Re-Thinking Turnout

Explaining Within-Individual Variation in Electoral Participation

James Dennison

Thesis submitted for assessment with a view to
obtaining the degree of Doctor of Political and Social Sciences
of the European University Institute

Florence, July, 2017

European University Institute
Department of Political and Social Sciences

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I James Dennison, certify that I am the author of the work 'Re-Thinking Turnout: Explaining Within-Individual Variation in Electoral Participation' I have presented for examination for the Ph.D. at the European University Institute. I also certify that this is solely my own original work, other than where I have clearly indicated, in this declaration and in the thesis, that it is the work of others.

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in a scientific manner, even if my natural predisposition might have been to get swept up in the hype, propaganda and hyperbole that surround both politics and academia. That made adapting myself to social science far easier and made me a better social scientist. It was great catching up on the progress of the thesis and other academic things so regularly, which also helped keep me grounded and focused. Moreover, he consistently gave me a simple but perhaps underrated piece of advice throughout the four years – 'just get it done, lad!' Finally, it is my Mum who knows me better than anyone and who convinced me that academia is right for me and that embarking on the PhD was such a worthwhile, positive choice. Keeping her informed on how things were progressing spurred me on and made me proud. She was always there to truly listen and provide fresh encouragement and advice. She gave me a sense of assurance in myself and in the value of what I was doing, without which the whole endeavour would have been impossible.

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Abstract

This thesis presents four essays that aim to explain within-individual variation in voter turnout. The motivation behind this thesis is not only the importance of voter turnout to democracy, both in theoretical and substantive terms, but also the methodological and theoretical weaknesses in the existing literature caused by the lack of attention given to why individuals vote at some points in their lives and not at others. This deficit stands in contrast to the vast literature explaining aggregate-level turnout – both within and between countries– and individual-level turnout solely between individuals. Each of the four essays seeks to re-think one of the explanatory models of individual-level voter turnout – mobilisation, resources, psychology and socialisation - by applying many of their determinants to within-individual variation, as well as, in some cases, adding new ones. The methodological approach to explaining within-individual variation is to use fixed effects panel data models, as well as intermittently random effects models, cross-sectional models and structural equations. The data for these models comes from the British Household Panel Survey, the Swiss Household Panel Survey and the British Election Study. This thesis makes a number of theoretical, methodological and substantive contributions. I show that within-individual variation in voting seems to be fairly unaffected by such issues as material resources, 'anti-political' sentiments, household politicisation or even feelings of personal ability to vote effectively – all of which have been mainstays of the between-individual literature. Rather, I conclude that individuals vote when they are interested in the politics of the time, feel affinity towards a party or when a party has bothered to contact them, on the one hand, and, more fundamentally, by the lifestyle of the individual at the time of the election – with lifestyles built on rootedness, social integration and roles demanding responsibility increasing the individual's desire to turnout to vote.

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Introduction

Voter turnout is one of the most consistently studied phenomena in political science. This is partly because of turnout's normative and substantive implications. Academics have given at least three major reasons for the on-going importance of turnout for democracy. First, turnout has been argued to be a barometer of democratic norms, with higher turnout indicating a more engaged and capable citizenry as well as greater demand for the political supply (Teixera, 1992; Dalton, 2007; Norris, 1999). Second, higher turnout aids democracy in performing its functions of ensuring legitimacy, accountability and representation (e.g. Franklin, 2004). Third, turnout has substantive policy effects; for example, scholars have shown that social spending is positively affected by aggregate turnout (Larcinese, 2007).

Unsurprisingly, there is a vast amount of research into the causes of variance in voter turnout, which Franklin (2004) calls the 'grand enchilada' of political science puzzles. Indeed, beyond the implications of turnout for democratic governance, the on-going inability of academics to produce conclusive models to explain either individual-level or aggregate-level voter turnout and the continuing emergence of contradictory test results have resulted in hundreds and possibly thousands of studies. In his meta-analysis of both cross-sectional and cross-time aggregate-level empirical studies into voter turnout, Geys (2005: 637) describes the amount of scholarly attention given to turnout as 'enormous' before reviewing 83 studies that respectively consider the effects of six socio-economic variables, three political variables, such as election closeness, and four institutional variables, such as registration rules. Part of the need for meta-analyses is that 'none of these [variables] … is omnipresent in the literature' (Geys, 2005: 641) due to studies tending to focus on 'some propositions on turnout' focusing on the effects of one variable (Hoffman-Martinot et al, 1996: 242).

Smets and van Ham (2013) built on Geys' aggregate-level meta-analysis by offering a meta-analysis of the determinants of turnout at the individual-level. Practically all of these studies were cross-sectional. They state (2013: 1) that 'despite decades of

research aiming to understand what drives citizens to the polls, the jury is still out on what the foundations of micro-level turnout are.' They review the findings of 90 studies between 2000 and 2010 that test the effects of 170 variables, together constituting six models of individual-level variation in voter turnout – rational choice, mobilisation, resources, psychology, socialisation and political-institutional. Like Geys, they note 'that authors rarely include the same control variables in their models [which] possibly leads to underspecified models and spurious inferences' (2013: 13).

Given the scale of meta-analyses at the both the aggregate- and individual-levels, even spanning only recent years, the turnout literature might appear to suffer from a surfeit rather than deficit of research. However, both of the aforementioned meta-analyses note instead the insular and patchy nature of both research streams, lamenting a tendency for both empirical and theoretical under-specification. I note a further, and perhaps more fundamental, flaw in the current literature. Whereas aggregate-level turnout has been studied in terms of variation both between and within (i.e. over time) countries and other political units, individual-level variation has been almost solely considered between individuals, rather that within them. This is the result of both the theoretical underpinnings of the voter turnout literature and methodological and data opportunities for research until recently.

The starting point for most explanations of individual-level voter turnout has been Riker and Ordeshook's (1968) calculus of voting. They showed that because the probability of a single vote affecting an election result is trivial, the instrumental benefits of voting (P(B)) are lower than the costs of voting in terms of time and effort (C). Therefore, a non-instrumental benefit of voting must be presumed (D) if voting is to have a net-positive outcome for individuals in terms of utility (P(B) – C + D > 0). Since then, vast swathes of studies have focused on which sections of the population are likely to have the 'D'. Such a search has had two major consequences for the literature. First, the majority of studies have treated the decision to vote or not as a function of a characteristic (the 'D') that is endogenous to the individual and unresponsive to external stimuli. Second, the 'D' has been presumed to be time-constant and as such not varying in individuals. As a result, there has been relatively limited research on why and how a particular individual's voter participation changes

longitudinally. Indeed, 'the bulk of the literature on individual turnout is based on cross-sectional survey data and, therefore, focuses on the impact of factors influencing the *level* of voter turnout at a given point in time or in a given contest (Tawfik et al, 2012: 352).' The absence of longitudinal individual-level studies has led researchers to presuppose longitudinal aggregate-level variance to be the result of new (and inherently different) voters coming into the electorate and old ones leaving.

These two consequences of the calculus of voting on the literature were exacerbated by the observation that, from the 1970s onwards, there was an apparent decline in voter turnout across advanced democracies. This phenomenon reinforced Riker and Ordeshook's emphasis on endogenous and unchanging citizen's normative attitudes because the seemingly consistent decline fitted well with the idea that aggregate-level variance is a function of new voters entering the electorate and old ones leaving it. Accordingly, increasing numbers of academics moved away from explaining voter turnout *per se* and instead attempted to account for decline (e.g. Franklin, 2004, Blais et al., 2004, Blais and Rubenson, 2012, Dalton, 2007 and Wattenberg, 2002, 2007). Generational replacement as an explanation hit something of an empirical wall at the end of the 20th century, when advanced democracies including the US, UK, France, Canada, Switzerland, Sweden, Japan and Germany experienced increases in voter turnout between at least two elections and a general levelling-out of turnout throughout the 21st century. Despite the fact that decline in voter turnout has halted, aforementioned studies such as Blais and Rubenson (2012) showcase the literature's persistent focus on *declining* voter turnout as opposed to *fluctuating* voter turnout.

However, the lack of within-individual studies is not only the result of a theoretical basis that assumes relatively stable predictors of voting within the individual, with variance mostly explicable by generation. To reliably study change within the individuals across time requires repeatedly observing those individuals with panel data. This is particularly problematic for studies of turnout given the time between general elections. To even have a two wave panel requires maintaining contact with citizens between two elections for four or five years. Moreover, certain predictors such as resources that have been relied on to explain between-individual variation are likely to only see high levels of within-variation over the course of decades.

Sufficiently lengthy panel data sets and the techniques to test these sets have only recently become available. For instance, in a recent longitudinal individual-level study, Spahn and Hindman (2014: 3) use panel data to create cross-tabulations between voting in consecutive elections to test 'the extent to which ostensibly high-propensity or low-propensity voters participate in or abstain from the same set of elections'. They attempt to test only the effect of turnout in one election on the next election, finding it to be a surprisingly poor predictor. Tawfik et al (2012) test the causes of voting in the canton of Geneva in both elections and referenda, locally and nationally, using a panel data. They find effects for age, period and generation. However, they note that their model is weakened by the omission of socio-economic variables, attitudinal variables and electoral variables such as closeness and campaigning.

The omission of studies testing within-individual variation – only highlighted by the glut of between-individual and within- and between-country studies – has important theoretical and methodological ramifications for the findings of the extant literature. Explaining the decision to vote by analysing which individual votes is distinct from doing so by analysing when an individual votes. For example, if turnout is a function of resources such as wealth, as Verba et al (1995) argue, we should expect individuals to be more likely to vote when they have greater wealth. Indeed, Verba et al make this argument by showing that, when controlling for other factors, richer individuals are more likely to vote. In basing their within-individual causal mechanism on between-individual variation, they risk the 'reverse ecological fallacy' (Hofstede, 2001). Not only can causal mechanisms at one level be not repeated at another level of analysis, but also they can be opposite. It may be that a wealthier individual *is* more likely to vote than a less wealth one, as a result of unobserved factors such as peer group and upbringing, yet, simultaneously, individuals are less likely to vote *when* they are wealthier because sudden drops in wealth mobilise individuals to express their grievance against the incumbent government. Many of the key causal mechanisms in individual-level research that would most appropriately be tested by within-individual variation have been tested instead by analysing between-individual variation, based on the assumption that the observed variables in such models capture the effective

factors of turnout, leaving any remaining variation between individuals random and, thus, unimportant. This assumption is unlikely to be met.

Given the central importance of voter turnout to democracy and the almost complete lack of research into one of the four potential avenues for explaining electoral participation – limiting the theoretical and methodological robustness of existing research – this thesis aims to re-think the causes of turnout by explaining within-individual variation. Substantively, I use the division of current individual-level literature offered by Smets and van Ham (2013) to structure the essays that comprise this thesis. As already mentioned, they divide existing research into six models: rational choice, mobilisation, resources, psychology, socialisation and political-institutional. The foundation of the first of these, the rational choice model, is already described above in terms of the calculus of voting. As we can see from the variables that Smets and van Ham place within this explanatory model - sense of civic duty, feeling personal benefits from voting and economic evaluation - rational choice explanations are substantively disparate, reflecting the single pre-cursor of the calculus of voting that individuals vote when faced with *any* non-instrumental benefit that outweighs the costs of doing so. As such, these factors can often more intuitively be placed within other models – such as economic evaluations within the resources model, sense of duty within the psychological model and closeness of the election within a political-institutional model. As such, rather than dedicate an entire paper to the rational choice model, I refer to it repeatedly throughout the other essays within this thesis.

The mobilisation model is far more self-contained and, compared to other models, largely exogenous to the individual. This model argues that 'people participate in electoral politics because someone encourages or inspires them to take part' (Rosenstone and Hansen, 1993: 161). The actors that have been supposed to encourage or inspire individuals to vote include parties and candidates, non-partisan Get-Out-The-Vote campaigns, media and religious and trade union organisations. By contrast, the resource model of voting is entirely endogenous to the individual. The resource model essentially argues that individuals vote if and when they have the capacity to do so. Most famously, this was argued by Verba et al (1995) as

encapsulated by skills, money and time, and this model is the most tested of all six. Other so-called resources that have been hypothesised as predictors of turnout include gender, race, marital status, residential mobility, geographical location, home ownership, occupational type, socio-economic class, parenthood and generation. One of the major advantages of this model is the non-political nature of many of the variables precluding risks of reverse causality. Conversely, their distal positioning from turnout and the explanatory power that that such fundamental socio-demographic have on other correlates of turnout makes tests of this set of variables highly subject to omitted variable biases.

Far more proximal, and thus susceptible to biases such as those caused by reverse causality and simultaneity, are psychological factors that measure feelings, opinions toward and understanding of politics, political actors and elections. Alongside resource-based accounts, psychological factors are some of the more often tested determinants of turnout – including partisan identification, political and electoral interest, external and internal political efficacy, political knowledge, trust and satisfaction with democracy. Less, though increasingly, considered are those variables that comprise a socialisation model of turnout, which argues that turnout is driven by the behaviour, characteristics and attitudes of those in the individual's life. Socialisation factors include upbringing, the turnout of cohabitees, neighbourhood turnout (and other highly local levels of turnout), and quantity and quality of political discussion. Like psychological factors, these predictors are highly vulnerable to reverse causality, and, like resource based accounts, to draw inferences on within-individual causation from between-individual studies is to make ones findings susceptible to omitted variable biases primarily owing to the homophilic tendencies inherent in the formation of social circles.

Finally, the politico-institutional model argues that turnout is a by-product of the political context and institutional framework of the time and place. Explanatory variables from this model include the closeness of the election, registration rules, the electoral system and the nature of the party system. Perhaps unsurprisingly, these explanatory factors have been investigated to a greater extent at the aggregate-level and, indeed, this thesis fails to consider them, aside from that of closeness, which is

used as a control to test the robustness of the effects of partisan mobilisation. Admittedly, this is a major omission from the thesis, the results of which become overwhelmingly focussed on the demand-, rather than supply-, side. As I note in the conclusion, rectifying the balance of the two when explaining within-individual variation in turnout will be a particularly exigent next step for research.

Methodologically, the essays primarily rely on the use of panel data and fixed effects panel models to test that data. Such models test the effects of change in the independent variables on change in the dependent variable within the individual while excluding all time-invariant independent variables and time-invariant individuals.[1] While it would be possible to test panels using models that do not drop such variables and observations, such as random effects models, I consider the assumptions that they make – such as randomly distributed error terms – to be unreasonable. This, coupled with the endogeneity and omitted variable bias issues that are already rife in the findings of the literature, lead me to conclude that the loss in efficiency caused by the fixed effects transformation is a price worth paying for the greater robustness of the findings that result from it. However, where I deem it appropriate – primarily as a means to bring in time invariant factors and turnout invariant individuals - I test random effects models and cross-sectional models intermittently in addition to the fixed effects models. Throughout the thesis the results of these models are presented in full, accompanied by graphically displayed marginal effects where appropriate.

[1] This means that only those individuals who vary in turning out to vote in the course of the respective panel are included in the models. The implications for the explanation of turnout of not focusing on consistent voters and consistent non-voters should be elaborated given that, both theoretically and practically, this is a major step in the specification of the research design. First, is this a problem theoretically? I argue that it is not. Though the panel datasets on which the analyses in this thesis rely do not cover the entire life courses of individuals, meaning that we cannot be sure whether these individuals *ever* vary in voting, for the years considered, the data suggests that those individuals that always or never vote do not vary in turnout. As such, in the case of these individuals – of which there are certainly many in the general population - there is no phenomenon to be explained. Just as attempting to explain between-individual turnout amongst a group of individuals who all vote would be both methodologically impossible and theoretically nonsensical, so too would be attempting to explain within-variation amongst individuals that always or never vote. In future, one may consider analysing the causes of within-variation in propensity to vote, a phenomenon that is more likely to vary and is captured by a continuous variable, unlike the binary actual voter turnout variable. This would then capture of the effects of independent variables that change the individual's chance of voting, yet do not push that chance over the individual's threshold of turnout or abstention.

Second, is this a problem methodologically? The most intuitive way in which it would be a problem is by creating a selection bias that sees the within-varying sub-sample appearing considerably different from the general population. However, in theoretical terms it is debatable whether this is actually an issue. Indeed, ideally, the within-varying subsample should replicate not the entire general population but the entire within-varying population, which, given the lack of any authoritative data on that group, could only be checked by comparing these survey with others. Alas, I demonstrate in the appendix to this thesis the descriptive differences between the entire sample of each of the two long-term data sources (BHPS and SHP) and their respective within-varying subsamples, showing that the differences are minor. On the other hand, in the short-term panel (BES), there are substantial differences, with the within-varying sample exhibiting traits that we would expect from less politicised individuals, which we can expect the short-term within-varying population to hold.

In total, the papers in this thesis use three sources of data. The first is the British Household Panel Survey (BHPS) and its successor the Understanding Society survey. One of the main advantages of the BHPS is its length, having started in 1991 and comprising 18 waves until it was integrated into the newer and broader Understanding Society survey. The data comes from annual interviews of all adults in a household and observes between 9,000 and 13,000 individuals per wave. The only socio-economic panel in Europe with greater length is the German Socio-Economic Panel (SOEP), which, unfortunately, lacks data on turnout. Similarly, while the BHPS does include data on turnout, it only includes a handful of other political variables – regarding party choice, partisan identification, political interest and external efficacy. By contrast, the panel includes hundreds of socio-economic variables, making it ideal when considering the effects of resources on turnout, and links respondents to other members of their households and to family members. Together, so far, the BHPS and Understanding Society surveys cover five general elections in the UK (1992, 1997, 2001, 2005 and 2010). Where appropriate, when using this data source I regress retrospective turnout from the year after the election on independent variables drawn from the year of election. This offers two advantages. First, it logically makes sense because the retrospective turnout variable refers to an action that was concurrent with variables from before the election. Second, by taking variables from differing waves I preclude reverse causality and substantially reduce the risks of simultaneity bias.

I also use a second European socio-economic panel. The shorter – though, in terms of political variables, richer – Swiss Household Panel (SHP). The Swiss Household Panel started in 1999 and is on-going. Until 2004, it contained 5,074 households containing 12,931 individuals. In 2004, 2,538 households of 6,569 individuals were added. Information on region and canton are available. Although the SHP does not include data that measures turnout at any level of government, it does ask the somewhat analogous though inferior hypothetical question of how many federal polls out of ten the respondent takes part in. Because of this omission, I rely on the SHP far less than the BHPS, except for when testing the effects of those political variables that are available in the former and not the latter relating to political trust and satisfaction with democracy.

The third data source used in the essays of this thesis is the British Election Study's 2014-2017 internet panel. This is an exemplary short-term panel for four reasons. First is its length, covering nine waves (and counting) between February 2014 and September 2016. Second is the richness of the data it captures, which offers a comprehensive view formed by hundreds of variables regarding the respondent's socio-demographic characteristics, political attitudes, social group and the political context in which they live, such as contact by parties. Third, the BES includes multiple elections and referendums, including the 2014 European Parliament elections, the 2014 Scottish Independence Referendum, the 2015 UK General Election, the 2016 Scottish Parliament Election and the 2016 referendum on EU membership. Fourth, the BES includes an impressive 57,535 individuals.

The thesis proceeds as follows. I present four essays that each re-considers one of the six models of voter turnout in terms of within-individual variation. The models considered, in order, are the mobilisation model, the resources model, the psychological model and the socialisation model. I then offer conclusions that tie the results together and place them within the broader voter turnout literature.

Partisan Mobilisation and Voter Turnout

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The positive effect of mobilisation on voter turnout has been well established by a number of seminal studies in the electoral behaviour literature. However, these studies have been heavily skewed towards investigating the effects of American, non-partisan 'get-out-the-vote' campaigns, in spite of the far greater prevalence of partisan campaigns. Moreover, the analytical robustness of the existing studies has rarely been accompanied by theoretical reasoning as to why mobilisation increases turnout. In this paper, I use fixed effects models from British panel data to test the predictors of voting in six types of election and make four contributions. First, I show that *partisan* contact has a similarly sized effect on turnout in five types of British elections to the *non-partisan* effects found in previous studies of US elections. Second, I consider whether contact increases one's chance of voting because it prompts, persuades or provokes the individual, finding that it mostly increases turnout because it convinces citizens that there is something for which it is worth voting. Third, I find no evidence from the 2015 UK General Election to support the findings of previous studies that personal forms of party contact are especially impactful. Finally, I find that more marginal constituencies have greater turnout because of greater partisan mobilisation.

Introduction and literature

Scholars have taken an increasing interest in the effects of mobilisation on voter turnout since the turn of the millennium. The basic causal mechanism at the heart of this stream of literature is that 'people participate in electoral politics because someone encourages or inspires them to take part' (Rosenstone and Hansen, 1993: 161). Recent studies considering the relationship between mobilisation and turnout have tended to follow on from Gerber and Green's (2000) finding that the effects of 'personal' (face-to-face) mobilisation techniques are stronger that 'non-personal' techniques (such as calls from telephone banks and direct mail). Gerber and Green's study, which confirmed early investigations that also used field experiments (Elderveld, 1956; Gosnell, 1927; Miller, Bositis and Baer, 1981), was highly impactful on the subsequent literature on mobilisation for two reasons. First, it offered an interesting theoretical contribution to the puzzle of why aggregate-level voter turnout declined in the United States in the second half of the twentieth century by positing that the increased prevalence of so-called impersonal forms of voter mobilisation, at the expense of personal forms, was the primary culprit for diminishing participation rates. Gerber and Green's results were drawn from a randomised field experiment of over 30,000 voters in Connecticut, which showed that voter turnout is increased 'substantially by [non-partisan] personal canvassing, slightly by direct mail and not at all by telephone calls', coupled with the observation that campaign contact, though as prevalent as ever, has become increasingly automated and centrally managed via email and telephone communications, rather than the traditional use of door-knocking in Get-Out-The-Vote campaigns (2000: 653).

Their theoretical justification for the greater effect of face-to-face contact on turnout was that people are more persuaded by those 'delivering urgent requests and making vivid the obligation to act and these blandishments seem particularly effective if delivered in person'. Later they succinctly compared mobilising a voter in person to 'inviting them to a social occasion' (Green and Gerber, 2004: 92). Impressively, this study showed that the marginal effects of being contacted in person are between 9.8 and 12.8 per cent, whereas the effects of receiving mail are just 2.5 percentage points

and the effects of receiving a telephone call are nil. Confidently, the authors go on to calculate that mobilisation campaigns had been mistaken to eschew the considerably better-value personalised campaigns – 'if the effective marginal costs of canvassing were doubled, face-to-face mobilisation would still be cost effective (2000: 661).'

The second reason for the high impact of Gerber and Green's study was its methodological approach of a field experiment analysis, which offered considerably greater robustness over previous survey-based investigations into the effects of mobilisation (Caldeira, Clausen and Patterson, 1990; Rosenstone and Hansen, 1993; Wielhouwer and Lockerbie, 1994). As Gerber and Green (2000: 653) note, previous studies that regressed reported turnout on reported party contact fail to isolate the endogeneity between being mobilised and likelihood of voting – namely that parties direct their campaigning literature strategically and, thus, non-randomly. A further source of endogeneity lies in the fact that 'voters who are easier to reach are also more likely to vote' (2000: 657). The substantive importance of this study and its robustness have lead to a number of similar methodologically similar investigations that supported earlier findings by showing that non-partisan phone calls and emails are relatively ineffective in increasing turnout (Michelson, 2003; Gerber and Green, 2005; Ramirez, 2005; Arceneaux et al, 2006).

However, simultaneously, other studies showed that impersonal contact could positively affect turnout. Nickerson (2005) used a field experiment in Michigan to show that *partisan* volunteer phone calls boosted turnout by an average of 3.2 per cent and that 'door hangers' boosted turnout by 1.2 per cent, undermining the so-called 'Social Occasion Theory' of personalised mobilisation, at least for partisan efforts. This finding was then expanded to show that partisan door hangers, volunteer phone calls and face-to-face visits all enjoy similar cost-effectiveness (Nickerson, Friedrichs, and King 2006), though the primacy in terms of sheer effect size of personal campaigning remained apparent. Moreover, by 2009, Dale and Strauss (2009: 787) could state that 'over the past 10 years, dozens of field experiments have shown that personal mobilisation tactics are the most effective approaches to increasing voter turnout.' They note that these results should, however, be treated with caution because those individuals that receive such non-partisan mobilisation have registered to vote

and thus already shown greater interest in voting, compared to those that have not. They also argue that any communication strategy that leads the citizen to internalising the message is likely to be successful. To support their theory, they use a field experiment to show that receiving a get-out-the-vote SMS reminder increases one's chance of voting by around 3 per cent. They argue that this shows that impersonal, noticeable communication can increase voter turnout by reminding or prompting that recipient, rather than transforming voting into a 'social occasion'. In other words, Dale and Strauss's study shows that social connectedness is not the only effective causal mechanism, but that a 'noticeable reminder' will also play this role. Similarly, Panagopoulos (2007) showed that a non-partisan street sign campaign had a positive effect on voter turnout of between 3.2 and 4.6 per cent. Wong (2005) was also able to show that non-partisan telephone calls and mail increased the probability of turnout amongst Asian-Americans living in Los Angeles.

Overall, the literature on the effect of mobilisation on turnout includes findings that seems strong and uncontroversial as well as those that are not. The first is fundamental and widely repeated finding that mobilisation – receiving contact from a party or candidate - increases the individual's probability of voting. The second is that *personal* forms of mobilisation have a greater positive effect on the individual's probability of voting than *non-personal* forms, though there remain incongruent findings regarding whether the effect of *non-personal* mobilisation is considerable or negligible. Four things stand out in the literature as it stands.

First, despite the call by Gerber and Green for further research to confirm the generalisability of their results in other contexts and election types, follow-up studies have been remarkably US-centric, with a handful of important exceptions. Karp et al (2007) take a cross-country approach by using logistic models to compare the effects of party canvassing on turnout across differing electoral systems, concluding that overall levels of party contact are far higher in candidate-based, rather than party-based, systems but that there are no consistent trends between the magnitude of the effect of party contact and the electoral system. In a single British parliamentary constituency, John and Brannan (2008) ran a *non-partisan* get-out-the-vote campaign that produced a treatment effect of 7.3 per cent for telephoned groups and 6.7 per cent

for door-to-door campaigns (the latter effect being of a considerably greater magnitude than equivalent 'door hanger' effects in the United States). This study was then expanded into a nationwide non-partisan campaign over the course of two waves covering the 2009 European Parliament elections and the 2010 UK general election (Fieldhouse et al, 2013). At the European Parliamentary elections, the telephone campaign had no statistically significant effect whereas the direct mail campaign had a small but statistically significant effect. One year later, at the general election, their telephone campaign increased turnout by around four per cent, while mail increased the chance of voting by just under two per cent. They admit, however, that 'extrapolation to partisan mobilisation efforts must be made with caution', though 'non-partisan campaigns similar to ours are increasingly common (Fieldhouse et al, 2013: 114).' The finding that mobilisation has a higher effect in general elections than European Parliamentary elections is consistent with the study of Arceneaux and Nickerson (2009) who show that face-to-face campaigns increase the chance of turnout by a greater margin in prominent elections, as measured by aggregate levels of voter interest.

Second, the literature has been remarkably focussed on non-partisan forms of mobilisation despite partisan forms of mobilisation being considerably more prevalent, particularly outside of the United States where the former are often scarce. Against this trend is one, again American, study that shows that 'the messages and strategies of the real-world campaign manager are highly effective in stimulating voter participation, more effective than might be extrapolated from previous academic studies using nonpartisan messages' (Alvarez et al, 2010: 31), suggesting likely incongruence between the effect sizes of partisan and non-partisan mobilisation strategies. This research finds a 5.6 per cent increase in one's chance of voting when contacted by a combination of face-to-face canvassing, phone calls, e-mails, and door hangers using the results of a field experiment on an - admittedly atypical - sample of 16,500 Democrats in Pasadena, California.

Third, the use of field of experiments in the United States has been notably constant since Gerber and Green's (2000) study, primarily because of the highly robust findings that they offer; yet there are shortcomings in using this method. First, these

data sources contain very limited information on the respondents, precluding nuanced investigation into theorised causal mechanisms. One notable exception is the study by Gerber et al (2013), which combines a Ten Item Personality Inventory survey with two field experiments to show that emotional stability and, particularly, openness, increase the effect of nonpartisan mobilisation, while agreeableness, conscientiousness and extraversion all have the opposite effect. Otherwise, however, those studies that consider the more commonplace partisan campaigns lack the theoretically key data on the relationship between party that contacted the individual and the individual's ultimate party choice. A second drawback of field experiments, although not entirely insurmountable, is the more obvious moral risk of interfering with the election result when treating would-be voters with a partisan message than when treating them with a non-partisan message (though, as mentioned by John and Brannan (2008), this is also a risk with the latter form of treatment). This issue is probably one of the primary reasons for the greater prevalence of non-partisan studies in the literature on mobilisation and turnout hitherto.

Fourth, the main preoccupation with the literature has been on the type of contact and the distinction between 'personal' and 'impersonal' canvassing – itself only contentiously theoretically underpinned and precluding a more fundamental theoretical consideration of why *any* form of contact increases turnout. Although the causal mechanism may seem obvious, there are at least three, not entirely mutually exclusive, hypothetical causal mechanisms that would see partisan mobilisation increase turnout. First, just as is the case with non-partisan mobilisation efforts, and as shown by Dale and Strauss (2009), partisan contact has the potential to remind, or *prompt*, its recipients, thus increasing the citizen's probability of voting for the contacting party as well as other non-contacting parties similarly. Second, unlike non-partisan forms of contact, partisan contact may increase turnout because it *persuades* the recipient that that contacting party is worth voting for and, thus, that voting itself is worthwhile. Third, and inversely, also unlike non-partisan contact, partisan contact may increase turnout because the contact of the message is so abhorrent to the recipient that she or he is *provoked* into voting for a different party to that which contacted them.

Marginality, mobilisation and turnout

Aside from seeking to explain the effects of mobilisation *per se*, historically, the effects of mobilization have also been investigated as part of a separate debate that seeks to explain why turnout tends to be higher in electoral districts with greater marginality (sometimes called closeness) between the winning and second place candidates in majoritarian voting systems. This trend has received generous academic attention because of its seeming consistency with the rational choice explanation for turnout that was popularised by Riker and Ordeshook (1968). In actual fact, this study showed that, according to the calculus of voting, the net benefits of participating in elections should be negative, given that the probability of affecting the result is trivial and the costs of participation, in terms of time and registration, are considerable, meaning that, in rational choice terms, no one should vote save for non-instrumental benefits (e.g. belief in the civic duty to vote). This did not stop researchers, compelled by the attractive simplicity of the calculus, from considering the effects of a closer race on turnout and subsequently whether this positive relationship was actually the result of voters' 'rational' considerations of the value of their efforts or whether it simply resulted from the greater campaigning resources that candidates and parties ploughed into close seats.

Cox and Munger (1989) test these two hypotheses in the United States using OLS models that analyse the effects of marginality and mobilisation, measured by party spending and controlling for various socio-demographic factors, to show that turnout rises with marginality as a result of mobilisation. Denver and Hands (1974: 35) also 'tentatively' conclude that higher turnout in closer British constituencies (where this debate has arguably been more pronounced) is more likely to be the result of greater campaigning than voter awareness of the political context. While these studies, along with findings by Pattie and Johnston (1998), highlight the likely primacy of partisan mobilisation over individual rational choice calculi in explaining the higher turnout in close constituencies, the sheer prevalence of the debate and the place of marginality within 'accepted wisdom' suggest that it should also be considered in any studies of the effects of mobilisation. Furthermore, Pattie and Johnson (2012) used the British Election Study's 2010 internet panel to show that party contact increased the

recipient's assessment of that party's chance of winning in that constituency and consequently their chance of voting for the contacting party, highlighting the likely causal mechanism whereby recipients of partisan mobilisation efforts are persuaded (though in this case whether to choose one party over another, rather than one party over abstention).

Overall, despite major advances in the literature - particularly regarding the overall positive effect of mobilisation on turnout and its greater strength when using face-to-face methods – there remain a number of unresolved questions that this study seeks to address. First, does partisan mobilisation affect voter turnout in a similar fashion to non-partisan mobilisation? Second, why does partisan mobilisation affect turnout - are would-be voters prompted, persuaded or provoked? Third, are the differences in effect magnitude between personal and non-personal *non-partisan* mobilisation mirrored in *partisan* efforts? Fourth, how consistent are the extant findings regarding effects when tested outside of the United States? Fifth, how consistent are the extant findings when tested in various electoral systems? Finally, sixth, what is covariate and interaction relationship between marginality and partisan mobilisation on turnout?

Methods and data

This study will use panel survey data models to test the effects of partisan mobilisation on turnout within individuals across time. This approach has several advantages. First, by using panel survey data we can expect to have access to far richer and varied information on the nature of would-be voters and partisan mobilisation efforts than would be the case using, for example, a field experiment. In particular, this allows for greater theoretical testing and more nuanced findings. Numerous election studies increasingly make use of their panel components, with perhaps the most advanced being the British Election Study, the American National Election Studies and the US-based National Annenberg Election Study. As Gerber and Green (2000) noted, a major limitation of survey-based methods had, until then, been the researcher's inability to measure the type or frequency of party contact using

the existing survey data at the time. The more advanced election panel studies, including those listed, now include these types of data.

Second, and crucially, panel data analyses offer far greater robustness than simple OLS or logistic regression models and, in doing so, overcome the primary methodological weaknesses of many survey based designs exploring mobilisation and turnout during the twentieth century. As Gerber and Green (2000: 653) note, previous studies that had regressed reported turnout on reported party contact failed to isolate the obvious potential endogeneity issues – because, first, parties direct their campaigning literature strategically and, thus, non-randomly and, second, because 'voters who are easier to reach are also more likely to vote' (2000: 657). Fixed effects models overcome such issues by controlling for difference between individuals, including their propensity to receive campaign literature. Substantively, these two issues are overcome in fixed effects models because all of those displaying variation in the independent variables must have been contacted at least once and not contacted at least at one other time. The fixed effects – or *within* - transformation can be understood first by considering the logistic unobserved effects model for N with T time periods:

$$log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right] = \alpha_i + \beta X_{it} + \varepsilon_{it} \text{ for } t = 1, \ldots, T \text{ and } i = 1, \ldots, N$$

where $log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right]$ is the log odds of turnout for individual i at time t, βX_{it} is the time-variant $1 \times k$ regressor matrix, α_i is the individual's unobserved time-invariant effect and ε_{it} is the error term. The fixed effects model is particularly robust because the individual's unobserved time-invariant effect can correlate with the regressor matrix without biasing the estimates. This is because the fixed effects model eliminates α_i via the within transformation, which estimates the effect of deviation from the individual's mean in the independent variables on the individual's probability of voting, with respect to their mean probability of voting. As such, if $log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right] = P(y_{it})$ then the fixed effects model can be described as:

$$P(y_{it}) - \overline{P(y_{it})} = (\alpha_i - \bar{\alpha}_i) + \beta(X_{it} - \bar{X}_i) + (\varepsilon_{it} - \bar{\varepsilon}_i) \Rightarrow P(\ddot{y}_{it}) = \ddot{\beta}_{it} X + \ddot{\varepsilon}_{it}$$

where $\overline{X_{it}} = \frac{1}{T}\sum_{t=1}^{T} X_{it}$ and $\overline{\varepsilon_i} = \frac{1}{T}\sum_{t=1}^{T} \varepsilon_{it}$. The individual's unobserved time-invariant effect, α_i, is eliminated from the equation because $\alpha_i = \bar{\alpha}_i \therefore (\alpha_i - \bar{\alpha}_i) = 0$. The fixed effects estimator is then obtained via a logistic regression of $P(\ddot{y}_{it})$ on $\ddot{X}_{it}$.

