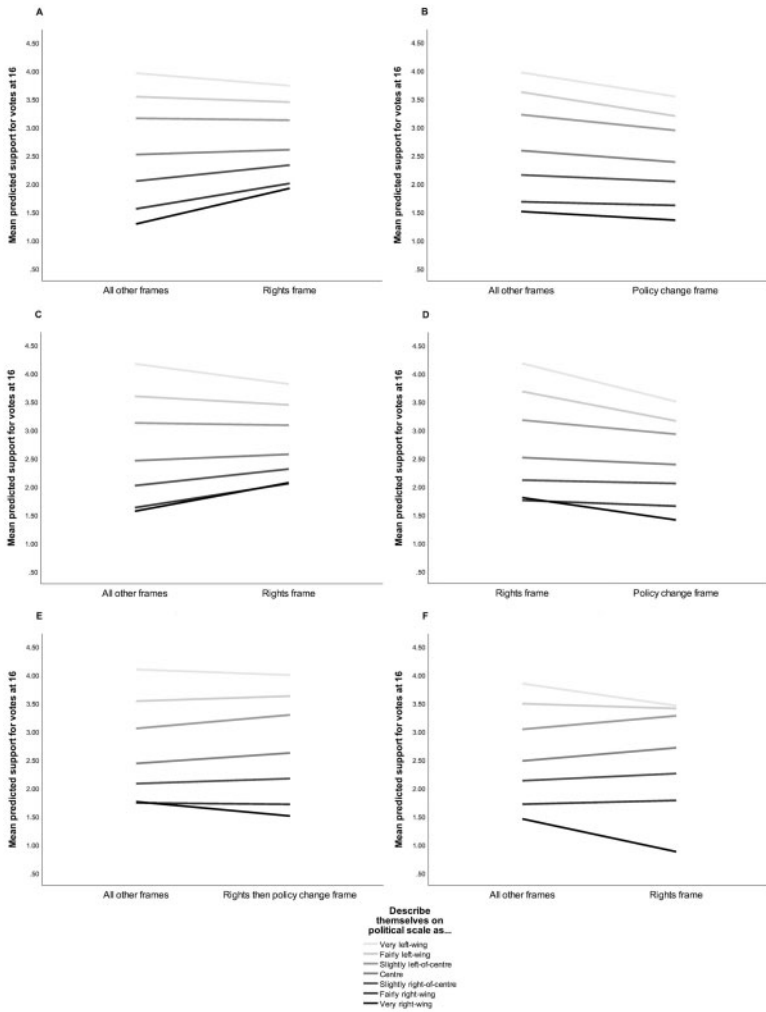


Figure 3. Ideology and ideology quadratic interactions with treatments in Experiments 1 and 2. Panel A: Rights frame interacted with ideology, Experiment 1. Panel B: Policy change frame interacted with ideology, Experiment 1. Panel C: Rights frame interacted with ideology squared, Experiment 1. Panel D: Policy change frame interacted with ideology squared, Experiment 1. Panel E: Rights then policy change frame interacted with ideology squared, Experiment 1. Panel F: Rights frame interacted with ideology squared, Experiment 2.

wing respondents. By contrast, the coefficients relating to the rights then policy change frame are insignificant, perhaps stemming from the reduced number of cases due to missing values on left-right self-placement. Despite this, it is clear that ideology interacts with the rights frame and the policy change frame to produce a negative effect amongst left-wingers and a positive or no effect amongst right-wingers. When we instead interact the treatments with ideology squared

(which allows for different treatment effects at the extremes of ideology), the effects of the rights frame are broadly replicated (Figure 3, Panel C) but the effect of the policy change frame (Panel D) becomes negative amongst very right-wing respondents. Further, the rights then policy change frame (Panel E) also has a negative effect amongst the very right-wing, suggesting that such respondents will reduce support for votes at 16 if presented with any frame but rights alone.¹⁶

Turning to the interaction effects in the second experiment (see Table 2) and using the same reference category (the policy change then rights frame), the story is remarkable for its absence of significant results. The only exception to this is the interaction of ideology squared with the rights treatment (Figure 3, Panel F), which again shows a markedly negative effect amongst the very right-wing. The weaker and less consistent interaction results stemming from the second experiment point in at least two possible directions.¹⁷ First, the results may indicate that the framing effects are moderated by the preceding primes relating to the consistency of legal ages in the UK. Secondly, the results may stem from the differing survey contexts in which the two experiments appeared, which were outlined in the methodology section. That said, no dramatic context effects were observed in relation support for votes at 16, or the direction or magnitude of the main effects coefficients, though this does not rule out their possible impact on interaction effects.

