

Divided by Brexit: A Quantitative Study
of Remain, Leave and Party Identities in
British Politics



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Abstract

The way individuals perceive themselves is frequently interconnected with their group memberships, giving rise to what is commonly referred to as their 'social identity'. As a result, individuals tend to perceive the world through a lens that enhances the reputation of the groups they belong to and exhibit a positive bias towards those who share the same in-group membership. Partisanship is an example of a social identity in the political sphere. People with a strong partisan identity evaluate policy outcomes more positively when their party holds power and view co-partisans more positively than out-partisans. In recent years, a new social identity has entered the political sphere in Britain, 'Brexit' identity, with many people exhibiting in-group and out-group bias according to the side they took in the EU referendum debate. In this thesis, I address how party and Brexit identity affected the British electorate in the context of this new, salient and divisive political issue. I show that the effects of campaigning during the lead-up to the Brexit referendum were mostly small, but differed significantly according to an individual's partisan identity. I show that when a person consumed more media sources like television they self-identified more strongly with their side of the Brexit debate. I demonstrate that partisan identity affected evaluations of the UK's performance during the COVID-19 pandemic, and attributions of responsibility for that outcome. Lastly, I find that Brexit identity had an even larger effect than partisanship on evaluations of pandemic performance, and also affected evaluations of how other countries performed during the pandemic. The findings have important implications for the literature on social identity, perceptual bias, and contemporary British politics.

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Chapter 1

Introduction

On 23 June 2016, the United Kingdom held a referendum on whether the country should leave the European Union. By a slim majority of 52%, the country voted for a British exit, or 'Brexit'. This was followed by an unprecedented period of political turmoil (Bale 2023); in a period of just six years, the UK held two general elections and was led by no fewer than five Prime Ministers. Long-standing political divisions over issues like immigration had burst into the foreground of British politics (Sobolewska and Ford 2020a, 2020b), and the result was a major realignment of the electorate around the issue of Brexit (Cutts et al. 2020; Fieldhouse et al. 2021).

Brexit caused a division within the country that endured over the years that followed the vote. During the referendum campaign, many British voters began to describe themselves as 'Leavers' or 'Remainers', reflecting their side in the debate about EU membership. They also exhibited the kind of in-group and out-group sentiments typically associated with partisan identity (Evans and Schaffner 2019). Indeed, Brexit identity actually became more prevalent and more relevant to some aspects of British politics than partisanship (Hobolt, Leeper, and Tilley 2020).

The 2016 referendum therefore reshaped Britain not only economically and politically, but socially as well. Voters who had long understood the political world primarily in terms of political parties were now given a new dimension to consider. Politicians were no longer labelled only as Conservative or Labour MPs, but as Remainers and Leavers

too. When evaluating candidates for a new BBC director general, the candidate's Brexit position mattered more to people than the candidate's party affiliation (Hobolt, Leeper, and Tilley 2020).

Brexit identity is particularly interesting because, unlike most other group identities, it is solely tied to politics. There are political consequences of groups like class (Evans and Tilley 2017), gender (Shorrocks 2018), and ethnicity (Heath et al. 2013; Sobolewska 2005). However, while individuals have a class, gender, and ethnicity irrespective of their interest in public policy, Brexit identity only makes sense in relation to a specific political issue. In this respect, Brexit identity is more akin to the purely political construct of partisan identity than to other group identities like class.

The inextricable link between Brexit identity and the specific matter of EU membership provides it with a temporal dimension. Before 2015, Britain's position in the European Union was a relatively fringe issue - mildly Eurosceptic voters had not been forced to sort themselves into one of two diametrically opposed camps. Brexit divided the country quite suddenly, in a period of months, in a way that no other social identity has done. At the 2019 General Election, Brexit was the central issue that helped decide the outcome (Cutts et al. 2020; Ford et al. 2021). By early 2020, the issue of Brexit was concluded, at least in the sense that the UK had officially left the European Union. The topics addressed by the empirical chapters in this thesis broadly relate to this interesting temporal dimension of Brexit identity, one feature that it does not share with partisan identity.

In the second chapter of this thesis, I consider the period just before the referendum itself. I address whether, at this early stage, the Brexit position of a voter already had a larger impact than party identity on their receptivity to campaign messages. Contrary to expectation, the findings suggest that party identity had a bigger effect than Brexit identity on the way an individual reacted to different campaigners.

In the third chapter, I consider whether, during the six years between the referendum

and Britain leaving the EU, the media played a role in reinforcing Brexit identity. I show that when individuals consumed more media sources, from reading newspapers to browsing the Internet, they felt more identified with their side of the Brexit debate.

In the fourth and fifth chapters, I turn to the period just after the UK officially withdrew from the EU. Having considered the formation of Brexit identity, I consider whether partisan and Brexit identity coloured the way in which voters evaluated national performance even after Britain had left the European Union. Specifically, I focus on perceptions of the UK's performance during the COVID-19 pandemic. There are good reasons to expect neither of these identities to have had a particularly large impact on these evaluations. Opposition partisans could well have sympathised with a government forced to deal with a major global event that caused significant problems for countries across the world. Brexit, meanwhile, was delivered, and the perceptions of Leavers and Remainers alike might have ceased to be coloured by this issue. On the contrary, I find that both partisan and Brexit identity affected perceptions of the UK's pandemic performance, and that Brexit identity affected how the performances of other countries were perceived too. Government partisans, and those more identified with the Leave than the Remain side of the debate, were significantly more positive about the UK's performance and, in the case of Brexit identity, significantly more negative about the performance of other countries.

The empirical chapters in this thesis therefore address a range of interesting questions in political sociology, all pertaining to the context of post-Brexit Britain. This is a context in which voters were divided not only by party identity, but also by Brexit identity. The research in this thesis shows that it took a little while for Brexit identity to rival partisan identity as a force in British politics, that the media reinforced this new identity, and that the importance of Brexit identity endured beyond the issue of Brexit itself. In the process of producing these broad findings I contribute to a wide array of literature, ranging from work on campaign effects to research on perceptual bias in politics. As a whole, this thesis enhances our understanding of the complex dynamics that politicised

identities can produce.

In the next section of this introductory chapter I briefly elaborate further on party and Brexit identity, introducing the idea that they are best understood as ‘social identities’. Next, I describe the data used for my analyses and the benefits this source provides. Lastly, I outline my empirical chapters and the individual contributions that each makes to the literature on social identity, perceptual bias, and contemporary British politics.

Party and Brexit as Social Identities

The Michigan model of electoral behaviour, which stresses the long term determinants of voting behaviour, has been hugely influential in political sociology (Campbell et al. 1960). Party identity is central to the theory, and remained central when the model was used to explain voting behaviour in Britain in the late 1960s (Butler and Stokes 1969). In addition to providing voters with a ‘homing instinct’ to one of the parties at each election (Converse 1969), the sense of belonging that partisan identity engenders helps to structure an individual’s beliefs and attitudes (Bowler 2018). Importantly, self-identification as a partisan is associated with a tendency to view the world in a way that reflects well on the favoured party (Bisgaard 2015; Slothuus and Bisgaard 2020). The boundaries of partisanship are permeable; anyone can choose to feel attached to a particular political party (Green, Palmquist, and Schickler 2002). However, the fact that partisanship is essentially a ‘choice’ in this respect does not undermine the importance of that psychological attachment to a voter’s self perception and, of particular relevance, their perceptions of the world (Green, Palmquist, and Schickler 2002).

Partisan identity is arguably best understood as a form of social identity (Greene 2004; Huddy, Mason, and Aarøe 2015). Social identities occur when an individual invests part of their self-esteem in a group to which they belong (Tajfel 1978b).¹ The result is an

1. Note that this mechanism reflects Tajfel’s social identity theory. Turner’s self-categorization theory is related but distinct, focusing more on the cognitive than the emotive aspect of self-identification

internal motivation to view their in-group more positively and their out-group more negatively, and to act in ways that will benefit the in-group and harm the out-group (Tajfel 1981). Understanding partisan identity in terms of social identity theory is not the only approach to using the concept, but it does help to explain the way in which partisan identity affects a person's perceptions. For example, that partisans view the economy more positively when their party is in power (Evans and Pickup 2010) can be interpreted as an expression of in-group bias on the part of voters who have invested some of their self-esteem in the competence of that particular party.

Brexit identity is also a social identity that pertains to a political division, as exhibited by the in-group and out-group bias displayed by Remain and Leave identifiers (Hobolt, Leeper, and Tilley 2020). For example, there is good evidence that Britons view the country's economic performance more or less positively depending on how they voted in the Brexit referendum (Sorace and Hobolt 2020). Indeed, when researchers measure Brexit identification with items developed in the field of social psychology, they identify the hallmarks of a genuine social identity (Evans and Schaffner 2019).

By focusing on partisan and Brexit identity, and by understanding their roles in British politics, I speak to a large body of existing scholarship in modern sociology. Political sociologists have long established the importance of understanding partisan identity for understanding the modern voter. Brexit identity is potentially as important for understanding voters in contemporary Britain. In this thesis, I therefore investigate how partisanship and Brexit identity function in a context where both identities are salient.

Data

The empirical chapters in this thesis all use data from the British Election Study Internet Panel (BESIP), the largest nationally representative and publicly available election study

(Turner 1987).

in Britain. To date, there have been twenty four separate BESIP waves fielded since 2014, each of which contains over 30,000 individuals. Alongside basic information about each respondent like their age, occupation, and voting history, the data include hundreds of survey items focussing on specific political issues of the day. The chapters in this thesis use the BESIP to address a wide range of empirical topics, ranging from sources of bias in evaluations of national and international responses to the COVID-19 pandemic, to the effect of reading newspapers and browsing the internet on identifying with one side of the Brexit debate.

Most BESIP respondents and variables are included in multiple waves, allowing for the use of methods that control for respondents' previous attitudes and behaviours. The analyses that follow use these panel data methods to infer causality, rather than mere association. In the first empirical chapter, for example, I use propensity score matching to control for the likelihood of a person being contacted by campaigners during the Brexit referendum campaign. This prevents the results, which concern the effectiveness of campaigning, from capturing the fact that some individuals are more likely than others to be contacted by campaigns.

An additional benefit of using the BESIP is that key variables are well measured. The clearest example is strength of Brexit identity, a variable that is important for multiple chapters in this thesis. Many surveys measure strength of identity by simply asking a variation of the question: 'how strongly do you feel this attachment?' The resulting variable is likely to capture related concepts, like strength of sentiment towards the European Union, rather than just the feeling of social identification that is of interest. In the BESIP, Brexit identity strength is measured with multiple items that are designed to tap genuine social identification by focusing on expressions of in-group bias. The items were first designed and tested in the field of social psychology, and have been found to have good external reliability when used in political sociology (Huddy 2013). As with the measurement strategy of other important variables in this thesis, this raises confidence that the analyses in this actually test the mechanisms which are hypothesised.

Outline of Papers

The chapters that together form this thesis all focus on the topics of Brexit and partisan identity in post-Brexit Britain. Each chapter answers a distinct empirical question and can therefore be read as a stand-alone article. It is for this reason that I have chosen to present my work as an integrated thesis (a collection of papers) rather than a single monograph.

In the first paper, which has been published in *Electoral Studies* and is presented here in Chapter 2, I consider the campaign period leading up to the Brexit referendum. The paper addresses whether traditional campaigning methods, like door-knocking, helped persuade voters to change their minds about Brexit. In particular, I consider whether the campaign affected voters differently according to their party identity and their referendum voting intention. I show that partisan identity, but not prior voting intention, significantly moderated the effectiveness of campaigner messaging. This result runs counter to the implications of Zaller's Receive-Accept-Sample model of opinion formation (Zaller 1992). The paper, therefore, offers an important contribution to the campaign literature by providing a counter-example to the prevailing theory of public opinion formation in the field. The paper also makes an empirical contribution by showing that traditional campaigning did little to influence the opinions of voters in the weeks leading up to the referendum.

In Chapter 3, I focus even more closely on the idea of social identity in the context of Brexit. I analyse the extent to which changes in media consumption affected how strongly an individual identified with their side of the referendum debate. Using a fixed effect model, I show that media consumption is significantly and substantively associated with the strength of a person's Brexit identity over time. More hours spent watching television, reading newspapers and browsing the Internet in a given period are all independently associated with the degree to which Remainers and Leavers exhibit in-group

bias towards others who voted the same way. The findings provide an important contribution to the literature on social identity formation by showing the extent to which media can exacerbate in-group bias in the context of a new and divisive political issue.

In Chapter 4, co-authored with Geoffrey Evans, we analyse whether partisan identification biased evaluations of the UK's performance on COVID-19 in June 2021, the midpoint of the pandemic. We make use of an experimental design to prime respondents to think about either a positive or a negative aspect of the crisis. We then assess their evaluations of the UK's performance, the degree to which the government was held responsible for this performance, and evaluations of the UK's *previous* performance one year ago. The findings provide evidence for bias in the case of overall evaluations, responsibility attribution and, of most interest, selective recall of past performance. The paper, therefore, contributes to a large literature on partisan motivated-reasoning by showing that the biased perceptions caused by partisanship apply even in cases of extreme non-economic crises. The novel findings concerning selective recall also mark an important contribution.

In Chapter 5, I concentrate on evaluations of COVID-19 performance, specifically focusing on how the UK's performance is perceived relative to other countries. The analysis incorporates both Brexit and partisan identity as social identities, using robust social psychology measures to examine how they influenced respondents' assessments of pandemic performance. The study reveals that both identities have distinct and measurable effects on these evaluations. Individuals exhibiting stronger government partisanship, as opposed to opposition partisanship, are more positive about the UK's performance compared to other countries. Those with a stronger Leave identification, rather than Remain identification, also tend to view the UK's relative performance more positively. Notably, these effects hold true even when both identities are analysed together, indicating that both partisanship and Brexit identity independently impact performance evaluations. Furthermore, both identities influence absolute ratings of the UK, but only Brexit identity significantly affects ratings of other countries. The finding that percep-

tual bias extends to an international context contributes to the literature on perceptual bias, which has primarily focused on biased perceptions within a national context.

Taken together, these chapters contribute to scholarship on contemporary British politics by exploring social identity as a means by which voters in the post-Brexit electorate came to form their political attitudes. The conclusions reached about the relationship between social identity and the attitudes and behaviours of voters will likely travel beyond the narrow empirical case study of post-Brexit Britain. In America, for example, voters across the political divide identify not only with their favoured political party but also with their ideology as a 'conservative' or 'liberal' more broadly (Malka and Lelkes 2010). The findings in this thesis suggest that, rather than replacing existing party identification, these ideological identities are more likely to complement partisanship and lead to compounded bias when evaluating outcomes, such as the US's economic prosperity.

Partisanship is in decline across most Western democracies (Dalton 2019), making non-economic 'second-dimension' issues increasingly salient (Kitschelt 1994; Kriesi 2008; Norris and Inglehart 2018; Stubager et al. 2021). One consequence of this development may be that emotionally driven divisions develop into new social identities. This thesis makes clear that these new identities will have the potential to restructure the political sphere, just as Brexit has left an enduring impact on the British electorate.

Chapter 2

Who Gets Persuaded? The Heterogeneity of Campaign Effects in the Brexit Referendum

Abstract

Political contexts affect the degree to which traditional campaign methods like door knocking and leafleting affect referendum outcomes. Campaign methods are most effective when issues are of low salience and political parties provide unified cues to their supporters. In this paper, I draw on Zaller's model of opinion formation to derive some theoretical expectations about the effectiveness of campaign contact in referendum campaigns when the issue is highly salient but the political parties provide mixed cues, arguing that prior voting intention, rather than partisan identity, should affect receptiveness to campaign messages. I test these expectations with regression analysis of British Election Study panel data, using propensity score matching to mitigate selection bias. My results suggest that the effects of campaigning during the Brexit referendum were mostly small, but differed significantly according to an individual's political attention and, contrary to the theoretical expectations, their partisan identity.

Introduction

It is now well established that political campaigns can persuade voters to change their minds about political questions and, sometimes, their actual choice at the ballot box. However, political contexts are an important moderator of campaign effectiveness. The success of referendum campaigners in particular is heavily dependent on the long-term saliency of the referendum issue, which affects how many voters have already made up their minds about the referendum question. Campaigners are also most likely to have success when political parties fail to provide unified cues to their supporters - clear party cues would offer voters an easy heuristic to rely on in place of actual engagement with campaigners' arguments (LeDuc 2002).

In this paper I estimate the effectiveness of traditional campaigning efforts in the lead-up to the 2016 'Brexit' referendum, in which the British electorate opted by a slim majority of 52% for the UK to leave the European Union. The case is particularly interesting for the study of campaign effects because the major political parties were internally divided over the question on EU membership, which existing theory suggests should have led to a volatile electorate susceptible to vote switching (LeDuc 2007). The referendum is also an important political event in its own right, resulting in a major turning point for Britain as the country terminated its four decade membership of the world's largest trading bloc (BBC News 2020a), and revealing stark and enduring social and political divisions in the electorate (Clarke, Goodwin, and Whiteley 2017; Curtice 2020; Evans and Tilley 2017; Goodwin and Milazzo 2017; Hobolt 2016; Hobolt, Leeper, and Tilley 2018, 2020).

Political contexts not only affect campaign effectiveness in general, but also which voters are most likely to be susceptible to campaign messages. In elections, for example, voters with strong partisan identities are more attached to their default party of choice (Huddy 2013) and are therefore less likely to be persuaded to change their voting in-

tention during the campaign (Butler and Stokes 1969; Campbell et al. 1960; Fieldhouse et al. 2021). The role of partisan identity in decreasing the likelihood of vote switching translates well to those referendums in which the political parties take clear and unified positions on the issue put to the public by the referendum (Butzer and Marquis 2002), where voters can follow their party's cues on which way to vote with little cognitive effort. However, much less is known about how campaign persuasion varies across an electorate when the main political parties appear to be internally divided over the question posed on the referendum ballot paper.

In this paper I therefore make a theoretical contribution to the literature on campaign effects by deriving expectations about campaign effectiveness in contexts where the major parties are internally divided and thereby provide mixed cues to voters. I build on existing theory, including Zaller's (1992) seminal Receive-Accept-Sample (RAS) model, to argue that individuals will, after arriving at an initial referendum voting intention, become biased against counter-arguments to that position. This universal human instinct, to be more critical of arguments that run counter to one's prior opinion, should result in referendum campaigners having more success at persuading voters who are already supportive of their campaign's message, and intending to vote in line with it, than at persuading voters who are planning to vote against the campaigner's side. This persuasion of current supporters can be considered a 'shoring up' of the vote, persuading sympathetic voters to not change sides, as opposed to persuading unsympathetic voters to change their voting intentions. My empirical contribution is the testing of this theoretical innovation, as well as more generally estimating whether traditional campaigning methods were effective at persuading voters overall.

To analyse campaign effectiveness in the Brexit referendum I use British Election Study panel data, collected in the months immediately before and after the Brexit referendum, modelled with propensity score matching to account for potential selection effects. I test whether traditional campaign efforts in the Brexit referendum affected individuals' voting decisions or their expectations about the likely impact of Brexit for the UK

across the three main issues of the campaign; the expected consequences of Brexit for the UK's economy, immigration numbers, and sovereignty. These expectations were the subject of much debate during the campaign, and were tied closely to vote choice in the referendum (Curtice 2017; Fisher and Renwick 2018). I include interaction terms in the models to test whether campaign effectiveness differed according to an individual's prior political opinion and attention.

My results suggest that traditional campaigning by either side had no, or at least very little impact on those who were contacted and, ultimately, on the outcome of the referendum. This is a notable finding given that the main political parties offered such mixed cues to their voters, which is expected to maximise campaign effects. It is also interesting empirically to note that, in a referendum campaign that generated much controversy around the subject of political advertising on social media, the traditional side of the campaign appears to have been fairly ineffective.

Despite the overall ineffectiveness of the campaign, my results do suggest that there was significant heterogeneity hidden within these minimal effects. The theoretical expectations derived in this paper were not borne out empirically - an individual's probability of being persuaded by a campaigner was not moderated by that individual's prior voting intention. However, contrary to expectation, partisan identity does appear to have moderated campaign effectiveness. Most notable is the fact that the Leave campaign had the most success at persuading those voters who did not hold a Conservative or Labour partisan identity. Contrary to Zaller's (1992) model of opinion formation, I also find that all campaigners had more success when contacting those with higher than average levels of political attention.

In the next subsection, I elaborate on my theoretical contribution to the literature on campaign effects, first summarising the broader pattern of scholarship and then expanding Zaller's theory of heterogeneous campaign effects to the context of the Brexit referendum. I then expand on my source of data, the British Election Study, and my

method for coding variables and using propensity score matching, before presenting my results. In the final subsection, I consider some limitations of my analysis and discuss the wider implications of my research for the literature on campaign effects. In particular, I argue that my findings suggest a need to further explain why partisans respond differently to campaign messages even when their parties offer mixed cues, and why voters do not appear to respond differently to campaign efforts on the basis of their prior voting intention.

Do Campaigns Matter?

Despite half a century of research on the topic, it remains contested whether traditional political campaign methods, such as door knocking and leafleting, can persuade voters to change their voting intention. In the United States, where much of this research has been conducted, ‘minimal effects’ constituted the prevailing paradigm for the latter half of the 20th century, with campaigns thought to persuade only a tiny minority of voters to support a different candidate in an election, and leaving the overwhelming majority of voters unpersuaded to do so (Hillygus 2010). This has been pushed further to the suggestion that campaigns have *no* persuasive effect on vote choice at all (Cardy 2005; Finkel 1993; Kalla and Broockman 2018), though campaigns are generally agreed to help mobilise individuals to turn out and vote (Green and Gerber 2004; Green, Gerber, and Nickerson 2003; Kramer 1970). However, other studies have found clear examples of campaigns directly persuading voters to alter their vote choice via traditional campaigning methods (Arceneaux 2007; Gerber, Green, and Green 2003), including in the case of relatively recent British elections (Johnston et al. 2013; Pattie and Johnston 2010; Pattie, Whitworth, and Johnston 2015), suggesting that the effectiveness of election campaigns depends upon contextual factors. Most current research therefore addresses specific questions about who is most persuaded by campaigns, and how contextual factors influence the effectiveness of campaigns (Farrell and Schmitt-Beck 2002; Fieldhouse, Fisher,

and Cutts 2019; Karp, Banducci, and Bowler 2007).

Referendum campaigns have received less attention than general election campaigns, in part because most countries hold referendums far less frequently than elections, limiting the availability of data (Farrell and Schmitt-Beck 2002). Nonetheless, understanding the role of campaigns in referendums is of substantial academic interest. Referendums are increasingly common occurrences across Europe (Bjørklund 2009; *The Economist* 2016), and can profoundly impact a country's future, as well as exposing divisions in the public that shape a country's politics for years after the ballots have been cast (Hobolt, Leeper, and Tilley 2020).

Existing research on referendum campaigns shows that the importance of parties and partisan attachments carries across from electoral contexts. Evidence from Switzerland, which holds three to four referendums per year, suggests that the outcome of a referendum and the individual decisions of partisan voters in that referendum depend heavily upon the alignment of parties over the referendum issue (Butzer and Marquis 2002; Sciarini and Tresch 2011). A broader investigation of EU referendums from 1972 to 2008 similarly highlighted the importance of partisan identity (Hobolt 2009). Likewise, there were clear partisan effects in the UK's 2011 alternative vote referendum (Vowles 2013), though internal division of the Labour party made it harder for Labour partisans to simply follow the party line (Curtice 2013). These findings tally with more general theories about elections which emphasise the important role played by parties in 'cueing' their supporters to take positions on political issues,¹ particularly when voters have given little thought to the specific issue beforehand (Butler and Stokes 1969; Campbell et al. 1960; Lau and Redlawsk 2001; Snyder and Ting 2002), though analysis of three UK referendums suggests that partisan effects are less strong in referendums than in general elections (Laycock 2013).