Not only do panel data analyses allow for the complete control of individual fixed effects but they also allow the researcher to control for time-dependent contextual factors that are exogenous to the individual. This is crucial when testing variation in fairly short-term electoral campaigns because the prevalence of partisan mobilisation is likely to be skewed towards the final wave of the analysis, due to its proximity to the election, and because that same final wave, in which individuals retrospectively report turnout, is likely to see lower turnout rates than the previous waves, reporting as they do prospective intention to vote. By allowing the possibility to control for waves, panel data models can in turn control for time-dependent contextual effects. As such, fixed effects panel data models enjoy the data richness of other survey-based methods while their findings, which control both between individuals and across time, lose far less of their robustness, also advantageously allowing for considerably more parsimonious models (Achen, 2002) than previous regression analyses on the subject.

Using a survey-based approach also has several disadvantages. First, most electoral surveys rely on self-reported turnout, which, due to social desirability bias amongst other things, is unlikely to be perfectly accurate. Using fixed effects models will control for this to a large extent, not least because such biases are likely to be fairly constant across time, thus being controlled for in the model, and because those that report abstention at any one time are considerably less likely to feel that they must misreport their voting behaviour in order to adhere to social norms at another time. The particular disadvantage that it does hold is that those reporting, particularly prospective, abstention are few, generally less than ten per cent of the sample. Second, although variation in actual partisan mobilisation is exogenous to *within* variation, this is not necessarily the case for self-reported contact (for example,

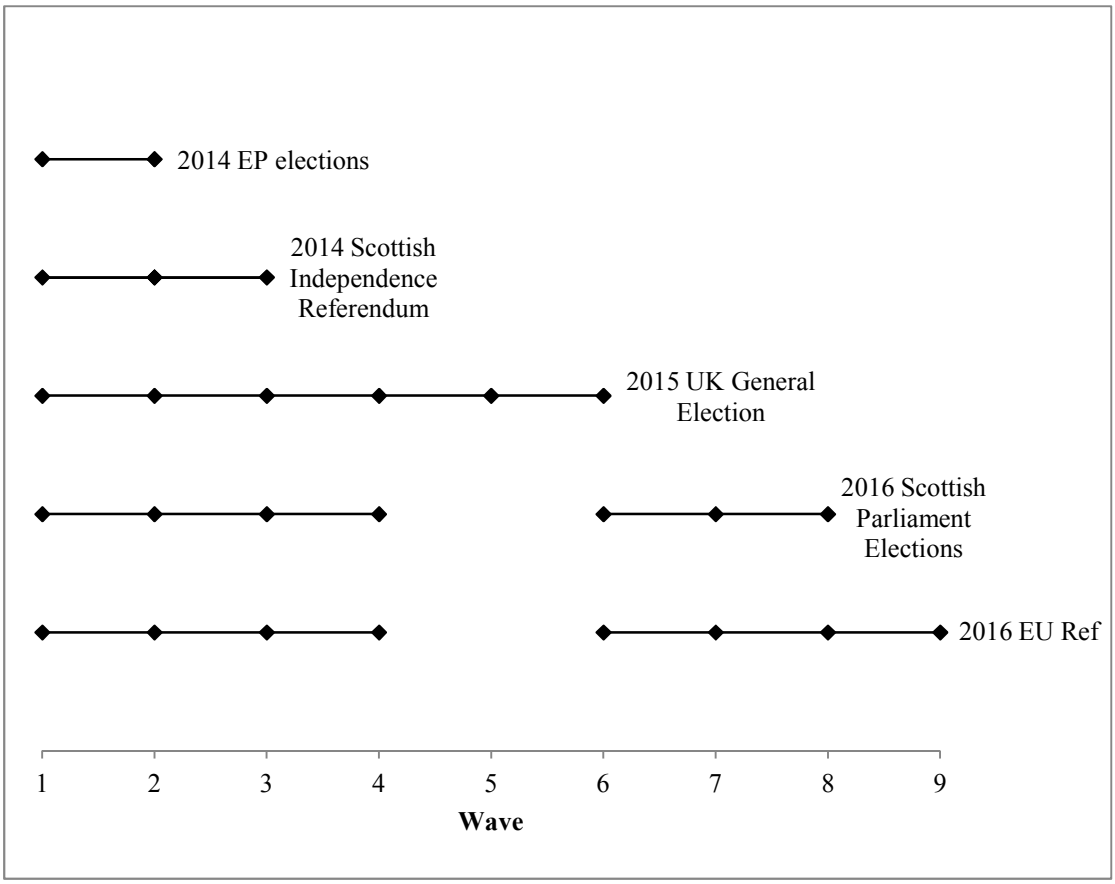
individuals may be more likely to notice having received partisan mobilisation once they have decided to vote), risking less robust results than those taken from field experiments.[2] Third, in order to attain the highest robustness, fixed effects, rather than random effects models must be used because the robustness of the latter relies on the, in this case highly unlikely, assumption that the effects are randomly distributed across the population. However, using fixed effects models creates the problem that the sample size is greatly diminished as all those not displaying variation are dropped from the model, both reducing efficiency and reliability. Moreover, those individuals that report variation are likely to be low- or mid-propensity voters, whom Arceneaux and Nickerson (2009) argue are likely to feel the strongest effects of mobilisation efforts. To (only partially) overcome these issues a particularly large panel study is required.

The data source that best fits the requirements of this study in terms of size and variables is the 2014-2017 British Election Study (BES) internet panel (Fieldhouse et al, 2016). So far, this panel includes nine waves, starting in February 2014 and ending in August 2016. In total, 57,535 individuals from Great Britain (i.e. excluding Northern Ireland) have taken part in the survey, which happened to take place during a particularly eventful period in British politics. As a result, the survey includes questions on voting behaviour, including turnout, on the 2014 European Parliamentary elections, the 2014 Scottish Independence Referendum, the 2015 UK General Election, the 2016 Scottish Parliament elections and the 2016 UK referendum on EU membership. The distribution of data on each of these elections by wave is provided in Figure 1. The BES also contains information on the 2014, 2015 and 2016 local elections, which due to only having one wave each are not available for within individual analyses. The array of elections in the BES is advantageous because it includes a proportional representation election (the European Parliamentary elections), two referendums, a single member plurality system (the UK General Election) and an additional member system (the Scottish Parliament elections) in which electors have two votes – one for a single member constituency member and

[2] Moreover, the accuracy of self-reported party contact may be biased by memory biases. The literature on memory biases in the psychology literature is vast. Perhaps the most pertinent of these biases in this case would be confirmation bias and cue-dependent forgetting. The former includes the tendency to recall events in a way that confirms beliefs and is consonant with actions at the time of recollection (e.g. Nickerson, 1998). The latter, also known as retrieval failure, concerns the greater inability to remember events without certain cues, in the form of events or context (e.g. Pastorino and Doyle-Portillo, 2011). Two respectively similar biases include consistency bias and context bias.

another for proportionally election regional members – data on both of which are collected in the BES.

Figure 1: Elections and referendums included in the 2014-2017 BES by wave



The BES includes rich data on partisan mobilisation. The study asks respondents in every wave whether they have been contacted by a party in the previous four weeks. It also asks in every wave which of the following parties has contacted them – the Conservatives, Labour, the Liberal Democrats, UKIP, the Greens, Plaid Cymru, the Scottish National Party or 'other'. Following this, respondents are asked *how* each of those parties has contacted them – via phone calls, leaflets, home visits, street stalls, email, SMS or other. In wave two and three, respondents in Scotland were specifically asked whether the two referendum campaigns – for and against independence - as well as the Scottish branches of the major parties had contacted them. Furthermore, in the wave prior and after the 2016 referendum on EU membership, respondents are asked if each of the main parties have contacted at any

time them to convince them to vote for either Remain or Leave. However, because of the lack of temporal component in this question, I will use the simple party contact question for this model.

First, I show the results of fixed effects models that regress turnout on party contact for each of the five elections, followed by marginal effects. Second, I then seek to offer additional insights into why party contact increases turnout by analysing models that show the effects of contact from each of the major English parties on voting for *any* party rather than abstaining, voting for that party specifically rather than abstaining and voting for any party *except* that which contacted the voter rather than abstaining. Third, I consider how each of the various forms of partisan mobilisation affect the individual's chance of voting. Fourth, I investigate the relationship between partisan mobilisation, constituency marginality and turnout at the 2015 UK general election.

The effects on partisan mobilisation on turnout by election type

First I consider whether partisan mobilisation increases individual turnout. There are reasons to doubt that this would be the case. Unlike non-partisan forms of mobilisation that are specifically concerned with increasing turnout and may appear more credible and genuine, partisan mobilisation is neither concerned with turnout *per se* – except as a means of increasing that candidate or party's vote share – and is plainly self-interested. As such, receiving partisan communication may have no impact on an individual's decision of whether to vote, both because of its contents and because of its source.

The analysis includes both fixed and random effects models. Even though the former are considerably more robust, presenting the outputs of both allows for comparison that indicates to what extent individual specific effects are correlated with the independent variables – most importantly in this case, to what extent individuals have been contacted because of their own characteristics, such as their socio-demographic profile or constituency in which they reside. Only those individuals who display

within variation in turnout are included in the fixed effects models. The number of observations from these within-varying individuals and their mean turnout is displayed by election type in Table 1, along with information about partisan contact and partisan contact with specific reference to the 2014 Scottish Independence Referendum.

Table 1: Observations and mean turnout of within-varying individuals

	Observations of within-varying individuals	Mean of within-varying individuals
European Parliament	7,852	0.5
Scottish Referendum	754	0.66
General election	10,361	0.62
Scottish Parliament List	921	0.62
Scottish Parliament Const.	1,966	0.69
EU referendum	14,170	0.67
Party contact	125,190	0.43
Scottish referendum contact	3,120	0.5

In Table 2, I present the twelve models. Partisan contact has a statistically significant positive effect in every random effects (RE) model. In the more robust fixed effects (FE) models, partisan contact has a positive effect in all six of the elections and these effects are statistically significant in voting in the European Parliament election, the General Election and the EU referendum. The positive effect is not significant in the three models regarding the Scottish independence referendum (with a very low N) and the two Scottish Parliament elections, each having a slightly larger N of around 300 individuals.

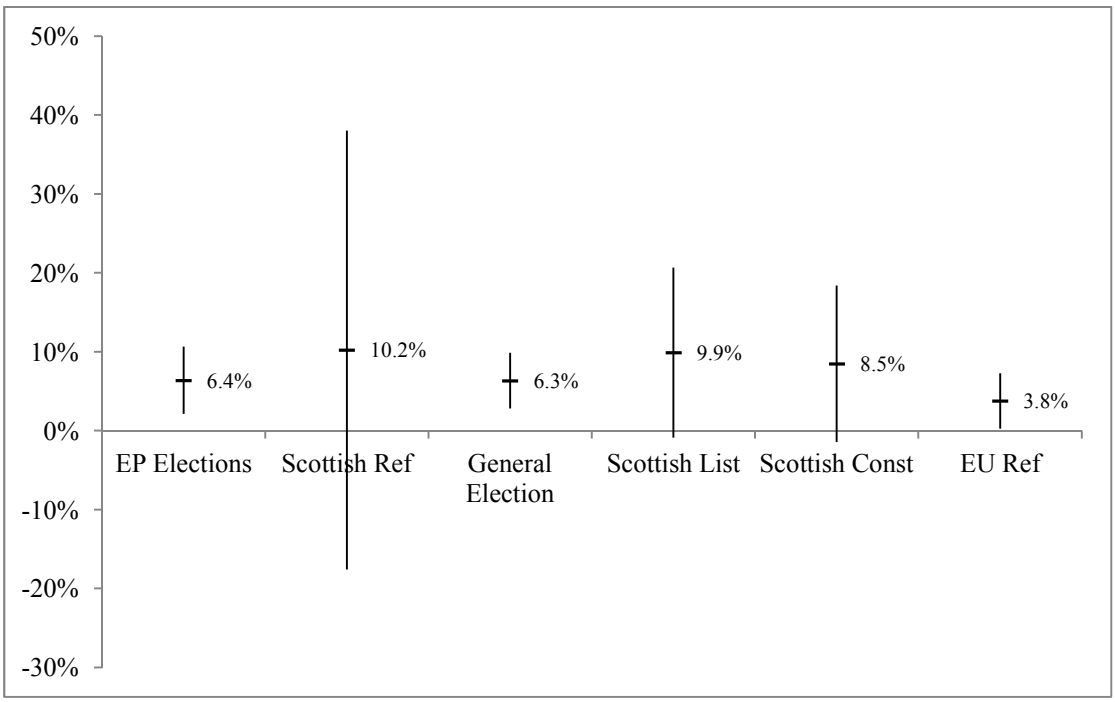
Table 2: Fixed and random effects models of the effect of party contact on turnout, by election type

	(1) EP elections FE	(2) EP elections RE	(3) Scottish Ref FE	(4) Scottish Ref RE	(5) General Election FE	(6) General Election RE	(7) Scottish List FE	(8) Scottish List RE	(9) Scottish Const. FE	(10) Scottish Const. RE	(11) EU Ref FE	(12) EU Ref RE
Contact	1.459**	4.977***	1.514	2.775***	1.321***	3.174***	1.662	4.099***	1.560	3.589***	1.209*	2.745***
	(0.188)	(0.296)	(0.891)	(0.834)	(0.107)	(0.189)	(0.473)	(0.804)	(0.422)	(0.660)	(0.111)	(0.188)
Wave (ref: 1)												
Wave 2	0.092***	0.067***			1.673***	1.392***					0.940	0.935
	(0.007)	(0.004)			(0.149)	(0.105)					(0.094)	(0.077)
Wave 3			0.987	1.074	1.722***	1.753***					1.125	1.145
			(0.351)	(0.278)	(0.155)	(0.130)					(0.103)	(0.085)
Wave 4					1.519***	1.389***			1.859*	1.587*	1.418***	1.208*
					(0.130)	(0.099)			(0.500)	(0.344)	(0.103)	(0.091)
Wave 5					2.350***	1.818***						
					(0.302)	(0.192)						
Wave 6					0.450***	0.383***	2.324**	1.501	2.476***	1.716*	1.962***	1.523***
					(0.036)	(0.026)	(0.661)	(0.331)	(0.706)	(0.398)	(0.195)	(0.127)
Wave 7							1.553	1.027	1.739	1.240	7.063***	5.679***
							(0.430)	(0.222)	(0.502)	(0.285)	(0.851)	(0.553)
Wave 8							0.157***	0.140***	0.133***	0.116***	9.872***	8.788***
							(0.035)	(0.025	(0.030)	(0.021)	(1.281)	(0.900)
Constant		68.93***		29250***		852.0***		1577***		2743***		83.573
		(6.060)		(8.490)		(52.71)		(341.2)		(0.049)		(55.65)
Observations	5982	42196	80	10263	7522	130530	850	17262	1117	22118	6749	171558
N	2991	25475	40	6307	2030	39090	279	7036	307	7266	1642	52277

Odds ratios reported; standard errors in parentheses *** p<0.001 ** p<0.01 * p<0.05

The marginal effects from the fixed effects models are presented in Figure 2. The marginal effects are reasonably similar by election. The size of the effects in the European Parliament elections and the General Election are practically identical at just over 6 per cent. The size of the effect in the EU referendum is smaller, at around 4 per cent.

Figure 2: Marginal effects of party contact on turnout by election type (fixed effects models with fixed effects set to zero)



Though there is considerable variation between the statistically marginal effects – varying from 3.8 per cent to 6.4 per cent, there is no evidence to suggest that the effect of partisan mobilisation on turnout varies consistently according to election type, as had been shown with non-partisan mobilisation, both in the United States (Arceneaux and Nickerson, 2009) and the United Kingdom (Fieldhouse et al, 2013). In the latter of those studies, the effect of a GOTV campaign was shown to be stronger at the more prominent (if we use aggregate turnout as a metric) 2010 general election than at the 2009 European election. Furthermore, the effect of partisan mobilisation seems to be constant according to the electoral system – at the contemporaneous 2016 Scottish Parliament elections, the effect of mobilisation on using one's (proportional representation) list vote were practically identical to its effect on using the single-member constituency vote.

Explaining the positive effect of partisan mobilisation on turnout: Prompted, Persuaded or Provoked?

I now turn to explaining the positive effect of partisan mobilisation on turnout by considering the relationship between the mobilising party and the party choice of the recipient would-be voter. First, in Table 3, I present the effects on turnout of being contacted by each of the five major national parties at the UK 2015 General Election. Each of the five models is a fixed effects logistic panel data model. In the case of every party we see a statistically significant positive effect on the odds of voting.

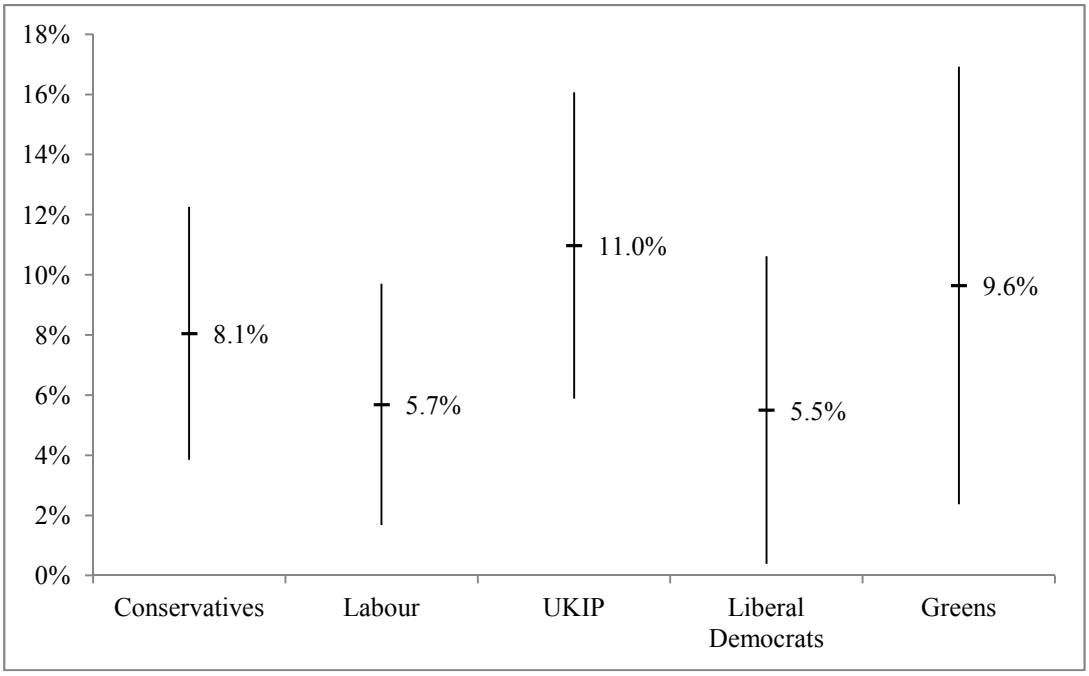
Table 3: Effect of partisan mobilisation on turnout by contacting party

	(1) Conservatives contact	(2) Labour contact	(3) UKIP contact	(4) Lib Dem contact	(5) Greens contact
Partisan contact	1.428***	1.284**	1.635***	1.273*	1.538*
	(0.140)	(0.118)	(0.200)	(0.148)	(0.266)
Wave (ref: 1)					
Wave 2	1.685***	1.702***	1.658***	1.715***	1.749***
	(0.149)	(0.151)	(0.148)	(0.152)	(0.154)
Wave 3	1.721***	1.721***	1.717***	1.716***	1.723***
	(0.155)	(0.155)	(0.154)	(0.154)	(0.155)
Wave 4	1.538***	1.541***	1.552***	1.556***	1.559***
	(0.131)	(0.131)	(0.132)	(0.132)	(0.132)
Wave 5	2.376***	2.391***	2.391***	2.435***	2.452***
	(0.305)	(0.307)	(0.306)	(0.312)	(0.313)
Wave 6	0.455***	0.459***	0.457***	0.472***	0.472***
	(0.036)	(0.037)	(0.036)	(0.037)	(0.037)
Observations	7,522	7,522	7,522	7,522	7,522
Number of id	2,030	2,030	2,030	2,030	2,030

Odds ratios reported; standard errors in parentheses *** p<0.001, ** p<0.01, * p<0.05

There is considerable variation in the magnitude of effect of being contacted by each of the five parties, as shown in Figure 3. If the size of each of these effects were uniform, we might conclude that partisan mobilisation increases turnout *solely* because it acts as a prompt to the voter. Instead, we see contact by UKIP and the Greens (two lesser known, non-mainstream parties) having particularly strong effects at around 10 per cent, whereas contact by Labour or the Liberal Democrats increases ones chance of voting by less than 6 per cent.

Figure 3: Marginal effects of partisan mobilisation on turnout by contacting party



I now restrict the dependent variable to either abstaining or voting for the *same* party that contacted the individual. The five models using this restricted dependent variable are presented in Table 4. Again, in every case, we see a positive effect, though it is not statistically significant in the case of being contacted by the Greens.

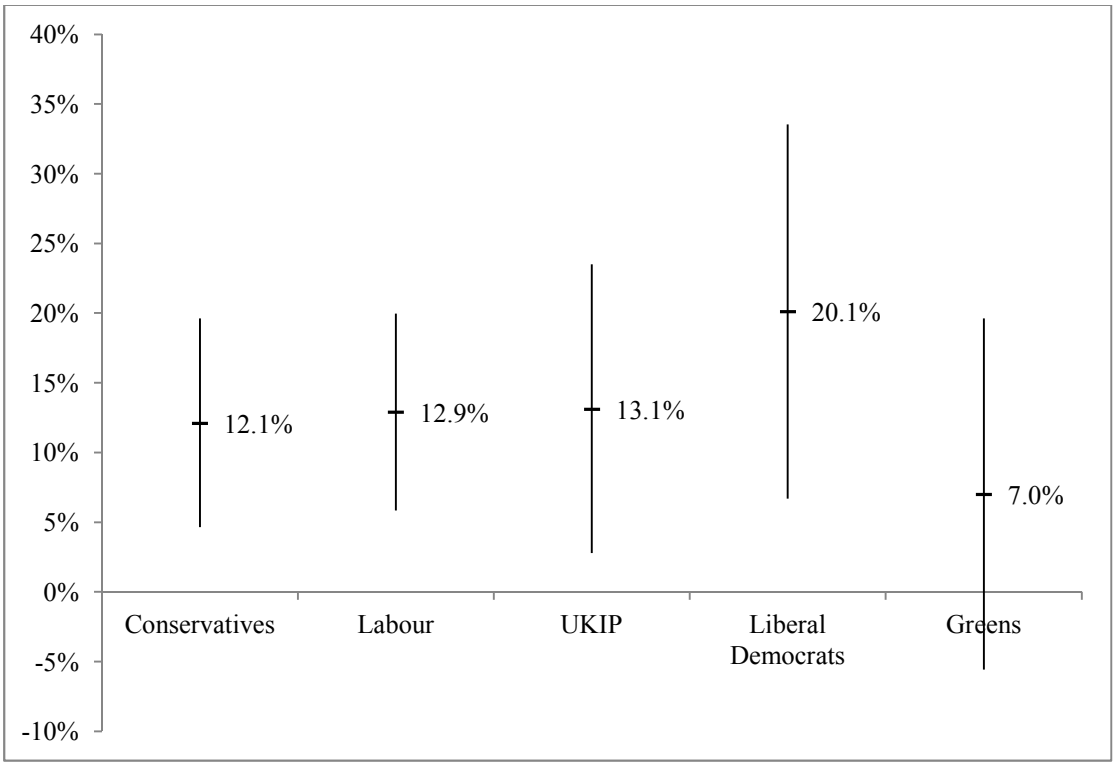
Table 4: Effect of partisan mobilisation on voting for the contacting party, rather than abstaining

	(1) Conservatives	(2) Labour	(3) UKIP	(4) Lib Dems	(5) Greens
Partisan Contact	1.747***	1.769***	1.938*	2.401**	1.465
	(0.328)	(0.294)	(0.552)	(0.778)	(0.266)
Wave (ref: 1)					
Wave 2	1.334*	1.065	2.452***	1.188	3.967***
	(0.225)	(0.171)	(0.469)	(0.364)	(1.037)
Wave 3	1.670***	1.322	2.222***	1.548	3.317***
	(0.294)	(0.222)	(0.435)	(0.481)	(0.895)
Wave 4	2.181***	1.431*	1.181	0.870	3.923***
	(0.348)	(0.229)	(0.221)	(0.245)	(1.071)
Wave 5	4.306***	1.540	1.979*	1.223	5.199***
	(1.060)	(0.361)	(0.587)	(0.525)	(2.059)
Wave 6	0.674***	0.315***	0.206***	0.423**	0.940
	(0.099)	(0.047)	(0.038)	(0.116)	(0.256)
Observations	1,987	2,204	1,729	589	770
Number of id	595	663	507	199	249

Odds ratios reported; standard errors in parentheses *** p<0.001, ** p<0.01, * p<0.05

The marginal effects of being contacted by a party on the probability of voting for that party, rather than abstaining, are shown in Figure 4. In every case, except for the Greens, we see that the effects of partisan mobilisation on voting for the contacting party have larger magnitudes than they do on the probability of voting *per se*. I interpret this as evidence that the partisan mobilisation increases one's chance of voting partially because of a *persuasive* causal mechanism, rather than simply because it acts as a reminder.

Figure 4: Marginal effects of partisan mobilisation on voting for the contacting party, rather than abstaining



However, there is a third possible reason for the positive effect of partisan mobilisation on turnout that is the opposite of persuasion. It could be that the recipient is so antipathetic to the contents of the messaging, that the messaging increases their chance of voting for another party. The effects of voting for a party *other than* that which contacted the individual, rather than abstaining, are presented in Table 5. Only being contacted by Labour or by UKIP has a statistically significant effect on one's probability of voting for a different party, with both having positive effects.

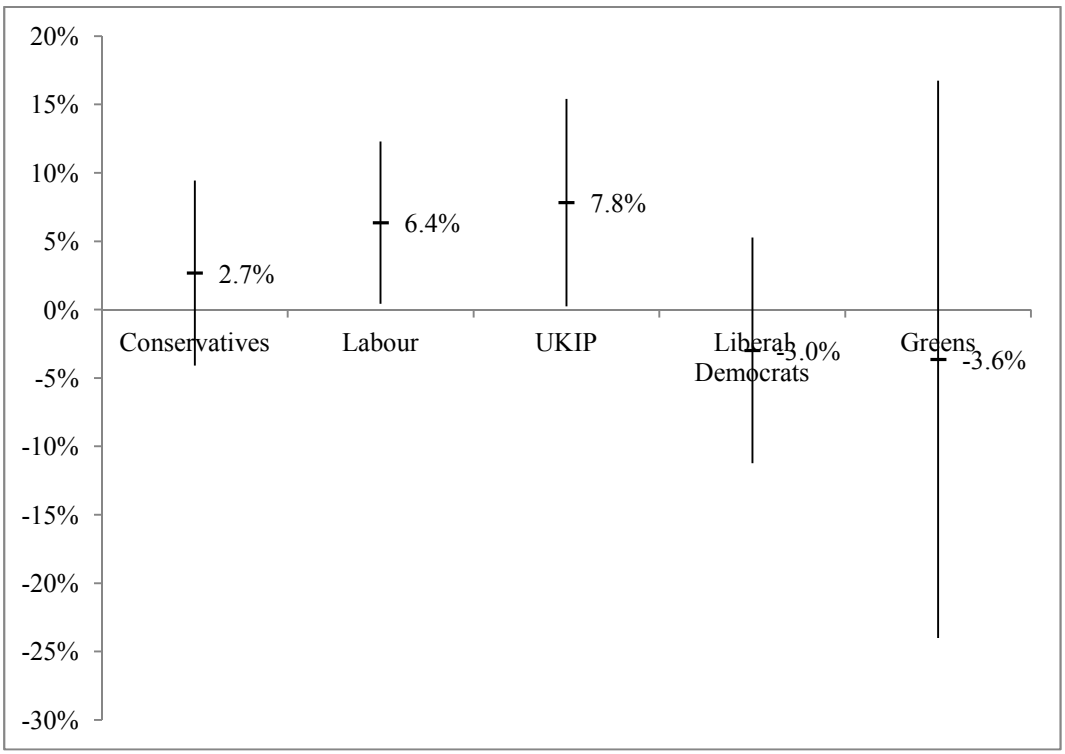
Table 5: Effect of partisan mobilisation on voting for a party other than the contacting party, rather than abstaining

	(1) Conservatives	(2) Labour	(3) UKIP	(4) Lib Dems	(5) Greens
Partisan Contact	1.144	1.431*	1.538*	0.857	0.833
	(0.202)	(0.258)	(0.350)	(0.183)	(0.428)
Wave (ref: 1)					
Wave 2	1.981***	2.487***	1.636***	2.115***	1.767***
	(0.231)	(0.306)	(0.195)	(0.231)	(0.195)
Wave 3	1.974***	2.283***	1.827***	2.069***	1.894***
	(0.240)	(0.288)	(0.228)	(0.236)	(0.223)
Wave 4	1.436***	1.794***	1.833***	1.943***	1.593***
	(0.169)	(0.218)	(0.216)	(0.213)	(0.178)
Wave 5	1.826***	3.087***	2.684***	2.937***	2.575***
	(0.301)	(0.513)	(0.423)	(0.440)	(0.391)
Wave 6	1.762e+08	9.656e+08	5.710e+08	9.089e+08	7.204e+08
	(8.791e+10)	(1.003e+12)	(4.380e+11)	(8.063e+11)	(5.989e+11)
Observations	3,116	3,097	3,333	3,775	3,636
Number of id	924	911	992	1,089	1,057

Odds ratios reported; standard errors in parentheses *** p<0.001, ** p<0.01, * p<0.05

The magnitudes of the marginal effects, as shown in Figure 5, are highly heterogeneous. This again suggests that any 'prompting' effect, whereby citizens are more likely to vote *per se,* is either only partially or not at all evident, since the size of this effect would be uniform by party. Indeed, whereas partisan mobilisation efforts by the Conservatives, Labour and UKIP all increase one's chance of voting for another party (as well as for those parties) rather than abstaining, the marginal effect of being contacted by the Liberal Democrats and the Greens is in fact negative.

Figure 5: Marginal effects of partisan mobilisation on voting for a party other than the contacting party, rather than abstaining



Overall, the evidence in this analysis suggests that the overwhelming reason why partisan mobilisation increases the individual's chance of voting is because it persuades them that the contacting party itself is worth voting for. Though this analysis by no means rules out the possibility that individuals are also prompted to vote by partisan mobilisation, the lack of uniformity amongst the effects on voting for a party other than that which contacted the individual suggests that any reminder mechanism is combined with a persuasive mechanism. Indeed, the strongest effect on voting for another party came when a controversial, radical right party (UKIP) contacted the voter, suggesting that some voters were mobilized by their distain for the message they received. However, these *provoking* effects were fewer and smaller than the strong and across-the-board persuasive effects of partisan mobilisation at the 2015 general election.

The effects of different types of partisan mobilisation on turnout

As already discussed, the major preoccupation within the literature in recent years has been determining whether personalised forms of non-partisan mobilisation are more effective than non-personalised forms, based on the theoretical explanation that face-to-face communication transforms voting into a social experience. In Table 6, we see the major types of partisan mobilisation in the six waves prior to the 2015 General Election. By some distance the most common form of partisan contact was receiving a party leaflet, with 46 per cent of the entire sample of 54,467 individuals being contacted in this was at some time. After this the most common forms of party contact were emails and home visits at 20 per cent and 17 per cent, respectively, followed by phone calls, street stalls, both at 7 per cent, and a negligible proportion of the sample receiving SMS and 'other' communication.

Table 6: Between and within frequency of each type of partisan mobilisation

	Percentage contacted at some point	Observations of within-varying individuals	Mean of within-varying individuals
Phone call	7.2	20,124	0.26
Leaflet	46.4	117,028	0.38
Home visit	17.3	48,445	0.26
Street	6.9	17,929	0.26
Email	19.8	47,625	0.39
SMS	2.9	7,608	0.27
Other	3.1	1,671	0.68

How should these six categories (not including 'other') be ranked in terms of 'personalisation'? Whereas home visits, phone calls and e-mail and SMS have already been ranked in descending order by previous studies, leaflets, and street stalls have not been categorized. Leaflets might be thought of as the British equivalent of door hangers and their contents overwhelmingly tend to be locally focused in general elections and will have been hand-delivered to the house, perhaps increasing their personalised quality above electronic forms of communication (see British Election Leaflet Project, 2017). Street stalls are clearly a face-to-face experience, albeit arguably less personal than the more intimate home visits. Overall, I conclude that

home visits and street stalls are the most personalized forms of mobilisation, followed by phone calls and leaflets, with emails and SMS being the least personalised.

In Table 7, I present the findings of models that test the effects of different types of partisan contact on voter turnout before presenting a single model (model 8) that includes all of the six types of contact and 'other' as independent variables. In both the separate models and the combined model, leafleting is the only statistically significant predictor of turnout as well as having the largest odds ratio.