The interaction effects emerging from our first experiment, then, are rather more striking than those emerging from our second experiment. In the former case, respondents on the ideological right have a consistently and significantly more positive response to framing the issue in terms of rights, or policy change, than do respondents on the ideological left. Framing on rights has a weaker positive, or sometimes negative, effect amongst left-wingers but a strikingly positive effect amongst right-wingers whereas framing in terms of policy change has a weaker negative effect amongst right-wingers than it does amongst left-wingers. The higher levels of support for votes at 16 amongst left-wingers and lower levels of support amongst right-wingers means that they have more scope to adjust their support down or up when confronted with negative or positive frames. Nevertheless, they do not have to respond as such, and the consistency of the results indicates a capacity to adjust positions on votes at 16 on both sides of the ideological divide. The effect of framing on rights then policy change is less

¹⁶Full ideology squared interaction results are available in Appendix G.

¹⁷A third possibility, that the results differ due to slightly different sample and treatment group sizes, does not seem to be the case on the basis of the robustness check outlined in Footnotes 11 and 15, in which we removed random cases to equalise the treatment group sizes and re-ran our models, with results presented in Appendix E.

striking, though perhaps this is unsurprising given that it is the closest treatment to the reference category, which frames in both ways but in the opposite order.

8. Discussion

Public opinion research on the votes at 16 debate demonstrates a lack of support for the initiative amongst the mass public (Birch *et al.*, 2015). Our study corroborates these findings: across two survey experiments, we find that nearly half of respondents either oppose or strongly oppose extending the franchise to 16-year-olds. However, as the issue lacks salience in public discourse, public opinion on votes at 16 is not solidified. Our main interest was to examine whether public opinion is malleable, depending on the way the issue is framed amongst elites and the media. To test our hypotheses, we manipulate whether votes at 16 are framed in terms of extending rights or as a change from the policy status quo (i.e. reducing the voting age). We find evidence in line with our main hypothesis that when the issue is framed in terms of extending rights, support for votes at 16 is higher than when the issue is framed in terms of policy change. Presenting respondents with a combination of these frames produces intermediate levels of support when the policy change frame precedes the rights frame. However, there is some support for the notion that when presented with the rights frame first; it elicits a primacy effect that leads to higher levels of support than the policy change frame conditions. It appears that the public is more willing to extend the franchise when the debate is framed in terms of democratic rights; however, when the debate is framed in terms of policy change, public support is depressed. We further examined whether priming the public on legal consistency arguments reduces support for votes at 16. Our results do not provide any evidence that this is the case. Asking respondents about other activities that 16-year-olds are not legally allowed to do, such as buying alcohol or marrying without parental consent, did not depress support for votes at 16.

We find that left-wingers are more supportive than right-wingers of votes at 16, though, interestingly, the impact of the frames across ideology did not bear out our predictions: left-wingers were not particularly persuaded to support the initiative by the rights frame and right-wingers were not particularly dissuaded to support the initiative by the policy change frame, except for the very right-wing. Indeed, it appears that framing votes at 16 as a matter of extending rights is a successful means of garnering support on the right. This may be due to the broad acceptance of liberal democratic norms in the UK. According to Cacciatore *et al.* (2016), frames that incorporate ‘culturally shared schemas among audiences’ (p. 14) may be able to elicit similar responses, despite individual-level differences. In this case, the rights frame brings the opinions of left- and right-wingers closer together. This finding is also in-line with work that shows that right-wingers can be persuaded to support liberal causes when issues are framed in a way that

comports with their underlying values and beliefs (Lammers and Baldwin, 2018). However, we also found that when presented with the policy change frame, either before or after the rights frame, or primes on legal consistency considerations, support was decreased amongst the very right-wing. As previous research suggests (e.g. Marx and Schumacher, 2016), people often respond to frames that are most in-line with their prior beliefs. It appears that most arguments against votes at 16 are more in line with very right-wing underlying views than are arguments for broadening political participation.

An important limitation of our study design is that it leaves out information that the public would likely use in the real world to help them determine their opinions on the votes at 16 debate, such as partisan cues. Whilst we control for party vote in our models, we cannot tease apart the relative role that party cues versus framing effects would play in the shaping public opinion, or whether party cues would trump ideological considerations. We expect that ideological considerations and party cues are bound together, making it difficult to fully disentangle their independent effects. Nonetheless, in the real world, certain frames may be more forceful in shaping the public debate, which can have important implications for those with an interest in the issue.

These findings highlight the power of elite framing to sway public opinion on socio-political issues. In particular, for stakeholders in the votes at 16 debate, our results show that members of the public who may initially be disinclined to support extending the vote can be persuaded by appealing to democratic norms.

Data availability statement

The data generated for this study, along with the replication files (SPSS syntax and STATA do-files) and full appendices, are available via Pure, the University of Strathclyde Research Management System. DOI: 10.15129/035e1ef3-763c-4dd9-9e0c-92aa4993c416.

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Conflict of Interest

The authors declare that they have no conflicts of interest relating to this article.

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