In summary, a higher level of late-stage vote switching during the referendum campaign

1. In some rare instances, cues from other actors can outweigh party cues in terms of influence, such as the role of the Church in the 2018 Irish referendum on abortion (Elkink et al. 2020).

is therefore expected in referendum contexts where voters receive weak or mixed partisan cues from political parties, and have given little prior thought to the issue at hand (LeDuc 2002, 2009; Vreese 2007). Existing literature therefore provides mixed expectations about the effectiveness of campaign contact in the lead up to the Brexit referendum. On the one hand, the two major parties were internally divided. The Conservative Party, which commanded a majority in the House of Commons, was openly split over the issue of Brexit and officially maintained a neutral stance on the issue. Leading figures, including Boris Johnson, vociferously advocated for Leave (Farrell and Goldsmith 2017), whilst then Prime Minister David Cameron publicly advocated for Remain, but attempted to avoid the campaign becoming tied to his personal popularity by stating that he would not resign if the UK voted for Brexit (Oliver 2016). Labour, the main opposition party, was objectively less divided than the Conservatives and officially campaigned for Remain, but left many voters confused about its position nonetheless. Jeremy Corbyn, then the party leader, belongs to a long-standing tradition of left-wing Euroscepticism, and stated live on television in the weeks before the referendum that he rated the EU only about 'a seven out of ten' (BBC News 2016). The majority of voters (and the majority of Labour supporters) were left unaware of whether most Labour MPs were in favour of Remain or Leave (Swales 2016). That the two major parties offered decidedly weak and mixed partisan cues to their supporters increases the probability of persuasion effects in the lead-up to the referendum:

H1: Campaign contact by a party or campaign makes voting for that campaigner's side in the referendum more likely.

Given the importance of issue positions and arguments for referendum vote choice (Colombo 2016; Hobolt 2005), I also expect that campaigners might have success at persuading voters to change their expectations about the impact of Brexit for the UK:

H2: Campaign contact by a party or campaign results in expectations of Brexit's likely outcome that more closely align with those of the campaigner.

On the other hand, the issue of EU membership was salient; it was selected by voters as the most important issue facing the country before the official campaign period had begun (Ipsos MORI 2016) and turnout on the day was relatively high at 72% (Uberoi 2016). This suggests that many voters had likely given considerable thought to the issue in advance of the final month and would have been less susceptible to persuasion during the main campaign period (Goodwin, Hix, and Pickup 2018; Hobolt 2007; LeDuc 2002, 2009). There is also evidence that local campaigning had very limited impact in the UK's 2011 referendum on an alternative voting system, despite the much lower saliency of that issue (Rallings, Thrasher, and Borisyuk 2013). Hypotheses **H₁** and **H₂** are therefore tested against the null hypothesis that campaign contact had no effect.

Heterogeneous Effects

Voters differ substantially with regards to both their political predispositions and the attention that they pay to politics. This observation forms the cornerstone of John Zaller's celebrated model of public opinion formation, which he terms the 'Receive-Accept-Sample' model (Zaller 1992). The model includes two crucial insights that are of particular relevance to the context of campaign effects during referendums, summarised as follows.

First, not all individuals are equally likely to 'receive' political communications. Those individuals who have more cognitive engagement with a political issue are more likely to understand messages about that issue. In the case of the Brexit referendum, for example, a Remain campaigner would have struggled to persuade a voter that tariff-free trade is of economic benefit to Britain if the voter did not understand the concept of tariffs. Voters with greater cognitive engagement are also more likely to receive the information in a more basic sense, for example by reading a political leaflet rather than simply discarding it immediately.

Second, individuals resist arguments that do not align with their pre-existing political

predispositions. This fact is well documented in political psychology, (Flynn, Nyhan, and Reifler 2017; Levendusky 2013; Lodge and Taber 2013), but also carries important implications when considering the type of voter who is most likely to be persuaded by political campaigners. Voters who are very cognitively engaged with an issue, and therefore most likely to 'receive' political messages, are also more likely to realise that the messages conflict with their existing predispositions and to accordingly reject them. Furthermore, an individual who has clear and defined political predispositions is likely to be more receptive to messages that broadly align with these than to messages that do not.

When Zaller's (1992) theory has been adapted, applied and tested in the case of referendum campaigns previously (Butzer and Marquis 2002), the focus has usually been placed on the role of partisan identity in moderating the effectiveness of campaigns. Research has noted that there should be greater campaign effects in referendums with divided parties because partisan cues will give voters less guidance (LeDuc 2007), but there has been no investigation of whether some voters will be more amenable to campaign persuasion than others. I argue that, in such contexts, campaigners will have much more success at affecting an individual's voting behaviour and political opinions when the voter is already inclined to agree with the campaigner on the referendum issue at stake. This inclination to agree can best be captured by pre-campaign voting intention. This follows from Zaller's (1996) original theory which discusses political predispositions rather than specifically partisan identity, though partisan identity often proves a useful operationalisation. In an emotive referendum debate over an issue that has long been salient, as is the case with Brexit, it seems reasonable to anticipate that voters will tend to accept arguments in favour of their prior opinion much more willingly than opposing arguments, a bias termed 'motivated reasoning' which is ubiquitous (Kunda 1990; Mercier and Sperber 2017). This bias should be more likely to operate at the level of a highly politicised campaign side than at the level of partisan identity, given that the parties themselves are providing mixed cues to their supporters, while popular campaign figures like Nigel Farage and Boris Johnson provided clear cues. In-

deed, evidence from Switzerland suggests that citizens' opinions on policy arguments align more closely with their intended referendum vote than with political party cues (Colombo and Kriesi 2017). I therefore build on previous scholarship by extending Zaller's theory to the context of salient referendums with divided parties, proposing that the political predispositions that matter in such cases should be best captured by prior voting intention in the referendum, rather than partisan identity.

I therefore expect that voters will respond differently to campaign efforts on the basis of their prior voting intention. I expect that those planning to vote for Remain were more receptive to Remain campaigners, and those planning to vote for Leave were more receptive to Leave campaigners. Note that if a Remain voter was contacted by the Remain campaign, I do not expect there to have been a 'persuasion' effect in the form of a change of vote. Rather, the persuasion can be thought of as a 'shoring up' of the prior voting intention, with the individual less likely to change to supporting Leave after being contacted by Remain:

H3: Campaign contact is more effective with regards to vote and expectations when aimed at those already planning to vote for the side of the campaigner.

An alternative hypothesis is that the more common cause of resistance to new information, partisan identity, causes different levels of receptivity to campaign contact, despite the mixed cues offered to voters by the main political parties:

H4: Campaign contact affects respondents differently according to their partisan identity.

I also test the impact of political attention, which Zaller also argues should moderate the effectiveness of messaging on public opinion (Zaller 1992). However, whilst Zaller

measures attention in the form of objective knowledge about the issue at hand, limited data leave me with a simpler self-reported measure of general political attention:

H5: Campaign contact affects respondents differently according to their political attention.

Data

The data analysed in this paper are from waves 7, 8 and 9 of the British Election Study Internet Panel (BESIP), administered by YouGov (Fieldhouse et al. 2022). Each wave contains over 30,000 respondents in total. 24,432 of those who responded in wave 8 had participated in wave 7, whilst 27,555 of those who took wave 9 had participated in wave 8. 7 and 8 were administered in consecutive months just prior to the referendum; wave 7 was fielded between 14 April 2016 and 4 May 2016, and wave 8 between 6 May 2016 and 7 June 2016. Wave 9 was conducted immediately after the referendum, between 24 June 2016 and 4 July 2016. I apply the post-stratification weights provided with the dataset to all models, which weight the sample to be more representative across a range of variables, including political attention.

Dependent Variables

The main dependent variable in my analysis is vote choice, derived from answers to the question: ‘Which way did you vote in the EU referendum?’, asked to all respondents who were surveyed immediately after the referendum.² I code this as a dummy variable, taking a value of one for individuals who voted for Remain, and zero for those who voted

2. The sample is only slightly skewed towards Remain when no post-stratification weights are applied (52% report having voted Remain), and when the wave 9 weights are applied the recall is 49% Remain against 51% Leave, only one percentage point closer than the actual referendum result.

for Leave.³

In addition to vote choice, I also consider whether the referendum affected opinions about Brexit. Referendums themselves can affect opinions concerning the issue to be voted on (Jung and Tavits 2021), and it stands to reason that campaigners may persuade people to change their opinions too, even if there is no persuasion to change their vote. Changes of political opinion that result from campaign contact are important for political science; opinion change can act as a mediator of campaign effects on voting intention, or can reveal that voters are receptive to campaigners even if their voting behaviour is not actually affected. This form of persuasion is of particular relevance in the case of Brexit because the divides in political opinion exposed by the referendum have endured long after the final voting slip was counted (Curtice 2020; Fieldhouse et al. 2021; Hobolt, Leeper, and Tilley 2018, 2020).

For Brexit, the political opinions that are of most relevance, and on which I focus, are expectations about the impact of Brexit upon the UK (Fisher and Renwick 2018). The campaigns focussed heavily on these expectations, with Remain mainly extolling the message that the economy would be damaged if Britain left the EU (Oliver 2016), whilst Leave campaigners focussed more on sovereignty and immigration (Farrell and Goldsmith 2017). I therefore coded an economic dimension that captures expectations about whether respondents anticipated that the general economic situation in the UK would improve if the country left the EU, whether unemployment levels would rise, and whether a respondent's personal economic circumstances would be affected positively or negatively (or not at all) by Britain's withdrawal from the EU. An immigration dimension captures whether a respondent expected immigration levels to increase, decrease, or stay the same in the event of Brexit. A sovereignty dimension measures the extent to which respondents believed that the EU has undermined British sovereignty, (the implication being that leaving the EU would increase sovereignty).

3. The 5037 respondents who did not recall voting or answered that they did not know which way they voted were coded as missing values.

For each scale, a higher number indicates a more pro-Remain position; that the economy would be worse in the case of Brexit, that immigration levels would be higher, and that sovereignty has not been undermined by the EU.⁴

Campaigning in the Lead-up to the Referendum

The main variable used for my analysis captures whether an individual was contacted by a traditional campaign method, such as leafleting or door knocking, during the Brexit referendum campaign. It is derived from respondents' answers to a series of questions about contact by parties and campaigns that were asked in the final month before the referendum (wave 8). Individuals were asked whether they had been contacted via traditional campaign methods during the past four weeks, and by which parties or campaign organisations. Importantly, those who were contacted by political parties but not by campaign organisations were also asked for which side the party urged them to vote, ensuring that respondents only had to correctly remember the message of the campaign contact, but not necessarily the source, in order to be coded correctly.⁵

I coded two dummy variables from these responses, capturing whether an individual had been contacted by the Remain side, and whether they had been contacted by the Leave side. In the case of Remain contact, for example, contact by any Remain campaign group or by any party to convince an individual to vote for Remain led to that individual being counted as contacted by Remain.

Overall there were relatively low rates of contact, with over 80% of the sample reporting no contact by either side. This stands in particular contrast to the general elections held before and after the referendum, as Table 2.1 shows, though is comparable to the 74% who reported not being canvassed before the 2015 Irish marriage referendum (Elkink

4. For the variables constructed from multiple questions I simply took the mean of a respondent's answer to each question, with an imputation to the mid-point in the case of 'don't know' responses if the respondent answered other than 'don't know' to at least one of the questions for that index.

5. Details of the exact questions can be found in the Appendix.

et al. 2016). It is possible that the low contact rates reflect in part the focus, particularly by the Leave campaign, on the digital element of campaigning (Farrell and Goldsmith 2017). Indeed, as Table 2.2 reveals, leafleting, door knocking and telephoning appear to have occurred less frequently in the referendum than in the election just prior to the referendum or the two that followed. Email contact, by contrast, was substantially higher. These numbers should be treated with caution, since the question asking about contact type actually relates to a general question about contact by parties rather than contact focussed on the referendum specifically, and therefore excludes contact by campaign organisations.⁶ However, these figures do offer some indication, at least, that traditional methods of contact were less of a priority for campaigners in the Brexit campaign than in general elections.

Table 2.1: Comparison of previous campaign contact rates

Campaign	% Reporting Contact
2015	49.9
Brexit	17.1
2017	47.6
2019	47.5

Table 2.2: Frequency of types of contact in previous campaigns

Campaign	Phone	Leaflet	At Home	On the Street	Email	Text / SM*	Other
2015	8	89	24	10	30	3	3
Brexit [†]	6	67	7	8	48	3	4
2017	10	91	28	9	33	17	3
2019	8	90	31	8	27	14	2

*In 2015 and Brexit the question referred to SMS texting, but this was changed to social media for 2017 and 2019 (hence the higher rates of reporting).

[†]The survey question about type of contact refers to a question about general contact, not contact specifically focussed on the referendum. The figures for type of Brexit contact should therefore be treated with caution, and may reflect methods used by campaigners for non-Brexit purposes like local elections.

6. It is for this reason that the main analysis does not focus on type of contact, but rather uses the simple but more reliable question of whether an individual received any contact at all.

Interaction Variables

The interaction term ‘prior referendum voting intention’ is coded as a categorical variable reflecting whether the respondent planned to vote for Remain or Leave, or was instead undecided or planning not to vote, in the pre-campaign survey wave. The interaction term for partisan identity is coded as a categorical variable reflecting whether a respondent felt closer to the Conservative Party, the Labour Party, a different party, or no party, also in the pre-campaign wave. These two variables overlap partially, as Table 2.3 shows, but there are still a substantial number of Remain-leaning Conservative identifiers and Leave-leaning Remain identifiers.

Table 2.3: Cross-tab of partisan identity and pre-campaign referendum voting intention

Party ID	Remain	Leave	No vote	Don't know
Conservative	35	59	0	6
Labour	60	33	1	6
Other or None	45	44	3	9

The variable capturing political attention is an 11-point score capturing answers to the question: ‘How much attention do you generally pay to politics?’.

For more detail on the exact coding process for all variables, see the Appendix.

Method

The use of a self-reported measure of campaign contact engenders concerns about biased reporting and self-selection. Previous research has used this type of data to study campaign effects in British election campaigns (Clarke et al. 2004; Fisher, Cutts, and Fieldhouse 2011; Pattie and Johnston 2010; Whiteley and Seyd 2003), and some concerns about bias can be partially mitigated by introducing appropriate control variables in a naive regression (Hobolt 2007), but the fact remains that some individuals are more likely than others to report campaign contact (Faas 2015). A more concerted effort to

obtain causal estimates, which I use in this paper, is to utilise propensity score matching, which attempts to mimic an experimental approach with observational data (Pattie, Whitworth, and Johnston 2015).⁷ A regression analysis is used to model the factors that make an individual more or less likely to be ‘treated’, in this case to be contacted by one of the campaigns. This model is then used to derive a ‘propensity score’ for each respondent, capturing how likely that individual is to have been treated (regardless of whether they did actually receive treatment). With full-matching, which I use for this analysis, weights are then constructed according to whether a treated individual has a very similar non-treated individual against whom they can be compared. Unlike nearest-neighbour matching, no individuals are discarded from the sample but are simply down-weighted if they cannot be appropriately matched.

Propensity score matching is an attempt to capture the essence of an experiment, with two groups as good as randomly assigned to treatment or not (since they were, in theory, nearly equally likely to have been contacted). The method is more suitable than a naive regression, which would run a particularly high risk of biasing coefficients because of issues with selection into treatment,⁸ but propensity score matching is not perfect and an experimental approach would still be preferable (Arceneaux, Gerber, and Green 2010; Sekhon 2009). There is some reason to think that, in the case of the Brexit referendum, campaign contact is less likely to be highly selective beyond the variables used for propensity score matching, given that the parties could not rely on past referendum vote to selectively contact those likely to support them and that there were serious concerns on both sides throughout about the failure to effectively coordinate ground campaigns (Shipman 2017). More pragmatically, however, no experiment was conducted during the referendum from which to draw data. Given the importance of the referendum as a rare case study for expanding understanding of referendum campaign effectiveness, I

7. An alternative would be a difference-in-difference design, but my use of multiple interaction terms in the main analysis would make it difficult to interpret the results of such a model, which includes additional interactions by design.

8. Interestingly, in the case of the analysis in this paper, matching does not seem to affect the results. As Tables A5.1 and A5.2 in the Appendix show, the main results are substantively the same when using matching as when simply using a regression that controls for all of the matching variables.

believe the use of propensity score matching to be adequate mitigation, but it remains a limitation that campaign contact was not truly randomised.

Matching the Sample

To generate my propensity scores I modelled campaign contact as a function of age, class, ethnicity, gender, educational attainment, vote in the 2015 UK general election, daily newspaper affiliation, political attention, partisan identity, intended referendum vote, and expectations about the impact of Brexit.⁹ Importantly, all of these variables were measured in the pre-campaign wave, ensuring that there are no potential issues of endogeneity arising from matching the sample on a model that includes them. I generated two separate propensity-matched samples; one matched on the propensity to be contacted by the Remain campaign, and one matched on the propensity to be contacted by the Leave campaign.

Figure 2.1 shows the absolute mean differences for each variable between those contacted and those not contacted.¹⁰

As can be seen, the absolute standardised mean differences between the 'treated' sample (those who were contacted) and the untreated sample (those who were not contacted) are within 0.1 for every variable.¹¹

A large amount of the variation in campaign contact pre-matching is explained by the variables included, though by far the largest difference relates to whether an individual was contacted by the other campaign side. This may indicate a clustering of campaign activities by both sides in certain regions, or simply the fact that some respondents are easier to reach than others, for example those whose house has a publicly listed phone

9. I use the three indicators outlined earlier, as well as an additional 'other' index which is constructed in the same way but from the remaining expectation items in the BESIP battery.

10. Table A2.1 in the Appendix shows the raw coefficients from the models used to derive the propensity scores.

11. Loveplots showing the Kolmogorov-Smirnov statistics can be found in the Appendix.

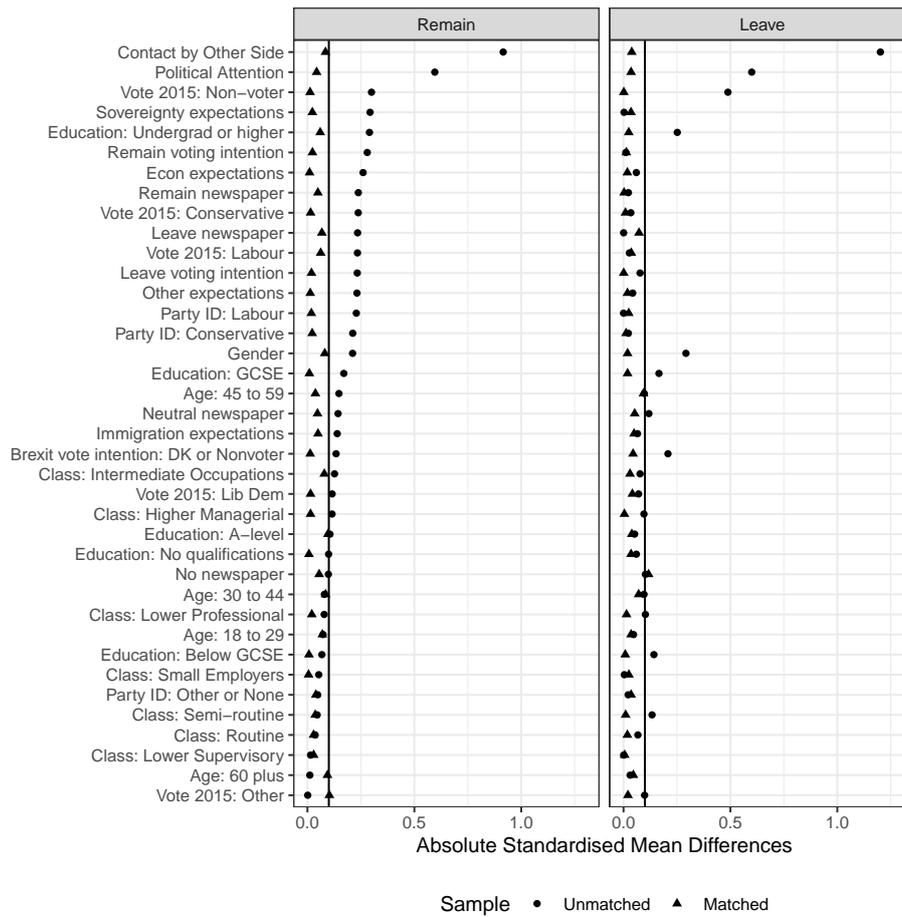


Figure 2.1: Loveplots of absolute standardised means before and after matching for Remain and Leave Contact, with all variables measured pre-campaign

number. Highly educated and more politically attentive individuals were also slightly more likely to have been contacted during the referendum campaign.

The propensity-matched samples are analysed with logistic regression (for referendum vote) and ordinary least squares regression (for expectations about Brexit), without the need to control for the variables used for matching. To analyse whether campaign effectiveness varied by partisan identity, pre-referendum voting intention, or political attention, I include an interaction term between each of these variables and campaign contact.

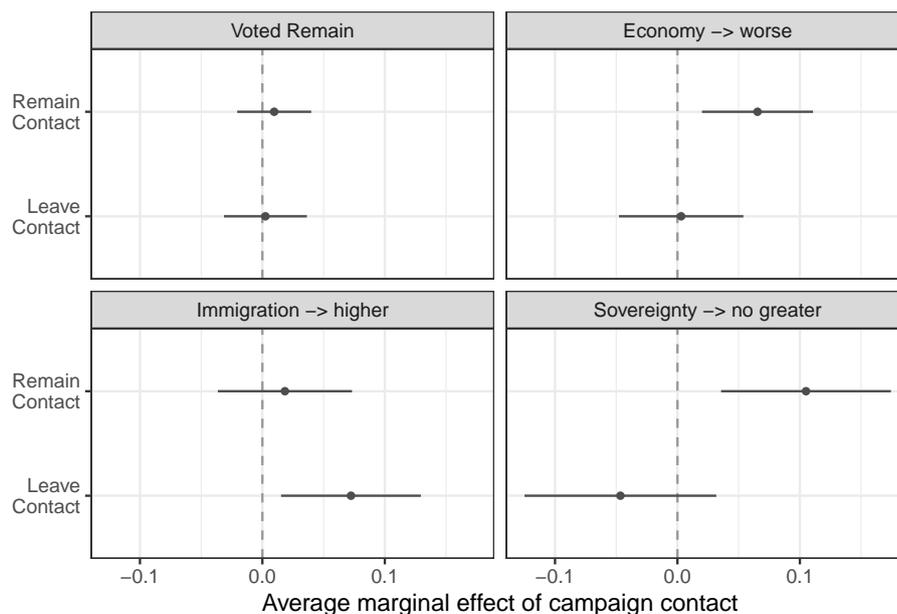


Figure 2.2: The effect of Leave and Remain contact on referendum vote and expectations about the likely impact of Brexit for Britain

Note: Dots indicate marginal effects, and whiskers show the 95% confidence interval. The average marginal effects for vote choice are calculated from logistic regression models, whilst the expectation models are from OLS regression. Full tables of these models can be found in the Appendix.

Results

Base Model Without Interactions

Figure 2.2 shows the marginal effect of contact by Remain and by Leave campaigners on referendum vote choice, and on expectations about Brexit’s impact on Britain’s economy, immigration, and sovereignty.

The first thing to note is that there is no significant effect of contact on voting intention. Both coefficients are relatively precise (confidence intervals of around 0.05 probability points) and almost exactly zero. The effectiveness of campaigning with regards to vote choice therefore accords with a null effects model of traditional campaigning.

However, there does appear to be a small effect of Remain contact on expectations about

the impact of Brexit; those contacted by Remain are significantly more likely to be pessimistic about the consequences of Brexit for the UK's economy and sovereignty, though there is no significant impact on expectations about immigration. Leave voters who were contacted are, contrary to expectations, significantly less likely to expect Brexit to reduce immigration levels, and not significantly affected with regards to expectations about the economy or sovereignty.

Looking at the results more broadly, Figure 2.2 shows a set of coefficients that are entirely consistent with a minimal effects paradigm of campaign contact. There is no support for Hypothesis H₁, though some limited support for Hypothesis H₂ in the case of contact by Remain campaigners. The effects of Remain contact are very small in magnitude; the largest point estimate is only a 0.1 movement towards the Remain position with regards to sovereignty; given that the dependent variable is a five point scale, this is a very small effect.