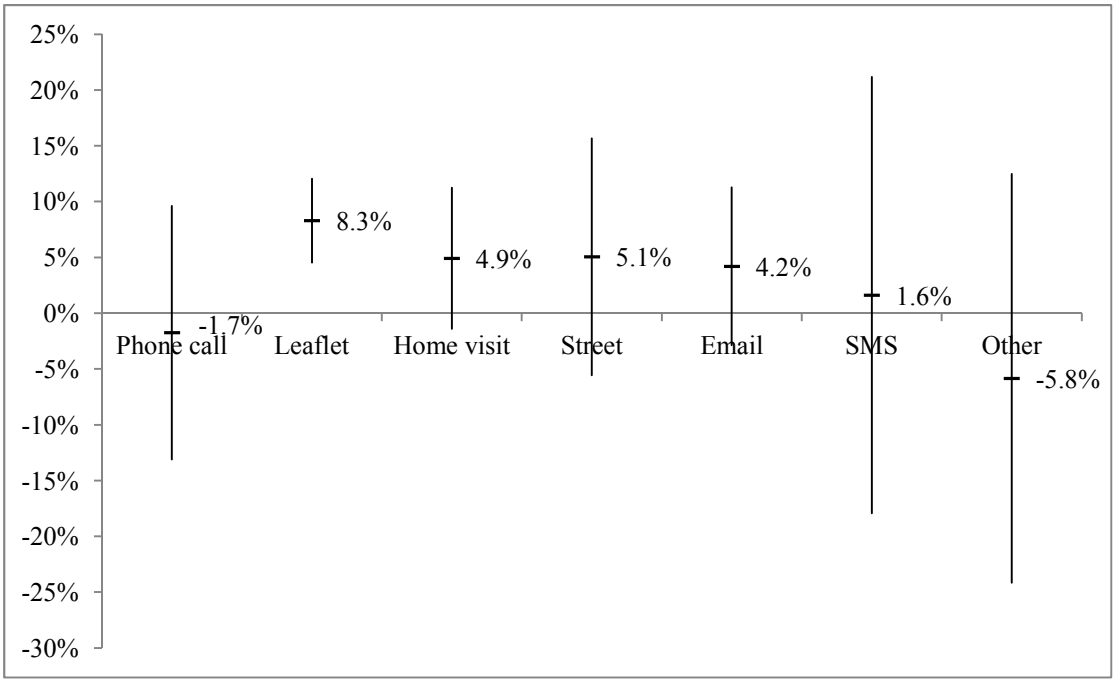
Table 7: Fixed effects logistic models determinants of turnout by type of party contact

	(1) Phone call FE	(2) Leaflet FE	(3) Home FE	(4) Street FE	(5) Email FE	(6) SMS FE	(7) Other FE	(8) Combined FE
Phone call	0.928							0.845
	(0.231)							(0.213)
Leaflet		1.445***						1.427***
		(0.127)						(0.131)
Home visit			1.241					1.104
			(0.178)					(0.164)
Street				1.248				1.083
				(0.301)				(0.270)
Email					1.201			1.076
					(0.192)			(0.177)
SMS						1.073		0.970
						(0.466)		(0.430)
Other							0.778	0.673
							(0.312)	(0.271)
Wave (ref: 1)								
Wave 2	1.754***	1.663***	1.741***	1.753***	1.754***	1.753***	1.752***	1.663***
	(0.154)	(0.148)	(0.153)	(0.154)	(0.154)	(0.154)	(0.154)	(0.148)
Wave 3	1.725***	1.729***	1.727***	1.727***	1.723***	1.724***	1.725***	1.731***
	(0.155)	(0.155)	(0.155)	(0.155)	(0.155)	(0.155)	(0.155)	(0.155)
Wave 4	1.575***	1.520***	1.568***	1.573***	1.568***	1.574***	1.576***	1.521***
	(0.133)	(0.130)	(0.133)	(0.133)	(0.133)	(0.133)	(0.134)	(0.130)
Wave 5	2.497***	2.319***	2.468***	2.484***	2.483***	2.494***	2.496***	2.312***
	(0.318)	(0.298)	(0.315)	(0.317)	(0.317)	(0.318)	(0.318)	(0.298)
Wave 6	0.492***	0.441***	0.481***	0.488***	0.485***	0.491***	0.492***	0.439***
	(0.038)	(0.036)	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)	(0.036)
Observations	7522	7522	7522	7522	7522	7522	7522	7522
Number of id	2030	2030	2030	2030	2030	2030	2030	2030

Odds ratios reported; standard errors in parentheses *** p<0.001, ** p<0.01, * p<0.05

In Figure 6, I present the marginal effects from the separate models. The marginal effect of receiving a party leaflet is to increase turnout by 8.3 per cent. After this, the effects of being contacted in the street, at home or via email are all relatively similar – at 5.1, 4.9 and 4.2 per cent respectively – yet all have p-values of slightly higher than 0.05. The effect of receiving a phone call is negative, whereas the effect of receiving an SMS is slightly positive though neither of these effects are anywhere near statistical significance.

Figure 6: Marginal effects of types of partisan mobilisation on turnout



Overall, there is little evidence that so-called personalised forms of partisan mobilisation, as defined as face-to-face communication, are more effective in increasing turnout than non-personalised mobilisation efforts within the context of British general elections. The most effective form of partisan communication in terms of marginal effect is also the most prevalent, leafleting. There is therefore no evidence to suggest that the UK's own aggregate-level decline in turnout, particularly between 1992 (77.7 per cent national turnout) and 2001 (59.4 per cent) from where it has since only partially recovered, had much to do with any switch towards non-personal forms of partisan mobilisation, following the logic of Gerber and Green (2000). Moreover, it may be that the positive effect of *partisan* mobilisation follows a different causal mechanism from *non-partisan* GOTV efforts, particularly since the primary message of the latter is solely to turnout. If partisan mobilisation is not about inviting citizens to a social occasion but instead results in higher turnout because it prompts, persuades or provokes, then the face-to-face component that has been identified as crucial to GOTV campaigns may be unnecessary. The above empirical analysis – showing no relationship between 'personalised' mobilisation and effectiveness of increasing turnout, supports this argument.

Analysis of marginality, mobilisation and turnout

I now turn to investigating the effects of marginality and partisan mobilisation on turnout at the 2015 UK General Election. As shown in Table 8, when turnout is regressed against just the individual's constituency majority in 2015, the latter has a statistically significant effect whereby the less close the eventual electoral outcome, the less likely one is to vote. Given that this is a fixed effect, I use a random effects model. In the second model (2), I introduce party contact, which retains its statistically significant effect, just as when predicting turnout without marginality being controlled for in Table 2. By contrast, marginality becomes insignificant and the size of its effect becomes far smaller when party contact is controlled for. In model 3, which uses the constituency's 2010 majority in order to overcome the temporal ordering weakness of using the (albeit probably more accurate measure of the citizen's perception of marginality) 2015 majority, party contact again maintains its statistically significant strong effect whereas 2010 majority has no effect.

Table 8: The effects of marginality and partisan mobilisation on turnout

	(1) 2015 majority alone (RE)	(2) Controlling for 2015 majority (RE)	(3) Controlling for 2010 majority (RE)	(4) Interacting with 2015 majority (FE)	(5) Interacting with 2010 majority (FE)	(6) 2015 Majority lower than mean (FE)	(7) 2015 Majority higher than mean (FE)
Party contact		3.162***	3.185***	1.492***	1.475***	1.459***	1.211
		(0.189)	(0.190)	(0.212)	(0.194)	(0.167)	(0.139)
2015 Majority	0.989***	0.997		1			
	(0.000)	(0.002)		(0)			
2010 Majority			1.001		1		
			(0.003)		(0)		
Party contact*2015 Majority				0.995			
				(0.005)			
Party contact*2010 Majority					0.993		
					(0.00623)		
Wave (ref: 1)							
Wave 2	1.811***	1.392***	1.391***	1.676***	1.675***	1.629***	1.709***
	(0.130)	(0.105)	(0.105)	(0.149)	(0.149)	(0.215)	(0.206)
Wave 3	1.772***	1.751***	1.753***	1.721***	1.720***	1.638***	1.785***
	(0.131)	(0.130)	(0.130)	(0.155)	(0.154)	(0.220)	(0.216)
Wave 4	1.473***	1.389***	1.387***	1.513***	1.513***	1.444**	1.563***
	(0.102)	(0.099)	(0.099)	(0.129)	(0.129)	(0.187)	(0.178)
Wave 5	2.425***	1.818***	1.816***	2.352***	2.351***	2.394***	2.312***
	(0.180)	(0.191)	(0.192)	(0.302)	(0.302)	(0.475)	(0.390)
Wave 6	0.472***	0.384***	0.383***	0.449***	0.448***	0.401***	0.489***
	(0.029)	(0.026)	(0.026)	(0.036)	(0.0361)	(0.050)	(0.052)
Constant	9255.9***	905.2***	831.5***				
	(712.7)	(76.0)	(65.1)				
Observations	162,409	130,530	130,530	7,522	7,522	3,455	4067
Number of id	40,723	39,090	39,090	2,030	2,030	955	1075

Odds ratios reported; standard errors in parentheses *** p<0.001, ** p<0.01, * p<0.05

Having clarified that partisan mobilisation, rather than instrumental reasoning regarding marginality, is the cause of the individual's greater likelihood of voting in more marginal constituencies, I now turn to the potential interaction effect between the two. Theoretically we might expect such an interaction effect because the contents of the partisan messaging are likely to highlight the marginality of the race in seats with small majorities (see British Election Leaflet Project, 2017). In models 4 and 5 the interactions effects are reported using the constituencies 2015 and 2010 majorities respectively. Though a larger majority (and thus a less marginal constituency) does indeed cause the effect of partisan mobilisation to be weaker, this interaction effect is not statistically significant. In models 6 and 7 this interaction effect is again displayed by dividing the sample between those residing in constituencies that have smaller than average majorities (22.4 per cent at the 2015 general election) and those that have larger than average majorities. Again, we see that in more marginal constituencies, party mobilisation has a stronger positive effect but we also see that it is only in these constituencies that this effect is statistically significant at the 95 per cent level (though it is at the 90 per cent level).

Robustness Check

The biases that can arise from interpreting interaction effects from regression models is increasingly recognised amongst social scientists (e.g. Brambor, Clark and Golder, 2006; Kam and Franzese, 2007; Berry, DeMeritt and Esarey, 2010). Addressing these biases, however, has been skewed heavily towards linear models (Clarke, Elliot and Stewart, 2015). Norton, Wang and Ai (2004) show that interpretation of interaction effects is even more complicated when using non-linear models, such as the fixed effects logistic models presented here. In short, 'the marginal effect of a change in both interacted variables is not equal to the marginal effect of changing just the interaction term [$\beta_{12}\mathrm{X}_1X_2$]. More surprisingly the sign may be different for different observations', which, alongside the variance in statistical significance and the interaction effect's reliance on other covariance, makes this a non-trivial matter (Norton, Wang and Ai, 2004: 154). They also caution against the use of odds ratios

interpretation of logistic coefficients for interaction effects. Rather than being the marginal effect of just the interaction term, the actual, full interaction effect is the cross-partial derivative of the expected value of *y* (in our case, turnout). When the interaction effect is between a continuous and discrete independent variable (as it is in our case between 2015 majority and whether the respondent had been contacted by a party or not), the interaction effect is the discrete difference with respect to X_2 of the single derivative with respect to X_1:

$$\frac{\Delta \frac{\partial F(u)}{\delta X_1}}{\Delta X_2} = \frac{\Delta\{(\beta_1 + \beta_{12} X_2) f(u)\}}{\Delta X_2}$$

$$= (\beta_1 + \beta_{12}) f\{(\beta_1 + \beta_{12}) X_1 + \beta_2 + X\} \beta - \beta f (\beta X_1 + X \beta)$$

I proceed to use this double derivative approach, using Stata's *Inteff* function, to perform a robustness check on the interaction effect between party contact and constituency marginality via logistic models that measure between-individual variation and include a number of socio-demographic, political psychological and previous turnout controls. Taking this approach also allows us to check the robustness of the previous results, which could be skewed by reducing the sample to only those individuals that display within variance. The results are shown in Table 9.

Table 9: Linear models of the interaction effect between marginality and party contact

	(1) Naïve model	(2) With socio-demographics	(3) With political psychological variables	(4) With 2010 turnout
2015 Majority	0.006**	0.003	-0.001	-0.001
	(0.002)	(0.003)	(0.004)	(0.004)
Party contact	1.318***	1.005***	0.467**	0.374**
	(0.098)	(0.137)	(0.170)	(0.179)
2015 Majority*Party contact	-0.005	-0.001	-0.002	-0.004
	(0.004)	(0.005)	(0.006)	(0.007)
Election interest			0.828***	0.742***
			(0.062)	(0.067)
Internal efficacy			-0.183**	-0.172**
			(0.069)	(0.073)
External efficacy			-0.098	-0.084
			(0.055)	(0.059)
Political knowledge			1.439***	0.811**
			(0.298)	(0.328)
Party ID			0.591***	0.381***
			(0.126)	(0.140)
Civic duty to vote			0.865***	0.665***
			(0.045)	(0.051)
Age		0.024***	0.002	-0.001
		(0.003)	(0.004)	(0.043)
Male		0.193**	0.223*	0.221**
		(0.069)	(0.091)	(0.096)
Household income		0.027*	-0.006	-0.004
		(0.012)	(0.014)	(0.015)
Age of leaving education		0.115***	-0.036	-0.059*
		(0.026)	(0.033)	(0.036)
Married		0.210**	0.187*	0.116
		(0.077)	(0.094)	(0.100)
Home owner		0.382***	0.378***	0.275***
		(0.080)	(0.099)	(0.106)
Number of children		0.000	0.000	0.000
		(0.000)	(0.000)	(0.000)
White ethnicity		0.355*	0.171	0.118
		(0.141)	(0.177)	(0.198)
2010 turnout				1.777***
				(0.100)
Constant	1.947***	-0.463*	-4.056***	-3.633***
	(0.063)	(0.243)	(0.437)	(0.471)
Observations	29,171	15,097	13,063	12,755

Standard errors in parentheses *** p<0.001, ** p<0.01, * p<0.05

Results of the double derivative approach of the interaction effect in the fully specified Model 4 show the mean interaction effect is -0.00005 with a high standard deviation of 0.000125 and a mean standard error of 0.00032 and a mean Z score of -0.118, all of which suggests that the interaction effect is negative but very weak,

supportive of the original findings in Table 8 that the interaction effect is minor and not statistically significant.

Conclusion

Partisan mobilisation – the act of parties and candidates contacting would-be voters – is considered a cornerstone of the electoral process. Indeed, we expect such mobilisation efforts in order to both inform us as electors and to show that the candidates 'mean business' enough that they are willing to forgo considerable resources in order to convince citizens of their merits. The prevalence of partisan mobilisation far overshadows non-partisan mobilisation in most elections across the world and, intuitively, we should desire significant partisan mobilisation in order to amplify the electoral process, increasing participation in the process.

In spite of this apparent importance, studies into the link between mobilisation and turnout have overwhelmingly been focussed on the less common non-partisan, Get-Out-The-Vote campaigns, primarily because of the greater ease of producing robust results using these campaigns as part of a field experiment methodological approach. Moreover, such studies have overwhelmingly been US-focussed and therefore only applicable to a certain political culture and a certain institutional framework, particularly in terms of the electoral system. Additionally, the constant use of field experiments has resulted in highly robust results yet has often precluded the ability to test causal mechanisms between mobilisation (either partisan or non-partisan) and turnout – outside of focussing on the somewhat niche issue of the medium of mobilisation – because of the deficit of additional information on the would-be voters that survey data provides. On the other hand, earlier survey-based studies in the twentieth century lacked robustness in their results.

In response to these shortcomings, this article uses fixed effects panel data models to test how partisan mobilisation affects intended and retrospective turnout within individuals across time. Such an approach is considerably more robust than cross-sectional survey approaches while containing greater data richness than field experiments. This study uses election study data that includes five types of elections

and referendums in the UK between 2014 and 2016 as well as information on the individual's party choice when voting (or planning to), their constituency and data on the type and source of partisan mobilisation.

In doing so, this study finds evidence that partisan mobilisation increased the individual's probability of voting in the 2014 European Parliament elections, the 2015 UK General Elections and the UK's 2016 referendum on EU membership. I also show that the effect of party mobilisation was to increase the chance of voting in the 2014 Scottish Independence Referendum and both votes in the 2016 Scottish Parliament election – though neither of these effects was statistically significant owing to considerably smaller sample sizes. The similarity of effect sizes in the European Parliament and General Election – just over 6 per cent – suggests that the electoral system has little consequence for how partisan mobilisation affects turnout.

I then move on to explaining why partisan mobilisation has a positive effect on turnout by offering three by-no-means mutually exclusive theoretical causal mechanisms – that voters are prompted, persuaded and/or provoked by the partisan messaging they receive. I seek to test these hypotheses by ascertaining the effects of each party's contact on, first, voting *per se*, second, voting for the contacting party and, third, voting for a party other than that which contacted the respondent. Partisan mobilisation by all parties has a positive impact on the citizen's chance of voting. However, in all cases but one this effect is considerably larger when the dependent variable is restrict to either abstaining or specifically voting for the contacting party – suggesting that the positive effect of partisan mobilisation on turnout is at least partially because it persuades citizens that voting for that party in particular is preferable to abstaining. A less weighty positive effect is found in the case of voting for parties *other* than that which contacted the respondent in the case of partisan mobilisation from Labour or UKIP at the 2015 general election, suggesting that some individuals were provoked into voting for another party, rather than abstaining, by the content of some partisan mobilisation towards which they felt antipathetic.

I then seek to test the effect of varying types of partisan mobilisation in order to test one of the major findings of the literature looking at the effects of nonpartisan mobilisation on turnout – that personal, face-to-face campaigning has a greater

positive effect than impersonal forms of campaigning. I find that, at the 2015 UK General Election, there is no evidence that personalised forms of campaigning had a greater effect on the individual's chance of voting. In fact, though not the only form of partisan mobilisation to have a positive effect, the strongest and only statistically significant effect came from leaflets. This supports the earlier finding that whereas non-partisan mobilisation is effective because it either transforms voting into a social experience or simply reminds the individual, the far more prevalent partisan mobilisation increases the individual's chance of voting by persuading them that voting is indeed a worthwhile act because there is someone worth voting for. As such, it seems unlikely that the UK's aggregate-level decline in turnout from 1992 onwards was the result of a move towards non-personal forms of mobilisation.

Finally, the role of greater partisan mobilisation in causing higher levels of turnout in constituencies with small majorities is presented. By contrast, the effect of tight marginality on turnout disappears when party contact is controlled for. However, this does not mean that the size of a constituency's majority has no effect on turnout. Indeed, I present some, albeit not entirely conclusive, evidence that partisan mobilisation has a stronger positive effect on turnout in the 50 per cent of constituencies that are most competitive constituencies rather than in the 50 per cent least competitive constituencies. However, this interaction effect is by no means consistent, being non-statistically significant when tested across all constituencies.

Resources and Voter Turnout

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The relationship between resources and electoral participation is perhaps the most studied aspect of the voter turnout literature. This 'resource model', which posits that citizens vote if and when they can, has included such diverse explanatory variables as wealth, employment, free time, skills, mobility, marital status and health. In spite of this, these variables have rarely been tested together and many of the findings have been based on fairly unsophisticated techniques. Using the British Household Panel Survey, I test the effects of these various resources on variation within individuals across time. I find evidence that greater financial resources, as measured by income and employment, do increase the individual's chance of voting. However, self-assessed financial well being has a negative relationship with voting, which I show is likely to reflect a grievance mechanism of voting against the incumbent party. Moreover, non-financial resources such as time, health, and stability and, above all, family overshadow the effects of these financial resources on turnout. I go on to argue that the uniquely strong positive effects of marriage and parenthood on turnout derive from the stability, support, discourse, and a sense of role and responsibility that they confer, resources that seem far more valuable to political participation than money.

Perhaps the most prominent strand of voter turnout research has been concerned with how resources such as wealth, skills and time affect an individual's likelihood of voting. The theoretical commonality of these resource-based explanations of voting is that people vote if and when the have sufficient capacity, which certain resources provide. The most intuitively obvious resource is wealth and, indeed, those with more wealth have consistently been shown to be more likely to vote. Throughout the second half of the twentieth century rates of turnout in US national elections among the most affluent citizens were nearly 35 percentage points higher than the rate of turnout among the poorest (Rosenstone and Hansen, 1993: 42). Verba, Schlozman and Brady (1995: 358) show a positive effect of family income on voting, even when other resources and political engagement variables are controlled for. More recently, in the 2008 US Presidential election, around 80 per cent of those in the top income quintile voted, while around half of those in the bottom income quintile did so (Leighley and Nagler, 2014). Mughan and Lacy (2002) found that a higher income increases one's chance of voting, though their multinomial models showed no statistically significant effect of personal finance evaluations or fear of losing one's job on turnout. Overall, it seems clear that the wealthier one is, the more likely they are to vote and scholars have repeatedly posited that this relationship is indeed causal (e.g. Verba, Schlozman and Brady, 1995: 273) whereby those who vote do so because they are able to.

However, although there is an unambiguous positive correlation between wealth and electoral participation between individuals, that same relationship within individuals across time remains contested. Indeed, there are two theories of political participation that make directly contrary predictions about how changes in financial resources might affect voter turnout. Rosenstone (1982) dubbed these - with respect to the effect of a negative change in wealth - the withdrawal and mobilisation hypotheses. The first of these assumes that as citizens get richer, their chance of voting increases and so any fall in an individual's wealth would see their chance of voting diminish as they *withdraw* from politics in order to focus on more pressing financial concerns. Verba, Schlozman and Brady (1995) spell this relationship out as one of the resources in their

broader Civic Voluntarism model and it has been further evidenced by Pacek (1994), Norris (2002) and Blais and Rubenson (2012). However, these studies make inferences based on between individual studies, reflecting the widespread use of cross-sectional survey research and a lack of longitudinal research in the field of political participation (Tawfik et al, 2012: 352).

A negative relationship between wealth and turnout has also been hypothesised. Long before advancing the Civic Voluntarism Model, Schlozman and Verba (1979; see Rosenstone, 1982) argued that people under economic strain blame the government for their situation and participate in politics to redress their grievances. Lipset, in 1960, had already stated that those facing economic difficulty would be likely to turn to the government for assistance and vote in higher numbers. Rosenstone (1982) found a positive effect of financial hardship on chance of voting but it was not statistically significant. Aside from electoral studies, this mobilising effect has long been a cornerstone of the social movement literature where it is known as grievance theory (Gamson, 1968; Wilkes, 2004). However, grievances about economic distress have only been shown to have a positive effect on the probability of political participation when the latter is measured either by an index-variable or by a form of non-institutional political participation, which does not include the voting. This naturally raises questions about the aptitude of using findings from grievance theory to explain voter turnout, given that variance in turnout has been found to have fundamentally different determinants than other forms of political activity (e.g. Verba, Schlozman and Brady, 1995: 359).

Overall, despite being a cornerstone of the voter turnout literature, the effect of changes in financial resources on electoral participation remains unresolved despite the existence of two clearly competing hypotheses. The methodological weaknesses hitherto are particularly problematic given that we can expect high levels of correlation between individual errors and financial indicators, which would push up the predicted effects of the latter because of unobserved effects of, for example, intelligence, ambition, upbringing or personality. Furthermore, when these two theories have been tested against each other, the results have been either inconclusive or, by attempting to apply lessons from the aggregate level to the individual level,

highly subject to the ecological fallacy. Blais (2006: 117) argues that the two effects (caused by grievance and lowered resources) are likely to cancel each other out, resulting in 'nil overall effect'. Blais concludes that there is no effect of economic conditions on turnout by testing the effects of macro-economic indicators. The findings of this study were similar to other studies before it (Arcelus and Meltzer 1975, Blais and Dobrzynska 1998, Blais 2000, Kostadinova 2003, Fornos et al. 2004). The risk of an ecological fallacy in this case is particularly acute because, as Wright (2012: 691) points out, a discrepancy 'between aggregate and individual analyses is nothing new in the study of economics and voting.'

However, there is an area of the political behaviour literature that has extensively tackled the question of how changes in resources affect voting at the individual level – the economic voting literature. Economic voting theory states that those who are dissatisfied with their economic situation are more likely to vote against the incumbent, with the contrary for those who are satisfied with it (Kramer, 1971, Tufte 1975; Hibbs et al 1982; Chappell and Keech 1988). Perhaps the key nuance that was later added to these early works is that, at the individual level, the economy is actually partisan rather than valence issue, meaning that the relationship between economic assessments and voting is likely to be tempered by partisanship, particularly with regard to the incumbent government (Kiewet, 1981; Wright, 2012). Though the economic voting literature is concerned with which party the individual votes for, scholars have intermittently – and without strong conclusions – linked this debate to turnout. Fiorina (1978), as an addendum to his article regarding retrospective economic voting, sought to test the hypothesis put forward by Arcelus and Meltzer (1975) that economic conditions affect partisan choice only via the effect they have on turnout. His results were inconclusive.

The effect of resources on turnout has by no means been confined to financial resources. Smets and van Ham (2013) list education, age, gender, race, marital status, residential mobility, region, occupational status, socio-economic position and children as resources that have been hypothesised to affect turnout, with most of these

explanatory variables repeated at the district or national levels.[3] Most famously, Verba, Schlozman and Brady (1995) identify wealth, time and skills as crucial resource determinants of participation.

Education has been consistently shown to have a positive effect on turnout (e.g. Shields and Goidel, 1997; Verba, Schlozman, and Brady, 1995; Wolfinger and Rosenstone, 1980). Moreover, these scholars all theoretically explain this positive relationship by arguing that education imparts the skills required to easy understand and follow politics and to, in turn, take a stance on the issues involved. Indeed, Hillygus (2005) uses longitudinal data to show that individuals are more likely to vote particularly after a social science and humanities higher education, suggesting that the contents of the education do matter. Sondheimer and Green (2010) use experimental evidence to argue that the relationship between education and turnout is indeed causal and based on the transfer of skills. That being said, the link between education and turnout has not solely been theorised in terms of resources. For example, Nie, Junn, and Stehlik-Barry (1996) argue that education simply serves as a proxy for social network position and therefore is unlikely to have much causal power in itself. They did, however, see this higher social network position as theoretically causative in resource terms because it made access to political information less costly. More divergent is the explanation of Herrnstein and Murray (1994: 253) who argue that education is simply a proxy for IQ, concluding 'education predicts political involvement in America because it is primarily a proxy for cognitive ability.' Regardless, as a single predictor, it seems that there is no shortage of evidence that education – and thus the 'skills' component of Verba, Schlozman and Brady's three-resource model – is a strong positive predictor of turnout both between and within individuals.

[3] It is arguably a theoretical stretch to include marriage and parenthood as resources. I do so for two reasons. First, the approach of this essay is to explain within-individual variation in turnout using resources, as classified in the between-individual literature on turnout. Smets and van Ham (2013) place all of the independent variables that this essay analyses within the 'Resource Model' of turnout. For consistency and complementarity, I do likewise. Second, the hypothetical causal mechanism behind a positive effect of marriage and parenthood on voting could be resource-based – in terms of providing a structure, stability and role – though, clearly, there could also be non-resource-based mechanisms at play as well, in terms of altered attitudes and socialisation. Even in these cases, however, attitudinal and socialisation mechanisms are likely to be more proximal causes and partially the result of more distal, resource-based mechanisms, such as those described, that are still likely to retain their own direct effect.

The relationship between greater geographic mobility on lower turnout has been repeatedly noted (Nichols and Beck, 1995; Squire, Wolfinger and Glass, 1987). Highton (2000) argues that Americans who move home have lower voter turnout than those who do not because of the need to re-register and the disruption to social ties and local political knowledge, with the former having the strongest effect. However, this study was based on a cross-sectional study that only controls for basic socio-demographic characteristics. Highton and Wolfinger (2001) expanded this research to comprehensively investigate the effects of an array of lifestyle changes in the young including residential stability, home ownership, employment, being a student, leaving home and age. Again, they take a cross-sectional approach, finding that residential stability, home ownership, employment and being a student all have a significant positive effect, whereas there is no evidence that marriage has an effect. While Highton and Wolfinger's contribution was useful both for its comprehensiveness and because it overcame previous limitations in the data, the robustness of its causal claims are weakened by the methodological approach of using cross-sectional data to test relationships that are highly vulnerable to omitted variable biases in a similar fashion to the effects of financial resources on turnout.

White-collar occupation and student status have both been hypothesised to confer resources such as development of skills germane to political participation, political knowledge and access to political discussion that increase the individual's chance of voting. Brady et al. (1995) show individuals with a higher occupational type – on a nine-point scale – are more likely to be involved in politics, contrasting the findings of other, earlier classic works such as Wolfinger and Rosenstone (1980), that occupational status's effect on turnout is no more than a proxy for education. For similar reasons, Tenn (2007: 451) hypothesises that a student 'may be more likely to vote since he is in an environment where people commonly engage in political discourse' before finding that being a student increases one's probability of voting by around 6 per cent. However, these findings were made by matching individuals – a methodological approach since criticised by Arceneaux et al (2006) as unlikely to truly control for unobserved variables in turnout studies.

Studies considering the effect of marriage have consistently found evidence that individuals who are married are more likely to vote than those who are not (Lane, 1959; Strate et al., 1989; Stoker and Jennings, 1995; Timpone, 1998). Most recently, Denver (2008) showed this relationship when controlling for socio-economic variables as well as social connectedness and sense of civic duty, to argue that married individuals have the traditional values that impel them to vote. As with other lifestyle factors, however, this would suggest that marriage itself is not the causal factor but simply a proxy for attitudinal issues that are likely to be fairly stable across the life course. If this were the case then we should expect to see no or little effect of marriage on voter turnout within the individual over time, unless marriage does confer certain non-financial resources such as stability, after all.

Moreover, Solt (2008: 52) argues that marriage could actually deprive individuals of the free time required to vote, stating that 'married people are more likely to remind each other to vote than single people, but they are less likely to have or spend free time to otherwise engage in politics; free time and therefore political engagement decline further as the number of children in the family increases' before going on to find that married individuals are more likely to vote, when controlling for socio-economic variables. Conversely, Solt also show that individuals with more children are less likely to vote, presumably because of the constraints that childrearing places on time, in contrast to the far earlier findings of Lane (1959, 218) who argued that parenting increases one's sense of social responsibility and the desire to set a good example. Overall, it is fair to conclude that, contrary to marriage, 'the impact of having children [on turnout] is much less frequently researched' (Smets and van Ham, 2013: 7) and the results are less consistent.

Given that the theoretical underpinning of the resources model of turnout is that people vote if and when they have the capacity to do so (Verba et al, 1995), it comes as some surprise that the effect of physical and psychological health has been relatively overlooked, particularly given the former's obvious positive relationship with age. The exceptions to this dearth of research includes that of Bazargan et al (1991), who find evidence that lower self-assessed health can decrease turnout in the elderly. Nygård and Jakobsson (2013), however, could find no such link in Finland

and Sweden when controlling for other resources. Latterly, Söderland and Rapeli (2015) use ESS data covering the five Nordic countries to show that individuals are actually more likely to vote when they are unwell, arguing that this shows that citizens with poor health are mobilised into action. Clearly, there is little in the way of consensus. In terms of psychological health, we might expect depression – with its associated feelings of loss of self-esteem, loss of energy and introversion – to decrease one's chance of voting. Indeed, in one of the few studies on the subject, Ojeda (2015) finds that depression decreases one's chance of voting when controlling for other factors in a multivariate logistic model.

Overall, evidence suggesting a causal mechanism between variations in non-financial resources and turnout suffers from many of the same pitfalls as the research looking into financial resources and turnout. First, the distribution of academic attention has arguably not been proportionate to the importance of various types of resources – with education being well considered with robust methodology while physical and psychological health have largely been overlooked. Second, in both the cases of financial and non-financial resources, we see competing hypotheses repeatedly arising – in the former case between a classic resource explanation and a grievance model and in the latter case between greater skills, responsibilities and access to information on the one hand and impingements on free time on the other. Third, both have been reliant on cross-sectional studies of individuals that have used control variables in an attempt to isolate confounding factors. The importance of often less easily controlled for predictors such as upbringing, norms and peer groups – all of which are likely to correlate with both turnout and resource-based independent variables – mean that we should treat the findings of these studies with caution. Moreover, given the likely correlations between financial resources and non-financial lifestyle resources, it is striking that these two groups of explanatory variables have not been considered more profoundly together when explaining turnout. As such, this paper seeks to broaden the study of both financial and non-financial resources and investigate their effects on turnout using more methodologically robust approaches.

Approach, methods and data

In order to test these relationships more robustly, one approach is to test for the effects within individuals across time. This is particularly appropriate when looking at resources because of their correlation with (fairly) fixed effects within adults over time as already discussed. The ideal approach is therefore to use fixed effects panel data models. The advantages of panel data are that repeated observations of individuals allow for the possibility of isolating the effects of unobserved between-individual variation, such as that deriving from intelligence, ambition, upbringing or personality. Importantly for voter turnout, using panel data enhances the ability to make causal conclusions by allowing for temporal ordering - *ex post* reported turnout can be regressed on variables reported before the election. Using this data source overcomes perhaps the biggest methodological problem of the economic voting literature, as diagnosed by Lewis-Beck and Stegmaier (2007: 531), which is the endogeneity between electoral behaviour and economic responses. They state that 'panel data, rather than cross-sectional data, are the way out of this endogeneity-exogeneity dilemma.' Indeed, Lewis-Beck et al (2008) use American, Canadian and British panel data to show that the effect of economic perceptions on voter choice are not only real but greater than previously found when using cross-sectional methods.

Panel data also includes the ability to control for waves. This is particularly useful for political science because it controls for the temporal political context that affects all respondents at that point in time and may have correlations with independent variables and turnout, which would otherwise bias the results. As such, the demand-side factors that this study is interested in can be better isolated from supply-side predictors of turnout. Overall, fixed effects models test the effects of deviation from the individual's mean in the independent variables on deviation from the individual's mean in the dependent variable. This is done via the fixed effects – or *within* – transformation, which can be understood first by considering the logistic unobserved effects model for N with T time periods:

$$log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right] = \alpha_i + \beta X_{it} + \varepsilon_{it} \text{ for } t = 1, \ldots, T \text{ and } i = 1, \ldots, N$$

where $log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right]$ is the log odds of turnout for individual *i* at time *t*, βX_{it} is the time-variant $1 \times k$ regressor matrix, α_i is the individual's unobserved time-invariant effect and ε_{it} is the error term. The fixed effects model is particularly robust because the individual's unobserved time-invariant effect can correlate with the regressor matrix without biasing the estimates. This is because the fixed effects model eliminates α_i via the within transformation, which estimates the effect of deviation from the individual's mean in the independent variables on the individual's probability of voting, with respect to their mean probability of voting. As such, if $log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right] = P(y_{it})$ then the fixed effects model can be described as:

$$P(y_{it}) - \overline{P(y_i)} = (\alpha_i - \bar{\alpha}_i) + \beta(X_{it} - \bar{X}_i) + (\varepsilon_{it} - \bar{\varepsilon}_i) \Rightarrow P(\ddot{y}_{it}) = \ddot{\beta}_{it}\ddot{X} + \ddot{\varepsilon}_{it}$$

where $\overline{X_{it}} = \frac{1}{T}\sum_{t=1}^{T} X_{it}$ and $\overline{\varepsilon_i} = \frac{1}{T}\sum_{t=1}^{T} \varepsilon_{it}$. The individual's unobserved time-invariant effect, α_i, is eliminated from the equation because $\alpha_i = \bar{\alpha}_i \therefore (\alpha_i - \bar{\alpha}_i) = 0$. The logistic fixed effects estimator is then obtained via a logistic regression of $P(\ddot{y}_{it})$ on $\ddot{X}_{it}$.

I select as a data source the British Household Panel Survey (BHPS) and Understanding Society panel, both socio-economic panels which include data on voter turnout and a raft of socio-economic indicators. Although there are other socio-economic panels in Europe, such as the German SOEP and the Swiss SHP, neither of these include data on voter turnout. The BHPS, by contrast, includes turnout as one of a handful of political indicators, alongside hundreds of economic and lifestyle variables. The BHPS started in 1991 and had 18 waves until it was integrated into the newer and broader Understanding Society survey. The data comes from annual interviews of all adults in a household and observed between 9,000 and 13,000 individuals per wave. The BHPS covers four general elections. The Understanding Society survey started in 2009 and has around 60,000 respondents. Of these, 6,700 continuing respondents from the BHPS were introduced into wave two. These individuals are given specific identifying variables to allow the two surveys to be

linked and treated as one. The Understanding Society survey covers one general election meaning that the merged data source covers a total of five general elections and, as of 2015, 22 waves.

Because the survey responses are yearly and general elections are usually around four and five years apart, the same questions and responses regarding voting are recorded repeatedly. As such, I chose to take the dependent variable from waves of the year immediately following the last general election. I match this dependent variable to independent variables from the wave immediately *before* the general election to ensure suitable temporal ordering and remove some sources simultaneity bias. The match between independent and dependent variables by waves is shown in Figure 1.

Figure 1: Merger of British Household Panel Survey and Understanding Society survey and retrospective turnout and pre-election resources into five waves

Study	British Household Panel Survey																		Understanding Society	
Year	91	92	93	94	95	96	97	98	99	00	01	02	03	04	05	06	07	08	09	10	11
Source of dependent variable (retrospective turnout)		X					X				X				X					X	
Source of independent variables	X					X				X				X					X		
Resultant five waves	1					2				3				4					5		

I control for the effects of each wave, removing a second source of simultaneity bias caused by trends in turnout that affect the mean response in the sample, such as those caused by the national political context. The merged four relevant waves from the British Household Panel Survey and one relevant wave from the Understanding Society survey produce a dataset of 55,878 observations by 24,487 individuals over the course of five waves[4]. The panel is unbalanced, meaning that the number of time observations (T_i) for each individual varies. The mean number of waves that are observed for each individual is 2.28; so most individuals are recorded over at least two waves. This is important because the testing of within effects for any individual requires at least two observations. Furthermore, the most common patterns for

[4] The five waves refer to the 1992, 1997, 2001, 2005 and 2010 UK General Elections.

observations are either to be observed in all of the last three waves (6,552 observations) or in all five waves (5,402 observations). Of this sample, 24.7 per cent of individuals show within-variation in turnout, making this the subsample that will be used for fixed effects models.

The British Household Panel Survey provides very apt data to test this study's hypotheses, most importantly self-reported turnout in the previous general election. The independent variables are described in Table 1:

Table 1: Descriptive data of BHPS variables

Variable	Obs	Mean	SD	Min	Max	Waves
Turnout	55878	0.75	0.43	0	1	1-5
Income (monthly (000s), adjusted for inflation)	63854	19.00	14.47	0	539	1-5
Financial situation ('How would you describe your financial situation? Living comfortably, doing alright, just about getting by, finding it quite difficult or finding it very difficult'? *reverse coded*)	62974	3.82	1.00	1	5	1-5
Unemployment	61877	0.06	0.24	0	1	1-5
Free time ('How satisfied are you with your amount of leisure time?')	51578	4.84	1.63	1	7	2-5
University Degree	61522	0.35	0.48	0	1	1-5
Moved in last year	62632	0.11	0.31	0	1	1-5
Class	60231	3.50	1.47	1	7	1-5
Student	65515	0.01	0.11	0	1	1-5
Married	65515	0.54	0.50	0	1	1-5
Children	65515	0.28	0.45	0	1	1-5
Health status ('How would you describe your health status over the last 12 months? Excellent, good, fair, poor or very poor?')	65481	2.19	0.95	1	5	1-5
Depressed ('Would you say you are unhappy or depressed? Not at all, no more than usual, rather more, much more')	65931	1.83	0.38	1	2	1-5
Age	65514	45.39	18.65	15	101	1-5
Wave	78869	3.24	1.33	1	5	1-5

In order to measure wealth, I use two variables, income and self-assessed financial situation. While income is more objective as a measure of resources, it does have some drawbacks. First, it not a comprehensive measure of wealth. For example, students, retirees and housewives often respond as having no income but may have sufficient resources to vote and may have no financial grievances. Second, a measure

such as income fails to take into account costs. It also fails to take into account other earnings, such as dividends. Overall, the subjective measure is more holistic. What of the endogeneity risks between a subjective indicator of financial resources and turnout? First, as discussed previously, the financial data is taken from the wave *prior* to the year of the election, whereas the vote data is taken that year. Second, Evans and Anderson (2006) show that, although sociotropic economic evaluations are conditioned by party sympathies, there is no relationship between egocentric financial evaluations and party support and thus the endogeneity risks are considerably lower. Moreover, it is useful to have a subjective, psychological measure of resources alongside a strictly objective, material one.

Because the dependent variable is dichotomous, I will use a logistic estimator and report the results as odds ratios. In order to test the partisan hypothesis regarding financial resources effects on turnout, I test further models that consider how the relationship between finances and turnout varies according to whether the respondent reports either that they 'feel closest' or 'would support' the incumbent party of the time or that do not. This will allow me to test whether the effects of personal financial indicators on voting participation are confounded by one's attitude towards the incumbent party. Naturally, I am unable to use a variable that reports the party that the respondent actually voted for *ex post* because all of those respondents will have voted.

I present marginal effects from the fixed effects models that use a linear estimator. Computing marginal effects from fixed effects logistic models is problematic because the former rely on the values of the fixed effects, which are not estimated, meaning that no meaningful marginal effect can be calculated (though an alternative is to arbitrarily set each observation's fixed effect to zero – for discussion and explanation see Longhi and Nandi, 2015: 205). Overall, though, the presentation of marginal effects is useful for the purposes of comparison even if the estimates are likely to suffer some inaccuracy based on the linear estimation of a dichotomous variable.

Financial resources

In Table 2 I present four fixed effects models that test the effects of financial resources on voter turnout. When tested separately (models 1, 2 and 3) and together (4), we see the three independent variables of interest have the same direction of effect and the same statistical significance – income increases one's chance of voting whereas an improved self-assessed financial situation decreases one's chance of voting, as does unemployment. As such, the resource model is supported for the objective indicators of financial resources whereas the grievance model is supported for the subjective and more holistic measure of financial well being – though this latter effect is not statistically significant.

Table 2: The effects of financial resources on turnout (fixed effects logistic models)

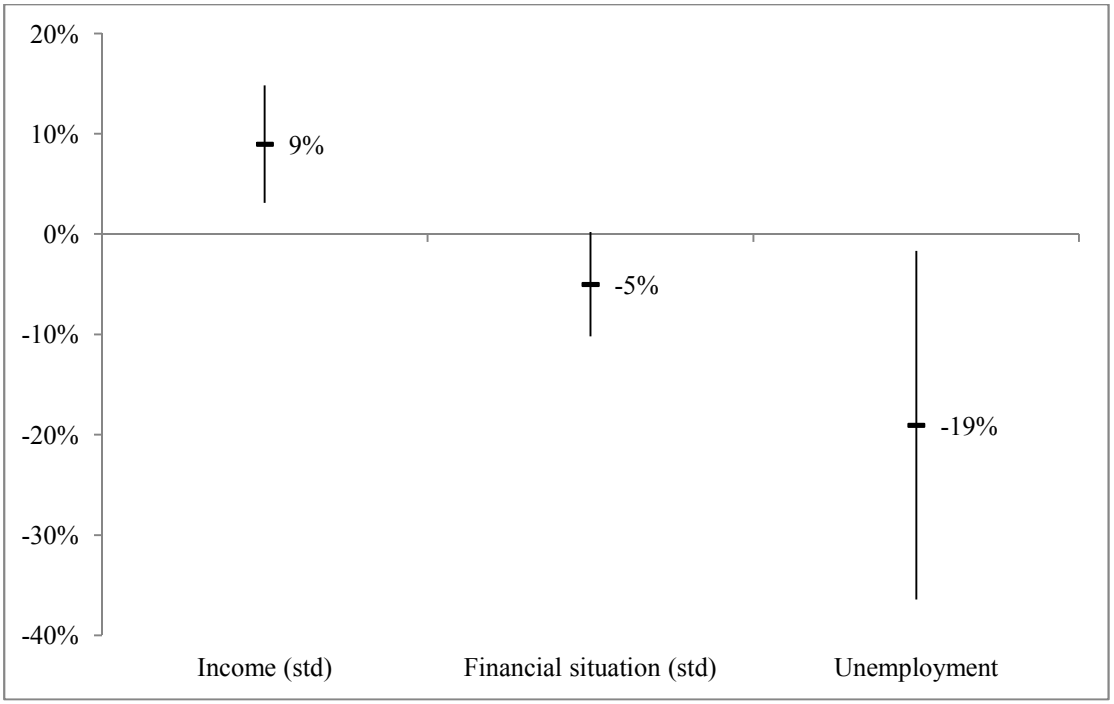
	(1) Income	(2) Financial Situation	(3) Unemployment	(4) Combined
Income	1.082***			1.094**
	(0.031)			(0.032)
Financial situation		0.970		0.951
		(0.025)		(0.025)
Unemployment			0.833**	0.827*
			(0.075)	(0.075)
Age	0.895	0.895	0.902	0.899
	(0.0659)	(0.0662)	(0.0669)	(0.0669)
Wave (ref: 1)				
Wave 2	1.278	1.283	1.223	1.244
	(0.471)	(0.475)	(0.454)	(0.463)
Wave 3	0.893	0.893	0.820	0.837
	(0.585)	(0.588)	(0.541)	(0.554)
Wave 4	1.668	1.693	1.499	1.547
	(1.602)	(1.632)	(1.450)	(1.501)
Wave 5	4.094	4.100	3.547	3.674
	(5.134)	(5.163)	(4.478)	(4.656)
Observations	12,537	12,481	12,348	12,322
Individuals	3,675	3,663	3,626	3,622

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

The marginal effects from the combined model (4) are presented in Figure 2. The measures of income and self-assessed financial situation are both standardised so that their effects are the change in probability of turnout that results from a one standard deviation increase in each of the independent variables. The unemployment measure is not standardised – partially accounting for its far greater magnitude, given its

relative rarity. Clearly, however, the effect of becoming unemployed is dramatic and negative on one's probability of voting. The positive effect of greater income and the negative effect of an improved financial situation are more modest by comparison. Overall, when assessed alone, it is clear that within-individual variation in financial resources have an important effect on turnout, though with somewhat conflicting theoretical ramifications.

Figure 2: Marginal effects of financial resources on voter turnout



I now turn towards considering how applicable the finding from the economic voting literature – that partisanship has a major confounding effect on the relationship between financial resources and electoral support for the incumbent party – is for the already established relationships between financial resources and turnout. In Table 3 I present the same three variables from Table 1 tested for those who do not support the governing party (models 1, 2 and 3) and those who do support the governing party (models 4, 5 and 6). The magnitudes of the odds ratios of each of the three variables are strengthened when only considering those who do not support the governing

party. The odds ratio of financial situation becomes statistically significant whereas that of unemployment loses its statistical significance. For those who do support the governing party, none of the effects are statistically significant and they are all weaker than when the entire sample is considered.

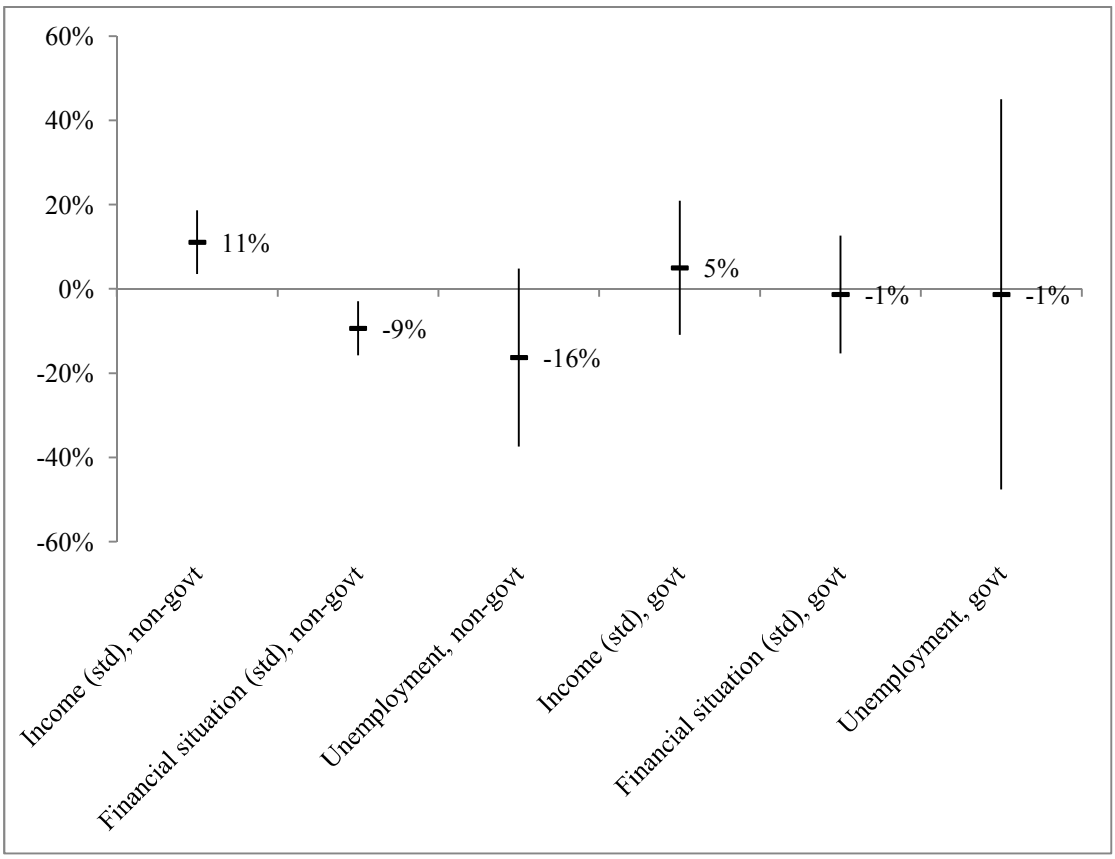
Table 3: The confounding effect of partisanship on the effects of financial resources on turnout (fixed effects logistic models)

	(1) Income (non-govt)	(2) Financial Situation (non-govt)	(3) Employment (non-govt)	(4) Income (govt)	(5) Financial Situation (govt)	(6) Unemployment (govt)
Income	1.093**			1.040		
	(0.041)			(0.081)		
Financial situation		0.928**			1.000	
		(0.030)			(0.070)	
Unemployment			0.886			0.989
			(0.095)			(0.251)
Age	0.921	0.905	0.917	0.956	0.970	0.958
	(0.087)	(0.086)	(0.087)	(0.171)	(0.173)	(0.172)
Wave (ref: 1)						
Wave 2	1.297	1.448	1.335	0.648	0.603	0.625
	(0.607)	(0.688)	(0.632)	(0.584)	(0.542)	(0.564)
Wave 3	0.730	0.865	0.753	0.728	0.650	0.689
	(0.610)	(0.733)	(0.636)	(1.172)	(1.043)	(1.111)
Wave 4	1.261	1.648	1.341	0.944	0.792	0.890
	(1.543)	(2.045)	(1.658)	(2.213)	(1.853)	(2.093)
Wave 5	3.031	4.166	3.258	1.178	0.933	1.085
	(4.840)	(6.750)	(5.258)	(3.604)	(2.849)	(3.330)
Observations	7,649	7,584	7,505	1,549	1,562	1,541
Individuals	2,560	2,540	2,515	657	662	654

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

Below are marginal effects from the six models in Table 3. Quite clearly, the effect of financial resources on turnout is only felt by those who do not support the governing party. Furthermore, the strength of the effects of income and financial situation are larger amongst those who do not support the governing party than they are for the entire sample. Particularly, the negative effect of an improved financial situation on turnout is nearly double for those non-incumbent supporters than it is for the sample as a whole and this effect disappears entirely for incumbent party supporters, lending strong evidence to the grievance theory.

Figure 3: The marginal effects of financial resources on turnout, by partisanship of the incumbent party



Non-financial resources

In Table 4 I present the effects of non-financial resources on turnout. An increased satisfaction with one's amount of free time (model 1), getting married (6) and having children (7) all increase the chance of turnout – with fairly similar odds ratios in the combined model (10). Conversely, moving house in the last year (2), being in a lower class of occupation (4), being a student (5), relatively poor physical health (8) and depression (9) all decreased the chance of voting – though the effect of physical health is not statistically significant in the combined model.

Table 4: The effects of non-financial resources on turnout (fixed effects logistic models)

	(1) Free time	(2) Education	(3) Mobility	(4) Occupation Type	(5) Student	(6) Marriage	(7) Children	(8) Physical health	(9) Mental health	(10) Combined
Free time	1.038*									1.045**
	(0.020)									(0.022)
University degree		1.043								0.834
		(0.086)								(0.087)
Moved in last year			0.580***							0.656***
			(0.038)							(0.052)
Occupation (ref: manager/technical)										
Professional				0.944						0.991
				(0.152)						(0.191)
Skilled non-manual				0.832**						0.814*
				(0.071)						(0.084)
Skilled manual				0.855						0.872
				(0.085)						(0.107)
Partly skilled				0.894						0.927
				(0.088)						(0.111)
Unskilled				0.880						0.938
				(0.128)						(0.168)
Armed forces				0.614**						0.972
				(0.135)						(0.301)
Student					0.454***					0.500
					(0.110)					(0.314)
Married						1.838***				1.805***
						(0.126)				(0.170)
Children							1.653***			1.683***
							(0.104)			(0.143)
Bad health								0.945**		0.980
								(0.027)		(0.037)
Depressed									0.915***	0.926**
									(0.027)	(0.035)
Age	0.868	0.885	0.894	0.883	0.896	0.902	0.909	0.900	0.881	0.848
	(0.074)	(0.066)	(0.066)	(0.067)	(0.066)	(0.067)	(0.066)	(0.066)	(0.070)	(0.077)
Wave (ref: 1)										
Wave 2		1.349	1.304	1.357	1.237	1.231	1.199	1.264	1.333	1.408
		(0.505)	(0.481)	(0.515)	(0.452)	(0.455)	(0.439)	(0.462)	(0.492)	(0.535)
Wave 3	0.737	0.984	0.899	0.976	0.861	0.820	0.777	0.870	0.960	1.050
	(0.249)	(0.655)	(0.591)	(0.659)	(0.560)	(0.539)	(0.506)	(0.565)	(0.631)	(0.710)
Wave 4	1.594	1.959	1.664	1.938	1.601	1.468	1.370	1.595	1.863	2.147
	(1.099)	(1.910)	(1.601)	(1.916)	(1.526)	(1.414)	(1.306)	(1.518)	(1.793)	(2.127)
Wave 5	4.475	4.895	4.015	5.553	3.876	3.413	3.134	3.808	4.561	5.722
	(4.604)	(6.232)	(5.046)	(7.170)	(4.823)	(4.291)	(3.902)	(4.733)	(5.733)	(7.405)
Observations	8,954	12,230	12,547	11,873	12,704	12,704	12,704	12,553	12,101	8,114
Individuals	3,038	3,582	3,674	3,530	3,721	3,721	3,721	3,667	3,552	2,809

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

Below, in Figure 4, are the marginal effects of those non-financial resources that had statistically significant effects in the combined model. All of the effects are standardised, with the exception of the categorical variable measuring occupational type (which explains its relatively large effect and confidence intervals). Two marginal effects standout – those of being married and having children – even when controlling for other variables such as free time and age, both of which have previously been argued to be the true causal mechanisms at the heart of the relationship between marriage, parenthood and voting. The effect sizes are large – 29% for marriage and 23% for having children. The positive marginal effects of satisfaction with free time and the negative effects of moving house and suffering

from depression are more comparable to those of the financial resources presented in Figure 2.

Figure 4: The marginal effects of non-financial resources on probability of turnout

Financial and non-financial resources

Finally, in Table 5, I present the results of two regressions testing the effects of both financial and non-financial resources on turnout. In the second of these I exclude the free time variable both because it was the only predictor not available in every wave, so necessitating the removal of that wave for the entire combined model, and because the effect of marriage and children has long been theoretically associated with the positive effect of stability and the negative effect of less free time. Again, more free time, being married and having children have statistically significant positive effects on within-individual variation in voting. An improved financial situation, moving in the last year, working in a skilled non-manual position rather than a managerial one, and depression all have negative effects on turnout. Unlike in the previous models, the effect of income is no longer statistically significant, nor is the effect of

unemployment. The effect of occupation is only statistically in the model that includes the effect of satisfaction with free time.

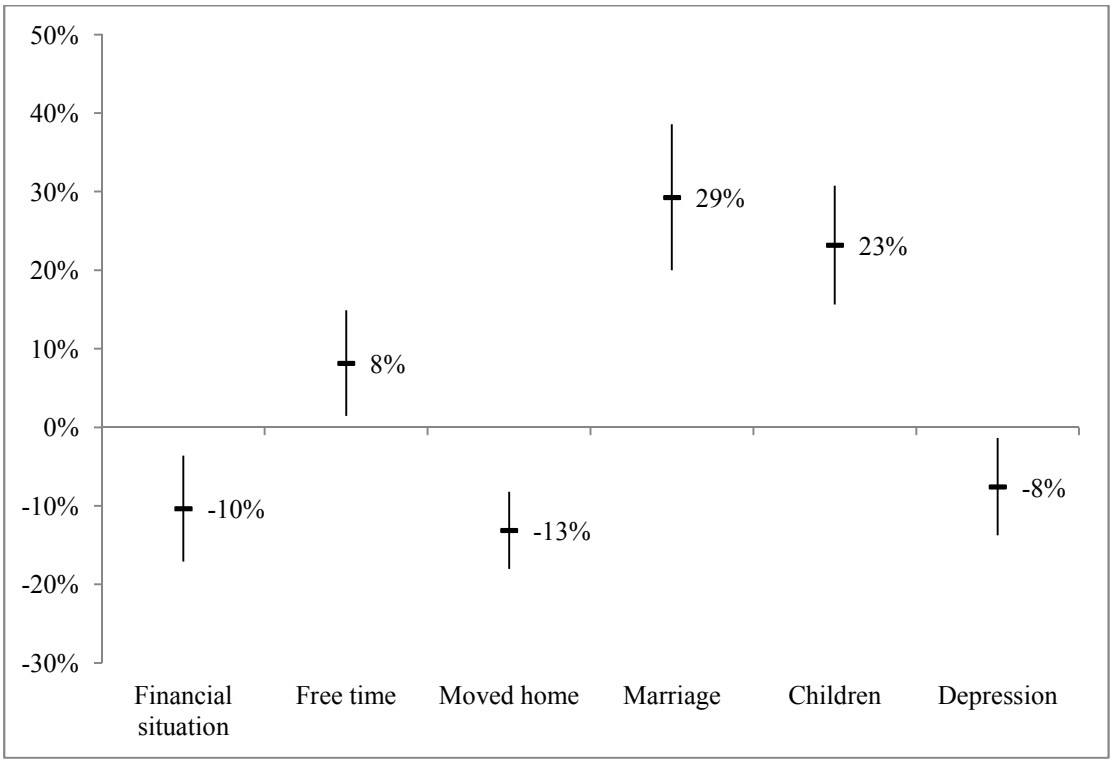
Table 5: The effects of financial and non-financial resources on turnout (fixed effects logistic models)

	(1) Including Free Time	(2) Excluding Free Time
Income (std)	1.059	1.045
	(0.039)	(0.0320)
Financial situation	0.902**	0.942**
	(0.031)	(0.027)
Unemployment	0.860	0.869
	(0.098)	(0.081)
Free time	1.085*	
	(0.037)	
University degree	0.832	0.900
	(0.088)	(0.080)
Moved in last year	0.658***	0.617***
	(0.053)	(0.043)
Occupation (ref: manager/tech)		
Professional	1.016	0.966
	(0.198)	(0.164)
Skilled non-manual	0.811*	0.848
	(0.084)	(0.076)
Skilled manual	0.857	0.864
	(0.106)	(0.090)
Partly skilled	0.924	0.885
	(0.111)	(0.091)
Unskilled	0.954	0.840
	(0.172)	(0.128)
Armed forces	0.968	0.746
	(0.301)	(0.178)
Student	0.479	0.638
	(0.301)	(0.169)
Married	1.799***	1.492***
	(0.171)	(0.115)
Children	1.672***	1.436***
	(0.143)	(0.099)
Health status	0.970	0.976
	(0.0364)	(0.031)
Depressed	0.927*	0.940*
	(0.029)	(0.025)
Age	0.856	0.873
	(0.078)	(0.070)
Wave (ref: 1)		
Wave 2		1.435
		(0.574)
Wave 3	0.734	1.051
	(0.266)	(0.749)
Wave 4	1.692	2.201
	(1.249)	(2.297)
Wave 5	5.234	6.343
	(5.763)	(8.647)
Observations	8,068	11,206
Individuals	2,798	3,346
Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05		

In Figure 5, I present the standardised marginal effects derived from the model 1 in Table 5. Now controlled for financial resources, the positive effects of marriage and children remain unchanged – statistically significant and very strong. Similarly, the

negative effect of having moved in the last year is identical. The negative effects of improvement in one's self-assessed financial situation and depression, as well as the positive effect of satisfaction with free time, are actually strengthened in the combined model.

Figure 5: Marginal effects of statistically significant financial and non-financial resources on turnout



On Marriage and Parenthood

The impressive effects of marriage and parenthood on voting warrant further attention. The above models suggest that the positive effects of marriage and parenthood cannot be explained as proxies for greater financial resources, socio-economic positioning, health or free time. In the previous section I considered simply whether one is married or not and whether one has a child in the house or not. In this section I will delve further into these relationships by considering the effects of other familial types and the effects of different numbers of children and at varying ages. First, I consider the effects of marriage in comparison to living together as a couple,

being widowed, being divorced, being separated, and never having been married. If marriage transfers certain skills, then those who leave marriage, either by way of widowhood or divorce and separation, may keep these skills. Furthermore, those who live together as a couple may be bestowed with the same resources of marriage – stability, security, support and socialising. Alternatively, it could be that married couples live qualitatively different lifestyles – presumably more interlocked – than couples living together in a relationship unmarried. Furthermore, it may be that marriage – societally rooted as it still is in tradition, reverence and prestige – has a uniquely positive effect on the individual's desire to take part in the social act of voting.

I also investigate further the effect of having children on turnout. It could be that parents are more likely to vote because the decision to become a parent represents a singular lifestyle choice that is constant thereafter, regardless of number of children. Furthermore, as mentioned above, Solt (2008) hypothesises that more children is likely to decrease the individual's chance of voting because of restraints on time. Alternatively, more parenting and more children may further reinforce the causal mechanism that makes being a parent increase the likelihood of voting – like marriage it may lead to a greater sense of stability and integration in the community as well as further reinforcing a sense of responsibility and need to provide a good role model as their lifestyle becomes more oriented around parenting. I also consider the effect of the age of the children in the household that the individual is responsible for. Again, such variation could have two alternative effects on turnout. On the one hand, parenthood may be a more intense lifestyle with younger children, amplifying its effect on voting. On the other hand, having older children may increase the desire to provide a good example on social matters like politics.

I present the findings of the fixed effects models, based on the above hypotheses, in Table 6. I include models that test each of the four independent variables both with and without a variable measuring the respondent's satisfaction with their amount of free time to account for the indication from the literature that the effects of marriage and children may be mitigated by the time restraints associated with marriage and parenthood.

Table 6: Effects of marriage and parenting on turnout

	(1) Marital status	(2) Marital status	(3) Number of children in HH	(4) Number of children in HH	(5) Responsible for child under 16	(6) Responsible for child under 16	(7) Responsible for child under 12	(8) Responsible for child under 12
		With free time control		With free time control		With free time control		With free time control
Marital status (ref: never married)								
Married	1.444***	1.617***						
	(0.156)	(0.219)						
Couple living together	0.934	0.881						
	(0.089)	(0.101)						
Widow	0.814	0.728						
	(0.174)	(0.195)						
Divorced	0.728*	0.709						
	(0.114)	(0.135)						
Separated	0.639**	0.504**						
	(0.111)	(0.112)						
Number of children in HH (ref: 0)								
1			1.411***	1.654***				
			(0.102)	(0.147)				
2			1.652***	2.100**				
			(0.134)	(0.216)				
3			1.560***	2.095***				
			(0.187)	(0.318)				
4 or more			2.192***	2.778***				
			(0.534)	(0.804)				
Responsible for child under 16					1.424***	1.629***		
					(0.120)	(0.172)		
Responsible for child under 12							1.267*	1.393*
							(0.135)	(0.185)
Free time		1.042*		1.055**		1.043*		1.034
		(0.020)		(0.021)		(0.020)		(0.028)
Age	0.964	0.939	0.967	0.948	0.957	0.944	0.923	0.858
	(0.073)	(0.082)	(0.072)	(0.083)	(0.072)	(0.082)	(0.073)	(0.099)
Age^2	0.999***	0.999***	0.999***	0.999***	0.999***	0.999***	0.999***	0.999***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Wave (ref: 1)								
Wave 2	1.249		1.194		1.315		1.764	
	(0.464)		(0.446)		(0.487)		(0.881)	
Wave 3	0.841	0.712	0.770	0.648	0.929	0.715	1.426	1.015
	(0.556)	(0.244)	(0.504)	(0.221)	(0.612)	(0.244)	(1.270)	(0.456)
Wave 4	1.536	1.516	1.355	1.266	1.786	1.528	3.724	3.657
	(1.488)	(1.057)	(1.300)	(0.880)	(1.723)	(1.061)	(4.852)	(3.331)
Wave 5	3.664	4.231	3.125	3.240	4.487	4.275	14.302	18.487*
	(4.635)	(4.401)	(3.912)	(3.363)	(5.65)	(4.430)	(24.338)	(25.124)
Observations	12,702	8,953	12,704	8,954	12,647	8,950	7,391	5,390
Individuals	3,721	3,038	3,721	3,038	3,708	3,036	2,300	1,907

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

The results from model 1 indicate strongly that it is only marriage, and not living together as a couple, that has a positive effect on voting. This model also shows that one's chance of voting when divorced or widowed is not only lower than when married but also lower than before marriage. It seems, therefore, that the institution of marriage that has a uniquely positive effect on voting that does not continue after a marriage ends and cannot be replicated by living together unmarried (I code same-sex 'civil partnerships' as 'married', rather than 'living together as a couple', given the institutionalised nature of that arrangement). In model 2, when a control for free time is introduced, the effect of marriage is only strengthened, suggesting that it does produce some negative effects on voting via its use of time otherwise. Model 3 considers the effects of number of children on voting, showing that more children living in the household leads to a higher chance of the respondent voting in a fairly linear fashion. Again, controlling for free time only enlarges these effects. Finally, from models 5 and 7 we can see that the effect of parenthood is stronger for those

whose children are under 16 than under 12, suggesting that the desire to provide a good role model to children via political participation increases as the children approach voting age. Both of these effects are strengthened when free time is controlled for.

Conclusion and discussion

In this paper I have analysed the effects of resources on turnout. Despite being perhaps the most studied relationships in the voter turnout literature, there were three major shortcomings in the studies hitherto. First, they often lacked the methodological sophistication to produce robust conclusions. Second, those studies that looked at financial resources had not sufficiently paid heed to the likely interplay with political allegiances that the economic voting literature had already identified. Third, the distribution of academic endeavour had not been proportionately allocated so that there have been exhaustive and impressive findings regarding education but far less clarity on the role of such major life changes as parenthood and health. In response, this paper sought to draw make more robust causal claims about resources and turnout. To do this, it used socio-economic panel data covering five general elections in the United Kingdom and produce fixed effects models that control for non-varying individual characteristics. In doing so, I present a number of findings.

First, financial resources are less important than non-financial ones. Indeed, when controlling for lifestyle factors, only the perception of financial wealth matters – and that has a *negative* relationship with voting, which, combined with the interaction effect for incumbent support that I present, strongly suggests that people vote out of economic grievance not economic reward. We already knew that financial resources were important predictors of who is likely to vote. However, this study casts doubt over their relative – and, in the case of the objective measure of income, absolute – importance when lifestyle factors are simultaneously considered. Essentially, this would suggest that most of the heavy lifting that income does when predicting which individuals vote is actually as a proxy for other factors such as intelligence,

upbringing and social circle, which vary little or not at all within individuals over time in adulthood.

Marriage and parenthood, as well as residential stability, are strong predictors of within-individual variation in voting. However, are being marriage and having children correctly classified as resources? Previously they had been included in so-called resource models of turnout (e.g. Smets and van Ham, 2013) because it was assumed that they conferred, or perhaps even diminished, resources of time and skills, as well as access to a political knowledge and a reminder to vote. This study shows the primacy of marriage and parenthood in causing an increase in the probability of voting over financial resources, even when controlling for free time (though this does cause the effect to weaken). In this case, it seems more likely that marriage and parenthood cause an increase in one's chance of voting because they provide stability, support, discourse, a sense of role and place and, arguably, promote more responsible behaviour.

When financial and non-financial resources are considered together, it is an increase in the non-financial resources – family, time, stability, health - that increase the individual's likelihood of voting, whereas the role of one's perception of money instead drives turnout via grievance not capacity. Future research should make use of the greater methodological capacity that current data provides to further unpick these relationships. The effects and causal mechanisms regarding both income and subjective financial situation warrant further clarification, not least because of their contradictory effects. That the positive effect of marriage on voting seemingly disappears when the marriage ends, either by death or separation, also suggests that the causal mechanism at play here is resource-based, rather than attitudinal, the latter of which would presumably remain. As such, when the resource disappears, the voting rate returns to pre-marriage levels. More detailed longitudinal analyses could unpick these relationships further. It would also be particularly interesting to test how these findings might be reproduced at the aggregate-level, perhaps explaining recent trends in national turnout rates. This paper would suggest that the post-war baby boom coincided with the post-war high turnout rates for a good reason. These results are consistent and important. On the one hand, money cannot buy political

participation and rootlessness makes for bad citizens. On the other, the gravity and sway of marriage and parenthood encourages individuals to ‘up their game’ when it comes to their social – and political – roles as citizens.