Differences by Prior Voting Intention and Partisan Identity

Having shown that there was no effect of contact on decisions at the ballot box, and only a small effect on expectations about Brexit, this subsection considers whether these apparent minimal results are in fact hiding heterogeneity in the effectiveness of campaign contact by an individual's pre-campaign political views.

Figure 2.3 shows the marginal effects of campaign contact on those who held a Remain voting intention and on those who held a Leave voting intention before the final month of the Brexit campaign.¹²

12. The full tables of the raw coefficients are shown in the Appendix. The choice to only report and interpret the marginal effects for Remain and Leave intentions, rather than those who were initially undecided, reflects the fact that the theoretical expectations derived in the first half of this article pertain to those who have already partially decided their position on the referendum issue. However, there is also the practical consideration that only 106 of the 1863 who did not express a Remain or Leave voting intention before the campaign reported being contacted in the final month of the campaign, leaving a very small sample size for analysing whether campaigners successfully targeted the undecideds (as indicated by the large standard errors on those coefficients).

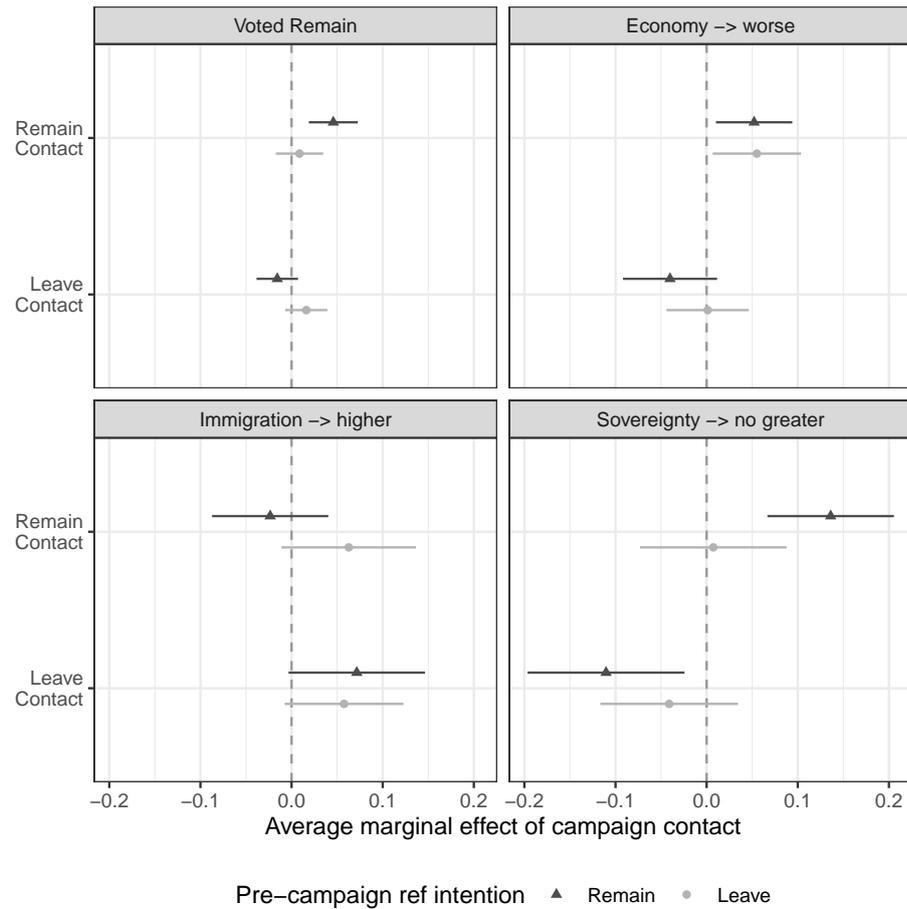


Figure 2.3: The moderating effect of prior referendum vote intention on Leave and Remain campaign contact effectiveness

Note: Dots indicate marginal effects, and whiskers show the 95% confidence interval. The average marginal effects for vote choice are calculated from logistic regression models, whilst the expectation models are from OLS regression. Full tables of these models can be found in the Appendix.

On the basis of these results, there is very little difference in the efficacy of campaigning efforts according to an individual's voting intention. Only two of the interaction terms were significant; those intending to vote for Remain were marginally more affected by Remain contact with regards to voting decision and expectations about sovereignty.¹³ The effect of Leave contact, by contrast, did not differ significantly according to an individual's voting intention for any of the dependent variables. Given the lack of a clear pattern across the dependent variables, and the very small magnitude of the two significant interaction effects, it is reasonable to conclude that there is almost no evidence to

13. See Tables A3.3 and A3.4 in the Appendix for the significance of the interaction terms.

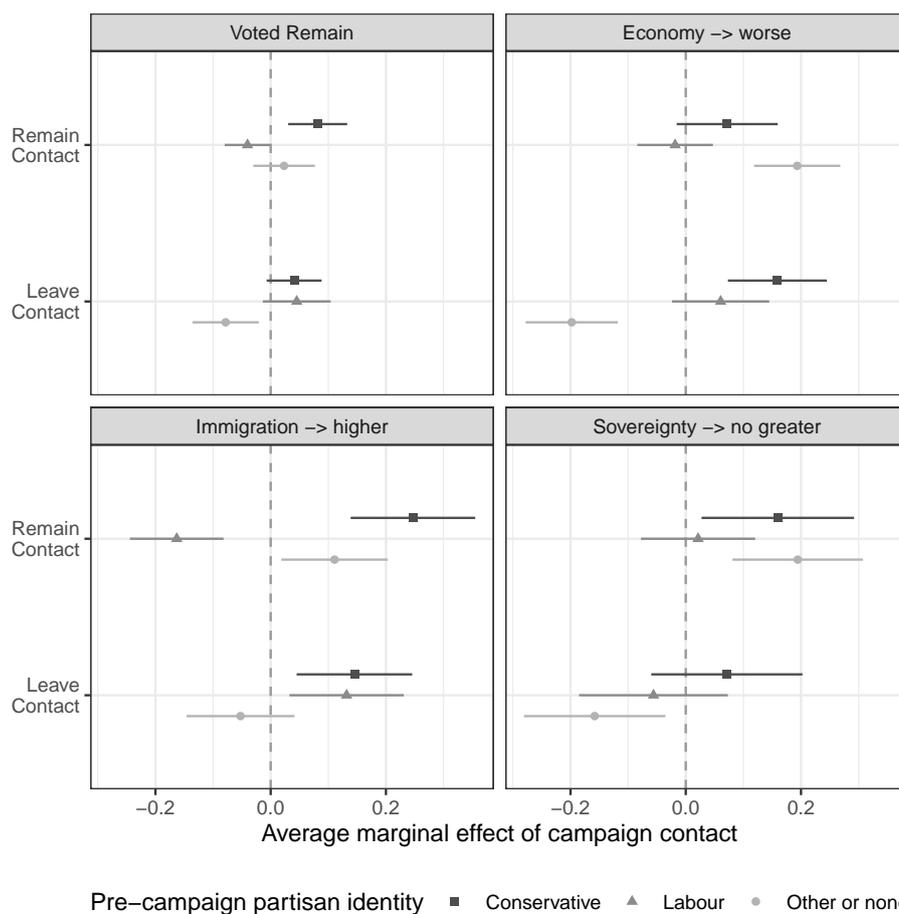


Figure 2.4: The moderating effect of partisan identity on Leave and Remain campaign contact effectiveness

Note: Dots indicate marginal effects, and whiskers show the 95% confidence interval. The average marginal effects for vote choice are calculated from logistic regression models, whilst the expectation models are from OLS regression. Full tables of these models can be found in the Appendix.

support Hypothesis H₃.

Partisan identity, by contrast, does seem to have a consistent impact on the effectiveness of campaign contact, as can be seen in Figure 2.4.

For all four dependent variables, Labour supporters were significantly less affected by Remain contact than those who did not identify with the two major parties. In the case of immigration, Labour partisans who were contacted by Remain seem to have actually ended up closer to the Leave position than Labour partisans who were not contacted at all. For Leave contact, both Labour and Conservative partisans seem to have been

particularly resistant to contact compared to the rest of the sample. Those who held no partisan identity or identified with a non-major party were significantly impacted by Leave contact in the expected direction for all four dependent variables, becoming more likely to vote for Leave and more positive about the potential impact of Brexit on the UK. Hypothesis H4 therefore seems well supported by the evidence; there is a significant difference in the effect of campaign contact according to an individual's partisan identity.

Differences by Political Attention

Figure 2.5 shows the predicted marginal impact of campaign contact according to whether an individual has low, medium, or high political attention.¹⁴

Both Remain and Leave campaign contact was, counter-intuitively, more effective at persuading voters with higher levels of political attention.¹⁵ This is significant in the case of immigration expectations for both Remain and Leave contact, and also in the case of vote choice and economic expectations in the case of Remain contact. The results therefore offer evidence in favour of Hypothesis H5 in the case of Remain contact, but less clearly in the case of Leave contact.

As before, it is worth noting that the magnitude of the results is still not particularly large, especially with regards to actual vote choice. Nonetheless, the results are relatively consistent, and, as I discuss in the next subsection, the fact that the panel sample I use displays high levels of political attention across the board is likely to be downwards biasing the estimate of political attention's moderating impact.

14. I have shown the results for the lower quartile, median value, and upper quartile, all of which are just one point apart on the ten-point scale of political attention. Although the effects would appear larger if I showed the marginal impact of being a ten compared to a zero, it would be misleading given that most of the sample are clustered around the top end of the scale, and a ten-point difference between two individuals is an extremely rare occurrence.

15. I also repeated this analysis with a categorical variable of political attention, to see whether there was a curvilinear relationship between political attention and the dependent variables. There was no such relationship (see Figure A4.1 and Tables A4.1 and A4.2 in the Appendix.)

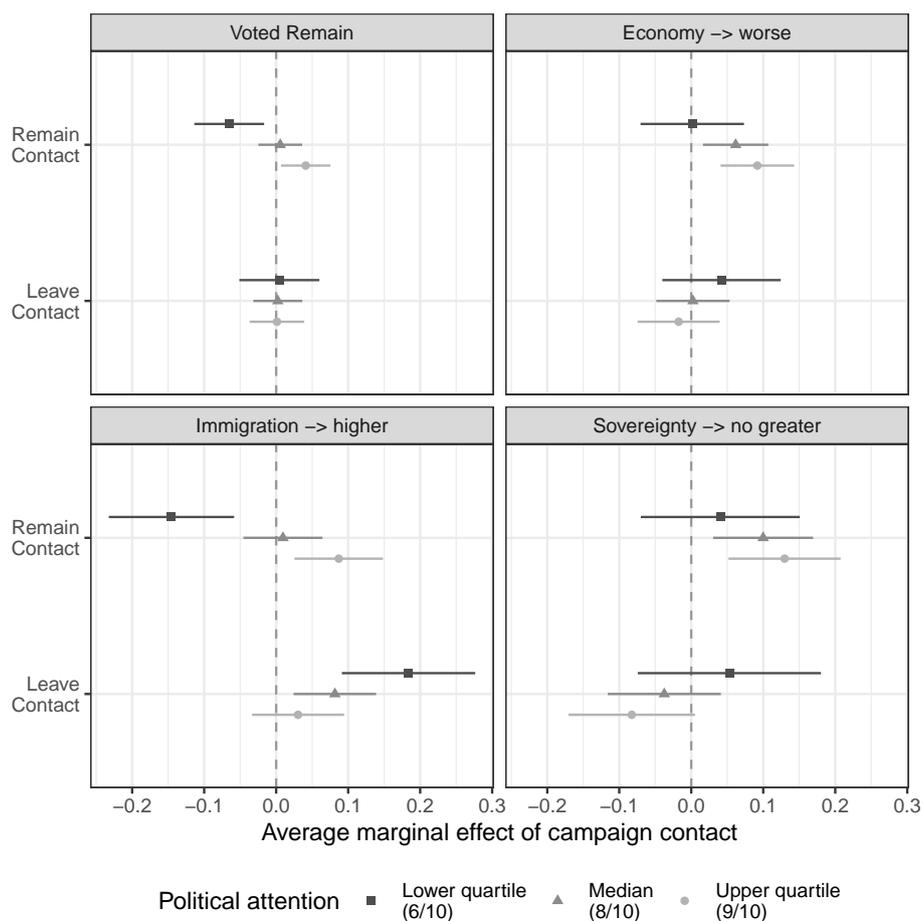


Figure 2.5: The moderating effect of political attention on Leave and Remain campaign contact effectiveness

Note: Dots indicate marginal effects, and whiskers show the 95% confidence interval. The average marginal effects for vote choice are calculated from logistic regression models, whilst the expectation models are from OLS regression. Full tables of these models can be found in the Appendix.

Discussion

In conclusion, my findings bolster the view that political campaigns, or at least specifically the traditional grass-roots aspects of these campaigns, do not have a particularly notable effect on referendum outcomes. The results suggest that the Brexit referendum is not, in fact, an exception to this general rule. Nor did grass-roots campaigners succeed at persuading many voters to change their expectations about Brexit's impact for the United Kingdom, despite the centrality of such expectations to the main campaign

and, ultimately, to voting decisions (Curtice 2017; Fisher and Renwick 2018). This is a particularly notable finding given that the major parties offered mixed cues to their supporters, a situation which is expected to produce a volatile electorate.

The very salience of the referendum highlighted at the beginning of this paper may be part of the explanation for this null empirical finding. The national newspapers covered the issue of EU membership extensively for a long period before the official referendum campaign period, meaning that many voters would have already been aware of and accounted for the main arguments advanced by campaigners when forming their opinions on Britain's membership of the EU. A simple model interacting newspaper readership with campaign contact lends some tentative support for this suggestion; individuals who read Remain-leaning newspapers were notably less susceptible to persuasion by Remain campaigners across all of the main dependent variables, suggesting that Remain-leaning newspapers affected their readers sufficiently to leave campaigners with little extra to add.¹⁶

An additional empirical finding highlighted by this paper is that traditional campaign contact was much less frequent in the Brexit campaign compared to other general elections in the same decade, with over 80% reporting no contact whilst the 2015, 2017 and 2019 election campaigns all resulted in only around 50% reporting no contact. The fact that these traditional methods were not particularly effective at persuading voters to change their votes or opinions offers some justification for the apparent lack of focus on traditional campaigning by campaign organisers, and supports the popular narrative that the Brexit campaign was characterised not by grass-roots momentum but by the use of advanced methods of contact, such as Facebook advertising. This is particularly relevant given that the campaigns seem to have been particularly unlikely to con-

16. The full results of these models, which can be found in the Appendix, are less clear for Leave newspapers - for vote at least, there seems to be no interaction effect between reading Leave newspapers and being contacted by Remain or Leave. Furthermore, there are some limitations to this analysis, not least the fact that newspaper readership is endogenous to a number of other relevant political variables. Because of this and other concerns, the results are not included in the main Results subsection of the paper, but are noted here because they offer some indication of a potential explanation for the null campaign effect.

tact those who were initially undecided; only 106 of the 1863 individuals in the sample who did not express a prior voting intention reported contact by either side.¹⁷ This stark finding adds further evidence to the general conclusion that the Brexit campaign was not a success story for traditional campaigning; few were contacted at all, few changed their minds, and those who would be expected most open to persuasion were unlikely to receive communication from either side via traditional channels.

The other major motivation for this paper was to test whether, in the case of divided parties, prior voting intention would have more of an impact on limiting the effect of campaign contact than partisan identity. On the contrary, this study makes clear the importance of partisan identity in this respect and the much lesser importance of voting intention. This is a surprising result because my theoretical expectations are a direct extension of the logic that underpins Zaller's (1992) model of opinion formation. The fact that partisan identity did moderate campaign effectiveness, whilst voting intention did not, implies an important avenue for further research; why is it that partisans respond differently to new campaign efforts even when their parties offer mixed messages? Perhaps more pointedly, why do voters not differ in their response to campaign contact according to their intended voting intention? Even in the absence of additional research, the findings in this paper imply that voting behaviour in referendums is better understood in light of partisan leanings than prior voting intentions, regardless of how the political parties choose to align on the referendum question.

The final finding of the paper, that higher levels of political attention appear to increase rather than decrease campaign effectiveness, is also a counter-intuitive finding. Without over-stressing the result, which was not particularly large in substantive terms, the fact that the politically attentive were most susceptible to campaigning stands in direct contrast to Zaller's (1992) model of opinion formation. One possible explanation for this is that the question of Britain's membership of the EU is sufficiently specialised

17. It is also worth noting that existing research has suggested that undecided voters were slightly more likely to vote for Remain after the campaign, suggesting that neither side was very successful at persuading this group (Fisher and Renwick 2018).

that only those with political attention were willing or able to grapple sufficiently with campaigner's messages to be persuaded. This would not be a total departure from Zaller's theory, since it would essentially imply that only politically attentive voters were able to 'receive' the message. But given how politically attentive the BESIP participants are (both by virtue of appearance in the sample and empirically according to the self-reported scale), it is notable to suggest that the barrier of sufficient attention to allow cognitive engagement with the referendum arguments should be placed at the very top of the attention scale. Further research into campaign effectiveness by political attention would be useful to corroborate, or oppose, these counter-intuitive results in other contexts.

Having noted the main conclusions of the paper, it is worth acknowledging the methodological limitations of the analysis. Most notable is that the measure of campaign contact used is self-reported. As discussed above, I made use of propensity score matching in an attempt to mitigate any possible selection effects resulting from possible non-random reporting of contact, but an experiment would still have been preferable since propensity score matching still relies on assuming that the matching model sufficiently captures potentially confounding variables. The fact that my results tally with the widely established minimal effects paradigm, rather than finding the large and significant effects that self-reported contact variables are feared to produce, helps bolster confidence in this assumption that the matching effectively controlled for self-selection bias - an inadequate model is more likely to have over-stated the effect of campaign contact rather than under-estimating it. An additional concern is raised by use of the British Election Study sample itself. This data has advantages - the panel structure in particular was important for allowing me to model contact as a function of pre-campaign variables, and to use voting intention and then recalled voting behaviour as measured both before and after the referendum itself. The main downside of such a panel is that it is likely to over-represent politically knowledgeable and attentive individuals, who may be affected by campaign contact differently to the average British voter. In particular, this means that the counter-intuitive findings about political attention should be accepted with utmost

caution. My use of the post-stratification weights provided with the dataset is not a perfect solution to this problem of sample composition, though it should at least help to reduce concerns about sample representativeness.

More research is needed to corroborate the findings of this paper, ideally with experimental approaches in future referendums given the limits of relying on self-reported data. At a substantive level, further work is also needed to establish exactly why partisan identity is more important than prior voting intention even in contexts where the parties are divided. Finally, additional research could investigate whether the theoretical puzzles raised in this paper apply equally well to the newer forms of contact that are increasingly prevalent in modern political campaigns.

Chapter 3

Polarising Press: The Effect of Media Consumption on Brexit Identity

Abstract

In the wake of the 2016 EU referendum, many British voters came to identify with their side of the Brexit debate, thinking of themselves as Remainers or Leavers. These new social identities rivalled partisan identity in prevalence and strength, continue to do so seven years after the referendum, and have been linked experimentally to affective polarisation. Little is understood about the formation of these identities, nor the mechanisms that led some individuals to form stronger attachments than others to their side of the Brexit divide. In this paper, I focus on one potential cause of Brexit identification; media consumption. By fitting models with individual-level fixed effects to British Election Study panel data, I find no evidence that identification over time is a product of media consumption, but the strength of that identification is reinforced by a person's level of media consumption. I find that, for both Remain and Leave identifiers, spending more time watching television, reading newspapers and browsing the Internet is associated with a stronger sense of social identification with their side of the Brexit debate. The findings have important implications for our understanding of the role that media can play in transforming political divides into social divisions, an important topic given how many democracies are currently exhibiting increasing political polarisation.

Introduction

On 23 June 2016 the United Kingdom (UK) held a referendum on whether the country should leave the European Union (EU). 52% of the electorate voted to leave, thereby substantially altering the course of British politics and of Britain itself (Bale 2023). One consequence, which provides the topic for this paper, is that a number of voters came to identify strongly with their side of the ‘Brexit’ debate, and to maintain this identification throughout the tumultuous period of British politics that soon ensued.

Many Britons had begun to self-identify as ‘Remainers’ or ‘Leavers’ during the build-up to the EU referendum. A large proportion of these individuals showed signs that this identification *as* a Remainer or Leaver complemented an identification *with* other people who had also voted for their preferred outcome. The Brexit debate had therefore resulted in new ‘social identities’; feelings of group belonging that extend beyond mere ideological agreement, and which produce in-group favouritism and out-group bias (Tajfel 1981). Experimental evidence suggests that Brexit identities result in affective polarisation; a respondent’s position on the referendum is associated with stereotyping, prejudice, and potentially discrimination (Hobolt, Leeper, and Tilley 2020).¹ Panel data suggests that Brexit identities also lead to cognitive bias when assessing economic conditions (Sorace and Hobolt 2020). Furthermore, Brexit identities, whether measured by the standard partisan identity question or by items developed in the social psychology literature, continued to rival partisan identity in both prevalence and in strength long after the referendum (Curtice 2018).

Existing research on Brexit identity has mostly focussed on the existence or the importance of this new social division, rather than considering the potential causes of this new social identity. More specifically, there has been little research asking *why* some individuals identified so strongly with their side of a debate. Social identity theory makes

1. For example, respondents were more willing to house a lodger in their home if the lodger’s position on Brexit matched their own.

clear that a minimal level of categorisation can produce social identification and associated consequences (Tajfel 1970) - even groupings that are allocated entirely randomly can result in in-group and out-group bias (Billig and Tajfel 1973). Yet group-level behaviours can hide individual variation. In the case of Brexit identity, not all Remain or Leave voters identified strongly at all times with people who voted the same way. Given the continued prevalence of Brexit identity, and the potential for new politicised social identities to emerge in the future, it is important to establish why some individuals developed a strong social identity around a single, albeit salient, political opinion.

In this paper, I focus on a single possible cause of Brexit identification; an individual's consumption of media. Existing research shows that media sources like newspapers can help transform political debates into divisive social identities (Lacombe 2019), and the media is well established as a driver of affective polarisation (Iyengar et al. 2019), which is itself a product of strong social identities. There is therefore good reason to anticipate that consumption of media sources like television and the Internet is associated with a strong sense of Brexit identification. Existing research has suggested that some personality types may simply be more prone to exhibiting affective polarisation (Tilley and Hobolt 2022), and has outlined the structural factors that helped shape the divisions that emerged during the Brexit debate (Sobolewska and Ford 2020b). But it is equally important to understand the contextual and environmental factors that can operate at an individual level to affect social identification, not least because these are likely to be the easiest factor for societies and individuals to alter. By focusing on the effect of media consumption, I offer a starting point for research on this important topic.

To test the effect of media consumption on the formation of Brexit identity, I use eight waves of the British Election Study Internet Panel, dating from April 2016, two months before the EU referendum, to June 2020, the start of the coronavirus pandemic. Each wave contains questions about Remain / Leave identification and the strength of this identification, measured with items used in the social psychology literature (Huddy 2001) that are designed to tap into feelings of identity beyond just political opinion.

The survey also contains questions that capture a respondent's consumption of media sources, including television, newspapers and the Internet. I employ a regression model with individual-level fixed effects to mitigate against self-selection into media consumption.

My results show that media consumption has no impact on the likelihood of an individual holding a Brexit identity at a given point in time, but it does significantly alter the strength of this identity. Both Remain and Leave identifiers exhibit stronger social identification in those time periods at which they pay more attention to media sources. The findings have important implications for our understanding not just of the formation of Brexit identity, but of the role media can play in creating social divisions more broadly.