Voter Psychology and Voter Turnout

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Variables measuring voter psychology, such as political interest, partisan identification, political efficacy and sense of civic duty, have been studied as potential determinants of turnout perhaps more so than any other single group of predictors with the exception of resources. Part of their popularity lay in the overtness of their causal mechanisms however it is this proximity with turnout itself that has historically produced questionable findings. I take a within-individual approach to untangle the effects of seven identified groups of political psychological variables using three data sets from the UK and Switzerland. This approach overcomes much of the risks of unobserved heterogeneity as well as some sources of simultaneity bias. The results of the resultant analyses are that the effects of political efficacy, political knowledge and populist attitudes are all very much secondary to the effects of interest in politics or the election and a sense of civic duty towards voting. I also consider the role of the Big Five personality traits – finding that, although their direct effects are very minor, they each affect turnout via their effects on political interest and sense of civic duty.

Introduction and literature

The commonality between psychological explanations for variation in voter turnout is that they focus on the individual's feelings, perceptions and cognition towards voting as an act, the political system, and political actors, as well as towards specific elections. The psychological model varies from other explanatory models of voter turnout, such as the resource model and the mobilisation model, for two reasons. Whereas resources focus on the demand-side of voting and mobilisation focuses on the supply-side of voting, psychological factors – such as political interest, party identification, political knowledge and political efficacy – are theoretically linked to both the demand and supply sides. For example, changes in an individual's life may lead them to taking a greater interest in politics, while simultaneously there may be events that increase the political interest of the population at large. Second, these psychological factors theoretically tend to be highly proximal to the act of voting itself in any explanatory model. For example, party identification is likely to very strongly correlate with party choice (notwithstanding phenomena such as tactical and protest voting – see Dennison and Birch, 2017) and, as such, voter turnout. As such, the explanatory power of psychological variables as exogenous predictors is likely to be highly intertwined with and difficult to separate from their endogeneity with the act of voting.

Similarly, explanations that directly focus on attitudes to the act of voting – such as asking respondents whether they believe they have a duty to vote – are subject to endogenous relationships with turnout via reverse causality whereby the independent variable may be used as a *post facto* justification for the dependent variable. This latter challenge, in particular, has led scholars to treat with scepticism the unsurprising findings of often-high correlations and effects between political psychological variables and turnout. Below, I group these political psychological explanatory factors into 7 groups – political interest, partisan identification, political efficacy, political knowledge, political trust and satisfaction with democracy, feelings towards voting as an act and personality types - and overview the findings in the literature hitherto regarding their respective relationships with electoral participation.

Political interest

Political interest is widely seen as a key determinant of political participation and one of the cornerstones of a healthy democracy. Intuitively, a citizenry with low political interest will lack the motivation to be politically critical and, therefore, will be unable to effectively hold policy-makers to account. Definitions and operationalisations of interest have varied considerably. Campbell et al (1960) used interest in the electoral campaign and, later, 'attentiveness' to or 'awareness' of politics was often used instead of interest (e.g. Zaller, 1992), with 'curiosity' being a slightly less commonly used term (e.g. van Deth, 1990). Van Deth and Elff (2004: 479) refer to political interest but again define it as 'the degree to which politics arouses a citizen's curiosity'. Also common as a term to either include or directly replace political interest is political involvement. Verba, Nie and Kim (1978) operationalize political involvement using two measures, one for political discussion and one for political interest.

The earliest major works on political interest and participation examined the relevance and effect of the former in simple cross-tabulations. Early classics such as Campbell et al (1960) and Key (1966) found that those with greater political interest are more likely to vote. Lazarsfeld et al (1944: 63, 79) also found that the more interested in politics one is, the sooner they decide for whom to vote and the more they expose themselves to political information. Since then, political interest has repeatedly been found to increase one's chance of voting (e.g. Verba, Schlozman and Brady, 1995: 334) and participating in politics more broadly (e.g. Schlozman, Verba and Brady, 2012: 212, 222). The findings of these works have led political interest to become established as one of the standard control variables used throughout the political behaviour literature.

More recently, the potential endogeneity between political interest and voting, whereby any estimated effects are likely to be biased by one or more common explanatory variables, has come under greater scrutiny (Rubenson et al, 2004). Most notably, Denny and Doyle (2008: 309) argue that 'while it is true that individuals who report a greater interest in politics are also more likely to vote … both political

interest and turnout are driven by common characteristics, both observable and unobservable, which generate a correlation between the two and this vitiates the common practice of modelling the latter as depending on the former. We argue that it is better to include the original factors that drive both outcomes, rather than using one to explain the other.' They form this argument by producing a series of bivariate probit models that test the effects of single variables (relating to socio-demographics, personality types and cognitive ability) on both interest and turnout and then summarising these as having fairly consistent similarities in terms of effect size and statistical significance.

However, on closer inspection, of the 18 variables Denny and Doyle used for bivariate analysis, only four – social class, civic duty, aggressiveness, and comprehension ability – *both* share the same direction of effect on interest and turnout and are statistically significant. Indeed, just as many variables – self-employment, marriage, gender and verbal ability - had opposite directions of effect when tested on interest and turnout, enough to suggest the two are sufficiently distinct for one to have a causal effect on the other, contrary to their conclusion that 'the positive association between political interest and turnout is unlikely to be causal as they are reflections of the same underlying construct (Denny and Doyle, 2008: 305). Clearly, however, more stringent efforts need to be taken when testing the effect of interest on turnout, rather than simply including both in a cross-sectional model based on contemporaneous survey data.

Partisan identification

Partisan identification – feelings of loyalty or congruency with a party - has most prominently been used to explain voter choice – particularly as the cornerstone of the Michigan Model (Campbell et al, 1960). Theoretically, partisan identification should also increase turnout by reducing the cost of information for deciding for whom to vote (Green, Palmquist, & Schickler, 2002). Empirically, at the aggregate level Rosenstone and Hansen (1993) argue that the modest decline in partisan loyalties played some role in late twentieth century decline in American turnout. At the individual-level Bartels (2000: 37) argued that 'individual turnout decisions are

increasingly sensitive to the strength of prospective voters' preferences for one candidate or the other, which derive in significant part from long-term partisan attachment.' Further studies have shown that aggregate-level turnout is supressed in votes in which there is no partisan identification on the ballot box, such as nonpartisan local and state elections (Schaffner, Streb, & Wright, 2001, Schaffner and Streb, 2002). The weakness of these studies is that they either rely on aggregate level cross-time variation in partisan identification – subject to the ecological fallacy when drawing conclusions at the individual level – or they use cross-sectional approaches that are particularly poor at separating the endogeneity between party choice survey responses and turnout survey responses – most notably that individuals are likely to respond that they identify with the party for whom they voted *post facto*.

Internal and external political efficacy

Political efficacy – the belief that, in this case, voting produces a desired or intended effect – has become a core explanatory and, increasingly, dependent variable within the political behaviour literature. Efficacy is generally divided between feelings that are directed internally and externally. Clarke and Acock (1992: 552) define internal political efficacy as 'the perception that one has the requisite skills and resources to influence the political system' and external political efficacy as 'the perception that government institutions and elites are responsive to ones attempts to exert political influence.' These feelings were quickly linked to voting. Regarding internal efficacy, Almond and Verba (1963: 257) argued that 'belief in one's competence is a key political attitude. The self-confident citizen … is likely to be more active.' Empirical studies have gone on to show that higher levels of internal efficacy are associated with political participation (Clarke & Acock, 1989; Scheufele & Nisbet, 2002). Furthermore, internal efficacy has been shown to be a significant predictor of turnout amongst young and first-time voters in cross-sectional studies (Kaid et al, 2007; Moeller et al, 2014).

Similarly, external efficacy has consistently been shown to have a positive association with turnout (Campbell et al, 1954; Campbell et al, 1960; Verba and Nie, 1972; Abramson and Aldrich, 1982; Pollock, 1983; Verba et al, 1995; Rosenstone and

Hansen, 1993; Norris, 2004). However, these studies rely on cross-sectional analyses, raising the question of whether the relationship is genuinely causal or, like many of the political psychological variables, raising endogeneity concerns not least regarding the potential for efficacy responses to be biased by actually reporting retrospective justifications for decisions to turnout or not.

Political Trust

The extent to which individuals trust politicians and the democratic system – deemed crucial for democratic legitimacy (e.g. Easton, 1965) - inspires two opposite theoretical effects on turnout. On the one hand, citizens who do not trust politicians may be motivated to vote in order to 'kick the rascals out' (Grönlund and Setälä, 2007). On the other hand, citizens who lack political trust may see voting as less worthwhile, in a similar vein to external efficacy, as they are unlikely to believe that campaign promises will be met or that any politician is deserving of their seal of approval in the ballot box. Perhaps because of these conflicting hypothetical effects, findings have been mixed at both the individual and aggregate levels. Citrin (1974) presented evidence of a weak relationship between political trust and voting while Hetherington (1999) concluded that aggregate level declines in voting could not be explained by declining trust. Grönlund and Setälä (2007) criticise these studies for not better delineating attitudes to, on the one hand, the political system and politicians as a whole and, on the other, specific governments, parties and politicians. They go on to show that, at the aggregate level, trust in parliament and satisfaction with democracy have positive effects on turnout across European countries, whereas at the individual level, only the former relationship is replicated. More recently, Wang (2016) used Taiwanese data to find that the effect of political trust on turnout is mediated by feelings of civic duty, casting doubt on to previous findings.

Voting as an act

Whereas other political psychological variables tend to focus on the individual's feelings towards politics or political actors, a handful of explanatory factors have been put forward that measure the citizen's feelings about the act of voting itself. Owing to

its key role in the rational choice literature, the most common of these by some distance is civic duty. The rational choice literature is based on Riker and Ordeshook's 1968 'calculus of voting', which shows that, if individuals voted according to an instrumental calculus, then no one would vote because the expected benefits of their single vote are overwhelmingly likely to be nil (given the rarity by which a single vote affects an electoral result) and the costs in terms of time and effort are non-trivial. Riker and Ordeshook (1968: 27) overcame this paradox by adding a 'D' to their equation, which represents satisfaction from 'compliance with the ethic of voting, which if the citizen is at all socialized into the democratic tradition is positive when he votes' – in short, the feeling of having a civic duty to vote. Recent works have gone on to show that, indeed, a higher sense of civic duty is associated with turnout (Blais, 2000; Blais and Achen, 2010) when tested using cross-sectional models. However, given the popularity of the calculus of voting, it comes as some surprise that the 'D' that is necessary to solve the equation has overwhelmingly been viewed and tested as if Riker and Ordeshook's suggestion of duty is the only possibility. In fact, any non-instrumental benefit that the citizen gains from voting has the potential to outweigh the costs of voting – themselves highly subjective and likely to vary – and therefore could solve the calculus.

Political Knowledge

The proposed theoretical links between political knowledge and turnout are numerous and relatively complex. For example, Fedderson and Pesendorfer (1996, 1999) posit that citizens with less knowledge have an incentive to delegate the responsibility of electoral choice to those whom they believe are more informed. Matsusaka (1995) theorises that better-informed voters gain additional utility from the act voting. Empirically, Palfrey and Poole (1987) analyse 1980 US presidential election voters to show that political knowledge increases turnout, a finding replicated by Delli Carpini and Keeter (1993), Lassen (2005, using a field experiemnt) and Larcinese (2007, using an instrumental variable approach). Overall, the evidence for political knowledge's positive effect on turnout is relatively consistent by political psychological standards.

Personality Traits

Personality traits – typically operationalised using the 'Big Five' personality traits of agreeableness, conscientiousness, extraversion, neuroticism and openness - have been increasingly used to explain political behaviour. The Big Five model of personality has been a particularly popular metric because it captures a wide range of individual variation and is formed early in life and is therefore relatively static (Srivastava et al, 2003; Johnson et al, 2005; Mondak, 2010). This makes it a relatively distinctive as an efficient and exogenous psychological predictor of political behaviour as well as useful for a range of other fields from scholastic achievement to healthy lifestyles (Ozer and Benet-Martínez 2006) as well as political attitudes and party choice (e.g. Bakker et al. 2015a, 2015b).

In particular, Gerber et al. (2010: 267) note and replicate the findings of the two most consistent effects between the Big Five and left-right placement and party choice. First, more conscientious individuals – defined as 'socially prescribed impulse control that facilitates task- and goal-directed behaviour' - are more conservative, theoretically explained by the greater heed that they take of norms, order and socially-prescribed accomplishments. The second consistent finding is that more openness to new experience – defined as 'the breadth, depth, originality, and complexity of an individuals' mental and experiential life' – leads to more left-wing views, theoretically explained by a greater acceptance of unorthodox behaviour, societal change, radicalism and unconventional policies that are generally associated with the left. A third trait, emotional instability or neuroticism – defined as negative emotionality in contrast to even-temperedness - has also been associated, albeit slightly less consistently, with left-wing views, resulting from greater anxiety, impulsive rejection of positive assessments of the status quo and a desire for firm state imposition of control rather than the seemingly chaotic free market and 'organic' society valued by conservatives.

Dennison (2015) expands on the simple left-right party choices to look at the rise of new anti-establishment parties in the UK. He finds that, in 2015, as expected Conservative voters were more conscientious than Labour voters, while Labour voters

were more neurotic and open to new experiences than Conservative voters. More surprising is that, though voters of the populist right UKIP and populist left Green Party followed the predicted left-right breakdown on openness, *both* were relatively un-conscientious, disagreeable, introverted and emotionally unstable. He concludes that 'individuals less likely to go with the flow and less energised by big social organisations have been the first to jump ship from Labour and the Conservative party to the protest parties.' This causal mechanism may be replicated with turnout, whereby higher conscientiousness, agreeableness, extroversion and emotional stability, as well as lower openness to new experience, will be correlated with higher turnout, given that voting is a highly orthodox and social prescribed activity.

Despite the attention given to attitudes and party choice, Denny and Doyle (2008: 292) could state that no studies 'to date have analysed the effect of personality on the decision to turn out to vote'. In their analysis they use a less common personality type framework that classifies individuals when they were children on the basis of six sets – 'Cautious/Impulsive, Moody/Even-Tempered, Timid/Aggressive, Flexible/Rigid, Sociable/Withdrawn and Lazy/Hardworking' (Denny and Doyle, 2008: 304). They find some evidence that greater even-temperedness (comparable to emotional stability), aggression (somewhat comparable to less agreeableness) and assiduousness (somewhat comparable to conscientiousness) are associated with turnout.

Despite these encouraging early findings, a number of scholars have since looked at the relationship between the Big Five and turnout in the United States with inconsistent results. Gerber et al (2011) and Mondak and Halperin (2008) show that agreeableness is associated with lower turnout – supporting Denny and Doyle (2008) and theoretically explained by the discomfort with which agreeable individuals react to political conflict – though Mondak et al (2010) find an opposite, positive effect. Against Denny and Doyle's finding regarding conscientiousness (or in their case, a hardworking personality), Mondak et al (2010) find that conscientiousness has no effect on turnout in the US, though Gallego and Oberski (2011) found that it did, albeit mediated via other psychological variables, for Spanish citizens at the 2011 European Parliament elections. More consistently, openness has been shown to positively affect political knowledge (Hambrick et al., 2008), as well as political

opinions and discussion (Mondak and Halperin, 2008), as well as political efficacy (Vecchione and Caprara, 2009). As such, its effect on turnout has tended to disappear when controlling for these variables (Mondak, 2010; Gallego and Oberski, 2011). Similarly, extraversion has been shown to affect efficacy and political socialisation, yet again its effect on turnout disappears when controlling for these more proximal psychological variables (Gallego and Oberski, 2011). Finally, neuroticism has been tested least – perhaps because of the lack of any obvious causal mechanism - and with no consistently shown results, though the party choice and personality literature would hypothesise that, on the one hand, more neurotic individuals would crave governance and, thus, should vote, whereas on the other hand, they would be less likely to see the positive benefits in voting. The respective causal mechanisms

Overview

Overall, psychological explanations of turnout are varied and numerous. However, there are three tendencies. First, with the exception of personality, they are highly proximal predictors of voting and, therefore, subject to endogeneity whereby both share a common actual determining variable. Denny and Doyle make this argument regarding political interest. Second, these variables often rely on self-assessment, increasing the risks of endogeneity, particularly where they are used as *post facto* justifications for concurrent self-reporting on turnout (e.g. in the case of civic duty) or when the independent variable is likely to by other actions related to turnout (e.g. party choice at the ballot box and self-reported partisan identity thereafter). Third, the similarities between a number of these variables (e.g. external efficacy and trust) and their obvious causal relationships (e.g. the effect of political knowledge on internal efficacy) means that any analysis of these variables on turnout must be designed to carefully avoid multicollinearity issues, as recommended by Gallego and Oberski (2011). Moreover, the reliance on cross-sectional studies to test variables in the psychology model of turnout is highly likely to amplify these three issues.

Methods and data

In order to overcome these challenges, I take two methodological approaches. First, I test for the effects of within-individual variation in the psychological predictors on within-individual variation in turnout. As I expand upon below, I do this using fixed effects panel data models that use British and Swiss socio-economic household panel surveys and a British election panel study. I take this first approach for four reasons. First, generally, testing within-individual is more robust than testing between-individuals because unobserved time constant variables – such as intelligence, upbringing and personality – are controlled for with certainty. Second, the use of panel data allows us to overcome some of the endogeneity caused by potential reverse causality between self-reported psychological variables and self-reported turnout. In short, the dependent variable can be regressed on that individual's psychological variables from the preceding wave. This makes sense for two reasons. First, it makes because the dependent variable is retrospective anyway (e.g. 'Did you vote in last year's general election?'), it is necessary for temporal ordering to use the preceding wave's independent variables. Second, it removes source of endogeneity such as *post facto* self-justification on self-perception variables, which include the majority of psychological variables, and as such overcomes a key drawback in the literature caused by the proximity of political psychology and turnout in explaining the latter. Third, the robustness of fixed effects models – controlling as they do for all fixed effects and including the capability to control for waves and thus wider political context (as well as other issues that are likely to affect all respondents because of the time of interview) – means that highly parsimonious models can be used, offering a way out of the problem of a potentially multicollinear set of independent variables. Fourth, only survey data includes the richness of data to test all of the seven groups of psychological predictors simultaneously. The fixed effects – or *within* – transformation can be understood first by considering the logistic unobserved effects model for N with T time periods:

$$log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right] = \alpha_i + \beta X_{it} + \varepsilon_{it} \text{ for } t = 1, \ldots, T \text{ and } i = 1, \ldots, N$$

where $log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right]$ is the log odds of turnout for individual *i* at time *t*, βX_{it} is the time-variant $1 \times k$ regressor matrix, α_i is the individual's unobserved time-invariant effect and ε_{it} is the error term. The fixed effects model is particularly robust because the individual's unobserved time-invariant effect can correlate with the regressor matrix without biasing the estimates. This is because the fixed effects model eliminates α_i via the within transformation, which estimates the effect of deviation from the individual's mean in the independent variables on the individual's probability of voting, with respect to their mean probability of voting. As such, if $log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right] = P(y_{it})$ then the fixed effects model can be described as:

$$P(y_{it}) - \overline{P(y_i)} = (\alpha_i - \bar{\alpha}_i) + \beta(X_{it} - \bar{X}_i) + (\varepsilon_{it} - \bar{\varepsilon}_i) \Rightarrow P(\ddot{y}_{it}) = \ddot{\beta}_{it} X + \ddot{\varepsilon}_{it}$$

where $\overline{X_{it}} = \frac{1}{T}\sum_{t=}^{T} X_{it}$ and $\overline{\varepsilon_i} = \frac{1}{T}\sum_{t=}^{T} \varepsilon_{it}$. The individual's unobserved time-invariant effect, α_i, is eliminated from the equation because $\alpha_i = \bar{\alpha}_i \therefore (\alpha_i - \bar{\alpha}_i) = 0$. The logistic fixed effects estimator is then obtained via a logistic regression of $P(\ddot{y}_{it})$ on $\ddot{X}_{it}$.

The second methodological approach is to regress turnout on a series of psychological and control variables using a simple between-individual logistic model using one of the same sources of panel data. I do this for three reasons. First, doing so allows for the testing of time constant variables – in this case, political knowledge and personality types. Second, fixed effects panel data analyses have the drawback that, though they are highly robust, they are not highly efficient – all those individuals whose dependent variable is constant (i.e. always voting or never voting) are dropped from the model, potentially causing biases. Third, by again using panel data to perform the simple between-individual logistic model, I am able to perform a robustness test for the fixed effects models with the same data and, importantly, I am able to again take the independent variables from a prior wave to the dependent variable (offering the same theoretical and methodological advantages as outlined above).

I use three sources of data, each of which offer advantages and disadvantages. First, I use the British Household Panel Survey (BHPS) which was later integrated into the far larger Understanding Society survey, the two of which can be linked together. These two studies have the advantages over other European socio-economic household panel studies for the purposes of this research of asking individuals about their turnout in the previous general election over the course of enough years to ensure sufficient variation in the dependent variable. Indeed, combined the two currently cover five UK general elections between 1992 and 2010 – the 2015 wave should be released shortly at the time of writing. The BHPS started in 1991 and had 18 waves until it was integrated into the Understanding Society survey. The data comes from annual interviews of all adults in a household and includes between 9,000 and 13,000 individuals per year. The Understanding Society survey started in 2009 and has around 60,000 respondents. Of these, 6,700 continuing respondents from the BHPS were introduced into wave two. These individuals are given specific identifying variables to allow the two surveys to be linked and treated as one.

The merged four relevant waves from the British Household Panel Survey and one relevant wave from the Understanding Society survey produce a dataset of 55,878 observations by 24,487 individuals over the course of five waves. The panel is unbalanced, meaning that the number of time observations (T_i) for each individual varies. The mean number of waves that are observed for each individual is 2.28; so most individuals are recorded over at least two waves. This is important because the testing of within effects for any individual requires at least two observations. Furthermore, the most common patterns for observations are either to be observed in all of the last three waves (6,552 observations) or in all five waves (5,402 observations). Of this sample, 24.7 per cent of individuals show within-variation in turnout, making this the subsample that will be used for fixed effects models. The main disadvantage of this dataset is that it has a relatively poor selection of political variables. Indeed, besides turnout, political questions are patchy with the exception of five that this study will use – political interest, partisan identification, strength of partisan identification, closeness to a party (asked only to those who respond that they do not identify with a party) and external efficacy (‘Do you agree that ordinary people cannot influence government?’). It is only with this data set that I regress the reported

retrospective turnout on independent variables from the previous year's survey. The match between independent and dependent variables by waves is shown in Figure 1.

Figure 1: Merger of British Household Panel Survey and Understanding Society survey and retrospective turnout and pre-election resources into five waves

Study	British Household Panel Survey																		Understanding Society	
Year	91	92	93	94	95	96	97	98	99	00	01	02	03	04	05	06	07	08	09	10	11
Source of dependent variable (retrospective turnout)		X					X				X				X					X	
Source of independent variables	X					X				X				X					X		
Resultant five waves	1					2				3				4					5		

A second European socio-economic panel survey contains considerably richer political variables but, alas, does not include as relevant a dependent variable. The Swiss Household Panel (SHP) instead asks respondents 'Let's suppose that there are 10 federal polls in a year. How many do you usually take part in?'. By asking a hypothetical question the responses are likely to be less accurate. However, the advantages of testing SHP data are that, because this question is asked every year there are more observations per individual. Indeed, the SHP was started in 1999 and I use data up until 2012. More importantly, the SHP has different political psychological variables to the BHPS, including external efficacy ('How much political influence do you feel that you have?' 0, none – 10, a very strong influence), trust in the federal government, and satisfaction with democracy. It also, like the BHPS, asks respondents how interested they are in politics. The merged fourteen waves from the SHP produce a dataset of 134,077 observations of 23,243 individuals. However, when only those eligible to vote are included this drops to 69,114 observations of 13,035 individuals per wave. On average, individuals participate in 5.3 waves and the most common patterns of observation are either in every wave or in each of the last nine waves (after a top up sample in 2005).

Finally, I use a third panel study for both the fixed effects models as well as the between-individual logistic regression, as explained above. The 2014-2017 British

Election Study (BES) internet panel has exceptionally rich political psychological variables. This panel includes nine waves, starting in February 2014 and ending in August 2016. In total, 57,535 individuals from Great Britain (i.e. excluding Northern Ireland) have taken part in the survey. The BES includes *intended* turnout in the 2015 General Election for five waves and then includes in the sixth wave retrospective turnout. The key drawback of using this study for the fixed effects models is that turnout for all but the sixth wave is intended, leading to potential bias. I take two steps to overcome this. First, I go on to produce a complimentary between-individual logistic regression and, second, I produce a robustness test, presented in the appendices, that drops the final wave and thus tests only like-for-like intended turnout. The impressive range of political psychological variables taken for this study from the BES are summarised in Table 1.

Table 1: Descriptive statistics of political psychological variables in the British Election Study's internet panel survey

Variable	Obs	Mean	Std. Dev.	Min	Max	Waves
Election interest	90828	3.43	0.78	1	4	4-6
Internal Efficacy (1)	240484	3.51	0.87	1	5	1-6
External Efficacy (2)	239394	3.39	1.03	1	5	1-6
Trust MPs	209804	3.14	1.52	1	7	1-4, 6
Satisfaction with democracy	228195	2.36	0.88	1	4	1-4, 6
Party ID + Closer (3)	244307	0.87	0.33	0	1	1-4, 6
Duty to vote	185665	4.17	0.93	1	5	1-4, 6
Enjoy voting	89391	3.30	0.93	1	5	1-4, 6
Voting is a lot/too much effort (4)	211943	2.84	0.99	1	5	1-4, 6
Interested in the EU referendum	63477	3.52	0.71	1	4	7, 8
Political knowledge (5)	36658	0.43	0.14	0	1	fixed
Agreeableness	51196	6.07	1.75	0	10	fixed
Conscientiousness	51196	6.70	1.86	0	10	fixed
Extraversion	51197	4.21	2.17	0	10	fixed
Neuroticism	51194	3.72	2.17	0	10	fixed
Openness	51194	5.61	1.70	0	10	fixed

Notes: (1) Mean of 'I have a pretty good understanding of the important political issues facing our country' and (reverse coded) 'It is often difficult for me to understand what is going on in government and politics'; (2) Mean of 'Politicians don't care what people like me think' and 'It doesn't matter which political party is in power'; (3) Coded 1 if respondent either identifies with a party or says they are closer to a party; (4) Mean of 'It takes too much time and effort to be active in politics and public affairs' and 'Going to vote is a lot of effort'; (5) Mean of correct answers identifying the positions of five British politicians and naming their local MP;

Analyses of BHPS

In Table 2 I present the results of the fixed effects logistic models of political psychological variables, first separately and then in three combined models. Of the five psychological explanatory variables, an increase in political interest, supporting a party (partisan identification), the strength of support for that party and being closer to a party (for those who do not respond as identifying with a party) all have statistically significant positive effects on the individual's probability of voting. The only variable that shows no statistically significant effect on voting in external efficacy and the magnitude of this variable's effect is considerably smaller than those of the other psychological factors. I then present three combined models in which partisan identification, strength of partisan identification and being closer to a party are respectively included in models with political interest and external efficacy. I do this to avoid multicollinearity between the first two and because the latter is only a possible response for those who do not have a partisan identification. In the first of these (model 6), I include partisan identification and strength of partisan identification, both of which maintain the statistical significance of their effects, as do strength of partisan identification (model 7) and being closer to a party (model 8). The effect of external efficacy is not statistically significant in any of the combined models.

Table 2: Fixed effects logistic regression of British Household Panel Survey and Understanding Society

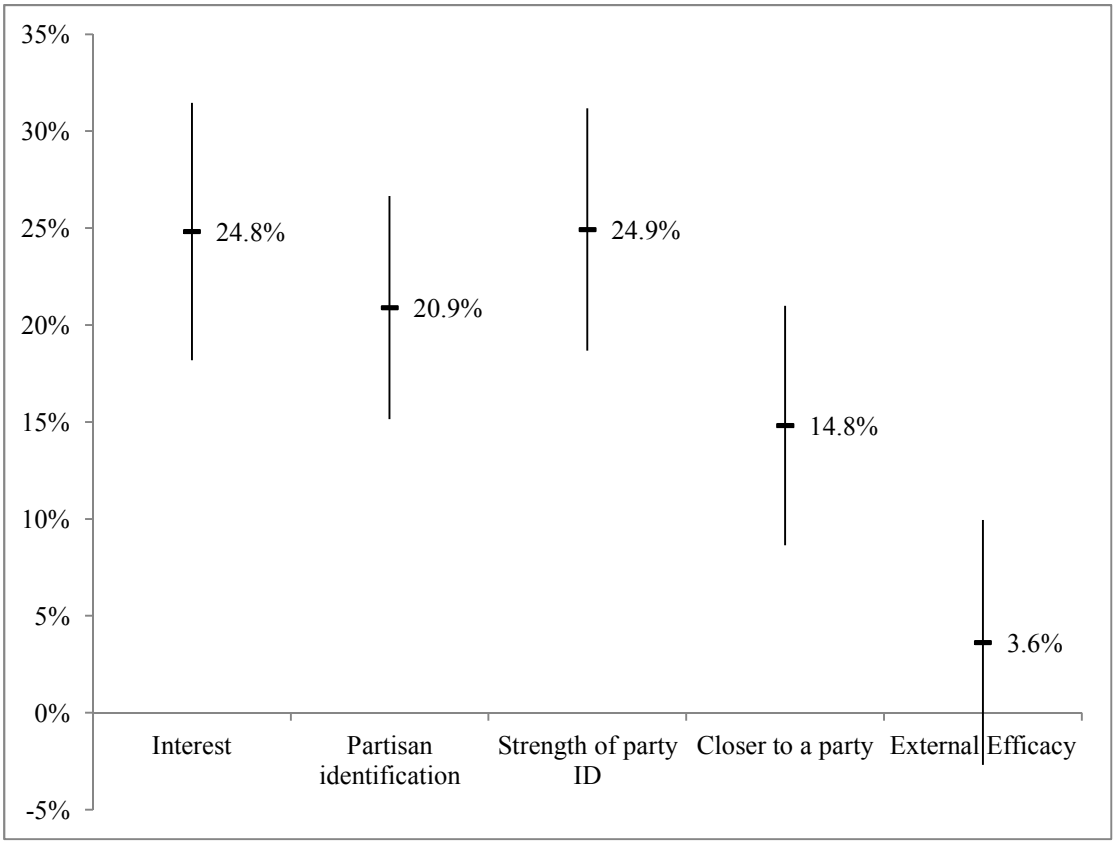
	(1) Interest	(2) Partisan identification	(3) Strength of Party ID	(4) Closer to a Party	(5) External efficacy	(6) Combined (Party ID)	(7) Combined (Strength)	(8) Combined (Closer)
Political interest	1.393*** (0.049)					1.343*** (0.049)	1.313*** (0.049)	1.323*** (0.060)
Partisan identification		1.606*** (0.093)				1.538*** (0.093)		
Strength of Party ID			1.306*** (0.096)				1.321*** (0.047)	
Closer to a party				1.424*** (0.091)				1.365*** (0.090)
External efficacy					1.049 (0.027)	1.045 (0.026)	1.044 (0.027)	1.038 (0.034)
Age	0.900 (0.0668)			0.918 (0.083)	0.859* (0.066)	0.864 (0.067)	0.867) (0.067)	0.886 (0.084)
Wave (ref: 1)								
Wave 2	1.313 (0.488)	0.754*** (0.047)	0.880 (0.089)	1.155 (0.523)	1.597 (0.609)	1.062 (0.691)	1.617 (0.626)	1.402 (0.666)
Wave 3	0.885 (0.585)	0.333*** (0.021)	0.356*** (0.035)	0.747 (0.602)	1.294 (0.878)	1.259 (0.874)	1.273 (0877)	1.007 (0.852)
Wave 4	1.669 (1.616)	0.407*** (0.026)	0.460*** (0.048)	1.342 (1.585)	2.926 (2.909)	2.901 (2.956)	2.864 (2.892)	2.161 (2.682)
Wave 5	3.891 (4.922)	0.637*** (0.047)	0.622*** (0.078)	3.266 (5.039)	8.766* (11.38)	8.375 (11.124)	8.229 (10.84)	6.177 (10.01)
Observations	12,524	12,459	4,510	7,895	12,100	11,919	11,989	7,572
N	3,675	3,653	1,477	2,542	3,508	3,462	3,484	2,424

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

In Figure 2, I present standardised linear marginal effects from the combined models in Table 1.[5] The marginal effects for political interest and external efficacy are each that of the smallest magnitude from the three models. A one standard deviation increase in political interest is associated with a 25 per cent increase in that individual's chance of voting, with the same effect found for a one standard deviation increase in strength of partisan identification. Partisan identification has a weaker, though still important, effect, with becoming closer to a party, rather than close to no party, have a weaker effect still. The effect of a one standard deviation increase in external efficacy is both small and statistically non-significant.

[5] I present marginal effects from the fixed effects models that use a linear estimator. Computing marginal effects from fixed effects logistic models is problematic because the former rely on the values of the fixed effects, which are not estimated, meaning that no meaningful marginal effect can be calculated (though an alternative is to arbitrarily set each observation's fixed effect to zero – for discussion and explanation see Longhi and Nandi, 2015: 205).

Figure 2: Standardised linear marginal effects from the combined fixed effects models



Analyses of SHP

I now move on to testing the effects of within-individual variation in psychological variables on turnout using the SHP data, as presented in Table 3. The dependent variable is the response of how many federal polls the individual participates in on average out of ten. As such, I use a linear regression fixed effects model. The four independent variables – political interest, satisfaction with democracy, external efficacy (measured by self-perception of political influence) and trust in federal government all show statistically significant positive effects. I then present two combined models (5 and 6) that test the effects of satisfaction with democracy and trust in the federal government respectively alongside political interest and external efficacy, in order to avoid multicollinearity issues with the former two. All four of the variables maintain their positive effects and statistical significance.

Table 3: Fixed effects linear regression models of SHP political psychological variables

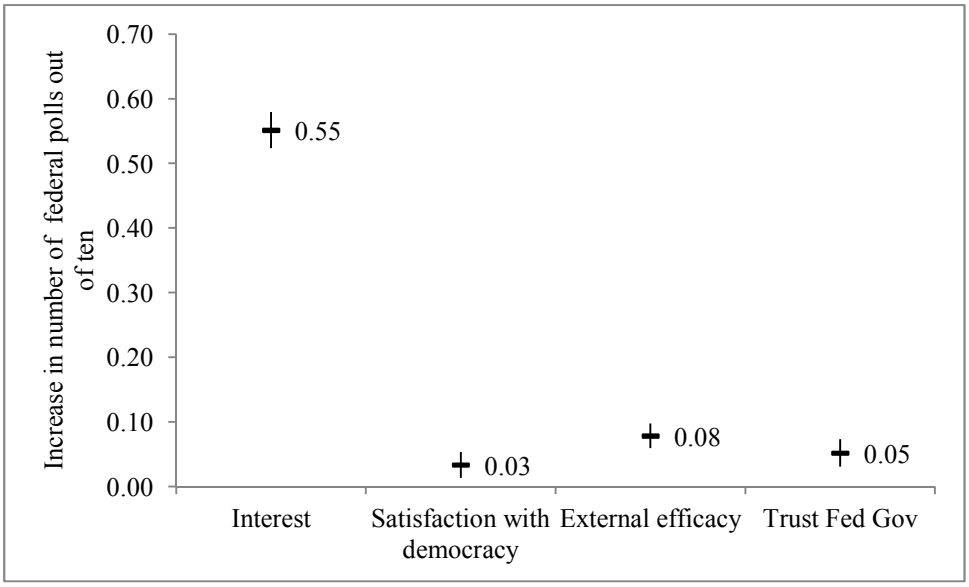
	(1) Political interest	(2) Satisfaction with democracy	(3) External efficacy	(4) Trust Federal Government	(5) Combined (satisfaction)	(6) Combined (trust)
Political interest	0.206***				0.197***	0.200***
	(0.005)				(0.005)	(0.005)
Satisfaction with democracy		0.048***			0.017***	
		(0.005)			(0.005)	
External efficacy			0.053***		0.0328***	0.029***
			(0.003)		(0.003)	(0.003)
Trust Fed. Gov.				0.048***		0.023***
				(0.004)		(0.005)
Wave (ref: 1)						
Wave 2	0.206***	0.244***	0.234***	0.247***	0.203***	0.200***
	(0.029)	(0.029)	(0.030)	(0.029)	(0.029)	(0.029)
Wave 3	0.184***	0.244***	0.235***	0.243***	0.170***	0.164***
	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)	(0.030)
Wave 4	0.383***	0.445***	0.433***	0.467***	0.371***	0.377***
	(0.031)	(0.031)	(0.032)	(0.032)	(0.031)	(0.031)
Wave 5	0.224***	0.300***	0.279***	0.326***	0.216***	0.230***
	(0.032)	(0.032)	(0.033)	(0.033)	(0.032)	(0.032)
Wave 6	0.457***	0.557***	0.535***	0.598***	0.434***	0.455***
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
Wave 7	0.565***	0.658***	0.646***	0.693***	0.555***	0.570***
	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
Wave 8	0.386***	0.460***	0.451***	0.496***	0.367***	0.383***
	(0.031)	(0.032)	(0.032)	(0.032)	(0.032)	(0.032)
Wave 9	0.292***	0.372***	0.351***	0.402***	0.278***	0.287***
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
Wave 10	0.293***	0.400***	0.384***	0.421***	0.282***	0.289***
	(0.031)	(0.032)	(0.032)	(0.031)	(0.031)	(0.031)
Wave 11	0.379***	0.486***	0.470***	0.521***	0.371***	0.385***
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
Wave 12	0.340***	0.335***	0.322***	0.362***	0.320***	0.335***
	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)	(0.031)
Constant	6.073***	6.958***	7.019***	6.940***	5.956***	5.902***
	(0.034)	(0.037)	(0.025)	(0.036)	(0.044)	(0.044)
Observations	69,072	68,017	68,232	68,292	67,348	67,575
N	13,031	12,950	12,978	12,959	12,900	12,911

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

In Figure 3, I present the standardised marginal effects from the analyses of the SHP data in Table 3. The effect of a one standard deviation increase in political interest – participating in an additional one-half federal poll out of ten - clearly overshadows the effects of the other variables, which, though statistically significant, are negligible. Data from the SHP, like that from the BHPS and Understanding Society, suggests that variation political interest is a far more important cause of within-individual variation

in voter turnout than is variation in efficacy or other types of attitudes towards the government or democracy.

Figure 3: Standardised marginal effects of SHP political psychological variables on electoral participation



Analyses of BES

Having tested the effects of political psychology on two long-term panels, I now analyse the shorter British Election Study panel survey. As already described, I take two approaches. The first of these is to use fixed effects panel data models that test intention to vote over five waves and retrospective actual turnout in a sixth wave. Because the dependent variable is not precisely comparable in this sixth wave, I include in the appendices a robustness test that only test the first five waves. In Table 4, I present the results of 11 models looking at the effects of increases in 10 psychological variables on turnout in the general election and one on turnout at the EU referendum. The effects are all positive, except for feeling that voting is too much effort, and all statistically significant, except for internal efficacy. In Appendix 1, the models of which do not include the sixth wave in which the dependent variable is retrospective, all of the directions of effect are the same as in Table 4 and all with

statistical significance, except for, again, internal efficacy and, probably because it is included in only two waves, enjoying voting.

Table 4: Fixed effects panel data logistic models of political psychological variables in the British Election Study

	(1) Election Interest	(2) EU Ref Interest	(3) Internal Efficacy	(4) External Efficacy	(5) Trust MPs	(6) Satisfaction Democracy	(7) Party ID	(8) Party ID + Closer	(9) Duty to vote	(10) Voting Too Much Effort	(11) Enjoy voting
Election Interest	2.338***										
	(0.158)										
EU Ref Interest		2.745***									
		(0.750)									
Internal Efficacy			1.034								
			(0.0467)								
External Efficacy				0.754***							
				(0.0281)							
Trust MPs					1.301***						
					(0.0360)						
Satisfaction w/ democracy						1.198***					
						(0.0547)					
Party ID							3.009***				
							(0.215)				
Party ID + closer								3.069***			
								(0.223)			
Duty to vote									2.131***		
									(0.0791)		
Voting too much effort										0.768***	
										(0.0271)	
Enjoy voting											1.459***
											(0.0788)
Wave (ref: 1)											
Wave 2			1.623***	1.592***	1.652***	1.584***	1.526***	1.553***	1.574***	1.491***	
			(0.110)	(0.110)	(0.114)	(0.118)	(0.102)	(0.105)	(0.113)	(0.104)	
Wave 3			1.666***	1.652***	1.663***	1.623***	1.608***	1.609***	1.484***	1.747***	
			(0.119)	(0.120)	(0.122)	(0.128)	(0.114)	(0.114)	(0.112)	(0.130)	
Wave 4			1.168**	1.046	1.194**	1.298***	1.019	1.036	1.079	1.087	0.774***
			(0.0795)	(0.0746)	(0.0834)	(0.0994)	(0.0682)	(0.0694)	(0.0783)	(0.0764)	(0.0594)
Wave 5	1.394***										
	(0.118)										
Wave 6	0.153***		0.289***	0.237***	0.275***	0.294***	0.211***	0.214***	0.227***	0.257***	0.181***
	(0.0121)		(0.0183)	(0.0166)	(0.0177)	(0.0203)	(0.0136)	(0.0137)	(0.0156)	(0.0172)	(0.0138)
Wave 8		1.373*									
		(0.263)									
Observations	4,822	260	10,838	10,670	10,513	8,839	11,856	11,856	10,459	10,514	4,882
Number of id	1,726	130	2,770	2,740	2,714	2,305	2,988	2,988	2,700	2,726	1,853

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

In Table 5, I present models that test the effects of each of the psychological variables together. I do not include internal efficacy because of its lack of statistical significance. I also choose to test the effects of the combined partisan identification and closer to party variable rather than partisan identification alone - only one can be tested simultaneously and, given that they both have very similar effects when tested separately, I choose to use that which includes greater variation. Because the dependent variable in each of the combined models is turnout at the general election, the EU referendum interest variable is not included. I also choose to use four different models partially for data availability reasons and partially for theoretical reasons. In the first model, I include all nine variables. However, the election interest is only included in Waves 4, 5 and 6 and because the other psychological variables are not included in Wave 5, this restricts the model to a rather limited two waves. Therefore, I

include a second model that does not include election interest, bringing in Wave 2 of the study. The other waves are similarly discounted because the 'Enjoy voting' variable is only included in Waves 2, 4 and 6. Therefore, I again present a third model that covers Waves 1 to 4 and Wave 6 but does not include the election interest or enjoy voting variables. Finally, I include a fourth model without feelings of civic duty, due to the theorised mediating affect that that variable has on the relationship between trust and voting (Wang, 2016).

In the first model in Table 5, election interest is statistically significant and its odds ratio is still large. However, in this model all of the other variables except for having the feeling of duty to vote and believing that voting is too much effort – both feelings directed towards voting as an act, rather than this specific election, political actors or the political system as a whole – lose their significance. The odds ratios of feeling a duty to vote and feeling that voting is too much effort remain practically identical, with the former's magnitude actually increasing when controlling for other variables. This suggests that the effects of external efficacy, trusting MPs and partisan identification – which all become statistically significant when *not* controlling for election interest in Models 2 and 3, are all mediated strongly by the individual's interest in the election. In Model 4, I remove feelings of civic duty, after which the effect of satisfaction with democracy becomes statistically significant, suggesting that its effect is mediated by seeing voting as a duty – the mediating effect on trust, as shown by Wang, is not apparent. In Appendix 2, I provide a version of these models without Wave 6 and, thus, without election interest and Model 1.

Table 5: Combined BES fixed effects panel data logistic models

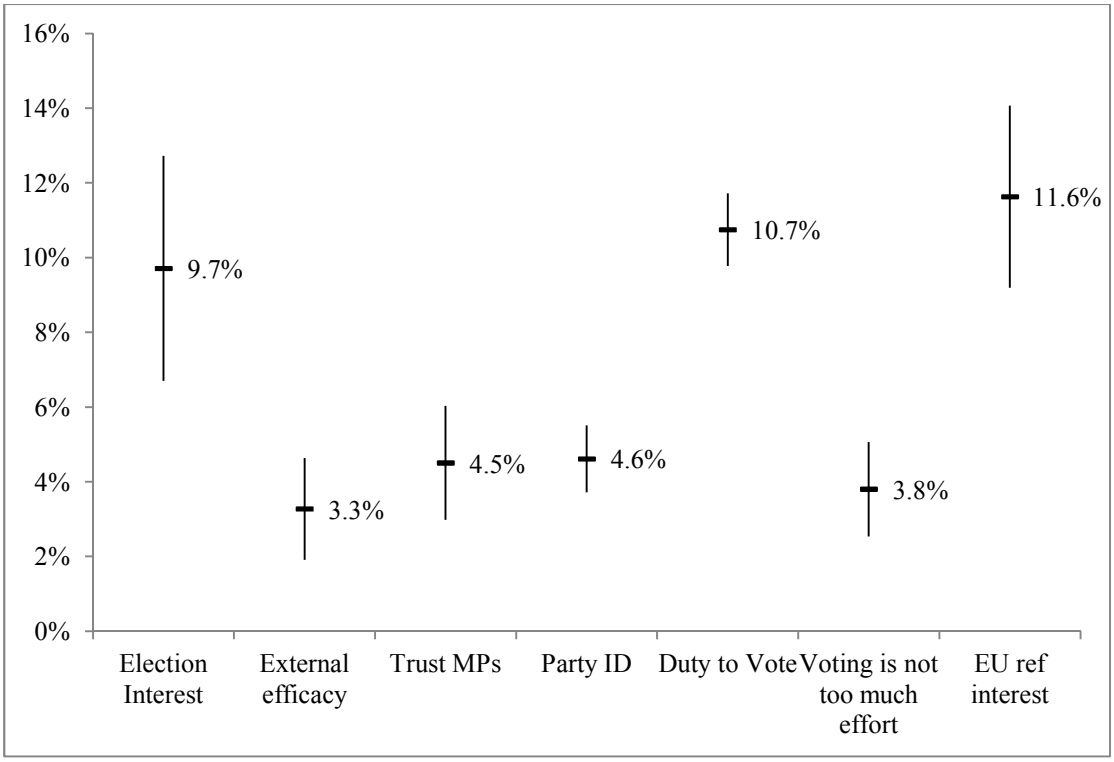
	(1) Full model (waves 4 & 6)	(2) Full model without Interest (waves 2, 4 & 6)	(3) Full model without Interest or Enjoy voting (waves 1-4, 6)	(4) Full model without Interest, Civic Duty or Enjoy voting (waves 1-4, 6)
Election interest	2.256***			
	(0.268)			
External inefficacy	0.826	0.737***	0.813***	0.805***
	(0.097)	(0.052)	(0.037)	(0.036)
Trust MPs	1.093	1.237***	1.214***	1.244***
	(0.078)	(0.062)	(0.042)	(0.042)
Satisfaction with Democracy	1.131	1.100	1.094	1.151**
	(0.131)	(0.086)	(0.057)	(0.057)
Party ID + closer	1.319	1.957***	2.471***	2.799***
	(0.299)	(0.282)	(0.227)	(0.247)
Duty to vote	2.188***	2.159***	2.126***	
	(0.223)	(0.146)	(0.092)	
Voting too much effort	0.770*	0.681***	0.778***	0.774***
	(0.089)	(0.052)	(0.034)	(0.032)
Enjoy voting	1.065	1.112		
	(0.117)	(0.078)		
Wave (Ref: 1)				
Wave 2			1.448***	1.476***
			(0.124)	(0.120)
Wave 3			1.506***	1.691***
			(0.136)	(0.146)
Wave 4		0.649***	0.974	1.074
		(0.066)	(0.088)	(0.092)
Wave 6	0.137***	0.108***	0.160***	0.195***
	(0.016)	(0.012)	(0.015)	(0.016)
Observations	2,038	3,761	8,067	8,182
N	1,019	1,445	2,150	2,172

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

The standardised marginal effects of those variables that are statistically significant in *any* of the models of Table 5 are presented in Figure 4. I take the marginal effect of election interest from Model 1 and the rest from Model 3. I also include the marginal effect of interest in the EU referendum, taken from Model 2 of Table 4. Overall, there is a clear story. The most important psychological predictors of voting during the 2015 general election were interest in the election and sense of having a duty to vote. A one standard deviation increase in either of these increased the individual's probability of planning to vote or actually voting by around 10 per cent. The effect of interest in the general election on voting was fairly similar to the effect of interest in the EU referendum on voting. There are also a handful of other statistically significant marginal effects, which have more modest effects on the individual's probability of

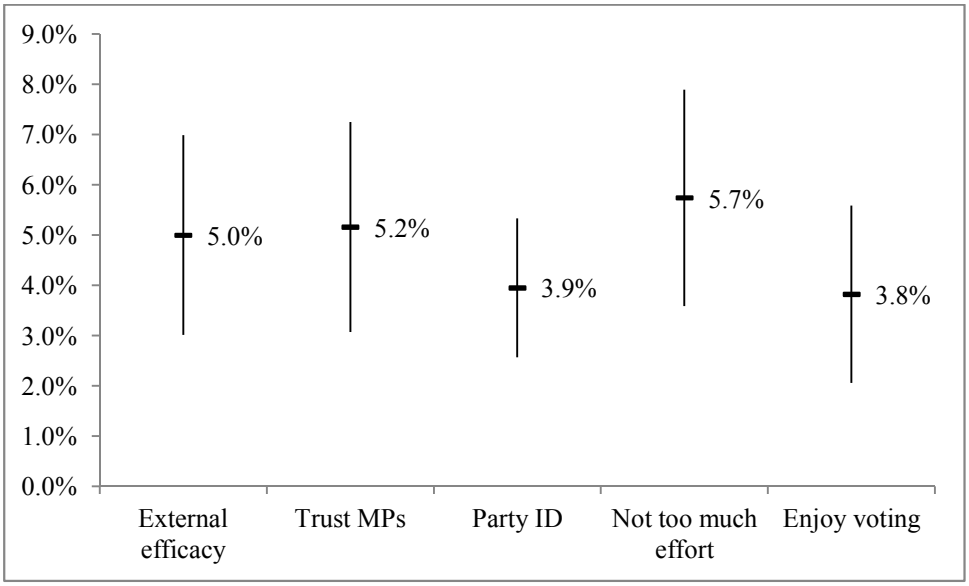
voting. A one standard deviation increase in external efficacy, trust in MPs, partisan identification, not feeling that voting is a lot of effort all increase the individual's chance of voting by between 3.3 and 4.6 per cent.

Figure 4: Standardised marginal effects of psychological predictors of voting in fixed effects logistics models using BES data



As already discussed in the literature, there is a possibility that election interest and sense of civic duty to vote act as mediating effects on some of the other psychological variables. For example, it is fair to assume that partisan identification is a more distal explanatory variable than election interest and, thus, in regression models its effect may get lost in the effect of the latter. Therefore, I present the marginal effects of those variables that are statistically significant in Model 4 when election interest and sense of civic duty are not included in Figure 5 below. These show clearly that the effects of these variables are larger when interest and duty are not included in the model, suggesting that the latter may act as mediating variables – though it should be noted that the effects may also be enlarged because of the more parsimonious model.

Figure 5: Standardised marginal effects of psychological predictors of voting in fixed effects logistics models using BES data with election interest and civic duty to vote removed from the model



Between-individual models

I now move onto a cross-sectional logistic model using the same explanatory variables as above alongside socio-demographic control variables. The disadvantage of this approach is that it entails a greater risk of unobserved variable bias in the form of those variables that are fixed within individuals. The advantage is that those variables that do not vary across time – in this case personality traits and political knowledge – can be tested alongside the other political psychological predictors. It is also fair to assume that a short-term panel survey like the BES will miss important variation in predictors that vary over long periods or relatively little compared to their between-individual variation, such as civic duty. Indeed, it is partially because of the relative lack of variation of some variables that fixed effects models, as used in this study hitherto, are relatively inefficient – they drop all individuals whose do not vary on one of the variables in the model. As such, also considering a between-individual model will bring into the analysis those individuals who were always or never planning on voting.

As discussed in sections 2 and 3, one of the greatest risks of the psychological model of voter turnout is that these explanatory variables are used as *post facto* justifications by the respondent for their turnout behaviour, inducing reverse causality and a source of simultaneity bias. The BES offers a way around this problem through its panel component, even when considering between individual variation using a standard logistic model. I take the individual's average response for each explanatory variable between waves 1 and 5 and regress retrospective voter turnout in wave 6 on these factors. This partially separates the two sides of the equation, lowering the endogeneity risks. I choose to take the average of each independent variable across the five waves prior to the general election to also tease out further variation in individuals.

In Table 6, I offer the four models that test between-individual variation in voter turnout, using the BES. Per Gallego and Oberski (2011) – and logically given their more distal positioning in any causal chain leading to turnout, whereby they are likely to be explanatory variables of more proximal psychological variables and even non-fixed socio-demographics – I first test the effects of the Big Five personality traits alongside only fixed socio-demographics (gender, race, age). In model two, I bring in political knowledge – the only other fixed psychological predictor in this panel survey (though of course political knowledge is not fixed over the longer term and, unlike personality, will not be the cause of time-varying socio-demographics. In model three, I bring in all of the other political psychological predictors with the exception of election interest and sense of civic duty (per Wang, 2016). Finally, Model 4 includes all of the explanatory and control variables.

In Model 1 we can see that, even with more proximal variables not included, the effects of the Big Five personality traits are minimal. Each of the five effects are small and only one – conscientiousness – has statistical significance, in the predicted positive direction. In Model 2, I add political knowledge, which has a positive statistically significant effect. Again the effects of the Big Five are negligible, with extraversion now being the only personality trait to have statistical significance, again in the predicted positive direction. In Model 3 I add all of the political psychological variables, except election interest and sense of civic duty to vote. In this model none

of the Big Five personality traits have statistically significant effects on turnout – nor does internal efficacy, trust of MPs or satisfaction with democracy. However, external efficacy, political knowledge, party identification, enjoying voting all have statistically significant positive effects, while believing that voting is too much effort has a statistically significant negative effect. In the final model, I add election interest and civic duty – both of which have strong, statistically significant, positive effects. External efficacy and enjoying voting lose their statistical significance in this model, though internal efficacy becomes statistically significant.

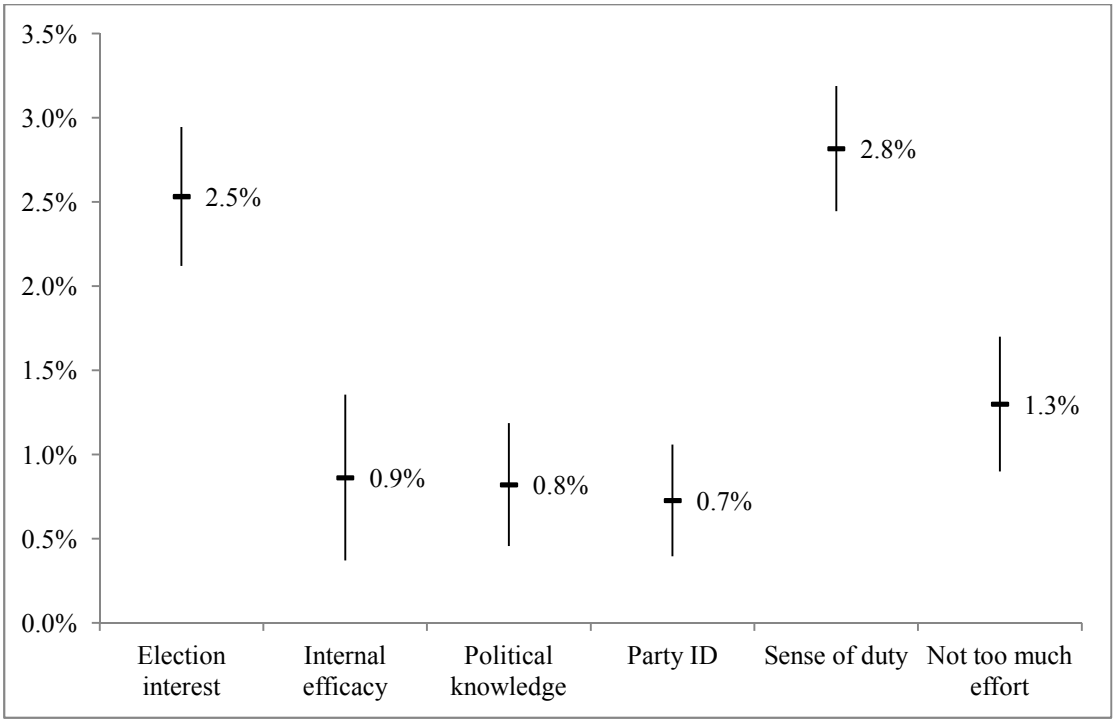
Table 6: Between-individual logistic models of voter turnout using the British Election Study's panel survey

	(1) Personality and fixed socio-demographics	(2) Personality, political knowledge and all socio-demographics	(3) Personality, Psychology and all socio-demographics Without interest or duty	(4) Personality, Psychology and all socio-demographics
Agreeableness	1.001	1.023	1.006	0.992
	(0.017)	(0.023)	(0.025)	(0.026)
Conscientiousness	1.033*	1.022	1.024	1.008
	(0.016)	(0.022)	(0.025)	(0.026)
Extraversion	1.014	1.036*	1.012	1.011
	(0.013)	(0.019)	(0.020)	(0.022)
Neuroticism	0.982	1.016	1.040	1.025
	(0.014)	(0.019)	(0.021)	(0.023)
Openness	0.981	0.972	0.961	0.942*
	(0.017)	(0.023)	(0.025)	(0.026)
Election Interest				2.191***
				(0.143)
Internal Efficacy			0.989	0.776***
			(0.0641)	(0.057)
External Efficacy			0.808***	0.889
			(0.048)	(0.057)
Political Knowledge (std)		1.780***	1.382***	1.214***
		(0.061)	(0.057)	(0.053)
Trust MPs			1.008	0.932
			(0.042)	(0.042)
Satisfaction with democracy			0.991	0.989
			(0.065)	(0.070)
Party ID + closer			3.183***	1.757***
			(0.377)	(0.231)
Duty to vote				2.157***
				(0.110)
Enjoy voting			1.650***	1.072
			(0.093)	(0.069)
Voting too much effort			0.621***	0.712***
			(0.029)	(0.037)
Age	1.029***	1.013***	1.002	0.999
	(0.002)	(0.003)	(0.004)	(0.004)
Male	1.243***	1.071	0.943	1.166
	(0.071)	(0.083)	(0.083)	(0.110)
Household income		1.014	1.001	1.002
		(0.012)	(0.014)	(0.015)
Age of leaving education		1.070*	0.985	0.964
		(0.031)	(0.031)	(0.033)
Married		1.258**	1.282**	1.191
		(0.103)	(0.116)	(0.115)
Home owner		1.401***	1.423	1.469***
		(0.120)	(0.136)	(0.149)
Number of children		1.000	1.000	1.000
		(0.000)	(0.000)	(0.000)
White ethnicity	1.484***	1.147	1.040	0.992
	(0.145)	(0.177)	(0.180)	(0.181)
Partisan contact		4.292***	2.339***	2.010***
		(0.642)	(0.387)	(0.349)
Constituency majority		1.002	0.999	0.999
		(0.003)	(0.003)	(0.003)
Constant	1.726*	1.678	4.064*	0.163***
	(0.383)	(0.623)	(2.424)	(0.105)
Observations	20,914	13,398	13,029	12,934

In Figure 6, I present the marginal effects from the Model 4 of Table 6 – the full combined model analysing BES data. As we can see, the marginal effects of two variables – election interest and sense of duty to vote - overshadow the rest.

Individuals with a one standard deviation higher response of each of these are around 2.5 per cent more likely to vote. Those individuals that believe that voting does not require too much effort are around 1.3 per cent more likely to vote. Finally, internally efficacious individuals, individuals with greater political knowledge and partisan individuals are each just under 1 per cent more likely to vote. All of these marginal effects are considerably more modest than when tested within individuals, primarily because a greater proportion of between-individual variation in turnout is unobserved than within-individuals.

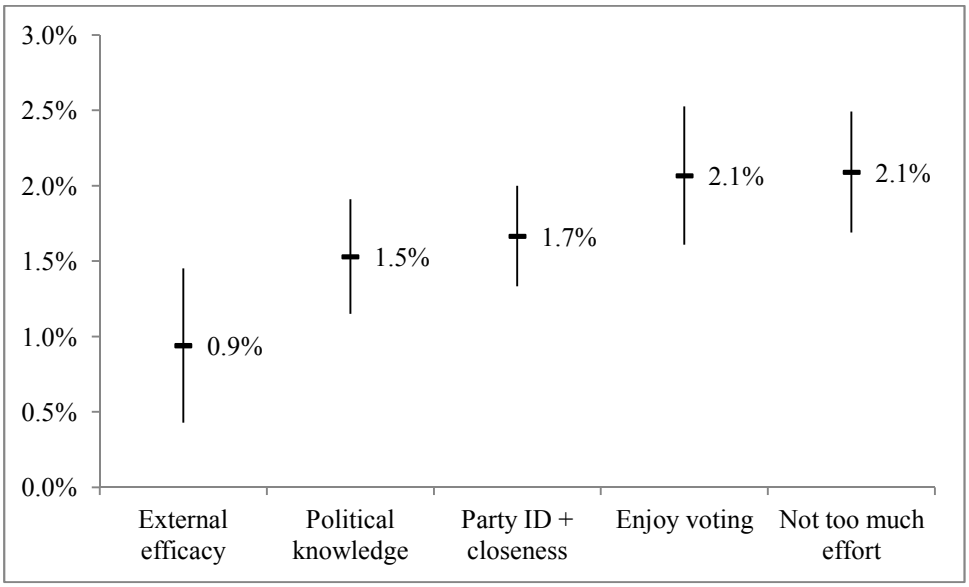
Figure 6: Standardised marginal effects of political psychological variables on turnout using between-individual logistic models of BES panel survey data



As discussed, sense of civic duty has been theorised to have a mediating effect on other political psychological variables and election interest (rather than more general political interest) is likely to be a more proximal cause of voting. As such, I also present the marginal effects from Model 3 in Figure 7, above. As we can see, the effects of political knowledge and partisan identification are greatly enhances, intuitively making sense given their more distal positioning. The effect of not believing that voting requires too much effort is also enhanced, as is enjoying voting.

The effect of external efficacy is also statistically significant in this model, though with a relatively weak effect size.

Figure 7: Standardised marginal effects of political psychological variables on turnout using logistic models of BES panel survey data with election interest and sense of civic duty removed



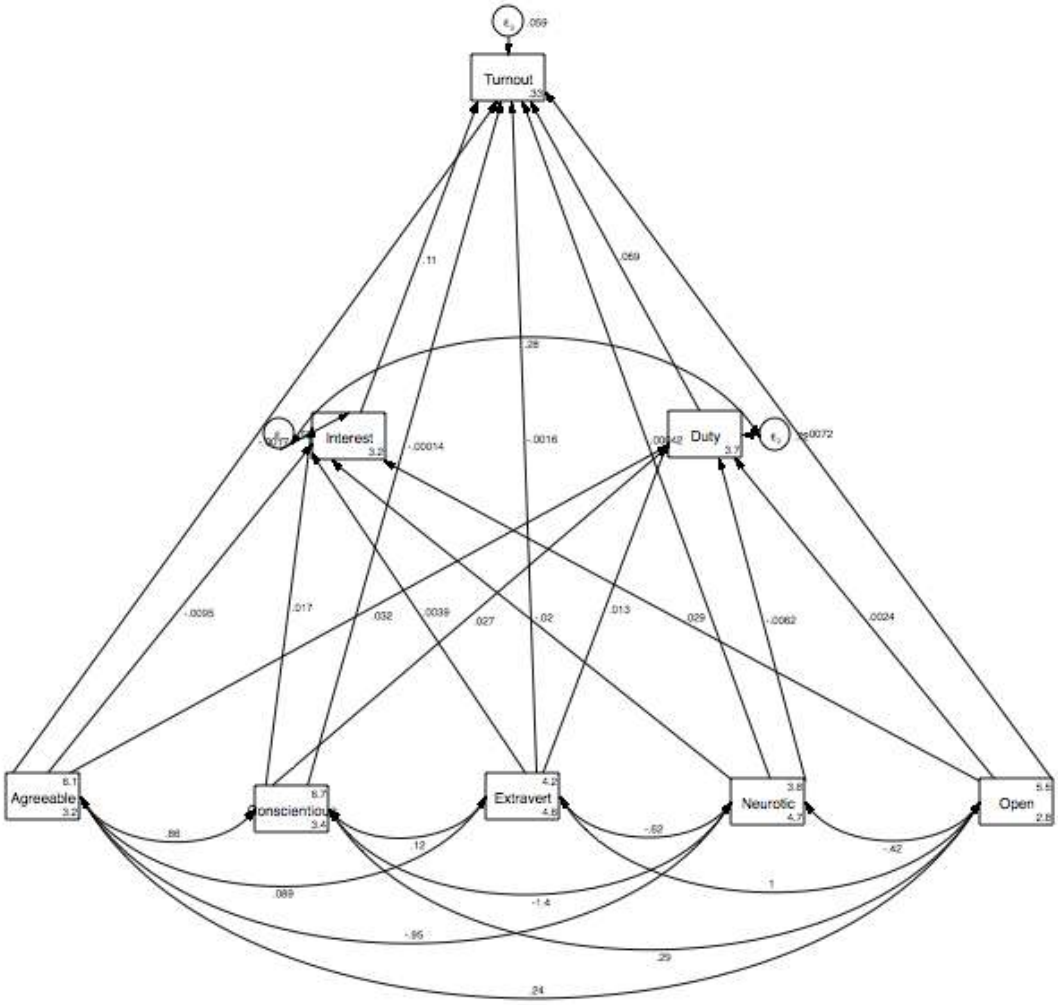
Personality traits, political interest, sense of duty and turnout

The primacy of interest in the election and feeling that it is one's duty to take part it, coupled with the relatively less explored effects of personality traits, warrant further investigation of the potential relationship between the two groups for two reasons. First, there is a fairly compelling theoretical case for such relationships because we can expect personality traits to affect political interest and sense of duty following the extant literature on personality and turnout. The primacy of interest in the election and feeling that it is one's duty to take part it, coupled with the relatively less explored effects of personality traits, warrant further investigation of the potential relationship between the two groups for two reasons. First, there is a fairly compelling theoretical case for such relationships based on the extant literature on personality. Individuals who score high on agreeableness, as already discussed, should be wary of the sometimes cantankerous nature of politics and thus be less interested in politics. On the other hand, we should expect agreeable individuals to be less questioning and

therefore more inclined toward a sense of civic duty. Individuals with conscientious personalities will likely be interested in politics, as is socially desirable, and, more so, feel a sense of civic duty to vote. Individuals with extraverted personalities should be inclined towards the social aspect of politics and, perhaps, towards the idea of there being civic duty at all. Individuals with neurotic personalities are likely to be less interested in politics and feel less of a sense of civic duty to vote, given their inclination towards negativity about the worth of either. Finally, individuals who are more open to experience should be more interested in politics but feel less of a compulsion to vote.

Furthermore, unlike other psychological variables, there is a clear logic in the causal chain between personality traits, the two more proximal psychological variables and, ultimately, turnout. I present a structural equation model of this relationship in Figure 8, with estimates more clearly presented in Table 7.

Figure 8: The mediating effects of political interest and civic duty on personality traits and turnout, SEM diagram



As we can see in Table 7 – and previously in Table 6 - although personality types have minimal effects on turnout directly (see Model 3 below), each of the Big Five has statistically significant effects on either election interest or sense of duty to vote. Greater agreeableness has a statistically significant negative effect on interest in the election and, unsurprisingly, a statistically significant positive effect on sense of duty to vote. Greater conscientiousness has statistically significant positive effects on both one's interest in the election and, again not surprisingly, one's sense of duty to vote. Extraversion has a positive, though not statistically significant, effect on election interest, and a statistically significant effect on sense of duty. Both of neuroticism's effects are negative, as expected, though it is only the effect on interest for which this is statistically significant. Finally, both of the effects of openness to experience are

positive, though it is only the effect on election interest that is statistically significant. Moreover, openness is the only personality trait to have a direct, statistically significant effect on turnout when the mediating effects are included, just as is the case in the full model 4 of Table 6 where its direct effect is also negative.

Table 7: The mediating effects of political interest and civic duty on personality traits and turnout, SEM tables

	(1) Interest	(2) Duty	(3) Turnout
Interest			0.106***
			(0.004)
Duty			0.069***
			(0.003)
Agreeable	-0.009**	0.032***	-0.002
	(0.004)	(0.004)	(0.001)
Conscientious	0.017***	0.027***	-0.000
	(0.004)	(0.004)	(0.001)
Extravert	0.004	0.013***	-0.002
	(0.003)	(0.003)	(0.001)
Neurotic	-0.020***	-0.006	0.000
	(0.003)	(0.004)	(0.001)
Open	0.029***	0.002	-0.007***
	(0.004)	(0.004)	(0.001)
Constant	3.169***	3.674***	0.334***
	(0.042)	(0.049)	(0.023)
Observations	21,239	21,239	21,239

Standard errors in parentheses; *** p<0.001, ** p<0.01, * p<0.05

Conclusion

In this paper, I have considered the effects of political psychology on voter turnout. Studies regarding these relationships have hitherto been stifled by four challenges. First, crucially, they have relied on cross-sectional data in which responses regarding political psychology are likely to have been biased by their contemporaneous responses regarding voter turnout. Second, and moreover, they have been face by the inevitable challenges regarding the studying the effects of a highly proximal set of explanatory variables on turnout – a risk heightened by the use of cross-sectional data that is relatively subject to considerable omitted variable biases. Third, studies have, at times, neglected to address biases caused by mediating variables. Fourth, studies

have not considered the role of political psychological variables together that allows for the comparison of respective effects.