Theory and Hypotheses

Social Identity Theory

Every person in society belongs to multiple social groups. Membership of some groups reflects personal characteristics, as is the case for gender and racial groupings. Sometimes, group identity is more of a choice, with identification as a group member being the condition for group membership. For example, a person who identifies as an Aston Villa supporter is an Aston Villa supporter precisely because they choose to identify as such. However, identification *as* a member of a group does not entail identification *with* that group (Green, Palmquist, and Schickler 2002). Only when a person both identifies *as* and also *with* a group does their group membership constitute a social identity. In such cases, the individual's self-esteem is partly tied to the successes or failures of the group to which they belong (Tajfel 1978a).² They will tend to exhibit in-group

2. Self categorization theory, which is related to but distinct from social identity theory, places slightly less emphasis on the motivation element of identification and more on the cognitive aspects (Turner

favouritism and out-group discrimination (Tajfel 1970). This bias will even occur in situations where it would be more profitable for them to not prioritise ‘winning’ against the out-group (Tajfel 1978a).

Perhaps the most obvious illustration of Tajfel’s impact on political science is the large number of scholars who now understand partisan identity as an example of a politicised social identity (Huddy and Bankert 2017). The thought that partisan identity is a social identity, rather than a simple running tally of party performance (M. Fiorina 1981), helps to explain the rise in affective polarisation, whereby partisans on both sides of the divide view each other with increasing hostility (Iyengar, Sood, and Lelkes 2012). Partisan identity also provides a good example of the point that politicised social identities do not merely reflect differences of opinion about politics. It is perfectly consistent with social identity theory for an individual to identify as a Democrat but then vote for the Republican party at an election because they believe that party to be better equipped for steering the country at that precise time (Green, Palmquist, and Schickler 2002). Social identity is about the linking of a person’s self-esteem to the success of their group, and of bias towards in-group and out-group members, not about holding the opinions most associated with their in-group. Another example of politicised social identities transcending their ideological origins is provided by the substantial minority of American self-identified conservatives who hold quite liberal economic and social policy stances (Ellis and Stimson 2012). Indeed, ideological identities can sometimes lead people to adopt positions that conflict with their actual principles (Malka and Lelkes 2010).

Partisan and ideology identities are not the only social identities of importance in the political sphere. In the UK, a new identity has arisen as a result of the 2016 referendum on whether the country should have remained a member of the European Union. In the immediate aftermath of the event, in which the country elected by a slim margin to leave the bloc (52% to 48%), voters began to identify themselves as ‘Leavers’ and ‘Remainers’ (Hobolt and Tilley 2019). Long after the actual referendum, survey questions designed to

1987). The implications of either theoretical framework are, in the case of this paper, essentially the same.

mirror the standard partisan identity measures continue to find more Brexit identifiers than partisans.³

Not only do self-identified Remainers and Leavers exhibit identification ‘as’, but they also show signs of identification ‘with’ their side of the Brexit debate. Measures from the social psychology literature designed to capture social identification, including such items as ‘When people criticise the *Remain / Leave* side it feels like a personal insult’, reveal higher levels of social identification with Brexit sides than equivalent measures of partisan identity (Evans and Schaffner 2019).

These new Brexit identities are associated with both in-group and out-group bias. Conjoint experiments show that Leave and Remain identifiers are uncomfortable about appointing an out-group member to a position of power in the UK’s publicly-funded media outlet, the BBC (Hobolt, Leeper, and Tilley 2020). Descriptively, Remain and Leave identifiers are uncomfortable with their child marrying someone from the other side of the Brexit debate, unwilling to talk politics with the other side, and likely to view people from the other side more negatively across a range of traits like intelligence, honesty and open-mindedness (Hobolt, Leeper, and Tilley 2020). The average Remain voter is uncomfortable with a Leave voter moving next door, whilst the average Leave voter views this as a positive characteristic in a new neighbour (Sobolewska and Ford 2020b). Leave and Remain voters also view the economy differently since the referendum, a finding consistent with the idea that their Brexit identities result in cognitive dissonance (Sorace and Hobolt 2020).

3. The identity question asked by YouGov take the form: ‘Since the EU referendum last year, some people now think of themselves as Leavers and Remainers, do you think of yourself as a Leaver, a Remainder, or neither a Leaver or Remainder?’ The partisan identity equivalent is: ‘Generally speaking, do you think of yourself as Labour, Conservative, Liberal Democrat or what?’

Causes of Brexit Identities

Social identities are remarkably quick to form. So-called ‘minimal intergroup experiments’ have demonstrated that the mere act of social division often suffices to result in the formation of social identity and the associated in-group and out-group bias. In one experiment, Tajfel (1970) divided schoolboys into groups on the basis of whether they under-counted or over-counted a number of dots during an arbitrary dot-counting exercise. In the follow-up task, which involved allocating real money, the boys exhibited clear in-group out-group bias when choosing how to distribute the money. Indeed, they even restricted resources to the out-group when the action carried no benefit to themselves or to their in-group. A few years later, Billig and Tajfel (1973) built on the experiment by allocating the schoolboys to groups completely randomly; there was not even the arbitrary dot-counting pretence for division. Again, the participants exhibited in-group bias.

Despite the minimal conditions required to generate social identification at the aggregate level, not *all* individuals form an equally strong identity when assigned to a group. This raises the question of why some individuals come to identify strongly with a particular grouping whilst others do not. This is particularly important in the case of Brexit identity. As the review above makes clear, Remain and Leave identification are an important force in British politics, resulting in both affective polarisation and cognitive dissonance among voters. The liberal-authoritarian dimension is increasingly important in contemporary politics (Kitschelt 1994; Kriesi 2008; Norris and Inglehart 2018; Stubager et al. 2021), creating divisions along new issues like, for example, the appropriate government response to the coronavirus pandemic (Mellon, Bailey, and Prosser 2021). It therefore seems reasonable to expect polarisation along new political identities to emerge. Understanding why some individuals were particularly prone to incorporating their Brexit position into their self-esteem may help with explaining the rise of new social identities in the future.

The existing literature on the question of why some individuals came to form an attachment to their side of the Brexit divide is surprisingly limited. Hobolt, Leeper, and Tilley (2020) include a table in the Appendix to their paper showing some socio-demographic predictors of Brexit identity. They find, for example, that younger individuals are more likely to hold a Remain identity than a Leave identity. However, they explicitly state that the purpose of their article is not to delineate the causes of the identities themselves, and they therefore extend the analysis no further. They also only measure identification 'as,' but do not make use of any measures tapping identification 'with'.

More recent research using survey data has suggested that part of the explanation for Brexit identification may lie in underlying personality traits. Narcissists are particularly susceptible to identification with their side of the debate and, as a result, are more likely to become affectively polarised (Tilley and Hobolt 2022). More broadly, it is also the case that the divisions exposed by the referendum along cultural, regional and ideological lines, long pre-dated the moment in June 2016 when the country went to the ballot box (Sobolewska and Ford 2020b). Accordingly, part of the explanation for which individuals developed strong Brexit identities may also lie in their pre-existing social and ideological characteristics.

The Effect of Media Consumption on Social Identity

One possible cause of Brexit identification that has not previously been examined is the consumption of media sources. It is remarkably easy to increase the saliency of group membership simply by encouraging individuals to think in terms of that identity (Haslam et al. 1999), or to heighten the salience of an identity by exposing individuals to rhetoric that is perceived as threatening to their social group (Pérez 2015). Media can serve to perform both of these functions, and empirical research has shown that this can result in the formation and strengthening of social identities.

For example, the increasing polarisation in American politics, which is itself a by-product

of strong social identification, has been linked to a broadly negative and polarising media (Iyengar et al. 2019). American media increasingly exposes its audience to extremely negative coverage of out-partisans (Berry and Sobieraj 2014), for example by paying disproportionate attention to out-party scandals (Puglisi and Snyder 2011), and there is both cross-sectional and experimental evidence that this partisan media is a partial cause of the polarisation that characterises US politics today (Levundusky 2009; Stroud 2010). Beyond the case of partisanship, there is strong qualitative evidence in the US of the role that media can play in forging social identities. The National Rifle Association's success at cultivating a gun-owner identity stemmed in part, it seems, from a sustained discourse in the association's official journal (Lacombe 2019).

There is good reason to expect the same mechanisms to apply in the context of the EU referendum divide. The British press has a long history of Eurosceptic reporting (Hobolt and Tilley 2014; Norris 2003), and the effect of this on voter attitudes to Europe has been shown quasi-experimentally (Foos and Bischof 2021). There is also evidence from time series data that newspaper coverage resulted in increased popular support the UK Independence Party, a highly Eurosceptic party that is partly responsible for the holding of the referendum in the first place (Murphy and Devine 2018). This long-standing tradition of polarised coverage of the EU in British media continued during the lead-up to the referendum, with media outlets on both sides of the campaign pushing fear-led narratives about the economy (in the case of Remain) and immigration (in the case of Leave) (Dan Jackson 2016). The intense antagonistic reporting on Brexit also continued throughout the four-year period of negotiation that followed the vote for Brexit (Gavin 2018; Ridge-Newman, León-Solís, and O'Donnell 2018), often with a strong 'us vs. them' narrative (for example, see: Phipps 2016). Social media too followed this format of polarisation on the subject of Brexit (Brändle, Galpin, and Trenz 2021).

Given the wider literature on the role of the media in producing social identification and the associated consequence of affective polarisation, as well as specific context of the British media's history of Eurosceptic reporting and the polarised coverage of the Brexit

debate, it seems reasonable to expect that higher consumption of media over time is associated with a heightened sense of social identification with a person's side of the Brexit divide.

Data

The data used for this paper are from the British Election Study Internet Panel (BESIP), administered by YouGov (Fieldhouse et al. 2022). For the analysis, I make use of the eight BESIP waves that include questions about Brexit identity and also about consumption of media sources. These are waves 7, 8, 12, 13, 15, 17, 19 and 20. Together, these survey waves cover a period of four years. Wave 7 was fielded between 14 April and 4 May 2016, and wave 20 between 3 and 21 June 2020. Full detail on the fieldwork dates and sample size for the other waves can be found in the British Election Study Internet Panel codebook.⁴

Dependent Variable

The main dependent variables for my analysis are a respondent's Brexit identity, and the strength of this identity. For the direction of identity, respondents were asked: 'In the EU referendum debate, do you think of yourself as closer to either the 'Leave' or 'Remain' side? If yes, which one?', echoing the British Election Study (BES) partisan identity question.⁵

Those who did identify as a Leave or a Remain identifier were then asked about the strength of this identity. The measure I make use of in this paper echoes the measurement strategy commonly used for social identities in social psychology (Huddy 2013). The question asks respondents to answer on a four-item scale of 'strongly disagree' to

4. Available for download at <https://www.britishelectionstudy.com>.

5. The long-standing formulation of which is: 'Generally speaking, do you think of yourself as closer to Labour, Conservative, Liberal Democrat or what?'

'strongly agree' their assent to the following statements, when thinking about the side of the referendum debate that they felt closer to (*Brexit ID*):

1. When I speak about the *Brexit ID* side, I usually say "we" instead of "they".
2. I am interested in what other people think about the *Brexit ID* side.
3. When people criticize the *Brexit ID* side, it feels like a personal insult.
4. I have a lot in common with other supporters of the *Brexit ID* side.
5. If the *Brexit ID* side does badly in opinion polls, my day is ruined.⁶
6. When I meet someone who supports the *Brexit ID* side, I feel connected with this person.
7. When people praise the *Brexit ID* side, it makes me feel good.

The Cronbach's Alpha score for these items is 0.89 for Remain identifiers and 0.87 for Leave identifiers, measured in March 2019. The figures are similar across all of the waves used in the analysis (see Table A3.1 in the Appendix).

The BESIP survey also includes two other variables that could be used to capture Brexit identity strength. A reported identity strength echoes the standard partisan identity measurement in the BES,⁷ reflecting answers to the question: 'Would you call yourself very strong, fairly strong, or not very strong *Brexit ID*?', where *Brexit ID* is replaced by either 'Remainer' or 'Leaver' as appropriate. A social affinity measures captures responses to the question: 'How much do you have in common with the people who want to *Leave / Remain in the EU* (apart from what they think about Europe)?', with answers given on an eleven-point scale from 'Nothing in common' to 'A great deal in common'.

6. Only asked in waves 7 and 8.

7. The partisan identity equivalent is: 'Would you call yourself very strong, fairly strong, or not very strong *Partisan ID*?'

Table 3.1 shows the correlation between these alternative measures of strength and the socio psychology measure that I use in my main analysis. The numbers show that both of these alternative measures are relatively well correlated with the social psychology measure of identity strength, raising confidence in the measurement validity of my main dependent variable.

Table 3.1: Correlation between different measures of identity strength for Remain and Leave identifiers, with variables measured in March 2019

	Huddy measure	Social affinity measure:	
		Remain	Leave
For Remain identifiers:			
Reported identity strength	0.41	0.34	-0.29
Huddy measure		0.47	-0.22
Remain social affinity			-0.31
For Leave identifiers:			
Reported identity strength	0.36	-0.24	0.35
Huddy measure		-0.13	0.47
Remain social affinity			-0.22

It is also reassuring to see that both of my main dependent variables, identification and strength of identification, are fairly stable over time in the aggregate. The proportion identifying with either side, and the average strength of these identities, saw only a slight up-tick around the time of the actual referendum and then remained fairly stable over a period of months after that, as Figure 3.1 shows. This makes clear that these identities are not just artifices of an intense referendum campaign, but are instead enduring and important political identities that remain a relevant phenomenon in British politics, though it is worth noting that some data extending beyond 2020 does suggest there has been a slight decrease in the strength of these identities in recent years (Sivathanan 2021).

The fact that there are far more respondents expressing no partisan identity than there are expressing no Brexit identity also remains unchanged over the entire five-year period for which there are data. It's also worth noting that this consistency of identity

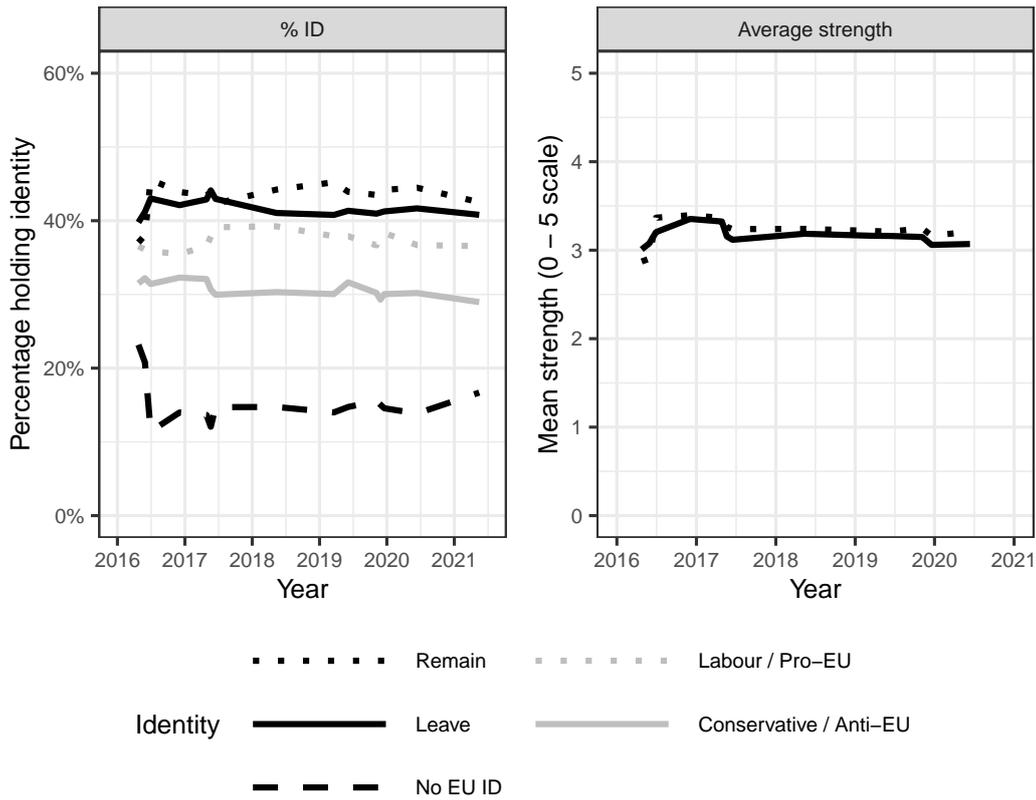


Figure 3.1: Change in prevalence and strength of Brexit identities since the referendum

strength is a product of consistency across *all* of the social psychology items used to create the average measure of strength (see Figure A3.1 in the Appendix).

Media Consumption

The main independent variable of interest is a respondent’s consumption of media sources. To capture this consumption I make use of a question included from the BESIP that asked respondents: ‘During the last seven days, on average how much time (if any) have you spent per day following news?’, where the ‘following news’ sources were television, newspaper (including online), radio and the Internet (excluding newspapers).⁸ Possible answers were ‘No time at all’, ‘Less than 1/2 hour’, ‘1/2 hour to 1 hour’, ‘1 to 2 hours’,

8. The survey question also includes ‘talking to other people’, but I exclude this from my analysis since this is not a media source in the traditional sense.

'More than 2 hours', or 'Don't know'.⁹

For the first part of the analysis I take the mean of these different answers, with 'Don't know' imputed to the mid-point for respondents who provided meaningful responses to at least one of the information sources questions. By using an aggregate measure I therefore capture the effect of media consumption in total, without needing to distinguish between different sources. The fact that respondents can be included even if they did not answer all of the information sources questions also helps to boost the sample size.

In a second part of the analysis I unpack this aggregate result by including consumption of each news source as an independent variable. This allows for the effect of each source, controlling for the other sources, to be estimated. Note that, for simplicity, I treat the variable as continuous in both parts of the analysis despite the fact that the underlying survey responses in terms of time use may not actually be continuous. For example, a movement from 1 to 2 on the scale indicates that the respondent has changed from spending no time consuming a particular source of media to spending less than 30 minutes doing so, whilst a movement from 3 to 4 on the same scale reflects a change from spending between one and two hours consuming that media to spending an unbounded 'more than two hours'. I treat the variable as continuous primarily for expediency; there is really no more sensible way to aggregate the different scales together to gain an average effect of 'media exposure'. For the sake of robustness, however, I replicate the analysis of media consumption (broken down by source) with the original categories included rather than a continuous version of the scale. The results, shown in Table A3.7 in the Appendix show that the substantive effects are the same as when making the simplification, and the coefficients remain significant across the different time-use levels.

9. Average media consumption, both on average and for the separate sources, was relatively consistent over the time period analysed in this paper, as Figure A3.2 in the Appendix shows.

Control Variables

As I discuss in the next section, I use a model with individual-level fixed effects which removes the need to control for time-invariant characteristics. Demographics, like age and gender, are constant within respondents by definition, whilst other variables like fundamental values are also expected to be constant over time (excluding measurement error). Two variables that may change, and which are likely to correlate both with Brexit identity and with media consumption, are a respondent's level of Euroscepticism and their partisan identity.

To control for Euroscepticism I make use of answers to the question: 'Some people feel that Britain should do all it can to unite fully with the European Union. Other people feel that Britain should do all it can to protect its independence from the European Union. Where would you place yourself on this scale?'. Answers are again given on an eleven point scale, ranging from 'Unite fully with the European Union', to 'Protect our independence'.

To control for partisan identity I include in all models a variable capturing responses to the standard partisan identity question used in Britain: 'Generally speaking, do you think of yourself as Labour, Conservative, Liberal Democrat or what?' From these answers I create a variable with four categories representing whether an individual identified with an Eurosceptic party (the Conservatives, UKIP, or the Brexit party in later waves), with the Labour party, with a pro-EU party (Liberal Democrats, the Greens, etc.), or with no party at all (the reference category).¹⁰

As a robustness check I also replicate the models without including these variables, and separately with the inclusion of lagged versions of these variables. The effect of these changes on the main coefficients of interest is , and the full results can be found in A3.2 in the Appendix.

10. The Labour party is left as a separate category because its explicit position on Brexit was left more ambiguous throughout the negotiation period than the other parties.

Method: Individual Fixed Effects

The main challenge to researching media effects is that people are likely to self-select into media consumption. For example, it may be that those who are strongly identified with their side of their Brexit divide simply watch more news than less strongly identified individuals, rather than the fact that they watch more news actually resulting in their identification.

The method used in this paper for overcoming this problem of causal inference is regression with individual-level fixed effects. This model controls for all unobservable time-invariant individual characteristics, like gender and birth cohort. The dependent variable essentially becomes a person's Brexit identification *relative to their average identification across all time periods*. The main independent variable is likewise their media consumption *relative to their average media consumption across all time periods*. Accordingly, the coefficients on the media variables basically reflect whether consuming more media than usual results in more Brexit identification than usual.

In addition, I also include time fixed effects. These ensure that the models do not simply capture the fact that some time periods are associated with both more media consumption and more Brexit identification across the whole sample. All analysis is also conducted with the post-stratification weights, which are provided with the BESIP dataset, to weight the sample to be more representative across a range of demographic and political variables.

Results

Table 3.2 shows the results from the fixed effect analysis of the post-referendum waves, with the models including the aggregated measure of media consumption. The first two

columns display the results of models where the dependent variable is simply identification rather than the strength of this identification. The sample is restricted to those who voted for that side in the referendum. For example, Column 1 shows the effect of media consumption and the control variables on a Remain voter’s likelihood of identifying as a Remainer. Columns 3 and 4 show the results from having identity strength as the dependent variable, where the sample is restricted to those who actually hold that identity. For example, Column 3 shows the marginal effect of media consumption and the other control variables on a Remain identifier’s strength of Remain identity.

Table 3.2: The effect of average media exposure on Remain and Leave identities and strengths - results from models with time and individual-level fixed effects

	<i>Dependent variable:</i>			
	Remain identity	Leave identity	Remain strength	Leave strength
Media:				
Average media exposure	−0.01*** (0.00)	0.00 (0.00)	0.06*** (0.00)	0.06*** (0.00)
Control variables:				
EU Integration scale	−0.03*** (0.00)	0.04*** (0.00)	−0.03*** (0.00)	0.03*** (0.00)
PID: Anti EU (including Con)	0.02* (0.01)	0.03*** (0.00)	−0.02 (0.02)	0.02 (0.01)
PID: Labour	0.04*** (0.01)	0.01 (0.01)	0.04*** (0.01)	−0.03* (0.01)
PID: Pro-EU	0.01* (0.01)	0.02* (0.01)	0.05*** (0.01)	0.01 (0.02)
PID: None (reference category)				
R ²	0.04	0.07	0.05	0.03
Num. obs.	69939	68032	70751	65908

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: Individual and time fixed effects are included but not shown. The Remain identity model includes only those individuals who voted for Remain in the referendum, whilst the Leave identity model includes only respondents who voted for Leave. The strength models only include individuals who hold that identity. The reference category for party ID is 'No party ID'. Nine survey waves are used for the analysis, together spanning the period from April 2016 to June 2020.

The results suggest no positive association between media consumption and identification as a Remainer or a Leaver. For Leave voters, there is a precisely estimated null effect of media consumption with regards to their likelihood of also identifying with their side of the Brexit divide. For Remain voters, there is actually a slightly negative effect of media consumption, but this is very close to zero and is also precisely estimated.

It is therefore clear that Brexit identities themselves are not, according to this analysis, the result of paying more attention to news sources.

The control variables, though not the main subject of this paper, do provide some explanatory power with regards to identification. Unsurprisingly, the degree of Euroscepticism that an individual feels is associated with their likelihood of holding a Brexit identity. Remain voters who were more pro-Europe were also more likely to feel their Brexit position as a social identity, as were more Eurosceptic Leave voters. Perhaps more surprisingly, it appears that partisan identity is associated with Brexit identity regardless of which party the individual identifies with. For example, Remain voters were more likely to hold a Remain identity in periods in which they also exhibited a Conservative or an anti-EU partisan identity. They were also more likely to identify with Remain if they held a Labour or other pro-EU partisan identity, but this is less surprising. Likewise, Leave voters were more likely to identify with Leave if they were identified with the Conservatives or an anti-EU party, or if they identified with a pro-EU party. It therefore seems that the likelihood of holding a political identity increases when an individual holds a separate political identity, regardless of the alignment of these two different identities.