In this paper, I attempted to overcome these challenges with approaches that both make use panel data, offering two main advantages. First, use this data for fixed effects models allows for the control of within individual fixed effects, leading to more robust estimates. Second, panel data – whether used in fixed effects models or cross-sectional models – allow for retrospective voter turnout to be regressed on variables taken from earlier responses in the panel, avoiding biases caused by reverse causality. I use this data to take two methodological approaches. First, I produced fixed effects logistic models that tested the causes of within-individual retrospective voting in UK general elections from 1992 to 2010, propensity to vote in Swiss federal polls and, intention to vote and retrospective turnout reported at six intervals over a year and a half prior to the 2015 UK general election. Second, I sought to triangulate these results and bring in fixed effect psychological determinants of voting - personality traits and political knowledge – by producing between-individual logistic models that regress turnout on average responses given prior to the vote.

The three sources of data each had advantages and disadvantages in terms of data availability. In spite of this, the results were fairly consistent across the three panel models and the one logistic model. The key psychological determinants of turnout are attitudes to voting as an act – primarily in terms of duty, but also in terms of effort and, to a lesser extent, enjoyment – and interest in politics and the election in question. The effects of all other determinants are considerably more patchy. Though partisanship and strength of partisanship have important roles in voter turnout over the long term, short term variations are subsumed into election interest and sense of duty to vote, which both overshadow partisanship's importance and act as a mediating variable between it and turnout. There is even less consistent evidence regarding attitudes to democracy and trust in MPs and the government and political knowledge as well as only patchy evidence of weak effects of internal and external efficacy on voting – though it does seem that their effects are *partially* mediated again by election interest and sense of duty to vote, as shown when the latter two are not included in models. Despite a recent flurry of academic interest in the effects of personality types

on political behaviour, this study finds very little evidence to support their direct effects, with the exception of openness to experience causing individuals to vote less. However, I find strong evidence that personality traits affect turnout via their effects on the two key psychological variables – election interest and sense of civic duty to vote. Being less agreeable, more conscientious, more emotionally stable and more open-minded lead one to be more interested in politics while being more agreeable, more conscientious and more extraverted cause individuals to see voting as a duty, with both affecting turnout thereafter.

Psychologically, individuals vote when they feel they should, in moral terms and regardless of outcomes, and when they are interested in the election. The decision to turn out is reliant on feelings towards voting as an act and the election that is being voted on, not feelings towards longer term or structural issues, though identifying with a party is likely to boost both interest and duty. By comparison, it is considerably less important whether the individual feels that their vote will make a difference, whether they feel they are capable of forming a reasonable opinion or, for that matter, whether they actually know much about political actors. Attitudes towards electoral officials – as a group – and the state of democracy are also important only insofar as they boost one's sense of duty to vote and, even then, their predictive power is relatively weaker. Disentangling, first, the effects of so many interrelated political psychological variables is difficult and, second, disentangling responses about political psychology and turnout itself is perhaps even trickier. However, this study goes some way towards showing that when citizens are faced with the drama of electoral politics, two psychological variables are the main performers – interest and duty – behind which stand a wider cast of minor, supporting actors.

Appendices

Appendix 1: Fixed effects panel data logistic models of political psychological variables in the British Election Study (no wave 6)

	(1) Election Interest	(2) Internal Efficacy	(3) External Efficacy	(4) Trust MPs	(5) Satisfaction Democracy	(6) Party ID	(7) Party ID + Closer	(8) Duty to vote	(9) Voting Too Much Effort	(10) Enjoy voting
Election Interest	1.748***									
	(0.200)									
Internal Efficacy		0.993								
		(0.0584)								
External Efficacy			0.789***							
			(0.0390)							
Trust MPs				1.337***						
				(0.0534)						
Satisfaction w/ democracy					1.166**					
					(0.0735)					
Party ID						2.810***				
						(0.268)				
Party ID + closer							2.846***			
							(0.270)			
Duty to vote								1.784***		
								(0.0856)		
Voting too much effort									0.848***	
									(0.0381)	
Enjoy voting										1.134
										(0.0988)
Wave (ref: 1)										
Wave 2		1.727***	1.703***	1.744***	1.689***	1.609***	1.635***	1.647***	1.612***	
		(0.127)	(0.127)	(0.131)	(0.139)	(0.116)	(0.118)	(0.126)	(0.122)	
Wave 3		1.821***	1.794***	1.827***	1.774***	1.754***	1.732***	1.637***	1.882***	
		(0.144)	(0.144)	(0.149)	(0.156)	(0.136)	(0.134)	(0.136)	(0.155)	
Wave 4		1.194**	1.096	1.213**	1.383***	1.002	1.018	1.068	1.137	0.716***
		(0.0923)	(0.0893)	(0.0963)	(0.121)	(0.0759)	(0.0770)	(0.0874)	(0.0908)	(0.0636)
Wave 5	1.464***									
	(0.141)									
Observations	998	4,772	4,664	4,559	3,730	5,171	5,171	4,558	4,564	1,048
Number of id	499	1,436	1,412	1,389	1,150	1,538	1,538	1,388	1,397	524

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

Appendix 2: Combined BES fixed effects panel data logistic models (no wave 6)

	(1) Full model without Interest (waves 2, 4 & 6)	(2) Full model without Interest or Enjoy voting (waves 1-4, 6)	(3) Full model without Interest, Civic Duty or Enjoy voting (waves 1-4, 6)
External inefficacy	0.692**	0.793***	0.791***
	(0.084)	(0.049)	(0.048)
Trust MPs	1.442***	1.256***	1.281***
	(0.157)	(0.064)	(0.064)
Satisfaction with Democracy	1.055	1.080	1.122*
	(0.156)	(0.078)	(0.078)
Party ID + closer	1.553	2.373***	2.684***
	(0.380)	(0.285)	(0.311)
Duty to vote	1.713***	1.771***	
	(0.201)	(0.102)	
Voting too much effort	0.643**	0.805***	0.816***
	(0.092)	(0.046)	(0.044)
Enjoy voting	0.983		
	(0.124)		
Wave (Ref: 1)			
Wave 2		1.536***	1.585***
		(0.145)	(0.144)
Wave 3		1.665***	1.845***
		(0.168)	(0.179)
Wave 4	0.608***	0.995	1.137
	(0.080)	(0.104)	(0.113)
Observations	736	3,318	3,381
Number of id	368	1,040	1,057

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

Socialisation and Voter Turnout

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Scholars seeking to explain turnout have, in recent years, increasingly looked toward the individual's social life. In particular, the existing literature considering the experiences of the individual's early years as well as the effects of one's local environment in adulthood. Less investigated, though already shown to matter greatly, are the effects of the political behaviour of one's cohabitees and family members, as well as the nature of one's political discussions, or lack of them. These explanatory phenomena are both highly subject to unobserved heterogeneity. As such, I take a within-individual approach using panel data from the British Household Panel Survey and the British Election Study. In doing so, I find that the political behaviour of one's cohabitees has strong effects on that of the individual but also that this socialisation effect is likely to be relatively less driven by politics and more driven by issues of practicality. Similarly, I show that the effects of political discussion are relatively unaffected by the nature of the relationship between the discussants – casting doubt over some previous findings – and instead dependents simply on the quantity of discussion. Finally, I show that political discussion is at least partly driven by another non-political variable, personality type, with more sociable personalities more likely to engage in such discussions.

Introduction and literature

Socialisation theories of voter turnout have thus far emphasised two key forms of social interaction by which the individual's probability of voting is increased (Smets and van Ham, 2013). The first of these relates to experiences prior to voting age when it is theorised that normative attitudes towards voting are developed and strongly affected by the behaviour of parents, peers, schooling, media and, to a lesser extent, the political context of the time. The second of these socialisation effects relates to the experience of the individual during adulthood and to the political content – or lack of it - within the individual's proximal environment and their social exchanges. In particular, the political behaviour of one's peers and their conversational matter are theorised to affect turnout, with political content in the latter expected to increase the individual's chance of voting by increasing political interest and a sense of civic duty to vote (Gerber and Rogers, 2009). Though the effects of experiences in childhood and young adulthood have been relatively well covered by the literature (e.g. Plutzner, 2002; Neundorf et al, 2016), we know considerably less about the effects of socialisation throughout adulthood.

Indeed, Pattie and Johnson (2000) noted that few studies have tested the hypothesis that 'people who talk together vote together.' They showed that, at the 1992 UK General Election, political discussion with family lead to a particularly strong clustering effect in terms of party choice and a weaker effect when the political discussion was with non-family members. Pattie and Johnston's (2000: 59) conclude that 'people listen most attentively, and are converted by, discussants from within their families. Non-family discussants are also influential, but not as strongly: the main sway occurs as a result of within-family discussions'.

Orford and Schuman (2002) focused on the socialisation effects in the individual's broader environment, noting that the causal mechanism behind the socialisation effect of the individual's neighbourhood on turnout has largely been neglected so far in the literature. They examine individual turnout at the individual-, household-, polling district- and ward- levels. They focus on one ward of around 10,000 voters and show that voters in two-person households are socialised into voting or not voting but that

this propensity to vote decreases as household size increases. They also show that more compact districts, with shorter walking distances to the polls (exacerbated by topography) and more intense socialisation effects increase turnout. However, and moreover, Johnston et al (2004) lament the difficulties with which the literature has been able to draw robust conclusions on socialisation effects at levels of analysis above that of the individual, as a result of the (often) inability to control for the effect of socio-demographic clustering whereby similar individuals live in proximity to each other, both in households and at higher levels. They control for local economic and demographic profiles, to show that party choice is influenced by neighbourhood milieu (see also Gimpel et al, 2004).

Zuckerman and Kotler-Berkowitz (1998) use multivariate models to show that in the 1992 UK General Election individuals were highly likely to vote if another individual in their household voted. They later (2004) showed that the longer a couple lived together, the more likely they were to share partisan affiliations and that households are the primary social arena in which political discussions and decisions take place. Kan and Heath (2003) also show that married couples conform to each other's party choices at elections, with non-working women particularly likely to conform to the male breadwinner's behaviour. Building on these studies of partisanship, Johnston et al (2005a: 202) argued that the household level is 'virtually ignored in most empirical studies' of voting, outside incorporating variables on the right side that account for one's father's occupation and partisan preferences. They support previous evidence regarding socialisation effects of party choice at the household-level by using the British Household Panel Survey (BHPS) to study vote switching between the 1992 and 1997 UK general elections. Amongst more robust findings regarding party choice, they also found that, of those households in which all members abstained in 1992, 42 per cent saw zero turnout rates in 1997 – a lower election-on-election consistency than that regarding party choice for the Labour, the Conservatives and the Liberal Democrats. Cutts and Fieldhouse (2009) support evidence that the household is the primary arena for socialisation by showing that turnout is clustered at that level to a far greater extent than at the postcode (neighbourhood) or ward (communities of around 10,000 people) levels. They suggest that, in light of their findings, future

research should investigate the role of how voter turnout habits are shared within the household and the nature of political discussions.

The strand of literature that considers the effect on voter turnout of political discussion rather than socialisation through proximity is perhaps more advanced. Eveland and Hively (2009, see also Schmitt-Bek and Lup, 2013, Rolfe, 2012) show that more frequent discussions of politics is strongly associated with political participation. As Eveland et al (2015) note, the causal mechanism for this increase has been hypothesised to be direct as well as mediated via increased exposure to media, political knowledge or political efficacy (Mcleod et al 1999; Scheufele et al 2004) or via mobilisation (Verba et al, 1995). Moreover, the type of relationship through which the political discussion takes place has been argued to affect the extent that political discussion increases turnout, with discussion with family members being the most powerful form (Zuckerman, et al, 2007). Though, as Schmitt-Bek and Lup (2013: 525) state, 'the validity of these findings might be questioned because of potential endogeneity.'

I seek to answer two lines of questioning that build on the findings of the socialisation and turnout literature. First, how does the political behaviour and the political attitudes of those with whom an individual shares a home influence the individual's propensity to vote, and how does this vary according to the nature of the relationship between the individual and their cohabitees? If it does, then why? Second, how does the amount of political discussion that an individual takes part in affect their propensity to vote? How is this affected by the nature of the relationship with whom they discuss politics? To what extent is this effect mediated by political psychological variables and, finally, to what extent is the effect of political discussion driven by an underlying propensity towards social interaction?

Approach, methods and data

To answer these questions, I take two approaches. The first is to remove potential sources of endogeneity caused by omitted variable biases that have left un-resolved

questions regarding the extant findings in the literature regarding both cohabitee behaviour and political discussion. I do this by taking a within-individual variation approach. I test within-individual variation in turnout by using fixed effects panel data models, thereby controlling for all time invariant characteristics of individuals without having to add a potentially incomplete batch of control variables. Though this means that time invariant individuals are dropped from the model – a considerable source of inefficiency – the few assumptions required by the model (as opposed to, for example, a random effects model – make it consistent and robust, relative to the methods on which the existing socialisation findings were made. I use logistic fixed effects models because of the dichotomous outcome variable – turnout. The fixed effects – or *within* - transformation can be understood first by considering the logistic unobserved effects model for N with T time periods:

$$log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right] = \alpha_i + \beta X_{it} + \varepsilon_{it} \text{ for } t = 1, \ldots, T \text{ and } i = 1, ..., N$$

where $log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right]$ is the log odds of turnout for individual i at time t, βX_{it} is the time-variant $1 \times k$ regressor matrix, α_i is the individual's unobserved time-invariant effect and ε_{it} is the error term. The fixed effects model is particularly robust because the individual's unobserved time invariant effect can correlate with the regressor matrix without biasing the estimates. This is because the fixed effects model eliminates α_i via the within transformation, which estimates the effect of deviation from the individual's mean in the independent variables on the individual's probability of voting, with respect to their mean probability of voting. As such, if $log\left[\frac{P(y_{it}=1)}{1-P(y_{it}=1)}\right] = P(y_{it})$ then the fixed effects model can be described as:

$$P(y_{it}) - \overline{P(y_i)} = (\alpha_i - \bar{\alpha}_i) + \beta(X_{it} - \bar{X}_i) + (\varepsilon_{it} - \bar{\varepsilon}_i) \Rightarrow P(\ddot{y}_{it}) = \beta \ddot{X}_{it} + \ddot{\varepsilon}_{it}$$

where $\overline{X_{it}} = \frac{1}{T}\sum_{t=1}^{T} X_{it}$ and $\overline{\varepsilon_i} = \frac{1}{T}\sum_{t=1}^{T} \varepsilon_{it}$. The individual's unobserved time-invariant effect, α_i, is eliminated from the equation because $\alpha_i = \bar{\alpha}_i \therefore (\alpha_i - \bar{\alpha}_i) = 0$. The fixed effects estimator is then obtained via a logistic regression of $P(\ddot{y}_{it})$ on $\ddot{X}_{it}$.

I use two sources of data for the fixed effects logistic models. The first of these is a combined dataset of the British Household Panel Survey and its larger successor, the Understanding Society survey. The advantage of this dataset is its length – 25 years, including five general elections – and the fact that all members of a household are simultaneously observed, allowing for the testing of variation in one individual on variation in another. I investigate the effect of the turnout of one's partner or spouse, their father and their mother. Once an individual is interviewed for the BHPS – by way of their residency in a certain household – efforts are made to then interview them in subsequent waves, even if they move, thus bringing in new participants in the survey as individuals move out of their original residences and begin cohabiting with new individuals. As such, a respondent's partner, father and mother are included in the BHPS, and remain liked to the original respondent, so long as they have lived with the respondent *just once*. The main disadvantage of this data source, for the purposes of this study, is that it includes only a handful of political variables – voter turnout, political interest, partisan identification, strength of partisan identification, closeness to one party (for those who do not identify as a supporter of a party) and external efficacy ('Do you agree that ordinary people cannot influence government?'). The data comes from annual interviews of all adults in a household and includes between 9,000 and 13,000 individuals per year.

The Understanding Society survey started in 2009 and has around 60,000 respondents. Of these, 6,700 continuing respondents from the BHPS were introduced into wave two. These individuals are given specific identifying variables to allow the two surveys to be linked and treated as one. The merged four relevant waves from the British Household Panel Survey and one relevant wave from the Understanding Society survey produce a dataset of 55,878 observations by 24,487 individuals over the course of five waves. The panel is unbalanced, meaning that the number of time observations (T_i) for each individual varies. The mean number of waves that are observed for each individual is 2.28; so most individuals are recorded over at least two waves. This is important because the testing of within effects for any individual requires at least two observations. Furthermore, the most common patterns for observations are to be observed in all of the last three waves (6,552 observations) or

in all five waves (5,402 observations). Of this sample, 24.7 per cent of individuals show within-variation in turnout, making this the subsample that will be used for the fixed effects models.

The second source of data for the fixed effects models comes from the 2014-2017 British Election Study (BES) internet panel. This panel includes nine waves, starting in February 2014 and ending, for now, in August 2016. In total, 57,535 individuals from Great Britain (i.e. excluding Northern Ireland) have taken part in the survey so far. The BES includes *intended* turnout in the 2015 General Election for five waves and then includes in the sixth wave retrospective turnout. This panel has exceptionally rich data on political discussion, including number of days per week discussing politics, the number of people that the individual discusses politics with, the relationship of the individual with their political discussants and their cohabitation status and the reported attitudes of the individual's political discussants both to politics and towards whether the individual votes. The main disadvantage of this panel, for the purposes of this study, is that, of the political discussion questions, only the days of political discussion per week variable is asked in a sufficient number of waves to be an efficient estimator for fixed effects models.

Because of this drawback to the BES, I also take a second, between-individual approach to investigating the effect of socialisation on turnout. Fortunately, the BES also includes hundreds of other political variables as well as sufficient socio-demographic variables, which can be used as controls. Particularly noteworthy for investigating the effect of political discussion is the inclusion of personality trait variables, which can be used to test an individual's underlying sociability and how this affects their propensity toward political discussion directly and indirectly. One advantage of using the BES panel for the study of between effects is that independent variables from earlier waves than that of the dependent variable can be used, thereby overcoming potential sources of endogeneity caused by reverse causality and simultaneity bias.

Cohabitee turnout models

In Table 4, I present the fixed effects logistic models that test the effect of the turnout of one's partner (model 1), father (2) and mother (3) on the individual's chance of voting, as well as further models (4) that combine these effects and (5) then control the combined variables for socio-demographic variation, using the BHPS data. The effects of one's partner turnout, one's father's turnout and one's mother's turnout are, respectively, all positive, of similar magnitudes and statistically significant when tested separately. However, when tested together, all three of the effects are weakened and only that of the partner's turnout remains statistically significant. This is essentially replicated when the socio-demographic controls are added.

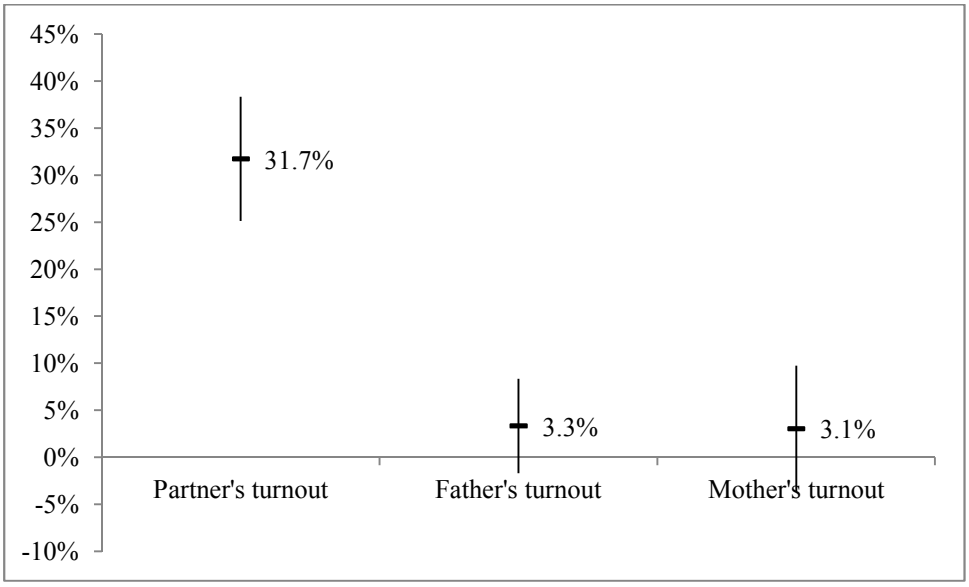
Table 1: Fixed effects logistic models of voter turnout (UK General Elections;1992-2010)

	(1) Partner's turnout	(2) Father's turnout	(3) Mother's turnout	(4) Combined	(5) Combined with socio-demographics
Partner's turnout	10.06***			6.702***	6.781***
	(0.918)			(2.783)	(2.842)
Father's turnout		10.60***		1.036	1.060
		(0.883)		(0.411)	(0.427)
Mother's turnout			12.22***	1.840	1.762
			(1.095)	(0.801)	(0.765)
Age	0.711***	0.740***	0.727***	0.695***	0.701**
	(0.0926)	(0.0857)	(0.0870)	(0.0974)	(0.101)
Financial Situation					1.083
					(0.054)
Married					1.917***
					(0.279)
Children					1.120
					(0.129)
Wave (ref: 1)					
Wave 2	4.309**	3.605**	3.832**	5.001**	4.777*
	(2.793)	(2.086)	(2.290)	(3.495)	(3.440)
Wave 3	9.101*	6.386*	7.293*	11.31*	9.757
	(10.54)	(6.579)	(7.762)	(14.12)	(12.552)
Wave 4	44.55**	27.41**	33.41**	64.38**	52.122*
	(75.61)	(41.41)	(52.12)	(117.7)	(98.209)
Wave 5	130.8**	135.1**	182.3**	238.5**	229.3***
	(291.9)	(266.5)	(371.4)	(587.4)	(581.4)
Observations	5,952	7,377	7,017	5,343	5,233
N	1,999	2,294	2,179	1,812	1,777

*** p<0.001, ** p<0.01, * p<0.05

In Figure 1, I present marginal effects from the combined model 5, above.[6] As we can see, the effects of the turnout of the individual's mother and father are small and non-statistically significant. By contrast, the marginal effect of the individual's partner is very strong - increasing the individual's probability of turnout by just under a third.

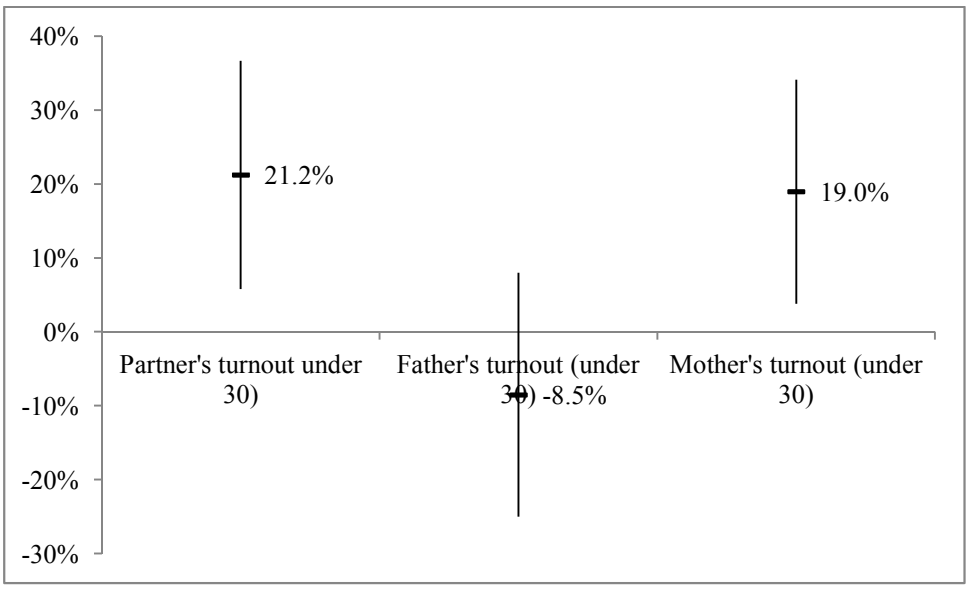
Figure 1: Marginal effects of fixed effects logistic models of voter turnout (UK General Elections; 1992-2010)



However, the above test is not entirely fair given that the individual is likely to be cohabiting with their partner for a longer period than with their parents, which is likely to be restricted primarily to youth. Therefore I present marginal effects from the above models that restrict the sample to the years in which the individual is under 30 years old, in Figure 2, below. In doing so, we can see that the effect of the mother's turnout is greatly magnified – almost to parity with the partner's, which is weaker in the 20s – and becomes statistically significant. What is most interesting is that the effect of the father's turnout remains statistically insignificant, suggesting that individuals in youth are strongly socialised by their mother's turnout but not by that

[6] I present marginal effects from the fixed effects models that use a linear estimator. Computing marginal effects from fixed effects logistic models is problematic because the former rely on the values of the fixed effects, which are not estimated, meaning that no meaningful marginal effect can be calculated (though an alternative is to arbitrarily set each observation's fixed effect to zero – for discussion and explanation see Longhi and Nandi, 2015: 205).

Figure 2: Marginal effects of fixed effects logistic models of voter turnout – under 30s only (UK General Elections; 1992-2010)



Socialisation and interest and partisanship models

How can we theoretically explain this? It could, on the one hand, be the result of individuals being more broadly politicised by their mothers than by their fathers. If this were the case, we would expect to see variation in other forms of political behaviour and attitudes rise and fall in line with those of the individual's mother, but not their father. Alternatively, the discrepancy between the effects of the mother and the father could be the result of less political and more practical matters – such as the greater propensity for the mother to be at home and, therefore, remind and accompany the individual to the ballot box. Furthermore, it could be that mothers pass on practical habits and actions, whereas the influence on children of fathers is passive in nature.

This second hypothesis is evidenced by the effects of the political interest and partisan identification of each household member on the individual's political interest and partisan identification, both of which are passive rather than active. As we can see in Table 2, it is the effect of variation in the father's political interest and two forms of partisan identification that affect the political interest and partisan identification of the individual – not that of either the partner or spouse or the mother. Each of the father's

three political variables have a positive, statistically significant effect on those of the individual, while none of the effects from the partner or spouse or mother are statistically significant.

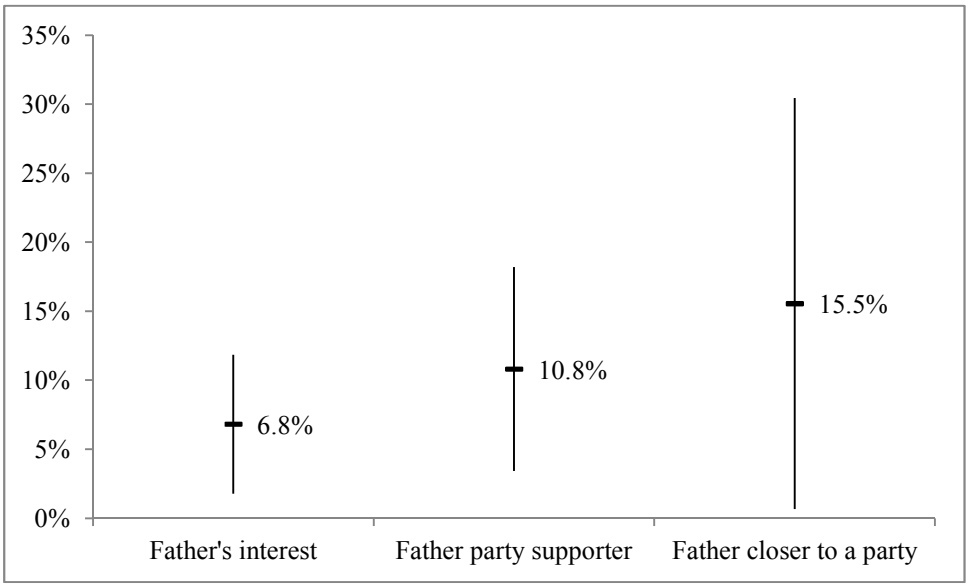
Table 2: Effect of political interest and partisan identification of household members on respondent's political interest and partisan identification (linear fixed effects models)

	(1) Interest	(2) Party supporter	(3) Closer to a party
Partner	0.017	0.065	-0.031
	(0.030)	(0.042)	(0.094)
Father	0.068**	0.108**	0.155*
	(0.025)	(0.037)	(0.076)
Mother	-0.020	-0.035	-0.035
	(0.031)	(0.044)	(0.096)
Age	0.019	-0.040	0.016
	(0.019)	(0.025)	(0.042)
Financial	-0.009	-0.003	-0.000
Situation	(0.007)	(0.009)	(0.018)
Married	0.035	-0.041	0.016
	(0.024)	(0.032)	(0.060)
Children	0.012	-0.003	-0.010
	(0.016)	(0.021)	(0.041)
Wave (ref: 1)			
Wave 2	-0.204**	0.198	-0.133
	(0.099)	(0.129)	(0.212)
Wave 3	-0.279	0.395*	-0.363
	(0.177)	(0.230)	(0.377)
Wave 4	-0.400	0.532	-0.462
	(0.259)	(0.337)	(0.553)
Wave 5	-0.392	0.676	-0.437
	(0.342)	(0.445)	(0.729)
Constant	-0.602	1.579*	-0.363
	(0.728)	(0.954)	(1.355)
Observations	28,926	28,568	12,363
N	14,863	14,700	8,007
	*** p<0.001, ** p<0.01, * p<0.05		

In Figure 2, I present the statistically significant marginal effects from the above models. A one standard deviation increase in the father's political interest increases the individual's political interest by just under 7 per cent, with larger increases in partisan identification caused by the father supporting a party (just under 11 per cent) and a larger still effect on the individual feeling closer to a party on the individual feeling closer to a party – over 15 per cent. Overall, whereas the partner – and, for the under 30s, the mother – socialise the individual into voting, it is the father that

socialises the individual into key political psychological factors that have repeatedly been shown to affect voting. Whereas spouses and mothers lead politically by example, the 'do what I say, not what I do' approach of the father to political socialisation in non-voting matters is effective. I interpret this as evidence that much of the effect of socialisation on turnout by proximate cohabitees is not the result of politicisation but more the effect of simply being reminded and accompanied to vote when those who are in the house with the individual vote.

Figure 2: Standardised marginal effects of political interest and partisan identification of household members on respondent's political interest and partisan identification



Political discussion

I now move onto investigating the more specific role of political discussion on turnout. First, I consider the effect that the quantity of political discussion by an individual has on their probability of voting. In five waves of the BES (June 2014, November 2014, March 2015, prior to the general election in May 2015 and after the general election in May 2015) respondents were asked how many days they had talked about politics in the last week (between 0 and 7). As shown, in Table 3, the within-effect of discussion on turnout is positive and statistically significant,

suggesting that previous between-individual findings were not (solely) the result of omitted variable biases and that, indeed, more political discussion increases the individual's likelihood of voting. The marginal effect of a one day increase in days of political discussion per week is to increase one's probability of voting by 2.2 per cent.

Table 3: Fixed effects logistic regression of turnout on days of political discussion per week (June 2014 – May 2015)

	Days of political discussion on turnout
Days of political discussion	1.108***
	(0.023)
Wave (ref: 2)	
Wave 3	3.843***
	(1.518)
Wave 4	0.774***
	(0.0605)
Wave 5	1.151
	(0.0950)
Wave 6	0.146***
	(0.0115)
Observations	7,010
ID	2,084
*** p<0.001, ** p<0.01, * p<0.05	

While the positive effect of the quantity of political discussion seems clear, the literature also points towards differences in the effect of political discussion according to the individual's relationship with their discussants. Respondents to the BES were asked to name up to three people that they sometimes discuss politics with in Wave 2 and again in Wave 4 (if they did not respond in Wave 2). The combined results from the two waves are shown in Table 4. Most individuals discuss politics with at least two individuals and by some distance the most common person to talk about politics with is one's spouse or partner.

Table 4: Who BES respondents discuss politics with (weighted)

Relationship	Person 1	Person 2	Person 3
Spouse or Partner	40.6	6.3	3.0
Other relative	17.8	24.5	14.7
Friend	16.1	19.1	16.7
Neighbour or co-worker	5.8	8.2	6.6
Other	4.6	2.8	2.9
None	15.3	39.0	56.1
N	23,147	23,147	23,146

The BES also asks, with reference to the 2014 European Parliamentary elections, whether anyone accompanied the respondents to the polling station and what was the relationship of that person to the respondent. By far the two most common responses are either alone or with one's partner or spouse, with the proportion who voted in 2014 with their spouse very similar to the proportion that discuss politics most with their spouse. The results are shown in Table 5.

Table 5: Who BES respondents voted with at the 2014 European Parliamentary elections (weighted)

	%
Alone	40.6
With partner or spouse	39.4
Discussant 1 (besides partner or spouse)	5.8
Discussant 2 (besides partner or spouse)	4.9
Discussant 3 (besides partner or spouse)	3.0
Other cohabitee	5.0
Other non-cohabitee	1.7
N	20,547

The effect of political discussion on turnout is positive and individuals are most likely to discuss politics, as well as attend the polls, with their spouses or partners. However, does the effect of political discussion with one's spouse – likely to be more intimate, frank and open than other forms of political discussion – have a greater effect on turnout that political discussion in other relationships? In Table 6, I present the effects of political discussion on turnout in various types of relationships, controlling for number of discussants with whom one discusses politics and a range of socio-demographic control variables, as well as two political context variables. Because the

BES only asks with whom one discusses politics once, I use between-individual logistic regression models.

However, because the independent variables are taken from waves prior to the retrospective reporting of turnout, risks of endogeneity caused by reverse causality and simultaneity bias are significantly reduced. As we can see in models 1 and 2, the effects of discussing politics with a spouse or a relative are positive and statistically significant while the effects of discussing politics with friends or neighbours and colleagues (models 3 and 4) are not statistically significant. The effect on turnout of discussing politics with no one (model 5) is negative and statistically significant. However, all of these effects lose their statistically significant when tested simultaneously with days of political discussion per week (model 7), suggesting that it is likely that the stronger effects of discussing with partners, spouses and family members is the result of the greater frequency of such discussions.