Turning now to the effect on identity strength, there is a clear and significant relationship between consumption of media sources and holding a strong Brexit identity rather than a weak one. The coefficient is the same for both Remain and Leave identifiers (0.06). In line with the expectations laid out above, there appears to be a direct link between media consumption and Brexit identification strength.

Once again, the control variables also provide additional explanatory power. As in the case of explaining identification, the effect of Euroscepticism is intuitive; more pro-Europe Remain voters feel a stronger sense of Remain identity whilst more Eurosceptic Leavers feel a stronger sense of Leave identity. There is also an association among Remain voters between possessing a Labour or pro-EU partisan identity and holding a strong Remain identity. For Leaver identifiers, there is just a negative association be-

tween possessing a Labour identity and the strength of their Leave identity.

Table 3.3 shows the result of splitting the media consumption measure out into its constituent four variables. As in Table 3.2, the first two columns have identification as the dependent variable whilst the last two have strength of identity as the dependent variable.

Table 3.3: The effect of television, newspaper, radio, and internet exposure on Remain and Leave identities and strengths - results from models with time and individual-level fixed effects

	<i>Dependent variable:</i>			
	Remain identity	Leave identity	Remain strength	Leave strength
<i>Media sources:</i>				
Television	-0.00 (0.00)	0.00 (0.00)	0.02*** (0.00)	0.01*** (0.00)
Newspaper	-0.00 (0.00)	0.00 (0.00)	0.01*** (0.00)	0.02*** (0.00)
Radio	-0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.01** (0.00)
Internet	-0.00* (0.00)	0.00 (0.00)	0.02*** (0.00)	0.02*** (0.00)
<i>Control variables:</i>				
EU Integration scale	-0.03*** (0.00)	0.04*** (0.00)	-0.03*** (0.00)	0.03*** (0.00)
PID: Conservative / Anti-EU	0.01 (0.01)	0.03*** (0.00)	-0.01 (0.02)	0.02 (0.01)
PID: Labour	0.04*** (0.01)	0.01 (0.01)	0.05*** (0.01)	-0.04* (0.01)
PID: Pro-EU	0.01 (0.01)	0.02* (0.01)	0.05*** (0.01)	0.01 (0.02)
R ²	0.04	0.07	0.05	0.03
Num. obs.	67664	65628	68663	63870

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: Individual and time fixed effects are included but not shown. The Remain identity model includes only those individuals who voted for Remain in the referendum, whilst the Leave identity model includes only respondents who voted for Leave. The strength models only include individuals who hold that identity. The reference category for party ID is 'No party ID'. Nine survey waves are used for the analysis, together spanning the period from April 2016 to June 2020.

The first and most striking point to note is the consistent and precisely estimated null effect for all four of the media source variables with regards to Remain or Leave identification. Even more than Table 3.2, the coefficients in Columns 1 and 2 of Table 3.3 show clearly that consumption of television, newspapers, radio or the Internet has no impact on the chance of Remain voter identifying as a Remainer, or a Leave voter identifying as

a Leaver.

Columns 3 and 4 of Table 3.3, in contrast, show a relationship between media consumption and strength of identification for all forms of media. The only coefficient that is not positive and significant pertains to the effect of radio among Remain identifiers. All of the other coefficients are remarkably similar in magnitude, and all are highly statistically significant. The expected association between media and Brexit identity strength therefore applies equally to television, newspapers, radio (in the case of Leave identifiers) and the Internet.

Discussion

The analysis in this paper shows that the media played an important role in affecting the strength of Brexit identities over time. The results make clear that, for both Remain and Leave identifiers, paying more attention to media sources in a given time period was associated with stronger identification with the Remain or Leave side of the debate. Furthermore, this appears to apply equally in the case of television, newspapers and the Internet. This suggests that media consumption in general, rather than any specific form of media, causes stronger Brexit identification.

It is worth stressing that the findings relate to individual variations in media consumption and Brexit identification over time, not to aggregate level trends in these variables. Both media consumption and Brexit identity strength remained relatively stable over the period of 2016 to 2020 analysed in this paper, with no net trend in either (see Section A3.1 in the Appendix). Accordingly, the relationship between individual-level fluctuations in media and identification strength clearly did not translate to shifts within the electorate on average. The implication is that, in periods where some individuals spent more time consuming media sources than they did on average, and accordingly strengthened their Brexit identities, other individuals must have spent less time than

usual consuming media and weakened their identities. However, this is not to suggest that changes in average media consumption would not have, in the aggregate, changed the average identity strength. The findings are entirely consistent with the notion that, if the British electorate had consumed less media during the post-Brexit period, the average level of Brexit identification strength could have been lower.

It is also important to note that the coefficients in the models reported in this paper almost certainly underestimate the effect of the media on Brexit identification. Indeed, the models were intentionally designed to yield conservative estimates of this causal effect. The inclusion of individual-level fixed effects removed any between-person disparities in media consumption or Brexit identification from the analysis. This strict approach ensured that the reported results are not influenced by the fact that some individuals are more prone than others to both consume more media sources and identify more strongly with their side of the Brexit debate. However, the confidence in causality comes at the expense of the models likely understating the broader association between the media and Brexit identity. There is a high probability that, in addition to within-individual effects, an individual's average media consumption is also associated with their average level of Brexit identification. On a related note, the results only reflect the relationship between media and identity among those who have variations in media consumption and identity strength vary over the period. People whose media consumption and identity remained extremely high throughout, for example, do not contribute to the coefficients in the models.

Furthermore, media consumption may have partly caused Brexit identity in advance of the four year period, starting in 2016, that is analysed in this paper. The divides that came to light during the referendum had roots in long-standing divisions within British society (Sobolewska and Ford 2020b). Although the social identities of 'Remainers' and 'Leavers' might not have existed before the referendum campaign, much of the groundwork for these identities was already laid. Some of the between-person effects of the media on Brexit identities, which was removed from the models by the inclusion of

individual-level fixed effects, could have occurred prior to the period analysed in this study. For instance, the media may have framed the debate in ways that linked existing divisions, such as educational and regional disparities, to the Brexit divide. The intensity of the divide over Brexit might have partly resulted from the overlap between Brexit identities and these pre-existing divisions. This would not undermine the overall conclusion of this paper, that media consumption affected Brexit identification. Rather, it would be an additional way in which media consumption was related to Brexit identity.

Future research would do well to focus on the specific mechanism by which media consumption can enhance social identity strength. The theory section of this paper mentioned several potential reasons for this relationship, such as the possibility that media sources may heighten group threat perceptions among their audience. However, the data did not allow for a direct test of these mechanisms. In particular, it is unknown whether the BESIP respondents tended to seek out information that aligned with their existing Brexit identity, or whether they were exposed to media sources which challenged their sense of identity. This potential self-selection into news sources may also have varied according to the type of media in question and whether the individual identified with the Leave or Remain side. For instance, readers of Leave-supporting tabloids are likely to have been exposed to more positive in-group rhetoric compared to readers who supported Remain (Simpson and Startin 2022). Both Leave and Remain identifiers, however, may well have entered 'echo chambers', engaging with increasingly reinforcing information and viewpoints, while browsing the Internet (Levy and Razin 2019).

An additional point for future research is to test one of the assumptions of this paper, that the media had a constant effect on Brexit identities across time. There were periods of intense political and, as a result, journalistic division over the issue of Brexit, and other periods where the issue was less visible in the media. It remains untested whether these periods of particularly intense coverage of the Brexit debate resulted in a larger effect of media consumption on Brexit identity than periods with less Brexit reporting.

In summary, this paper provided an intentionally tough test of whether the media causally affected Brexit identities. The findings provide no evidence that identification itself reflected an individual's media consumption, but there is robust evidence that strength of identity did fluctuate in line with a person's time spent watching television, reading newspapers, and browsing the Internet. Given the toughness of the test, these results might reasonably be supposed to be the tip of a much larger media influence; if the media has a causal effect on over-time fluctuations in Brexit identity within the same person, it seems likely that the media also helped to play a role in causing some people to form strong Brexit identities in the first place. More research testing this is welcome, as would be future scholarship focussing on the precise mechanisms by which the media heightens social identification. The ferocity of the Brexit debate, particularly in the aftermath of the referendum, took many by surprise. On the basis of the results in this article, both old and new forms of media played a role in reinforcing these divisions.

Chapter 4

Partisanship in a Pandemic: Biased Voter Assessments of Past and Present Government Performance

Abstract

Accountability relies on voters accurately evaluating government performance in addressing the important issues of the day. This requirement arguably applies to an even greater extent when addressing fundamental societal crises. However, partisanship can bias evaluations, with government partisans perceiving outcomes more favourably, or attributing less responsibility for bad outcomes. We examine partisan motivated reasoning in the context of the COVID-19 pandemic crisis, using panel data and a survey experiment of over 6000 respondents in which vignettes prime respondents about the UK government's successes and failures in tackling the pandemic. We also propose a novel extension of the partisan bias thesis - partisans arrive at biased judgements of government competence by recalling the past performance of the government differently, according to whether or not their favoured party held power at that time. We find that even in the relatively consensual partisan context of the UK's response to COVID-19, where both major parties endorsed both lockdown and vaccination programs, there is evidence of both current and recall partisan biases: opposition partisans are more likely to blame the government for negative outcomes and less likely to recall positive aspects of the government's recent and past performance unless prompted to do so. Our findings have implications for understanding the limits of democratic accountability under crisis conditions.

Introduction

Democracy relies on citizens holding governments to account at the ballot box for the outcomes of their policies (M. Fiorina 1981; Key 1966; Powell 2000). Yet research has repeatedly cast doubt on the ability of voters to objectively evaluate government performance on important issues. One frequently raised concern is that many voters have a strong attachment to a particular political party, a ‘partisan identity’, and that this leads to biased assessments of real-world conditions (Anderson 2007; Green, Palmquist, and Schickler 2002; Sniderman, Brody, and Tetlock 1991). When outcomes are hard to dispute, government and opposition partisans can agree about the current state of affairs but attribute responsibility for the outcomes to different actors (Bisgaard 2015; Tilley and Hobolt 2011). In this paper, we use panel data analysis combined with an experimental approach to examine whether these consequences of partisan bias are present in the context of a highly salient non-economic issue; the UK government’s handling of the COVID-19 pandemic, which was noticeably less party polarized than in the more commonly studied US context. We also use the opportunity to test a new potential consequence of partisan reasoning; that partisans may exhibit biased recall of the government’s past performance.

The COVID-19 pandemic provides an excellent opportunity for the study of partisan motivated reasoning and its effects on evaluations of government competency.¹ Perceiving real-world conditions in line with partisan leanings is to be expected when issues are of low salience to voters and signals about the government’s performance are mixed. The COVID-19 pandemic, however, was highly salient and performance signals were relatively clear. People cared about the government’s handling of the pandemic for over

1. An alternative explanation for partisan differences is that voters instead engage in a form of ‘Bayesian updating’ (Gerber and Green 1999; Gerber and Huber 2010; Graham and Singh 2023). According to this theory, voters strive to reach accurate conclusions but give consideration to prior beliefs when evaluating new information. For example, a Conservative partisan who has concluded that Prime Minister Johnson is competent may, upon exposure to evidence of government incompetence, conclude that Johnson cannot have had much control over that area of government. In practice, these two explanations overlap considerably, and we therefore follow the majority of the literature by discussing partisan bias as a form of motivated reasoning.

two years,² and the media provided constant coverage and offered continual information about simple indicators like death and vaccination rates throughout. Moreover, COVID-19 handling in the UK was not strongly polarized by party: the government was unchanged throughout the pandemic and the main parties were relatively consensual in their positions on the measures adopted to address the pandemic and implementation of vaccination programmes (Klymak and Vlandas 2022). This contrasts with the polarisation over COVID-19 seen in, for example, the United States (Rodriguez et al. 2020). Accordingly, the UK pandemic provides a particularly tough test of partisan motivated reasoning and the thesis that partisans view the world through a perceptual lens (Achen and Bartels 2016; Campbell et al. 1960; Green, Palmquist, and Schickler 2002). In this paper we assess whether that lens is strong enough to distort perceptions of the worst global health crisis of recent decades.

In this context, the UK government is widely acknowledged to have performed both extremely badly on some aspects of pandemic management and extremely well on other aspects, thereby creating two strong but opposing performance signals. This allows us to prime respondents to focus on a positive or a negative aspect of handling without lying or distorting reality, increasing the external validity of our results, as well as allowing causal inference about the impact of partisanship. As a pre-pandemic wave of our survey was fielded at the time of the UK's last general election, in December 2019, it further allows us to incorporate pre-Covid partisanship into our models and to thus provide a temporal basis for inferring possible causal impact.

In addition to testing whether partisans display bias when evaluating performance and attributing responsibility for outcomes, we also examine an additional and novel way in which partisan voters may draw different conclusions about the competence of the governing party. Existing literature shows that motivated reasoning can affect the accuracy of a person's memory (Greene, Nash, and Murphy 2021; Murphy et al. 2019). We build

2. Health was cited as the most important problem facing the country in over 60% of the YouGov surveys asking that question over the period of 2020 to 2022: <https://yougov.co.uk/topics/education/trackers/the-most-important-issues-facing-the-country>

on these findings by hypothesising that government partisans will choose to selectively recall positive aspects of the government's past performance, whilst opposition partisans choose to focus on more negative aspects. These hypotheses have not been tested in previous research.

Overall, our results show that voters assess government performance in a partisan-biased manner. Panel data analysis provides evidence for almost all of our proposed mechanisms. Government partisans are more positive about the UK's coronavirus performance than opposition partisans, slightly less likely to attribute responsibility to the government for the pandemic overall, and more positive when recalling the government's handling of the pandemic a year before our experiment when compared to how they perceived it at the time.

Our experimental results mostly support our panel data findings. We find evidence that government partisans focus on positive aspects of the crisis when forming evaluations of the UK's performance overall, whilst opposition partisans are less likely to consider the success of the UK's vaccination program unless explicitly forced to do so. We also find that government partisans are less likely to attribute responsibility for the UK's pandemic experience to the government when first reminded of the UK's high death toll.

Our findings indicate that, when recalling past performance, opposition partisans are significantly less likely than government partisans to think about a positive aspect of crisis management, lending weight to the notion of partisan-biased recall.

Theory and Hypotheses

People are not only motivated to reach accurate conclusions, but also to reach conclusions that accord with their prior opinions (Mercier and Sperber 2017). This process of (directionally) 'motivated reasoning' (Kunda 1990) results in a range of cognitive bi-

ases that are frequently observed when voters reason about politics (Flynn, Nyhan, and Reifler 2017; Leeper and Slothuus 2014; Lodge and Taber 2013; Taber and Lodge 2006).

The role of motivated reasoning in politics is most clearly apparent in the case of partisanship. Though some scholars have previously taken partisan identity to essentially reflect a ‘running tally’ of party performances (M. Fiorina 1981), it is in fact the case that partisan identity can also create a ‘perceptual screen’ through which voters see different realities (Bartels 2002a; Butler and Stokes 1969; Campbell et al. 1960). In particular, partisans tend to view conditions more favourably when their party is in power (‘selective evaluation’), and to attribute more responsibility for positive outcomes and less responsibility for negative outcomes to the government when their party is in power (‘selective attribution’). As we shall elaborate below, there are also reasons to expect partisans to recall the government’s past performance differently according to whether or not their party held power (‘selective recall’). These processes allow voters to reduce the cognitive dissonance that results from supporting a political party which is failing to deliver desirable outcomes.

The tendency to twist new information in service to prior opinions is not unbounded (Lebo and Cassino 2007; Redlawsk, Civettini, and Emmerson 2010), however. Even committed partisans acknowledge economic reality when conditions are extreme or signals about performance are particularly clear. The case of COVID-19 allows us to test the limits of partisan reasoning during an extreme and highly salient health crisis, such as occurred in response to the economic crisis of 2008-09 when the endogenous nature of economic perceptions were mitigated by the strength of the signals resulting from the financial crisis (Chzhen, Evans, and Pickup 2014; Parker-Stephen 2013). For partisanship to affect evaluations of the government during the COVID-19 crisis, and in a relatively non-polarized political environment, would be strong evidence of the resilience of partisan biases.

Selective Evaluation

The simplest way for partisans to avoid confronting harsh truths about the failures of their favoured party or the successes of other parties is to ignore these truths altogether. For example, when inflation rates are worryingly high, government partisans might focus on the low level of unemployment and therefore judge that the economy is performing well.

Existing literature has established that partisans view conditions more positively when their favoured party holds office than when it is in opposition. Panel data analysis of UK voters in the 1990s shows clearly that socio-tropic perceptions of the economy are themselves affected by prior opinions about the incumbent party and by party choice at the last election (Anderson, Mendes, and Tverdova 2004; Evans and Andersen 2006), as are egocentric economic evaluations (Johnston et al. 2005). The causal relationship between vote choice and economic perceptions extends beyond the UK (Wlezien, Franklin, and Twiggs 1997), as does the persistent effect of partisanship on perceptions of not just the economy (Wilcox and Wlezien 1993) but also foreign policy (Bartels 2002a).

Accordingly, we expect to find descriptive partisan differences in how respondents view the UK's experience of the COVID-19 pandemic. We anticipate that government partisans will be more positive about the UK's coronavirus performance than opposition partisans, because they are motivated to believe that the government has handled the crisis successfully:

H1: Government partisans are more positive about the UK's overall pandemic performance compared to opposition partisans.

One way for government partisans to conclude that the government has handled the pandemic well is for them to focus on positive aspects of the crisis management like the vaccine rollout. Conversely, opposition partisans can arrive at a negative evaluation of

the crisis handling by focusing on the UK's comparatively high pandemic death toll. We therefore expect our negative experimental manipulation to affect government partisans more than opposition partisans because opposition partisans are already thinking about the negative aspect of the pandemic, and conversely for the positive manipulation:

H2: Government partisans who are reminded about a negative (positive) aspect of the UK's experience of coronavirus are more negative (less positive) about the UK's overall pandemic performance compared to opposition partisans.

Selective Attribution

When faced with uncomfortable and undeniable facts about reality, there remain a number of possible options for engaging in motivated reasoning (Gaines et al. 2007). One of these options is selective attribution. Voters who hold a partisan identity may accept objective facts about policy outcomes like the state of the country's economy or health service, but choose to attribute responsibility for this outcome differently according to whether their favoured party holds power. Multiple studies have found evidence that partisans engage in this form of motivated reasoning, raising concerns about democratic accountability (Gomez and Wilson 2003; Rudolph and Grant 2002). The opportunities to engage in selective attribution are expanded by the fact that attributing responsibility for policy outcomes is often difficult even for a non-partisan voter (Anderson 2000; Hobolt, Tilley, and Banducci 2013; Powell and Whitten 1993), particularly given the clear political incentives for parties to intentionally blur the lines of responsibility (Hellwig 2012; Hobolt and Tilley 2014).

Previous research has shown that partisans in the United States are more likely to attribute responsibility for good economic outcomes and less likely to attribute blame for

bad economic outcomes to officials from their favoured party (Brown 2010; Rudolph 2003a, 2003b, 2006). They are also more likely to selectively blame officials for the handling of natural disasters (Healy, Kuo, and Malhotra 2014; Malhotra and Kuo 2008), foreign policy (Nawara 2015; Sirin and Villalobos 2011) and health care (McCabe 2016).

Less research has considered the UK context, but some studies have found that the findings from the U.S. extend quite well to the UK context. UK partisans are more likely to blame the government for negative economic outcomes when their party does not hold power (Bisgaard 2015; Marsh and Tilley 2009), and similar effects have been found for health care outcomes (Tilley and Hobolt 2011).

In the case of COVID-19, our expectations follow the findings from existing literature. We anticipate that government partisans will attribute less responsibility for negative aspects of crisis management than opposition partisans, and more responsibility for positive aspects:

H3: Government partisans attribute less (more) responsibility for the UK's high coronavirus death toll (successful vaccine rollout) to the government than opposition partisans.

Attributions of responsibility for the pandemic overall are likely to depend on how positively partisans feel about the UK's performance. If government partisans feel positively about the performance then, according to the theory outlined in this section, they will likely attribute more responsibility to the government for this outcome than if they feel negatively about the performance. The opposite is expected for opposition partisans:

H4: Feeling positively about the UK's pandemic performance is positively associated with attributing more responsibility for the pandemic outcome to the government among government partisans, and negatively associated among opposition partisans.

Likewise in our experiment, we anticipate that prompting respondents to think about the UK's high death toll will result in government partisans affording less responsibility for the pandemic overall to the government compared to opposition partisans. We expect the exact reverse when we prompt respondents with details of the UK's vaccine success:

H5: Government partisans who are reminded about a negative (positive) aspect of the UK's experience of coronavirus attribute less (more) responsibility to the government for the handling of the pandemic overall compared to opposition partisans.

Selective Recall

Far less research has investigated the effect of partisanship on memory of political events. Literature from psychology shows that human memories are frequently distorted or even patently false (for an overview, see: Koriat, Goldsmith, and Pansky 2000). There is also a clear neurological link between motivated reasoning and memory (Bavel and Pereira 2018). Given that partisans are willing to selectively evaluate present conditions differently according to whether their favoured party holds power, it stands to reason that they are also likely to evaluate past performance differently according to whether their party held power at that time. When attempting to reach a motivated conclusion about the competence of the governing party, individuals may selectively recall or ignore elements of the party's past performance. For example, a government partisan may forget about some of the government's past failings and instead conclude that the government was performing well, even if they thought the government was performing badly *at the time*. Despite the intuition of this possibility, we are not aware of any studies directly testing this potential consequence of partisan bias, which we term 'selective recall'.³

3. Our concept of selective recall is distinct from voter myopia. The conventional wisdom in the economic voting literature is that voters evaluate conditions myopically, placing more emphasis on recent

Existing studies suggest that selective recall is likely to be a consequence of partisanship. There is evidence that citizens are generally fairly poor at recalling past economic performance even without factoring in motivated reasoning (Hellwig and Marinova 2014). Furthermore, recent work has shown that some voters exhibit false memories of political events and that these are more likely to occur when the memories align with their current political predispositions and attitudes. For example, Irish voters falsely recalled seeing news stories that aligned with their views on the Irish abortion referendum in 2018 (Murphy et al. 2019), and UK voters falsely remembered stories about the EU referendum depending on their own opinions concerning Brexit (Greene, Nash, and Murphy 2021). Along partisan lines, U.S. Republicans were more likely to recall President Obama shaking hands with the President of Iran, whilst Democrats were more likely to recall President Bush taking a vacation during the Katrina hurricane, despite neither of these events actually having occurred (Frenda et al. 2013).

Two articles come close to testing the type of selective recall examined in this paper. Castelli and Carraro (2011) used an experimental approach to show that ideologically conservative participants were more likely to recall negative facts about immigrants than ideological liberal participants. It seems reasonable to expect similar differences in memory according to partisanship. Similarly, Jacobson (2010) used panel data analysis to show that perceptions of the Iraq war in the U.S. changed over time in line with partisan identity, with respondents falsely recalling their earlier opinions. For example, many Democrats forgot that they had once believed Iraq to possess weapons of mass destruction (Jacobson 2010).

Our paper builds on these findings by considering whether past memories about government performance, rather than specific events or facts, can be affected by partisan identity. The question of whether voters can accurately recall past performance, or whether this is biased by partisan identity, is crucial for understanding the relationship

economic performance than on past performance and that this poses a problem for democratic accountability (Achen and Bartels 2016; Tufte 1978). In this paper we are not interested in the question of whether voters care about recent performance more than past performance, but rather whether voters are can even manage to accurately recall the past performance without bias.

between time and the limits of democratic accountability.

Specifically, we propose that motivated reasoning may result in government partisans recalling the government's performance a year ago more favourably than opposition partisans:

H6: Government partisans recall the government's past handling more positively than opposition partisans.

Part of the explanation for **H6** may be that government partisans were simply more positive about the pandemic at the time. We also anticipate that the bias of partisan motivated reasoning may extend further to actually distort recollection of the past. Accordingly, we expect that:

H7: Government (opposition) partisans recall the government's past handling more positively (negatively) than they perceived it at the time.

We expect this mechanism of selective recall to operate similarly to selective evaluation; government partisans selectively avoid thinking about the negative aspects of past performance when evaluating earlier government competence. We therefore anticipate that reminding government partisans of the UK's high pandemic death toll will have a bigger negative effect than reminding opposition partisans of this fact (opposition partisans are likely to think about the death toll even without a prompt to do so) and conversely for the vaccine success:

H8: Government partisans who are reminded about a negative (positive) aspect of the UK's experience of coronavirus will become more negative (less positive) about the government's past handling compared to opposition partisans.

Analytical Strategy

To test our hypotheses we employ both panel data analysis and a survey experiment. Our data is from the British Election Study Internet Panel (BESIP), and we make use of three waves (19-21) from this survey (Fieldhouse et al. 2022). Wave 19 was fielded between 13 and 23 December 2019, wave 20 between 3 and 22 June 2020, and wave 21 between 7 and 25 May 2021. Our experiment was fielded at the end of the full wave 21 survey, with a sample comprising 6884 respondents who were randomly selected from the full panel ($n = 30,821$). For tables showing the distribution of the sample across a range of demographic and non-demographic variables, see Section A4.3 of the Appendix.

The timing of these survey waves allows us to exploit exogenous variation in the British experience of the COVID-19 pandemic. COVID-19 first reached Europe in early 2020, just after a UK general election that gave then Prime Minister Johnson a large majority in the House of Commons. Yet by May 2020, the UK government was perceived by many as too slow to react to the pandemic, resulting in comparatively high infection and death rates as the virus spread fast during a period of very few restrictions on social activity. By June 2020, when the BESIP wave 20 was in the field, UK citizens had been exposed to prolonged media criticism of the government's COVID-19 strategy, and had endured multiple weeks of 'lockdown', with a police-enforced 'stay at home' order that prohibited leaving one's home except to shop for necessities or engage in one hour of daily exercise. By May 2021, the time at which we fielded our experiment, the UK situation had dramatically reversed. Though there had been additional (less extreme) lockdowns in the eleven interceding months, May 2021 was a period of very few restrictions, and a time at which the UK's vaccination program was proving extremely successful. Vaccination rates outstripped those seen across Europe, partly due to the government's successful procurement program, and the UK's pandemic performance at that point had been praised in the media for a number of months.

Our panel data analysis uses this exogenous variation to examine evaluations of the government's handling of COVID-19 as a product of pre-COVID-19 evaluations. Our experiment exploits the variation by priming respondents to think about negative and positive aspects of the crisis without lying or distorting reality; we simply call attention to either Britain's initial struggle with the virus or to the later success that came from the vaccine program. It is also worth noting that early evidence suggests that voters differ substantially in their evaluation of the government's performance but tend to agree on what the goal should be (Green, Evans, and Snow 2020), which avoids the conflation of performance evaluations with ideological preferences.

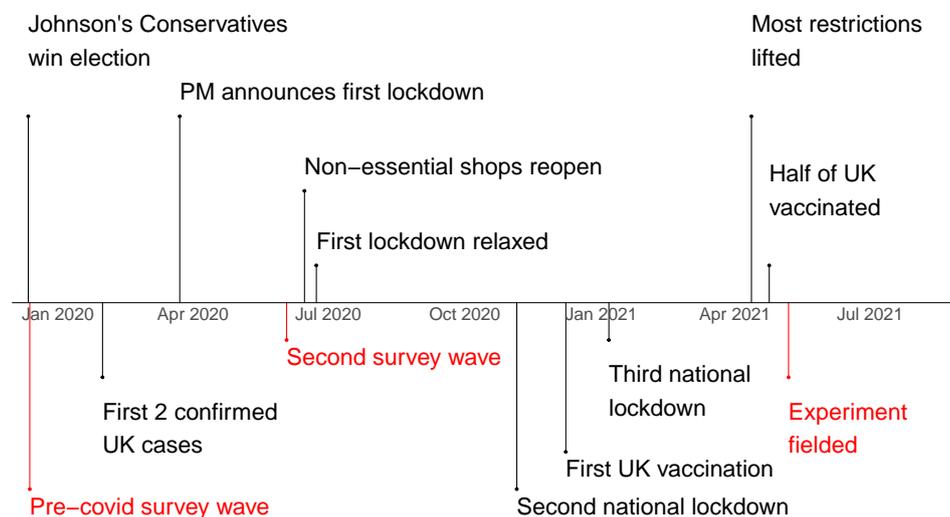


Figure 4.1: Timeline of data collection

Note: Sources are the Institute for Government Analysis, BBC news, and the BESIP code-book

The panel data analysis consists of examining the relationship between pre-pandemic partisanship and evaluations of the government's handling of the pandemic two years later. On the question of recalled past-handling, we also make use of the 2021 survey wave to compare respondents' recollections of pandemic handling 'this time last year' to how they actually responded at the time. Over half ($n = 3796$) of the full sample had participated in wave 20 of the BESIP, which was fielded one year before our experiment, and 3784 had participated in wave 19 of the BESIP, providing a reasonable sample size

for panel data analysis.

To corroborate the findings of our panel data analysis we also make use of an experimental approach. We primed respondents to think about a positive or negative aspect of the UK's COVID-19 performance, either the successful vaccine program or the high pre-vaccine death toll, and observed how this treatment affected responses to questions about the government's handling of the pandemic.

Experimental Treatment

We randomly assigned respondents to one of two treatment conditions or to a control group.⁴ For experimental treatments we used short vignettes that highlighted either a negative or a positive aspect of the UK's experience with the COVID-19 pandemic. By comparing the responses of those in the control group (who received no vignette) to those who did receive a vignette, we can infer whether respondents would have been thinking about the points raised in our vignettes even had we not prompted them first.

The exact wording was as follows:

Negative performance reminder:

“Before the rollout of the coronavirus vaccine, the UK had one of the highest coronavirus death tolls per capita in the entire world. Over 125,000 Britons have died after contracting the virus.”

Positive performance reminder:

“Well over half of the UK adult population have already received their first dose of the COVID-19 vaccine, and the UK continues to have one of the best

4. Because assignment was genuinely random, the groups are only roughly equal sized. 2042 were assigned to the control group, 2160 to the negative treatment group, and 2292 to the positive treatment group.

vaccination rates in the entire world.”

Dependent Variables

After receiving treatment in the form of vignettes, respondents were asked a number of questions that allow us to test our hypotheses about selective evaluation, attribution and recall. First, to test for selective evaluation we asked: *“How well has the UK performed overall in dealing with the coronavirus crisis?”*. Answers were given on a five-point scale from ‘very well’ to ‘very badly’, with a ‘don’t know’ option available. This dependent variable also acts as a useful manipulation check, allowing us to assess whether respondents were affected by our treatments at all.

We then tested selective attribution by asking three questions about the government’s responsibility for crisis management. Respondents were asked: *“To what extent are the following the result of decisions taken by the UK government?”* and asked to rate responsibility for three outcomes (in a randomised order) on a scale from 0 (“Not at all due to government decisions”) to 10 (“Entirely due to government decisions”), again with an option for “Don’t know”. The three outcomes we asked about were *“The UK’s overall experience of the coronavirus crisis”*, *“The UK’s high coronavirus death toll”* and *“The fast pace of the UK’s vaccine rollout”*.

To test our novel theoretical expectations about retrospective evaluations of distant past performance, we asked respondents: *“Thinking back to **this time last year**, how well do you think the UK Government had handled the coronavirus outbreak in Britain?”*⁵ This was measured on a 5-point response scale ranging from ‘Very well’ to ‘Very badly’ in order to match the question about the UK’s overall performance and matches the question in the BESIP survey for wave 20 in which respondents were asked: *“How well do you think the UK Government has handled the coronavirus outbreak in Britain?”*⁶

5. The bold highlighting of the text to focus on ‘this time last year’ was present in the actual survey question shown to respondents.

6. Technically the gap between wave 20 and our experiment is closer to 13 months, and varies slightly

Independent Variables

Partisan identity is our main independent variable of interest, which we interact with treatment status to see whether government partisans responded differently to our vignettes compared to opposition partisans. We make use of the standard BESIP question for partisan identity: “Generally speaking, do you think of yourself as Labour, Conservative, Liberal Democrat or what?”, coding all Conservative partisans as ‘government partisans’ and all those expressing *any* other partisan identity as opposition partisans. This was measured prior to our experiment in the main survey. Of those who also participated in wave 20 of the BESIP, we were able to check whether their partisan identity had changed during the pandemic; just 3% of the sample switched from the Conservatives to another party, or vice versa, and the results reported below remove these switchers from the sample. We also remove the 1% of the sample who were Brexit party identifiers because they are not ‘government partisans’, but cannot really be considered ‘opposition partisans’ given the ideological overlap between the Brexit and Conservative parties. The effect of not removing these individuals can be seen in Section A3.2 of the Appendix.

Method

We analyse our data with ordinary least squares regression models, with each dependent variable modelled in turn as a function of treatment status. For hypotheses concerning differences of treatment effect by partisan identity we also include the interaction of treatment status with these variables. In the case of our evaluation measures (overall performance and retrospective evaluation) we also re-ran the analysis using ordered logit models - the results of this robustness check were not substantively different

across individual respondents since each survey is conducted over four weeks and respondents can choose when exactly they take part within that time frame. Our use of the phrase, ‘thinking back to this time last year’ reflects a desire for a question that is simple to read and understand. For our intents in this experiment, it only really matters that respondents are prompted to this question to think back to before the vaccine rollout, when the UK’s performance became much more positive relative to other countries.

from using OLS, and can be found in Section A3.2 of the Appendix.

The use of randomised allocation to treatment theoretically removes the need to include control variables, since demographics do not differ systematically according to treatment status, but demographic variables can help to increase the precision of other coefficient estimates. Accordingly, all models reported below include full demographic controls for gender, ethnicity, age, class and educational attainment. Full details of how these variables are coded can be found in Section A5.2 of the Appendix.

Surveys typically under-represent individuals with low political attention, which may bias our experimental results if these individuals differ in response to treatment compared to individuals with high political attention. Accordingly, we adapt the weighting variable for our analysis to match the levels of self-reported political attention in our experimental sample to those found in the random probability British Election Study (BES). An additional concern is that our vignette praising the success of the UK's vaccine program might have an opposite effect on those individuals who oppose vaccination on ideological grounds. All reported models therefore omit those individuals who indicated earlier in the survey that they are not generally in favour of vaccinations (3% of the sample).

The effect of including these individuals and of not weighting by political attention can be found in Section A4.5 of the Appendix. The model specification has no systematic effect on coefficient sizes or standard errors and does not affect our substantive conclusions.

Results - Panel Data Analysis

Figure 4.2 shows the main results of the panel data analysis. The models show clearly that there are partisan differences across almost all dependent variables. In line with

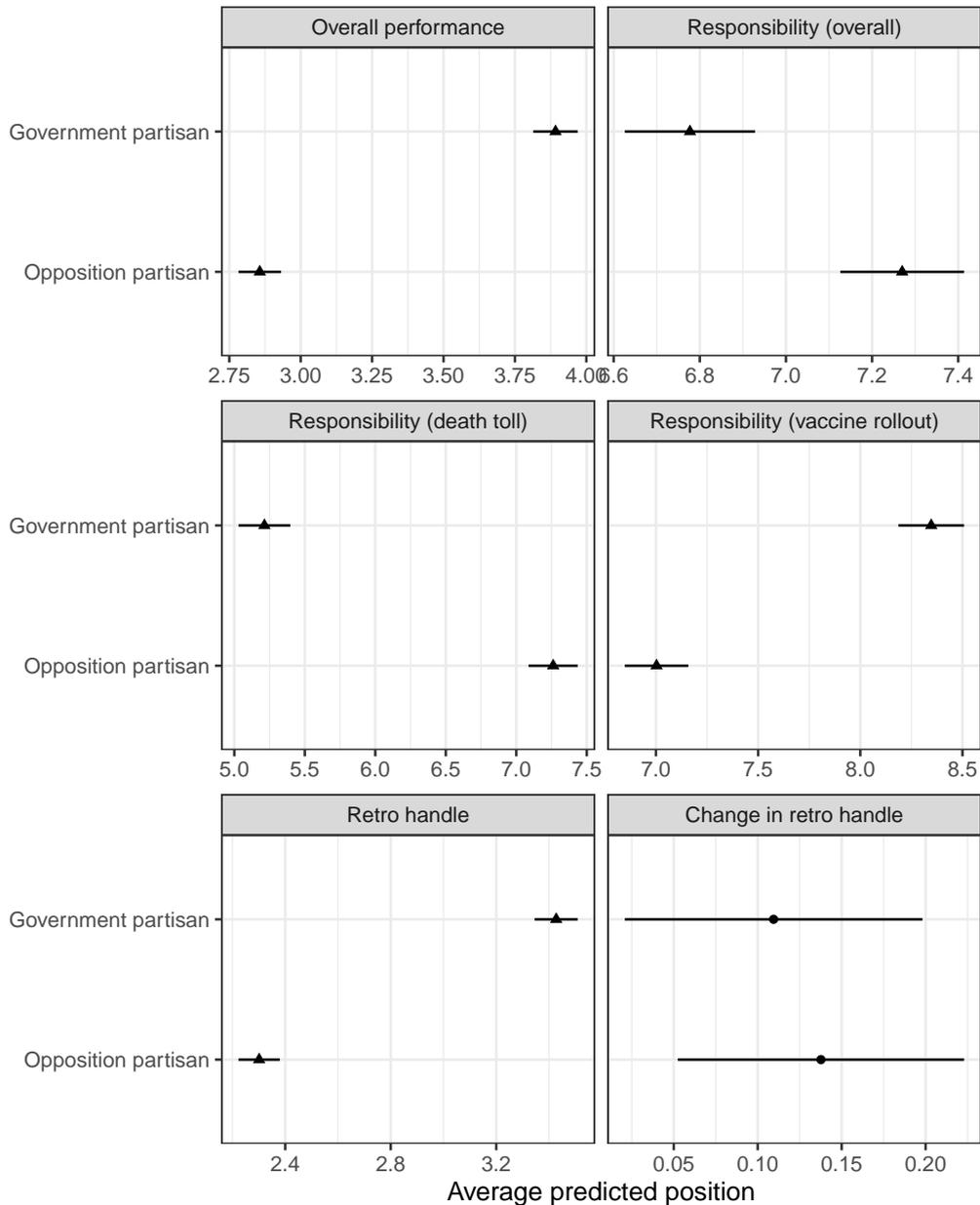


Figure 4.2: Average predicted position for government and opposition partisans across all six dependent variables

Note: Triangular points indicate that there is a significant difference in the predicted response between government and opposition partisans at the 95% level. The ‘Change in retro handle’ model only includes those individuals who were also surveyed in the pre-experiment wave one year prior to the experiment. The predicted positions displayed in this figure are calculated from a model that also includes controls for gender, ethnicity, age, class and education. The ‘whiskers’ shown reflect 95% confidence intervals. The full results of these models can be found in Section A4.4 of the Appendix.

H1, individuals who were government partisans before the pandemic were more positive about the government’s handling of the pandemic by May 2021 compared to those

respondents who were opposition partisans in 2019. In confirmation of **H3** government partisans were less likely than opposition partisans to attribute responsibility for the death toll to the government, but more likely to attribute responsibility for the success of the vaccine rollout.

We also hypothesised that the relationship between evaluations of the UK's pandemic experience and of overall responsibility would differ according to partisanship. Figure 4.3 displays the bivariate relationships between attribution of responsibility for the pandemic overall and evaluations of the UK's overall performance, attribution of responsibility for the UK death toll, and attribution of responsibility for the vaccine rollout. The first panel provides supports for **H4**; government partisans who are more positive about the UK's overall performance attribute less overall responsibility for the pandemic to the government, whilst opposition partisans who are more positive about the performance attribute more responsibility to the government.

One way in which partisans may arrive at different responsibility attributions is by focusing on different aspects of crisis management when assessing responsibility for the pandemic as a whole. Panel 3 in Figure 4.3 offers some support for this explanation. There is a very strong association between vaccine responsibility and overall responsibility for government partisans, but for opposition partisans the two are almost entirely unrelated. Opposition partisans who gave full credit to the government for the vaccine rollout attributed the same amount of responsibility to the government for the pandemic outcome overall as those who gave the government no credit for the vaccine rollout. This suggests that opposition partisans are unwilling to account for the government's role in the vaccine rollout, which is widely accepted to have been highly successful, when attributing responsibility for the pandemic overall, and that they differ in this respect from government partisans. We do find a similar but less extreme pattern in the case of responsibility for the death toll, where both opposition and government partisans seem to incorporate responsibility for this outcome into their perceptions of overall responsibility, but government partisans do so to a lesser degree.

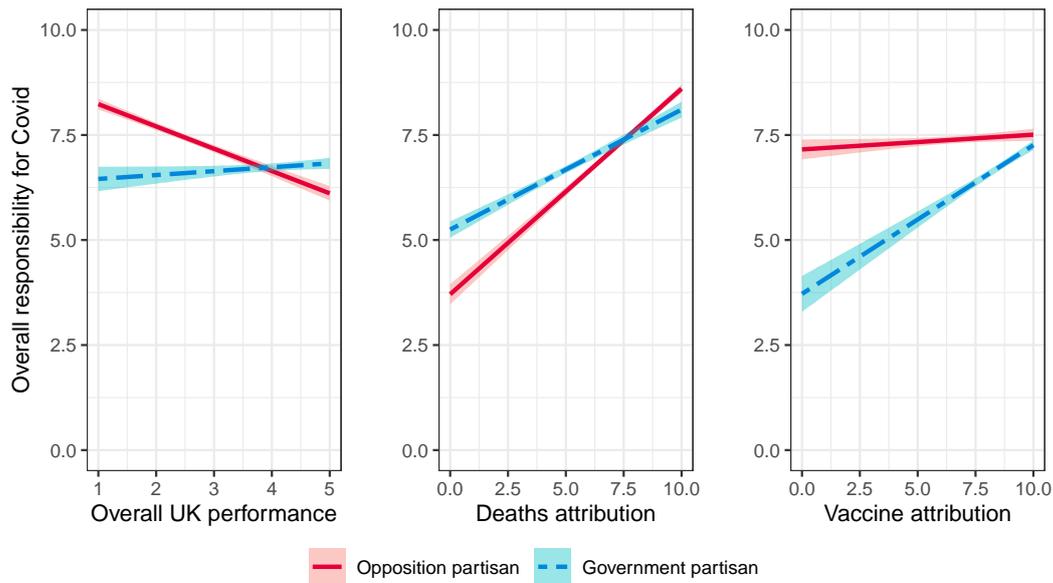


Figure 4.3: Bivariate relationships between overall performance, death toll and vaccine success attributions and overall responsibility for the pandemic

Returning to Figure 4.2, we find confirmation of **H6**; government partisans were more positive when recalling the government's performance than opposition partisans. We also find evidence that this reflects genuine bias among the government partisans, who are actually more positive than they recalled at the time, in line with **H7**. Surprisingly, and contrary to the expectations of **H7**, opposition partisans are significantly more positive about the government's handling of the pandemic one year ago than they stated at the time.

To give some substantive context to how extreme this last result is, we considered the relationship between current and past evaluations of the UK's coronavirus experience, and respondents' recollections of the government's past handling. As Table A4.8 shows, there is a greater association between evaluations of the UK's current coronavirus experience and *recall* of the government's earlier handling than there is with individuals' actual evaluations of the government's handling *at the time*.

Furthermore, government partisans rely significantly more on present evaluations when forming their opinions about the past than opposition partisans. This adds further evidence to our suggestion that partisanship affects the way in which individuals recall the

Table 4.1: Results from a regression of retrospective handling evaluations on perceptions of the UK's current Covid-19 performance and on evaluations of the performance one year ago

	1	2
Current UK Covid-19 performance	0.50*** (0.02)	0.42*** (0.03)
Covid-19 handling one year ago	0.35*** (0.02)	0.39*** (0.03)
Government partisan (ref = opposition)		-0.02 (0.16)
Current UK Covid-19 performance * gov partisan		0.20*** (0.05)
Covid-19 handling one year ago * gov partisan		-0.16** (0.05)
R ²	0.64	0.67
Adj. R ²	0.64	0.66
Num. obs.	2464	1645

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: Results are from an OLS regression with retrospective handling evaluations, measured in May 2021 as the dependent variable. Partisanship is measured in the pre-pandemic survey wave, and measures of Covid-19 handling one year ago were measured in June 2020. Controls are added but not shown for treatment, gender, ethnicity, age, class, and education. The full table of the models can be found in the Appendix.

past, with government partisans extrapolating back from the sunny present and forgetting about their actual feelings concerning the government's performance one year ago.

Results - Experiment

Our first dependent variable, perceptions of the UK's pandemic performance overall, can be used as a manipulation check for our treatments. As Table 4.2 shows, our treatments had the desired effect. Those individuals who were informed (or reminded) about the UK's high death toll were significantly more negative about the UK's experience of the coronavirus pandemic overall than the control group receiving no vignette. Those who were informed (or reminded) about the success of the UK's vaccination program

were significantly more positive about the UK's experience of the pandemic than the control group.

Table 4.2: Percentage breakdown of UK overall performance by treatment condition

Treatment	Very badly	Fairly badly	Neither well nor badly	Fairly well	Very well
Negative	21	22	18	30	10
Positive	9	17	16	31	28
Control	14	21	16	36	13

The results of our main analysis, testing whether our successful manipulation affected government and opposition partisans differently, can be found in Figure 4.4. The figure shows the marginal effect of our treatment conditions on each of our dependent variables, separated by partisanship. Triangular points indicate that the interaction term was significant in the model, providing evidence at the 95% level that government and opposition partisans reacted differently to our treatment vignettes.

With regards to our hypotheses about selective evaluation, we find partial support for **H2**. In line with our expectations, opposition partisans were significantly more affected by a reminder about the UK's vaccine success than government partisans, indicating that they were less likely than government partisans to recall the vaccine success when evaluating the UK's overall experience of coronavirus unless explicitly prompted to do so. However, the negative treatment was equally effective at dampening evaluations of the UK's performance for government and opposition partisans, and so does not provide further evidence for **H2**.