Table 6: Effects of political discussion in various relationships on turnout (between-individual logistic models)

	(1) Partner or spouse	(2) Relative	(3) Friend	(4) Neighb. Or Colleag.	(5) No one	(6) Days of political discuss	(7) Combine
Discussion with partner or spouse	1.215**						1.063
	(0.116)						(0.116)
Discussion with relative		1.232**					1.180
		(0.122)					(0.142)
Discussion with friend			1.020				0.980
			(0.103)				(0.122)
Discussion with neighbour				0.834			0.896
				(0.097)			(0.121)
Discuss with no one					0.492***		
					(0.045)		
Days of political discussion per week						1.470***	1.464***
						(0.041)	(0.041)
Number of political discussants (0-3)	1.298***	1.279***	1.331***	1.360***		1.031	1.007
	(0.047)	(0.050)	(0.052)	(0.049)		(0.040)	(0.070)
Age	1.025***	1.025***	1.025***	1.025***	1.026***	1.030***	1.030***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Male	1.190**	1.195**	1.172**	1.180**	1.191**	1.116	1.144*
	(0.094)	(0.095)	(0.092)	(0.093)	(0.094)	(0.089)	(0.093)
Household income	1.028**	1.031**	1.030**	1.033**	1.034**	1.022	1.023*
	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.013)	(0.014)
Education (age of leaving)	1.114***	1.115***	1.113***	1.112***	1.128***	1.057*	1.058*
	(0.033)	(0.033)	(0.033)	(0.033)	(0.034)	(0.032)	(0.032)
Married	1.184*	1.285***	1.275***	1.263***	1.251***	1.168*	1.142
	(0.111)	(0.112)	(0.112)	(0.110)	(0.108)	(0.102)	(0.108)
Home ownership	1.475***	1.477***	1.478***	1.477***	1.473***	1.495***	1.493***
	(0.133)	(0.134)	(0.134)	(0.133)	(0.133)	(0.138)	(0.137)
White ethnicity	1.473**	1.464**	1.493**	1.489**	1.521***	1.483**	1.449**
	(0.239)	(0.237)	(0.242)	(0.241)	(0.246)	(0.246)	(0.241)
Partisan contact	5.055***	5.061***	5.074***	5.081***	5.633***	3.064***	3.036***
	(0.832)	(0.834)	(0.837)	(0.838)	(0.920)	(0.522)	(0.518)
2015 majority	1.003	1.003	1.003	1.003	1.003	1.001	1.001
	(0.002)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Constant	0.379***	0.367***	0.372***	0.374***	0.616*	0.247***	0.251***
	(0.105)	(0.102)	(0.103)	(0.104)	(0.172)	(0.0712)	(0.0728)
Observations	11,758	11,758	11,758	11,758	11,758	11,734	11,734

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

Furthermore, the above models do not control for political psychological factors that are likely to affect one's propensity to vote, such as election interest, partisan identification and efficacy. When these are controlled for, in Table 6, we can see that the effects on turnout of political discussion in various types of relationships become non-statistically significant. Conversely, the positive effect of days of political discussion per week, albeit weakened, retains its statistical significance when tested alone and in the combined model. Notwithstanding potential covariance (likely to be heavily skewed between individuals towards the effects of political psychology on political discussion), this suggests that the statistically significant positive effect of political discussion with partners, spouses and family members is in fact the result of

individuals who discuss politics having higher levels of political interest, efficacy, partisanship, political knowledge and a sense of civic duty to vote.

Table 6: Effects of political discussion in various relationships on turnout with political psychological controls (between-individual logistic models)

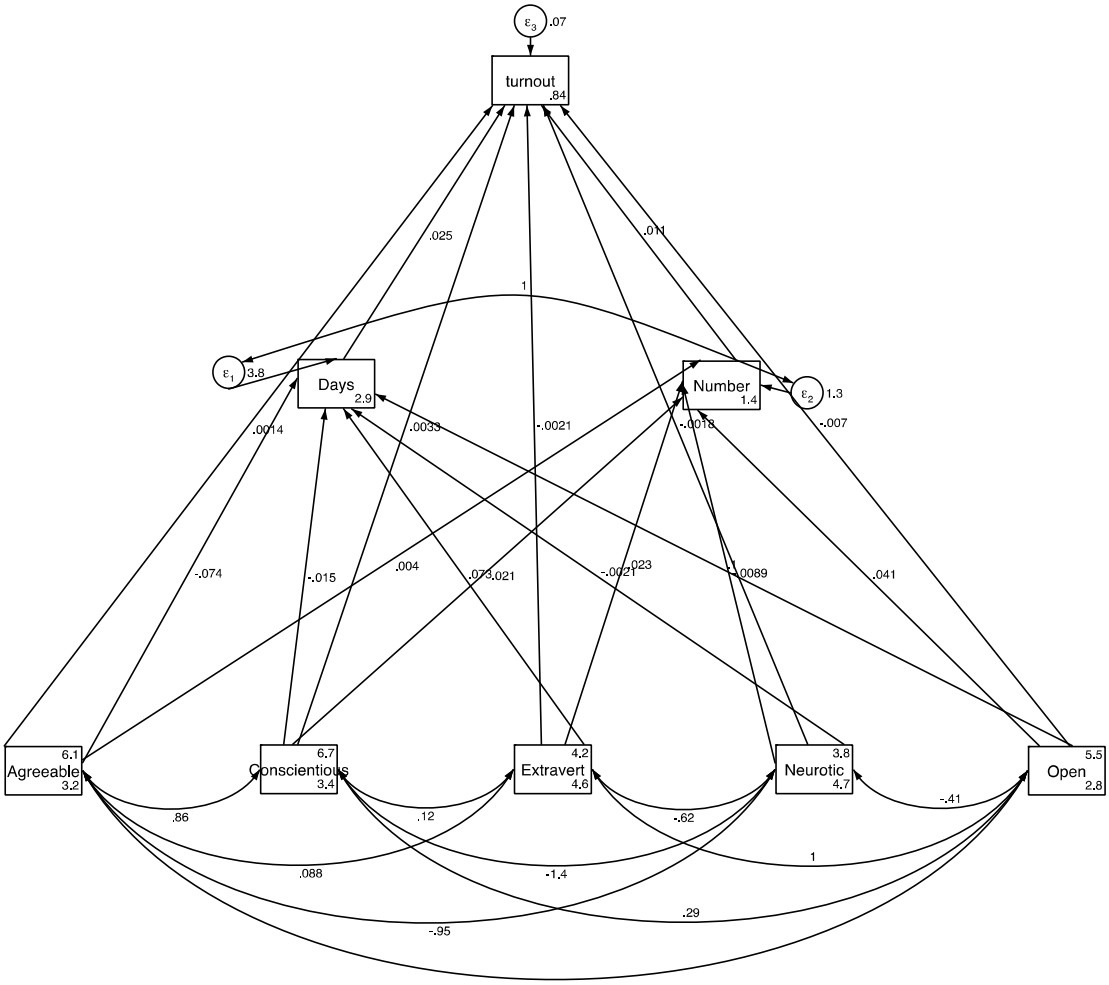
	(1) Spouse	(2) Relative	(3) Friend	(4) Neighb. Or Colleag.	(5) No one	(6) Days of political discuss	(7) Combine
Number of political discussants (0-3)	0.919	0.906	0.946	0.940		0.885**	0.948
	(0.0416)	(0.0433)	(0.0449)	(0.0423)		(0.0401)	(0.0740)
Discussion with spouse	1.006						0.938
	(0.108)						(0.114)
Discussion with relative		1.080					0.998
		(0.119)					(0.131)
Discussion with friend			0.852				0.805
			(0.0958)				(0.110)
Discussion with neighbour				0.814			0.783
				(0.105)			(0.116)
Discuss with no one					1.204		
					(0.141)		
Days of political discussion						1.096***	1.101***
						(0.0372)	(0.0377)
Election interest	2.422***	2.423***	2.428***	2.423***	2.417***	2.269***	2.283***
	(0.162)	(0.162)	(0.162)	(0.162)	(0.162)	(0.160)	(0.162)
Internal efficacy	0.842**	0.841**	0.843**	0.838**	0.831**	0.806***	0.800***
	(0.0630)	(0.0629)	(0.0630)	(0.0627)	(0.0616)	(0.0617)	(0.0614)
External efficacy	0.905*	0.906*	0.905*	0.905*	0.906*	0.904*	0.903*
	(0.0537)	(0.0538)	(0.0538)	(0.0537)	(0.0539)	(0.0539)	(0.0539)
Political knowledge	4.736***	4.693***	4.727***	4.827***	4.640***	4.817***	5.008***
	(1.453)	(1.440)	(1.450)	(1.481)	(1.418)	(1.478)	(1.547)
Party ID (or closer)	1.907***	1.902***	1.915***	1.910***	1.899***	1.880***	1.898***
	(0.253)	(0.253)	(0.255)	(0.254)	(0.252)	(0.250)	(0.253)
Civic duty to vote	2.339***	2.338***	2.341***	2.336***	2.334***	2.349***	2.349***
	(0.114)	(0.114)	(0.114)	(0.114)	(0.114)	(0.115)	(0.115)
Age	1.004	1.004	1.004	1.003	1.004	1.006	1.006
	(0.00392)	(0.00393)	(0.00394)	(0.00393)	(0.00392)	(0.00401)	(0.00404)
Male	1.156	1.165	1.165	1.168	1.156	1.181*	1.203*
	(0.112)	(0.113)	(0.112)	(0.113)	(0.111)	(0.114)	(0.118)
Household income	1.004	1.004	1.003	1.007	1.003	1.005	1.009
	(0.0154)	(0.0153)	(0.0153)	(0.0155)	(0.0153)	(0.0154)	(0.0157)
Education (age of leaving)	0.976	0.976	0.979	0.974	0.974	0.969	0.972
	(0.0343)	(0.0344)	(0.0345)	(0.0343)	(0.0342)	(0.0342)	(0.0345)
Married	1.200*	1.207*	1.185*	1.191*	1.207*	1.179*	1.168
	(0.126)	(0.120)	(0.118)	(0.118)	(0.120)	(0.117)	(0.123)
Home ownership	1.488***	1.486***	1.486***	1.482***	1.492***	1.488***	1.481***
	(0.156)	(0.155)	(0.155)	(0.155)	(0.156)	(0.156)	(0.155)
White ethnicity	1.084	1.076	1.077	1.089	1.079	1.116	1.122
	(0.204)	(0.203)	(0.203)	(0.205)	(0.203)	(0.211)	(0.213)
Partisan contact	1.932***	1.930***	1.946***	1.930***	1.889***	1.805***	1.821***
	(0.354)	(0.354)	(0.357)	(0.354)	(0.345)	(0.335)	(0.338)
2015 majority	1.001	1.001	1.001	1.001	1.001	1.001	1.001
	(0.00334)	(0.00334)	(0.00334)	(0.00334)	(0.00333)	(0.00334)	(0.00335)
Constant	0.0148***	0.0147***	0.0143***	0.0149***	0.0133***	0.0158***	0.0148***
	(0.00682)	(0.00677)	(0.00658)	(0.00688)	(0.00625)	(0.00736)	(0.00694)
Observations	11,713	11,713	11,713	11,713	11,713	11,698	11,698

Odds ratios and (standard errors) reported; *** p<0.001, ** p<0.01, * p<0.05

Personality, political discussion and turnout

One area of psychology that does not risk such covariance issues and which is plainly more distal from turnout than political discussion is that of personality types. Personality types - typically operationalised using the 'Big Five' personality traits of agreeableness, conscientiousness, extraversion, neuroticism and openness - have become increasingly common explanatory variables of political behaviour (see the 'Psychology and Voter Turnout' article in this thesis). The Big Five model of personality has been a particularly popular metric because it captures a wide range of individual variation and is formed early in life and is therefore relatively static (Srivastava et al, 2003; Johnson et al, 2005; Mondak, 2010). In particular, personality types have been shown to be a strong predictor of social behaviour (e.g. Selfhout et al, 2010), with individuals with high extraversion, openness and agreeableness tending to have wider social networks. As such, we might expect these traits to have an effect on turnout that is mediated by two of the political discussion variables with the strongest effects on turnout from the previous models – days discussing politics per week and number of the people that the individual discusses politics with. To capture this potential mediating effect I use structural equation modelling, the result of which is shown in Figure 3, with the effects given in Table 7. In Table 7 I also present the direct effects of the Big Five personality types for the purposes of comparison.

Figure 3: The effects of personality traits on turnout, mediated by number of days discussing politics per week and number of political discussants (SEM model)



From Figure 3 and Table 7, we can see that days of political discussion per week and number of political discussants both mediate the effect of the Big Five in a number of ways. First, higher openness and extraversion both increase the individual's regularity of political discussion and the number of individual's with whom they discuss politics (Models 1 and 2). Both of these effects are statistically significant. Furthermore, greater agreeableness increases the days per week in which one discusses politics and conscientiousness increases the number of individuals with whom one discusses politics. As we already know, these two political discussion variables, in turn, have statistically significant positive effects on turnout (Model 3). Besides the effect they have on turnout via the two political discussion variables, the Big Five continue to have statistically significant direct effects on turnout – conscientiousness and openness have positive effects on turnout, outside of their effect on political

discussion, while extraversion has a negative effect on turnout, outside of its effect on the two political discussion variables. In the case of extraversion and openness, these effects – negative and positive respectively - are opposite to their respective effects on turnout when the mediating effects of days of political discussion and number of political discussants is modelled.

Table 7: The effects of personality traits on turnout, mediated by number of days discussing politics per week and number of political discussants as well as direct effects of personality traits (regression models)

	(1)	(2)	(3)	(4)
	Days of political discussion	Number of political discussants	Turnout	Big Five (Direct model)
Days			0.0253***	
			(0.0015)	
Number			0.010***	
			(0.0029)	
Agreeable	0.073***	0.003	0.001	0.001
	(0.009)	(0.006)	(0.001)	(0.001)
Conscientious	-0.0153	0.020***	0.003**	0.004***
	(0.009)	(0.006)	(0.001)	(0.001)
Extravert	0.072***	0.022***	-0.002*	0.001***
	(0.007)	(0.005)	(0.001)	(0.001)
Neurotic	-0.002	-0.008	-0.001	-0.003***
	(0.007)	(0.005)	(0.001)	(0.001)
Open	0.104***	0.0406***	0.007***	-0.004***
	(0.009)	(0.006)	(0.001)	(0.001)
Constant	2.926***	1.437***	0.839***	0.922***
	(0.108)	(0.072)	(0.016)	(0.010)
Observations	21,230	21,230	21,230	29,219

Conclusion

Socialisation – both at the household level and within social circles - has strong effects on the individual's chance of voting. Though previous research had already argued that this is the case, this study has improved on prior findings in terms of both robustness and nuance. In terms of robustness, I show that when looking at the effects of cohabitee voting behaviour on *only* within-individual variation in turnout, we see a positive effect. This brings the robustness of one of the key socialisation and turnout

effects into line with that of the socialisation and party choice literature. I also show, again purely within individuals, that individuals are more likely to vote during times of greater political discussion in their lives than at other times. This improved upon previous findings that show that individuals who discuss politics more are more likely to vote, given the obvious risks of unobserved heterogeneity whereby individuals discussing politics were already more likely to vote.

In terms of nuance, this study also provides a number of key insights. First, the effects of cohabitee turnout on the turnout of the individual seem unlikely to be the result of politicisation and are instead likely to be the result of more pragmatic issues such as reminders and greater willingness to vote together. I come to this conclusion following a number of findings. First, the turnout of one's partner or spouse has a very large – over 30 per cent – marginal effect on one's own turnout. On the other hand, the electoral participation of one's mother and father have very small and non-statistically significant positive effects on turnout, despite the fact that we would expect these familial channels to also provide sources of politicisation. Indeed, when we only consider these effect on respondents who were under 30, we see that the effect of the partner and spouse's turnout is weaker – though still strong and statistically significant – and the effect of the turnout of the mother becomes far stronger and statistically significant, unlike that of the father, which remains non-statistically significant. More compelling, and perhaps surprising, is the finding that the political interest and partisan identification of one's partner or spouse has no statistically significant effect on one's own political interest and partisan identification. By contrast, it is the political interest and partisan identification of the father that affects the individual in these respects – the only relationship to show no evidence of increasing one's chance of voting - further dampening the idea that the socialisation effect in the household has much political substance to it and is far more to do with practical matters.

The findings regarding political discussion reflect those about the behaviour of cohabitees. As already mentioned, I find strong evidence that turnout is affected by the amount of political discussion during the electoral campaign. However, contrary to previous findings in the literature, there is no evidence that the nature of one's

relationship to their political discussants affects one's probability of voting, when controlling for the amount of political discussion per week. This suggests that, in actual fact, the stronger effects of discussing politics with one's spouse or family are actually simply a reflection of the greater amount of discussing that occurs in these relationships. Similarly, even when one's regularity of political discussion is not controlled for but political psychological variables such as election interest and sense of duty to vote are, the nature of the relationships become unimportant. By contrast, there is strong evidence that quantity of political discussion matters, even when controlling for highly proximal political psychological variables.

Finally, this paper also finds evidence that those psychological variables that are clearly distal from turnout in their causality – personality traits – have strong indirect effects on turnout via their affects on political socialisation measured in terms of regularity of political discussion and number of political discussants. In short, being more outgoing, more open-minded and more agreeable makes for a more politically social citizen, which in turn increases electoral participation.

Overall, the most impactful terms of political socialisation, in terms of their effects on turnout, seem relatively non-political. People vote when those around them in their households vote – be it their husbands and wives or, in the case of young people, their mothers. Yet this seems to be driven far more by issues of convenience and practicality than issues of politicisation. Similarly, discussion increases turnout in terms of its sheer quantity, yet not in terms of its quality as measured by the relationship to the person with whom one is discussing. We might think that the open, frank and intimate nature of long-term relationships or of other familial ties would lead to more deeply political discussions that, therefore, would have stronger socialising effects. However, there is very scant evidence to suggest that the effects of discussing politics with co-workers or neighbours is any less impactful than doing so with family, outside of the fact that the latter is more regular. Indeed, the drivers of political discussion – both in terms of quantity and in terms of number of discussants – are, at least to some extent, what we would expect of any social interaction – a sociable personality. The relationship between socialisation and turnout is strong and nuanced, yet it seems far more practical than political.

Conclusion

The four essays in this thesis aim to explain within-individual variation in voter turnout. The motivation behind this aim was not only the importance of voter turnout to democracy, both in theoretical and substantive terms, but also the methodological and theoretical weaknesses in the existing literature caused by the lack of attention given to why individuals vote at some points in their lives and not at others. This deficit stands in contrast to the vast literature explaining aggregate-level turnout – both within and between countries and other political units – and individual-level turnout solely between individuals. Each of the four essays seeks to re-think one of the explanatory models of individual-level voter turnout – mobilisation, resources, psychology and socialisation - by applying many of their between-individual determinants to within-individual variation, as well as, in some cases, adding new ones.

In each essay I draw on panel data from the UK, which I analyse using fixed effects models that are intermittently accompanied by random effects panel models and standard cross-sectional models. The data includes the long-term, socio-economic, household British Household Panel Survey, encompassing five general elections in the UK, and the short-term British Election Study, thus far encompassing nine waves over the course of two and a half years. I also use data from the Swiss Household Panel in one of the essays. This approach overcomes important sources of endogeneity and simultaneity bias in two ways. First, it controls for all time invariant effects, removing potential sources of omitted variable biases that cast doubt on some of the existing findings. Second, in the models testing data drawn from the BHPS I regress retrospective turnout on independent variables taken from the preceding year's survey. The resulting dataset thus includes five waves each covering one general election. This precludes the possibility of reverse causality bias and reduces potential for simultaneity bias, also reduced by controlling for waves. By explaining within-individual variation in voter turnout, the essays in this thesis will hopefully underline the importance of better delineating between studies that seek to explain why some individuals vote, on the one hand, and why individuals vote at some

elections and not others, on the other hand, for both substantive and methodological reasons.

Regarding a number of predictors of turnout, the essays in this thesis offer more robust conclusions than previous studies, sometimes with congruent and sometimes contrary results to the between individual literature. As previous studies have shown – albeit primarily by testing the effects of non-partisan mobilisation – I show that partisan mobilisation increases the individual's chance of voting. Unlike these previous studies, however, I find no evidence that type of election – six of which I consider – has any obvious effect on the size of the marginal effect of mobilisation. Despite previous claims that the effect of mobilisation is likely to be larger in more salient elections, the effects of party contact in the 2014 European Parliament campaign and the 2015 General Elections campaign were practically identical. Similarly, personalised forms of campaigning – as opposed to non-personal forms such as e-mail, that have been the focus of so much of the turnout literature show no signs of having stronger effects on turnout. However, it may be that partisan contact is treated by recipients differently to non-partisan contact. Regardless, this is an important finding given that partisan mobilisation is far more pervasive. Like previous studies have shown, I show that electoral closeness has no direct effect on turnout and its indirect effect is purely via the greater partisan campaigning activity in close races – though admittedly this finding is based on the less robust random effects approach.

One new contribution of this paper is to posit three mutually non-exclusive causal mechanisms between partisan mobilisation and voter turnout – that recipients are prompted, persuaded and provoked into voting. I find the strongest evidence that individuals are persuaded by the campaigning literature they receive as well as strong evidence that they are prompted into voting. I also find, in the case of two parties, including the radical right UKIP, at receiving partisan mobilisation from one of these parties increases the individual's probability of voting for another party altogether.

Perhaps the results that are most contrary to those drawn from the between-individual literature came from the essay entitled Resources and Voter Turnout. While income

and unemployment have positive and negative effects on turnout respectively, as the existing literature would predict, these variables both become non-statistically significant when non-financial resources are controlled for. Given the centrality with which material wealth has been used to explain turnout previously, I consider this an important finding as it suggests that resource explanations have largely been based on biases emanating from unobserved variables and that the effect of financial resources is primarily a proxy for lifestyle factors that better nurture a life of political participation. Furthermore, the only financial factor that maintains its statistical significance when controlling for non-financial resources is that of self-assessed financial situation. However, this has the exact opposite effect that the resource model would predict, with those stating that they are finding it difficult financially considerably more likely to vote than those who say they are living comfortably. Again, this is an important finding because it suggests that the primary effect of resources is one of grievance whereby individuals who consider themselves worse off are mobilised to vote against the government. It is also worth remembering that the limitations on the use of household income as a measure of financial capacity to vote are numerous, again lending additional relative credence to the self-assessed financial situation finding.

An additional new contribution of this paper is to take some of the approaches of the economic voting literature – primarily the division of economic effects between those who support the incumbent party and those who do not – and apply them to turnout. By doing so, I show that the effects of financial resources are only felt amongst those who do not support the governing party, adding further weight to the idea of a grievance-based causal mechanism between finances and turnout.

The key finding regarding resources, however, is of the very large marginal effects of change in non-financial resources on turnout. In particular, the positive effects of bring married, having children and being residentially stationary on turnout are highly impactful. I also find evidence that moving into a managerial job role increases the probability of voting. All of these findings lend evidence to the theory that within-individual variation in turnout is caused by taking up a lifestyle based on rootedness and responsibility. This is further evidenced by the fact that, amongst marital statuses,

only marriage has a strong positive effect on turnout – individuals again become less likely to vote when divorced or widowed. Similarly, as an individual has more children and their lifestyle revolves more around parenthood, their chances of voting continue to increase. Whereas the famous resource model – originally called the baseline model (Verba and Nie, 1972) argued that abstention was the result of voters being incapacitated in terms of time, money and skills, this thesis finds little evidence to support those arguments and even more to suggest the contrary. In some ways, citizens are *more* likely to vote when they are busy, although indeed an increase in self-assessed free time is associated with a greater probability of voting when actual lifestyle changes are controlled for. In terms of skills, this thesis only finds sporadic evidence that internal efficacy – the belief that one has the ability to make accurate voting decisions – has an impact on turnout, with similarly muted effects of political knowledge, though these findings are based more on the psychological essay. Another original contribution of this thesis is to consider both physical and mental health as resources that affect turnout, with both having positive effects on turnout – again amenable to the view that a certain lifestyle is what causes turnout within-individuals. Overall, if these findings can be transposed to the aggregate level, it may be that the decline in voting in the twentieth century was the result of changing lifestyles that became more transient, more atomised and more individualistic.

With regard to psychological variables, we see a broad variation between the effects on within-individual variation in turnout of variables that are regularly used to explain electoral participation. Over the long-term, individuals vote when they are interested in politics and when they identify with a political party, with partisan identification of greater strength further increasing the probability of voting. Conversely, issues such as internal and external political efficacy and trust in politicians and satisfaction with democracy are unimportant. Similarly, in the short-term, being interested in the election and feeling a sense of duty to vote increase the individual's chance of voting. Variation in both forms of efficacy, attitudes to politicians, political knowledge and even partisan identification are relatively less important. The two key psychological variables are caring about the election and feeling obliged to vote in it. Again, if these findings can be transposed successfully to the aggregate-level, then it seems unlikely

that political disenchantment, populism or so-called anti-politics sentiment has been the key driver of declines in national turnout figures.

An additional contribution to the literature regarding the effects of psychology on turnout relate to personality types, albeit necessarily based on between-individual findings. Though there has been a recent flurry of research into this area, I find that the direct effects of personality types are muted, with the exception that individuals who are more open to experience seem less likely to go to the polls. However, personality types do affect the two key political psychological drivers of turnout, with more open-minded, emotional stable, conscientious and *dis*agreeable individuals being more interested in the election, and more agreeable, conscientious and extraverted individuals feeling more of a sense of duty to vote.

In terms of socialisation, I argue that the effects of the behaviour of cohabitees on turnout is likely to be less political than we might have thought, again pointing towards the primacy of lifestyle as the determinant of within-individual variation in turnout. Though indeed individuals are highly influenced by the political behaviour of their cohabitees, the evidence presented in this thesis suggests that this is far more the result of having company when voting and having someone to act as a reminder to vote. Indeed, the very relationship through which individuals are seemingly politicised the most – via the father – is the one that has the least direct effect on actual turnout, which is likely due to the greater absence of the father from home during the day. This is not to say that greater political discussion does not increase turnout. I find robust evidence that the more one discusses politics, the likelier they are to vote. In contrast to previous works, I find only partial evidence that the relationship of the discussant to the individual has any effect on the magnitude of the influence of discussion. Instead, it appears that the reason that discussion with spouses and family members matters more is primarily because this is considerably more frequent. Moreover, I again use personality types to provide evidence that the causes of political discussion are at least in part non-political – with more open-minded, outgoing and agreeable individuals likely to discuss politics, which in turn drives them to the polls.

Within-individual variation in turnout seems to be a function of one's lifestyle, on the one hand, and an interesting and active political supply, on the other. Given the relatively impracticality and undesirability of policy-makers affecting change in lifestyles in order to increase the proportion of citizens with lifestyles amenable to voting, does this mean that such officials should give up attempts to increase aggregate level turnout? Not necessarily. Rather, it is voting and turnout *as-is* that is unsuited to many of the characteristics of modern life that became more prominent in the second-half of the twentieth century – namely greater transience and relative social atomisation. As rules and information regarding voting and elections become more amenable to such trends – for example, via various e-initiatives and voting via mobile applications – we may expect to see the effects of mobility and social networks diminish in their impact on within-variation in turnout. Moreover, as this thesis noted when forming its methodological approach, aggregate level cross-time trends in turnout – primarily decline in Western countries that have only partially recovered recently – are not necessarily the same as individual-level ones. Testing the possibility of such ecological issues should be a component of any future aggregate-level works that explain cross-time trends with the lifestyle factors that this thesis diagnoses at the individual level. Furthermore, individuals seem responsive to the political supply – with interesting political context, parties that people identify with and political actors that directly contact voters all increasing the individual's chance of voting.

While this thesis goes some way towards addressing the relative deficit of investigations of within-individual variance in the turnout literature, there remain numerous areas of potential further study. As mentioned in the introduction, the explanatory variables in these essays are overwhelmingly related to the demand-side. Important supply-side sources of variation, such as the changing party system both nationally and locally, the popularity of leaders, and the political context of the time, are not considered. Though we can consider it likely that the positive effects of within-variation in political and election interest and partisan identification as shown in this thesis – and supported partially by the finding that partisan mobilisation affects turnout most powerfully through persuasion – are the result of the political supply to

some extent, no direct evidence of this is presented. An important part of further research will be to address this relative imbalance.

Another next step for research will be to test more comprehensive models of turnout. While Smets and van Ham's (2013) six models of turnout – four of which are directly tested in this thesis – is one way to divide explanations for turnout, it is by no means the only way and, in some ways, is overly disparate substantially. Clarke et al (2009) summarise alternative, more connected models of turnout such as the cognitive engagement mode, based on education, attention to politics, media consumption and evaluations of the government, the general incentives model, built on political efficacy, costs of participation, and individual and group benefits, and the valence model, built on leader images, economic evaluations, perceptions of party competence and satisfaction with democracy. The essays in this thesis consider many of the variables in these theoretically more unified models, yet neither do so completely nor jointly. Similarly, by separating the predictors of turnout into the four essays as I have done, we can logically deduce the position of each within a causal chain leading towards turnout. However, such a unified model remains untested.

Conversely, going forward each independent variable should be tested as the sole subject of investigation both to better check whether its effects are genuinely causal and in order to attain a more nuanced understanding of what the effects are and how their effects vary when interactions are considered. In this thesis, I aimed to make a first step towards explaining within-individual variation in voter turnout. As such, dozens of independent variables are employed to test the major theories from the between-individual literature. While I am of the opinion that this approach ensured that the most progress could be made within the limits of single PhD thesis in understanding within-individual variation *per se*, and while retaining robust methodological approaches, one downside is that a more nuanced and specific hypothesis testing was not possible. Future research will investigate this, particularly regarding the variables that showed very strong or surprising effects in this thesis.

On specific methodological matters, there remain a number of potential sources of bias that future research should consider. First, is the sampling biases that may ensue

from panel attrition, particularly in the longer-term models. For example, individuals who find themselves pressed for time because of new job responsibilities or care-giving responsibilities may decide to opt out of future participation in the panel survey. This could see individuals drop from the panel at their times of being busy and thus lead to models that underestimate the effects of free time on voting. Similarly, there could be an issue regarding the correlations between political interest and participation in panel surveys. If individuals leave panels when not political interested, the models that test the effects of political interest and related variables could be biased. Multiple imputation methods should be employed in future to estimate the effects of within-sample attrition.

Second, the models in this thesis are single equation static specifications. Going forward, it would be useful to employ dynamic models to leverage the power of the multi-wave panel survey data. The models in this thesis are static, primarily, as a result of data limitations that already see small samples when non-time variant individuals are excluded from the models. This means that there remain sources of reverse causality and I am not able to show how the effects of the independent variables carry on well after the time that they take place (a particularly prescient case would be that of partisan mobilisation, the effect of which may last for some time after occurring).[7]

Finally, this thesis heavily relies upon data from just one country, the United Kingdom. To draw conclusions on universal causal mechanism from just one country case should be done with caution. As such, future research should broaden the country cases where data permits, as it already does with some American sources and will likely increasingly do so with other European socio-economic and election panel studies.

Overall, however, this thesis makes a number of theoretical, methodological and substantive contributions. I show how the neglect of within-individual variation is an

[7] However, dynamic models create their own statistical issues, notably that the fixed effects components engender a problem of simultaneity bias that is addressed by first differencing, which itself creates a second simultaneity bias problem because the first differenced lagged endogenous variable is correlated by construction with the non-invertible error term. This problem is addressed by using instrumental variables in a GMM (generalised methods of moments) framework (for a recent treatment, see Roodman (2009).

important omission from the voter turnout literature and why this is likely to have significant consequences. I also show how only some of the findings from the between-individual literature are evidenced within-individuals, in some cases leading us to question the reliability of the causal mechanisms built on prior findings. Moreover, from the findings in the essays of this thesis, I show that within-individual variation in voting seems to be fairly unaffected by such issues as material resources, 'anti-political' sentiments, household politicisation or even feelings of personal ability to vote effectively. Rather, I conclude that within-individual variation can be primarily accounted for by the lifestyle of the individual at the time of the election – with lifestyles built on rootedness, social integration and roles demanding responsibility increasing the individual's desire to turnout to vote, particularly when they are interested in the politics of the time, feel affinity towards a party or when a party has bothered to contact them.

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Appendices

Appendix 1: Vote switching in the BHPS, SHP and BES

	BHPS	SHP (10 point variable)	BES (general election only)
% of the total sample that report variation in turnout	24.67	74.06	6.40

Appendix 2: Comparison statistics of the BHPS within-varying and total sample

Variable	Within-varying sample (FE sample)	Total sample
N	17,448	55,878
Average turnout	56.9	74.7
% Male	44.4	46.2
Age	42.4	45.4
Education		
None/primary school	22.0	24.3
Secondary / Sixth form	40.6	40.8
University Degree	37.5	35.0
Household Income	19.1	19.0
Financial situation		
% Living Comfortably	3.0	2.6
% Doing alright	6.8	6.1
% Just about getting by	28.0	26.4
% Finding it quite difficult	36.8	35.8
% Finding it very difficult	25.4	29.1
% Unemployment	6.6	6.0
% Free time	4.7	4.8
% Moved in last year	12.0	11.0
% Student	0.7	1.2
% Married	51.3	53.7
% Children in HH	36.2	28.5
% Bad health	16.0	17.0
% Depressed	2.0	1.9
% Responsible for child under 16	21.9	16.6
% Responsible for child under 12	18.1	14.8
External political efficacy (1 less / 5 more)	2.4	2.4
Political interest (1 more/4 less)	2.9	2.7
% Partisan identification	27.3	37.9
Strength of Party ID (0/3)	0.66	0.89
% Closer to a party	31.6	0.35

Appendix 3: Comparison statistics of the SHP within-varying and total sample

Variable	Within-varying sample (FE sample)	Total sample
N	49,655	69,114
Polls	7.1	7.6
Political interest	5.3	5.5
Satisfaction with democracy	6.0	6.0
External efficacy	3.8	3.7
Trust in federal government	5.6	5.6

Appendix 4: Comparison statistics of the BES within-varying and total sample

Variable	Within-varying sample (FE sample)	Total sample
N	10,219	162,409
Turnout	0.62	0.95
% Party contact	19.8	32.4
% Party contact by type:		
Phone call	1.2	2.7
Leaflet	14.4	24.6
Home visit	3.7	6.4
Street stall	1.2	2.5
Email	3.8	11
SMS	0.5	1.1
Other	0.5	1.0
Election interest	2.7	3.4
Internal efficacy	3.1	3.5
External efficacy	3.7	3.4
Trust MPs	2.5	3.1
Sat. w/ dem.	2.2	2.4
% Party ID	50.7	78.0
% Party ID + closer	62.2	87.2
Duty to vote	3.3	4.2
Voting too much effort	3.2	2.8
Enjoy voting	2.7	3.3