Turning to responsibility attribution, we find the reverse of our findings on performance evaluations, with a significant partisan difference for the effect of the negative treatment but not for the positive treatment. In line with **H5**, we find that government partisans attribute significantly less responsibility to the government for the UK's pandemic performance overall when they are first reminded about the UK's high coronavirus death toll. In this respect they differ significantly from opposition partisans. However, the positive treatment has no effect on either government or opposition par-

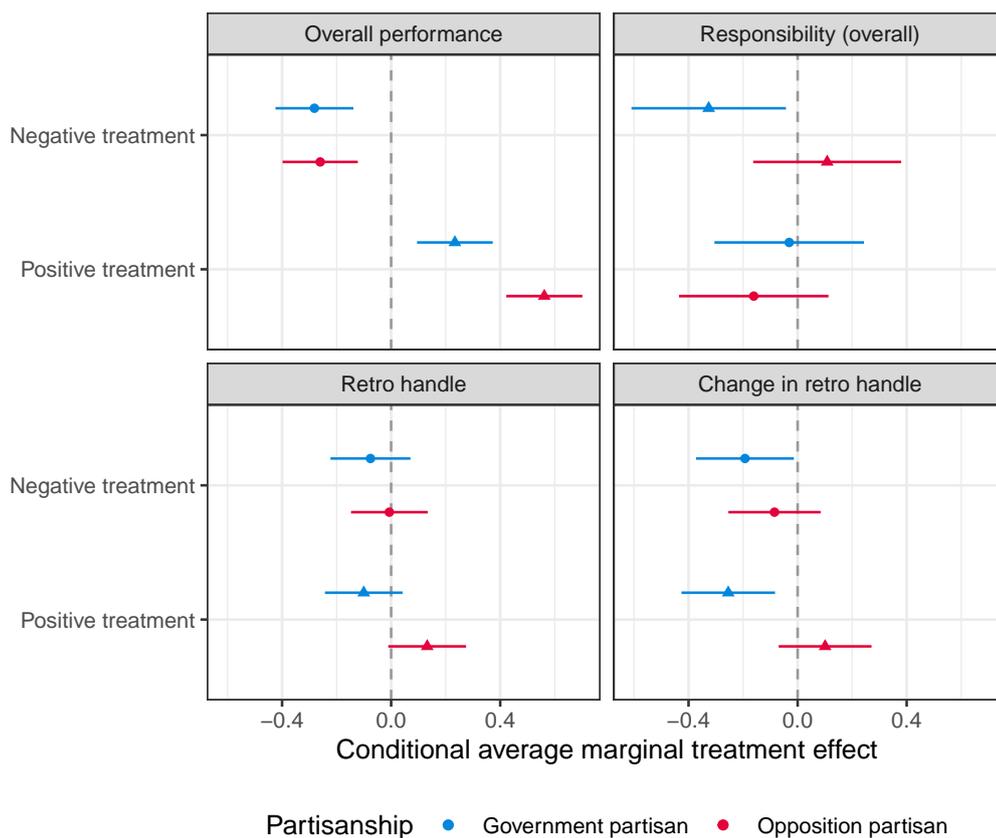


Figure 4.4: Average marginal treatment effects for government and opposition partisans for all six dependent variables

Note: Triangular points indicate that the interaction term is significant at the 95% level, i.e. that government partisans differ significantly from opposition partisans in response to that treatment condition for that dependent variable. The ‘Change in retro handle’ model only includes those individuals who were also surveyed in the pre-experiment wave one year prior to the experiment. The marginal effects displayed in this figure are calculated from a model that also includes controls for gender, ethnicity, age, class and education. The ‘whiskers’ shown reflect 95% confidence intervals. The full results of these models can be found in Section A4.4 of the Appendix.

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For our final dependent variable, retrospective handling evaluations, we find no significant partisan differences in the effect of the negative treatment but a significant difference for the positive treatment. When reminding respondents about the UK’s high death toll, we do not find a significant difference in the resulting evaluations of past handling compared to the control group who received no reminder. This is true for both government and opposition partisans.

There is some evidence that, for those respondents who were surveyed a year before the experiment as well as during the experiment, government partisans became slightly more negative about the past performance compared to how they felt at the time. Yet this does not significantly differ from opposition partisans.

We do however find a significant interaction effect in the case of the positive treatment, as hypothesised in H8. Opposition partisans reminded about a positive aspect of the UK's coronavirus handling became significantly more positive about the government's response to the pandemic a year prior to the experiment. Government partisans, by contrast, appear to have become more negative, though this is not quite significant in the model for the full sample. When focusing on those who took the survey one year before the experiment, we also find a significant interaction effect and, in this case, we find that government partisans reminded about the vaccine success actually became more negative about the government's past performance compared to how they rated it at the time.

Discussion

The results in this paper offer an important confirmation that government evaluations are heavily endogenous to partisan identity. The findings, derived from panel data analysis corroborated by a survey experiment, suggest that partisans evaluate performance, past performance and responsibility in a manner that reflects positively on their favoured party, even in the context of a highly salient health crisis like the COVID-19 pandemic.

This article provides an important update to a body of research that has tended to be limited either in scope or by a lack of external validity. Much scholarship on the topic of partisan bias focusses exclusively on the consequences for economic evaluations, often in the United States. This leaves open questions about whether partisan bias extends beyond the economy to other issues, and about how well the results replicate

to less politically polarised countries. Studies that attempt to move beyond the economy or the United States, however, tend to struggle with providing experimental treatments that are realistic. For example, experimenters sometimes present respondents with hypothetical scenarios or non-factual prompts, such as claiming to respondents in the treatment group that ‘experts say that... the British economy is doing considerably worse than most other countries’ (Tilley and Hobolt 2011). There is good reason to doubt whether voters react to real-world information in the same way that they react to these kinds of hypothetical vignettes. Alternatively, some papers focus on natural disasters like hurricanes or floods (Malhotra and Kuo 2008), which have a short-term saliency and are often quite localised. Our study builds on existing research by addressing both of these shortcomings - we manipulated factually accurate information about a real-world and enduringly salient non-economic crisis.

It is also worth noting that our experimental set-up provided a very tough test of the mechanisms of partisan motivated reasoning established in the literature. COVID-19 was a highly salient event, covered extensively in the national media for a period of months with easy to grasp figures like death rates that could be compared to other countries by ordinary voters. In such a context, finding evidence of motivated reasoning testifies to the strength of the partisan perceptual screen.

It is therefore worth noting that our experiment setup may have downwards-biased our estimates of motivated reasoning. We fielded the experiment at the end of wave 21 of the British Election Study Internet Panel, meaning that our sample comprised relatively knowledgeable and politically interested respondents who had just answered a number of questions about politics and COVID-19. Accordingly, our treatment vignettes might have been expected to have no detectable effect on these respondents. The fact that we detected changes in the attitudes of our respondents after treatment, and that these changes fell along predictable patterns of partisanship, further emphasises the robustness of the partisan perceptual screen.

One interesting nuance of our findings is that the effect of our negative and positive prompts were asymmetric. In the case of selective evaluation, our positive prime affected opposition respondents more than government respondents, whilst the negative prime affected both equally. In contrast, our negative treatment affected opposition partisans significantly more for a question of overall responsibility, but the positive treatment was equally ineffective at changing attributions of responsibility for opposition or government partisans. One possible explanation for this is that respondents reacted to our primes by engaging in one of two forms of motivated reasoning. When reminded of a positive, they chose to update their overall evaluation, but when reminded of a negative they instead changed their attribution of responsibility. In neither case was there a need for partisans to both selectively evaluate performance and to selectively attribute responsibility. This accords with findings from previous literature that partisans need only engage in one form of motivated reasoning to arrive at a biased conclusion, for example by attributing responsibility differently only when forced to confront the fact that conditions are getting worse (Bisgaard 2015). However, more work is needed to establish whether the specific positive / negative asymmetry we found in our paper generalises to other cases, or whether it is merely a feature of the context in which we conducted our experiment. For example, the UK's high death toll may simply be too strong a performance signal for even ardent government partisans to ignore, instead leading them to allocate responsibility differently. It is also worth highlighting that for the panel data analysis we do not find this asymmetry, further suggesting that these caveats may not generalise to other settings.

Our paper is also the first to posit and test that voters may differ with regard to how they recall the distant past performance of governments. Our results suggest that recall differs according to partisan identity. Our findings are not meant to suggest that voters care less about the past, though this may be the case, but that they genuinely recall past performance differently. For example, we find experimental evidence that opposition partisans are unlikely to recall the vaccine success when evaluating early government performance unless explicitly reminded about it. Relatedly, government partisans seem

more likely than opposition partisans to essentially extrapolate back from the positive present context when evaluating the government's performance at a far more negative stage in the pandemic. This carries obvious implications for democratic accountability in a context of increasing political polarisation, and invites further research.

In conclusion, our findings suggest that voters evaluate the government's performance with bias even in the context of a deadly worldwide pandemic. Not only are voters biased in their evaluation of conditions and their attributions of responsibility, but they appear to have short and biased memories too. There are many challenges to democratic accountability - one is undoubtedly the bias that voters themselves bring into the ballot box. Democracy relies on voters accurately holding governments to account without bias, and our findings concerning selective evaluation, selective attribution and selective recall therefore have important implications for the limits of democratic accountability.

Chapter 5

Biased Perceptions: Social Identity and Evaluations of National and International COVID-19 Pandemic Performance

Abstract

Many voters hold misperceptions about their country's performance on important outcomes. One reason is that individuals with a social identity are motivated to view the world in a way that reflects well on their group. This bias can become salient in the political sphere. For example, committed partisans tend to view the economy more positively when their party is in power. In this paper, I expand scholarship on the perceptual bias caused by social identities by considering whether party and Brexit identities shaped perceptions of how countries performed during the COVID-19 pandemic. Using British Election Study panel data, which includes high quality measures of social identification, I show that both party and Brexit identity were associated with how well the UK performed during the pandemic. Those individuals who were closer to the government end of the party identity scale were more positive about the UK's performance. Those individuals who were closer to the Leave end of the Brexit identity scale were also more positive about the UK's performance and, additionally, were more negative about the performance of other countries. This latter result provides a novel contribution to the literature on perceptual bias by showing that social identities can bias the perception of international, as well as national, outcomes.

Introduction

When political parties perform badly in office, citizens can punish them by voting for a different party at the next election. This is the cornerstone of democratic theories of accountability (M. Fiorina 1981; Key 1966; Powell 2000). If citizens struggle to accurately assess the quality of outcomes produced by governing parties, this mechanism is at risk. Social identities bias how individuals view the world (Tajfel 1981), in part by creating cognitive dissonance (Cooper 2007). When social identities exist in the political sphere, they therefore threaten the ability of voters to hold governments to account.

In the case of partisan identity, for example, committed partisans tend to view outcomes more favourably when their party is in power (Bartels 2002a; Evans and Andersen 2006). Incumbent governments can therefore rely on a substantial portion of the electorate failing to notice when the economy is performing sub-optimally, and this reduces the incentive to deliver objectively good economic outcomes.

Partisan identity is by far the most researched social identity in the political sphere, but many voters also maintain other relevant social identities. A large majority of British voters continue to have a social identity based on whether they believe that the UK should have left or remained in the European Union, a question posed to the nation via a referendum in 2016. These individuals feel a sense of belonging to other 'Remainers' or 'Leavers' that goes beyond mere ideological agreement, and they view in-group members more positively than out-group members (Hobolt, Leeper, and Tilley 2018). Recent evidence also suggests that, just like partisan identity, this 'Brexit' identity impacts perceptions of the national economy (Hobolt, Leeper, and Tilley 2020; Sorace and Hobolt 2020).

In this paper, I consider the role of partisan and Brexit identity in shaping the perceptions of UK citizens. Specifically, I focus on perceptions of the UK's performance during the COVID-19 pandemic, as compared to other countries. The comparison aspect is

important, and marks a novel contribution to the literature on perceptual bias. While past research has made clear the role of social identities in affecting perceptions of performance, the focus has always been a domestic context. However, competence evaluations are not made in a vacuum; global context matters to voters (M. A. Kayser and Peress 2012). COVID-19 affected countries across the world, and many faced similar difficulties to the UK in dealing with the pandemic. Assessing whether party and Brexit identity affect not only national, but also international evaluations of pandemic performance, is therefore important for fully understanding how these social identities affect the basic mechanism of democratic accountability.

I conduct my analysis using data from the British Election Study Internet Panel. The dependent variables are derived from a question included in the survey during the pandemic which asked respondents to rate the COVID-19 performance of the UK and six other countries. I exploit the panel structure of the data to measure social identity before the pandemic began, ensuring that the findings do not reflect reverse causality. I also control for a range of variables that could confound the relationship between partisan / Brexit identity and UK performance evaluations, also measured pre-pandemic.

The variables capturing both Brexit and partisan identity are of notably high quality, increasing confidence in the findings of the paper. The variables reflect a measurement strategy first developed in social psychology, which involves asking respondents a series of questions directly relating to feelings of in-group bias, and then averaging across the responses on these items.

The results show clearly that perceptual bias arising from both partisan and Brexit identity affected how British citizens evaluated their country's performance with respect to the COVID-19 pandemic. In line with my expectations, partisanship and Brexit identity were both associated with evaluations of the UK's relative performance. Those towards the government and the Leave ends of the identity scales were significantly more positive about the UK's relative performance. The effect of Brexit identity is arguably quite

surprising, given that the UK voted to leave the EU nearly four years before the COVID-19 pandemic, and officially left the bloc at the end of 2019, though the enduring nature of Brexit identities has been noted in existing scholarship (Sivathasan 2021).

In the case of partisanship, the effect is due to differing perceptions of the UK's performance rather than perceptions of other countries, which did not systematically vary according to an individual's partisan identity. Brexit identity, in contrast, is associated with both perceptions of the UK and perceptions of the other countries asked about in the survey. Respondents towards the Leave end of the scale were significantly more negative about the performance of EU and non-EU countries. The perceptual bias resulting from Brexit identity is therefore revealed to have an international component. This finding is a novel contribution to a literature that has, to date, focussed exclusively on perceptual bias with regards to domestic performance.

Theory and Hypotheses

The classic theory of democratic accountability holds that voters evaluate the outcomes of the government's policies and subsequently cast their ballots based on how well the government has performed (M. Fiorina 1981; Key 1966; Powell 2000). This model is particularly apt in the case of so-called 'valence issues' where the electorate broadly agrees on what constitutes a desirable outcome (Clarke et al. 2004, 2009; Stokes 1963).¹ For example, it is well established that voters reward governments based on the state of the economy, as measured by both objective measures (Dassonneville and Lewis-Beck 2014; Kramer 1971; Markus 1992) and subjective measures (M. P. Fiorina 1978; Kinder and Kiewiet 1981; Lewis-Beck and Stegmaier 2000).

This model of electoral accountability is potentially threatened if voters struggle to ac-

1. Valence issues stand in contrast to 'spatial issues' in which the desirable end-point is itself contested (Downs 1957).

curately perceive real world conditions without bias.² The psychological literature on cognitive dissonance provides good reasons to be sceptical about whether voters are able to perform this function well. When individuals hold beliefs that are partially or wholly inconsistent, or ‘dissonant’, they experience psychological discomfort (Cooper 2007; Festinger 1962). One way of alleviating this discomfort is to search for information that allows them to reconcile these beliefs. For example, a person who knows the health risks associated with smoking but is aware that they themselves smoke frequently is likely to experience cognitive dissonance. As a result, they may attempt to mitigate this dissonance by reading and accepting the conclusions of scientific studies which cast doubt on the health risks associated with smoking (Festinger 1962). One implication of this theory is that individuals reason not only with the goal of reaching accurate conclusions about the world, but also of reaching conclusions that accord with their other prior beliefs in order to reduce cognitive dissonance. Research has repeatedly demonstrated that people reason in this biased manner (Kunda 1990; Mercier and Sperber 2017). The tendency to twist facts to accord with prior opinion is highly prevalent in the context of political opinion (Leeper and Slothuus 2014; Lodge and Taber 2013; Taber and Lodge 2006).³

Social identities are one feature of contemporary politics likely to result in cognitive dissonance and, as a result, biased perceptions among many voters. All people belong to multiple social groups, like race and class, whether or not they are aware of these group memberships. If a person self-identifies as a member of a group *and* attaches emotional significance to that membership, then this constitutes a ‘social identity’ (Tajfel 1981). Individuals who have a social identity view their group more positively because part of their self-esteem is tied to the identity (Tajfel 1978a). When these individuals are exposed to information that does not conform to their rose-tinted view of their in-group, they are left holding competing beliefs which they are motivated to reconcile. One way

2. Though for the argument that ill-informed electorates can produce better outcomes than informed electorates, see Ashworth and Mesquita 2014.

3. It should be noted that tendency to twist new information in service to prior opinions is not totally unbounded (Groenendyk 2013; Lavine, Johnston, and Steenbergen 2012; Lebo and Cassino 2007).

of reconciling the beliefs is to find reasons to disregard the evidence against their in-group. Indeed, they are fairly likely to have avoided the negative information in the first place, since individuals will tend to seek out information that reduces rather than increases cognitive dissonance (Festinger 1962). In this manner, cognitive dissonance provides a clear theoretical link between social identities and perceptual bias.

Applying this theory to a political context, it is clear that individuals who view politicians as part of their in-group have a motivation to seek out and agree with information that those politicians are performing well. Likewise, when these in-group politicians publicly advocate for certain policies, like Brexit, there is a motivation to view the policies as successful. In the case of the UK, the two most important social identities in the modern political context are partisan identity and Brexit identity. These identities are the main variables of interest in this paper. In the next two subsections, I briefly outline these identities and highlight the myriad ways in which they affect how voters perceive national and government performance, particularly with regards to the economy. The overall conclusion of existing research accords with the theory just explicated; voters are more positive about national performance when members of their in-group are in power.

Partisan Identity

Partisan identity can be considered an archetypical social identity (Greene 1999, 2004; Huddy, Mason, and Aarøe 2015). According to the Michigan school of thought, partisan identity is a psychological attachment to a political party that develops during one's formative years via parental socialisation (Campbell et al. 1960). It leads to a strong 'homing' instinct at elections, with partisans tending to vote for their party unless they perceive a very good reason not to do so (Converse 1969).⁴

4. Note that there is an alternative view of partisan identity, in which the affiliation is not a psychological attachment but rather a running tally of how well the party has performed over time, offering a useful heuristic to voters for how well that party would govern and thereby providing the 'homing instinct' to that party at elections (M. Fiorina 1981).

Partisans find their self-esteem connected to the success of their party, and will tend to view the world with a degree of bias, interpreting new information in ways that reflect favourably on it (Flynn, Nyhan, and Reifler 2017; Lodge and Taber 2013). Specifically, partisan identifiers evaluate the economy more positively when their party is in government (Bisgaard 2015; Enns, Kellstedt, and McAvoy 2012; Enns and McAvoy 2011; Evans and Pickup 2010), prefer policies associated with their favoured party (Colombo and Kriesi 2017), perceive candidates from their own party more positively (Bartels 2002b; Johnston, Hagen, and Jamieson 2004) and are less likely to blame the government for negative outcomes if their party holds the reigns of power (Bisgaard and Slothuus 2018; Healy, Kuo, and Malhotra 2014; Malhotra and Kuo 2008; Rudolph 2003b). There is also solid evidence of affective polarisation, whereby partisans view other partisans more negatively and co-partisans more positively, in America (Iyengar et al. 2019) and beyond (Wagner 2021).

The importance of partisan identity in British politics has long been understood (Butler and Stokes 1969),⁵ and studies have replicated in Britain the findings from America and other countries. For example, Tilley and Hobolt (2011) use an experimental approach to show that government partisans are more positive about the UK's recent economic performance when they are primed by a statement that the government is responsible for this performance. Using a panel data approach, Marsh and Tilley (2009) show evidence that government partisans are more willing to credit the government for positive outcomes than opposition partisans. Despite the continued relevance of partisan identity in UK politics, the prevalence and average strength of identification has fallen over recent decades (Dalton 2019; Heath 2018), leading to an increasingly volatile electorate (Fieldhouse et al. 2021). It is into this context of declining partisan identity that a new political social identity has emerged: Brexit identity.

5. One caveat is that partisan identity in the UK seems less stable than in America; British voters are more likely to change their partisan identity when they change their vote, partially undermining the notion that identity is distinct from voting intention (Butler and Stokes 1969). However, it is hard to be certain of whether this lack of stability is caused by differences in survey question order (Heath and Pierce 1992), differences in question wording (Sanders, Burton, and Kneeshaw 2002), or because there is genuinely less attachment to parties in Britain.

Brexit Identity

In June 2016, the UK held a referendum to determine whether the country should withdraw from the European Union (EU). The country voted by a slim majority of 52% to leave. Soon after the vote, people began to identify themselves in surveys as ‘Leavers’ or ‘Remainers’ (Curtice 2018; Hobolt, Leeper, and Tilley 2018). Indeed, when a question is asked in the format of the partisan identity question,⁶ more individuals claim to have a Remain or Leave identity than claim to have a partisan identity (Hobolt, Leeper, and Tilley 2020). Additional questions that are well established as measures of social identities, such as ‘When I speak about the (Remain / Leave) side, I usually say “we” instead of “they”’, reveal a similar pattern, suggesting that the apparent emergence of a new identity is not purely a linguistic matter (Evans and Schaffner 2019).

Very little research to date has examined the extent to which Brexit identity creates perceptual biases in the same way that partisan identity does. There are certainly differences between Remain and Leave identifiers with regards to policy stances. For example, Brexit identity was correlated with preferences concerning the negotiation of the UK’s exit from the EU (Curtice 2018). Likewise, there is some evidence that perceptions of whether the recent economic situation in the UK has improved or worsened is associated with Brexit identity, even after controlling for partisan identity (Hobolt, Leeper, and Tilley 2020). However, these studies leave unclear whether the perceptual differences reflect a causal effect of Brexit identity, or whether the association is confounded by other variables. Only one paper has attempted to establish a causal relationship. The researchers used panel data and a survey experiment to show that Remainers became more pessimistic about the UK economy after the referendum whilst Leavers continued to be fairly optimistic, and that priming respondents to think about their referendum

6. In the BES this takes the form of ‘In the EU referendum debate, do you think of yourself as closer to either the ‘Leave’ or ‘Remain’ side? If yes, which one?’ or, in later waves, ‘Thinking about the EU referendum, do you think of yourself as closer to either the “Leave” or “Remain” side? If yes, which one?’ The partisan identity equivalent is: ‘Generally speaking, do you think of yourself as Labour, Conservative, Liberal Democrat or what?’

in-group enhanced these biased perceptions (Sorace and Hobolt 2020). No research, however, has used panel data methods to consider whether Brexit identity causally conditions perceptions of non-economic performance indicators, particularly during a period when the UK had already left the EU.

COVID-19 as a Competence Issue

On 21 December 2019, some patients in the Wuhan region of China were recorded as suffering from a 'pneumonia of unknown cause' (Aspinall 2022). It later became apparent that these patients were in fact suffering from an infectious disease caused by the SARS-CoV-2 virus; COVID-19. This new 'coronavirus' spread across China and, soon after, the world. Many countries rushed to close their borders and curtail the freedom of their citizens in an effort to curb the spread of the virus (BBC News 2020b). The UK government soon instigated a 'lockdown', using police to shut down public places and requiring all individuals to stay within their own homes except for essential movements like shopping or exercising (UK Government 2020).

The UK is generally agreed to have performed relatively well on some areas of pandemic response, and fairly poorly on others. Britain's initial response was compared unfavourably to other countries on a number of grounds, such as the slow speed with which the first lockdown was implemented (Scally, Jacobson, and Abbasi 2020). The UK's rollout of the COVID-19 vaccine was, in contrast, praised for being one of the best and fastest in the world (Baraniuk 2021).

The COVID-19 pandemic provides an excellent case study for investigating the relationship between social identity and evaluations of government performance. The majority of research into the area of perceptual bias in politics has focussed on the performance of the economy. This is because the economy is: (a) a valence issue where voters can agree on a number of desirable outcomes, like low unemployment, meaning that differing perceptions reflect bias rather than ideological disagreement (b) an issue with clear

factual indicators, like GDP and inflation rates, meaning that voters should in theory be able to agree on the objective performance and (c) a highly salient matter about which voters care deeply, meaning that voters should be aware of the basic factual indicators and therefore able to judge the performance. The point about salience also makes the topic more intrinsically interesting, since biased perceptions of salient issues are more likely to have actual impacts on electoral outcomes. Issues other than the economy can become salient to voters and affect electoral outcomes (Green and Jennings 2012, 2017; Meyer and Müller 2013) and it is therefore important to understand whether voters exhibit perceptual bias in the context of salient non-economic issues.

The COVID-19 is a non-economic event characterised by the three desiderata that lead so many political scientists to study economic evaluations. Voters in the UK broadly agreed on the extent to which the government should prioritise fighting the virus at a cost to the economy and to personal freedom, and the pandemic therefore acted as a valence issue in UK politics (Green, Evans, and Snow 2020)⁷. The pandemic had clear factual indicators for voters to grasp and understand, like death and vaccination rates, hospitalisations, and severity of lockdown measures. The pandemic was a highly salient matter to voters, with health being cited as the most important problem facing the country in 60% of YouGov's surveys between 2020 and 2022 (YouGov 2023). The COVID-19 pandemic therefore offers an excellent chance to see whether the partisan and Brexit biases that cloud voter judgements of the economy have an equal effect in the context of a highly salient public health crisis.

Relative Evaluations

It is important to focus on voters' perceptions of the UK's performance during the pandemic *relative* to their perceptions of other countries. Voters are not blind to the diffi-

7. This stands in contrast to the United States, where the pandemic can also be considered a positional issue, with issues concerning the appropriate pandemic responses were highly politicised (Bolsen and Palm 2022).

culties posed by external events beyond the control of the government. For example, scholarship on retrospective economic voting suggests that voters take global context into account when voting on valence issues (Aytaç 2018b, 2020; M. A. Kayser and Peress 2012).⁸ External shocks also play a role in affecting whether voters reward or punish governments (Ahlquist, Copelovitch, and Walter 2020; Campello and Zucco 2016), but the role of luck in terms of global conditions coincides with the role of comparative performance rather than entirely replacing it (Leigh 2009).

COVID-19 is a clear case in which voters are likely to evaluate performance indicators in light of the global context. The origin of the virus was not the fault of the UK government, and caused negative outcomes like excess deaths across the world (Our World in Data 2023). Indeed, many governments benefited from increased support in the early months of the pandemic due to a ‘rally round the flag’ effect (Baekgaard et al. 2020; Johansson, Hopmann, and Shehata 2021; Kritzinger et al. 2021), suggesting awareness on the part of citizens that national governments were dealing with a difficult situation for which they were not to blame. Accordingly, the important measure of performance that is likely to be of most electoral relevance is whether people believe that the UK performed *comparatively worse than other countries* during the pandemic. If the analysis focussed only on absolute values, this would leave open the possibility that some respondents were simply more negative about COVID-19 in general, rather than specifically about the UK’s performance compared to the global average.

Hypotheses

Applying the theory from the literature outlined above to the context of the COVID-19 pandemic yields two straight forward hypotheses. First, and most simply, is the expectation that government partisans have a strong incentive to perceive the UK’s experience of the pandemic in a positive light compared to other countries, whilst opposition parti-

8. For a criticism of the analytical strategy behind these empirical findings, see Arel-Bundock, Blais, and Dassonneville (2019), and for responses see M. Kayser and Peress (2019) and Aytaç (2018).

sans have the opposite incentive. If the UK is perceived to have performed well then this reflects well on the party in which they have invested part of a government partisan's self-esteem, whilst the opposite is true for an opposition partisan:

H1: A stronger sense of government (opposition) partisanship is associated with a more positive (negative) evaluation of the UK's COVID-19 performance, relative to other countries.

For Brexit identity, Leave identifiers had an incentive to perceive the UK as performing comparatively well. The then Prime Minister, Boris Johnson, was strongly identified with the Leave side of the EU debate both before and after the referendum (Farrell and Goldsmith 2017), as were many of his cabinet (The i 2023), meaning that in-group bias should have resulted in a belief that the UK handled the crisis well. Additionally, the terms of the UK's exit from the European Union were negotiated by these staunchly pro-Leave advocates, providing further motivation for Leave voters to believe that a post-Brexit Britain performed well on the world stage. The converse is true for Remain identifiers:

H2: A stronger sense of Leave (Remain) identification is associated with a more positive (negative) evaluation of the UK's COVID-19 performance, relative to other countries.

Note that I only hypothesise about whether Brexit identity causes biased perceptions of the UK relative to the average of all other countries, not the average of just EU countries. Evaluating the UK's pandemic performance in the wake of a Brexit deal delivered by high profile members of the Leave camp relates to Britain's position in the world, not just its position in relation to Europe. If Britain is doing better than countries like Korea, as well as European countries like Germany, then this reflects well on the Leave in-group, and Leave identifiers in the electorate therefore have a motivation to perceive the UK's performance as better than European and non-European countries alike.

Analytical Strategy

Survey Data

To test my hypotheses, I use data from the British Election Study Internet Panel (BESIP), a survey administered by YouGov (Fieldhouse et al. 2022). I avoid issues of reverse causality by measuring all independent variables before the COVID-19 pandemic, making use of BESIP survey wave 17. This wave was fielded just prior to the 2019 general election, between 1 and 12 November 2019, and contains 34,366 respondents when not accounting for missing values or panel attrition.⁹ For the measurement of the dependent variables I use wave 21, which was fielded between 7 and 21 May 2021, during the pandemic. This wave contains 30,281 respondents, again not accounting for sample attrition or missing values.

The pre-pandemic wave, in which the independent variables are measured, occurred before COVID-19 had even been discovered in China. Wave 21, at which point the respondents were asked to evaluate the UK's performance, occurred after successive lockdowns, thousands of COVID-19 related deaths, and the start of the UK's vaccine roll-out, but months before the pandemic could be considered to have 'ended'. The timing is therefore ideal to see how voters rated the UK's performance, because the issue remained highly salient and widely reported on by the media, but was also old enough for voters to have accumulated a wealth of information and experience to draw on when making their judgements.

9. Panel retention rates are around 60% wave-on-wave, though some individuals drop out of consecutive waves and return to later waves in the survey.

Dependent Variables

The dependent variables are derived from respondents' answers to the question: 'How do you rate each of these countries' responses to the coronavirus outbreak?'. Answers were given on a scale from zero ('very bad') to 100 ('very good') for seven countries presented to respondents in a randomised order; China, Germany, Italy, South Korea, Sweden, the UK, and the USA. Figure 5.1 shows the distribution of scores for each of these variables relative to the UK.

The main variable of interest for my analysis is each respondent's score for the UK minus their mean average rating of the other countries. Respondents who answered 'don't know' to all of the evaluation questions were dropped from the analysis, but respondents who only answered 'don't know' to some questions were imputed to the mid-point (50) for those countries. While this measure allows for a simple assessment of whether partisan and Brexit identities affect how respondents perceive the UK's performance compared to the rest of the world, I also break these scores down further in a series of robustness checks later in the article.

Independent Variables

There are two independent variables of interest, partisan identity and Brexit identity. It is central to the argument of this paper that social identity, not only attitudes to Brexit or the government, affects an individual's evaluation of the UK's COVID-19 performance. High quality measurement of social identity is therefore crucial to the empirical validity of the analysis. Fortunately, the British Election Study contains a number of questions that allow for a highly reliable measure of social identity that was first developed and tested in the field of social psychology, and which involves asking respondents to agree or disagree with a number of statements about their feelings towards the in-group and out-groups of interest (Huddy 2013). These questions therefore allow for a scale of social

identification, reflecting the strength with which an individual feels personally attached to their party or Brexit side, rather than simplistic binary measures. Though note that I also conduct a robustness check where I do not use the strength element and the results, found in Table A5.6 of the Appendix, also show independent effects of both partisan and Brexit identity.

I standardise the social identity variables across the sample, by dividing by twice the standard deviation, so that the coefficients are made directly comparable and I can ascertain the importance of Brexit identity relative to partisan identity.¹⁰

In the case of partisan identity, respondents were asked about the following statements: 'Earlier you said that you tend to identify as *Party ID*. Thinking about this party, how much do you agree with these statements?'

1. When I speak about this party, I usually say "we" instead of "they".
2. I am interested in what other people think about this party.
3. When people criticize this party, it feels like a personal insult.
4. I have a lot in common with other supporters of this party.

For each item respondents selected from the following list of responses: 'strongly agree', 'agree', 'disagree', 'strongly disagree', or 'don't know'. I created a five-point score based on these responses, ranging from 'strongly disagree' (1) to 'strongly agree' (5), with answers of 'don't know' assigned to the middle of the scale (3). If a respondent answered 'don't know' to all questions, they were dropped from the analysis. These scores were averaged within each respondent, and given a direction based on which party the individual identified with, based on answers to the question: 'Generally speaking, do you think of yourself as Labour, Conservative, Liberal Democrat or what?' The Conservatives were in

10. For an explanation of why it is generally preferable to scale by twice the standard deviation, see Gelman (2008).

power throughout the pandemic, so I code Conservative partisans as government partisans (a positive party identification) and identifiers of any other party as opposition partisans (a negative party identification). A score of zero was given to those without a partisan identity, though note that dropping respondents who did not have a partisan identity does not affect the results, as is shown in the robustness checks. When separating the sample into those with a government and an opposition identity, Cronbach's alpha for the composite scores is 0.764 and 0.795 respectively.

The measurement of and coding process for Brexit identity is very similar to that of partisan identity. The question to ascertain the direction of the identity is: 'In the EU referendum debate, do you think of yourself as closer to either the 'Leave' or 'Remain' side? If yes, which one?' Remain identifiers then received positive scores and Leave identifiers negative scores on a scale constructed from responses to the following items:

1. When I speak about the *Brexit ID* side, I usually say "we" instead of "they".
2. I am interested in what other people think about the *Brexit ID* side.
3. When people criticize the *Brexit ID* side, it feels like a personal insult.
4. I have a lot in common with other supporters of the *Brexit ID* side.
5. When I meet someone who supports the *Brexit ID* side, I feel connected with this person.
6. When people praise the *Brexit ID* side, it makes me feel good.

When separating the sample into those with a Leave and a Remain identity, Cronbach's alpha for the composite scores is 0.895 and 0.883 respectively. The correlation between the directional strength scales of partisan and Brexit identity, coded as outlined above, is 0.51, when measured in November 2019. Though sizeable, this should not cause any issues in terms of multicollinearity within the model. The fact that there is only around a 0.5 correlation also emphasises that these are distinct social identities in British politics.

Control Variables

It is important to control for any variables that are plausibly correlated with both the identity measures and causally prior to the country evaluations.

All models reported in this paper therefore control for age, ethnicity, gender, education, and position on the left-right and liberal-authoritarian scales, measured pre-pandemic. For reasons of space the tables below do not display the coefficients for these control variables, but the full model tables can be found in A5.3 of the Appendix. Below, I also check that the results are robust to the inclusion of a number of other control variables, showing that the findings are not affected by controlling for class, media consumption, preferred daily newspaper, preferences on COVID-19 policy, or national identity. Full details about the coding of all control variables variables can be found in Section A5.2 of the Appendix.

Method

The method used is simple ordinary least squares regression of the COVID-19 performance evaluations on the standardised measures of identity. I display the results of including just one of the identity variables in the model at a time, as well as the version with both included. This allows for an informal investigation of whether the measures overlap in their effects; if the coefficients are substantially reduced in the full model then this implies that the effects of each identity are not independent of each other.

Results

Table 5.1 shows the main results of the analysis, using partisan and Brexit identification as continuous variables to explain evaluations of the UK's performance relative to other

countries. To recapitulate, these evaluations reflect the difference between the score given by respondents to the UK and an average of the scores they gave to the other countries asked about. Positive figures on this variable therefore indicate more positive assessments of the UK's relative performance.

Table 5.1: Results from an OLS regression of UK relative performance scores on partisan and Brexit identity scales, with controls included but not shown

	<i>Dependent variable: UK relative score</i>		
	1	2	3
Pro-Gov Partisan ID (scale)	7.86*** (0.56)		4.87*** (0.57)
Pro-Leave Brexit ID (scale)		12.52*** (0.57)	11.14*** (0.59)
Controls inc.	✓	✓	✓
R ²	0.29	0.31	0.31
Adj. R ²	0.29	0.31	0.31
Num. obs	9423	9423	9423

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: All continuous independent variables have been standardised (divided by two times the standard deviation) to make coefficients comparable. The dependent variable was measured in May 2021, and all independent variables in November 2019 (before the pandemic). Higher values on the partisan identity scale equate to pro-government (Conservative) partisan strength, low values to pro-opposition (Labour / other) partisan strength. Higher values on the Brexit identity scale relate to stronger Leave identity, and lower values to stronger Remain identity. Non-identifiers were assigned to the midpoint of the respective scale. All models include controls for age, education, gender, ethnicity, and position on the left-right and authoritarian-libertarian scales. The coefficients for these variables can be found in Table A5.2 of A5.3 in the Supplementary Material.

The first point to note is the relatively high adjusted R² values across all three models, indicating that the model is a relatively good fit for the data (Ozili 2022). This is partly because of the additional controls, but should increase confidence that the measures of the UK's relative score is not purely noise; a full 31% of the variation in scores of the UK compared to other countries can be explained by the parsimonious model fitted in this paper.

Turning to the coefficients, there is strong support for Hypothesis H1. A one standard

deviation on the partisan ID scale is associated with a 5 point change in a respondent's evaluation of the UK's relative COVID-19 performance, and this relationship is statistically significant at the 1% level. The more towards the government end of the scale the more positive they felt about the UK's relative performance. Or, equivalently, the more towards the opposition end of the scale they were the less positive they felt about it.

The coefficient on Brexit identity is also positive and also significant, providing support for Hypothesis H2. Indeed, this coefficient is over twice as large as the coefficient on partisan identity, indicating a strong relationship between an individual's Brexit identification and their positivity about the UK's performance during the COVID-20 pandemic five years after the referendum. The more towards the Leave end of the scale an individual was placed the more positive they were about the UK's comparative performance. Or, again, the equivalent formulation is that the more towards the Remain end they were the less positive they were in their evaluation.

It is also interesting to note that the two identities seem to be somewhat orthogonal in their associations with the UK's score relative to other countries, as can be seen by comparing the coefficients in Column 1 and 2 with the coefficients from the combined model in Column 3. Controlling for Brexit identity only reduces the coefficient on partisan identity by around 3 points, a reduction of less than fifty percent, whilst the coefficient on Brexit identity is only reduced by about 1.5 when controlling for partisan identity. There are therefore two distinct and independent associations between these identities and perceptions of the UK's COVID-19 performance.

Positive About the UK or Negative About Other Countries?

Do the differences by partisanship and Brexit identity shown above predominantly reflect perceptions of the UK, or perceptions of the other countries in the sample? Though I did not offer explicit hypotheses concerning this question, it is worth unpacking the results in this respect to make the empirical picture more complete. Table 5.2 shows

the effect of partisan and Brexit identity on the absolute scores for the UK, and on the average of the EU countries and the non-EU countries asked about in the survey.

Table 5.2: Results from an OLS of performance scores for the UK, EU and non-EU countries regressed on partisan and Brexit identity scales

	<i>Dependent variable:</i>		
	UK	EU average	Non-EU average
Pro-Gov Partisan ID (scale)	5.42*** (0.55)	-0.39 (0.36)	0.72 (0.38)
Pro-Leave Brexit ID (scale)	9.14*** (0.57)	-1.70*** (0.37)	-1.70*** (0.40)
Controls inc.	✓	✓	✓
R ²	0.32	0.02	0.04
Adj. R ²	0.32	0.02	0.04
Num. obs	9722	9053	9400

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: All continuous independent variables have been standardised (divided by two times the standard deviation) to make coefficients comparable. The dependent variables were measured in May 2021, and all independent variables in November 2019 (before the pandemic). Higher values on the partisan identity scale equate to pro-government (Conservative) partisan strength, low values to pro-opposition (Labour / other) partisan strength. Higher values on the Brexit identity scale relate to stronger Leave identity, and lower values to stronger Remain identity. Non-identifiers were assigned to the midpoint of the respective scale. All models include controls for age, education, gender, ethnicity, and position on the left-right and authoritarian-libertarian scales. The coefficients for these variables can be found in Table A5.2 of A5.3 in the Supplementary Material.

The relationship between partisanship and perceptions of the UK's relative performance appears to be driven entirely by the fact that those towards the government end of the partisanship scale are more positive about the UK's absolute performance. There is no significant relationship between partisanship and perceptions of the EU or non-EU country groups, when controlling for the basic demographics that the models include.

Conversely, Brexit identity is significantly associated with ratings of these other countries. The largest coefficient for Brexit identity concerns perceptions of the UK, showing that this is the primary driver of the positive relationship between being more identified with the Leave side and evaluating the UK's relative performance positively. But there is also a significant relationship with both the EU and non-EU averages. A one

standard deviation movement towards the Leave end of the Brexit identity scale is associated with a 1.70 decrease in the positivity, on the 100 point scale, about the EU and non-EU countries in the sample.¹¹ Leave identity not only leads to positive perceptions of the UK but to negative perceptions of other countries too.

Comparisons by Country

The countries in the sample had varying approaches to dealing with COVID-19, and it is certainly the case descriptively that perceptions of the other countries' relative performances differed widely within the sample, as Figure 5.1 shows.¹²

For example, there appears to be an overall consensus that the United States performed worse than the UK, with the vast bulk of the distribution in the negative half of scale, but a far more dispersed range of opinion on South Korea's performance compared to the UK. There is sufficient variation in the distribution of scores across countries to suggest that most respondents were not simply assigning all countries to the mid-point of the scale. It's also notable that the distribution is not bimodal for any of the countries concerned. It is therefore not the case, for example, that all Remainers are much more negative about Germany relative to the UK while all Leavers are much more positive.

It therefore makes sense to consider whether the two social identities under investigation relate to the raw, not relative, evaluations of the seven countries asked about. Figure 5.2 shows the coefficients from a set of models where the dependent variable is the absolute score for each given country in turn.

The first point to note is that the models reaffirm the general point that partisan identity is associated with perceptions of the UK's performance in absolute terms, but not particularly associated with perceptions of the other countries. Those towards the gov-

11. To recapitulate, the EU countries asked about were Germany, Italy, and Sweden, and the non-EU countries were China, Korea and the USA.

12. For a graph showing the distribution for the other two post-pandemic survey waves, see Figure A5.1 in the Appendix.

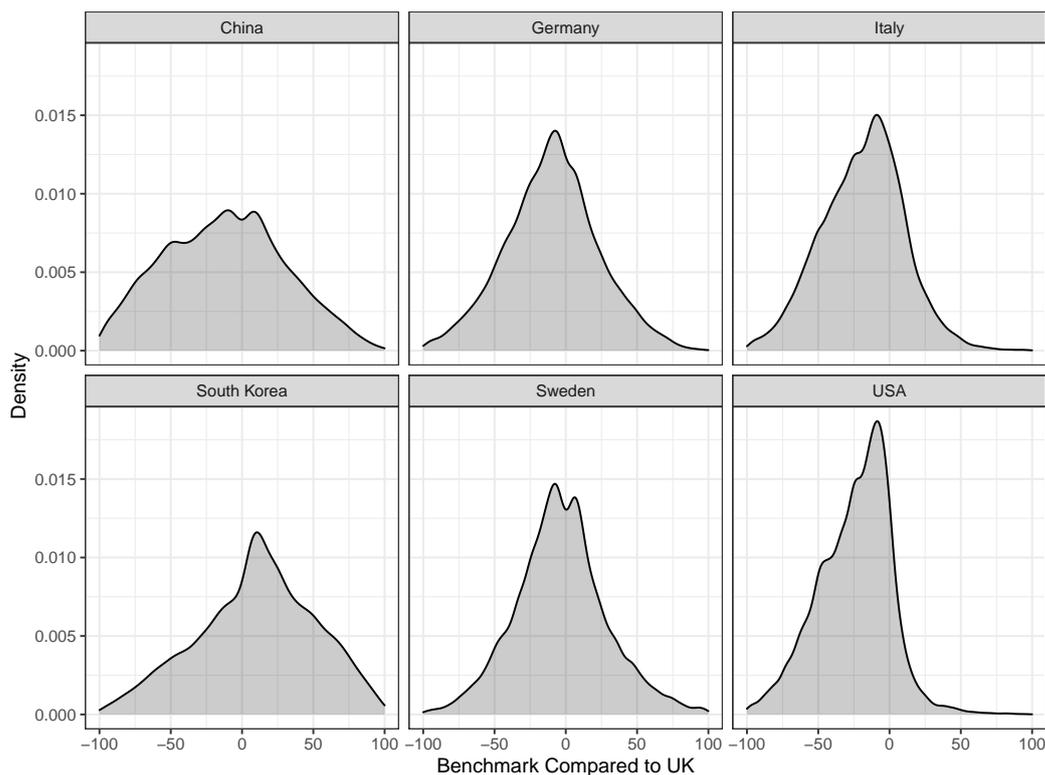


Figure 5.1: Distribution of country scores relative to the UK in May 2021

Note: Positive values indicate more positivity about a given country compared to the UK. The score is calculated by subtracting the UK's absolute score (on a scale of 0-100) from the country in question's absolute score (also on a scale of 0-100). The x-axis above therefore ranges from -100, for those respondents who rated the UK as 100 and the country in question as 0, to 100, for those respondents who rated the UK as 0 and the country in question as 100.

ernment end of partisanship scale are slightly more positive about the performance of Korea and the USA, but these coefficients are very small and there is no theoretical reason for expecting this result, so these can perhaps best be considered small quirks of the data rather than major empirical findings.

Brexit identity, in contrast, has a significant association with perceptions of every country in the sample. Those towards the Leave end of the scale have significantly less positive evaluations, in absolute terms, of the performance of China, Germany, Italy and Korea, and significantly more positive views of Sweden, the USA, and the UK.

The obvious intuition for this pattern is that the results reflect sentiments about the most appropriate policy response to COVID-19. The countries about which those to-

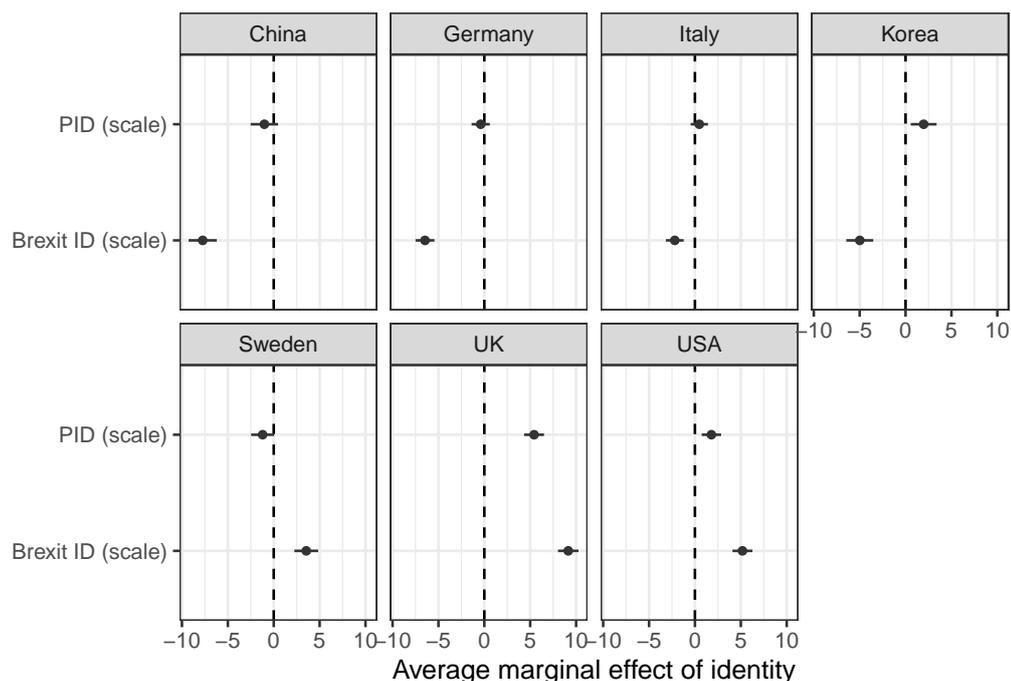


Figure 5.2: Results from an OLS regression of absolute country scores on the party and Brexit identity scales

Note: Positive values indicate a more positive evaluation of a country's performance. Government partisans are those individuals with a Conservative partisan identity, whilst opposition partisans are those who identify with any other UK political party. All models include controls for age, education, class, gender, ethnicity, and position on the left-right and authoritarian-libertarian scales, measured pre-pandemic. The coefficients for these variables can be found in Tables A5.4 and A5.5 in of the Appendix.

wards the Leave end of the scale are more negative tend to be those countries that appeared more willing to curb individual freedom in order to tackle the virus. Conversely, the countries about which those towards the Leave end of the scale felt more positive took the opposite approach. For example, Sweden leant heavily towards voluntary rather than mandatory restrictions (Pashakhanlou 2021), whilst China instigated an extreme 'zero Covid' policy (The Guardian 2022). Accordingly, it might be thought that Brexit identity is associated with the degree to which an individual has a pro-freedom stance on the best approach to tackling COVID-19. Given how central the question of 'sovereignty' was to the Brexit campaign, as well as the idea that Britain can buck international trends to its benefit, this seems a plausible explanation.

However, the Brexit coefficient for each of these models is almost entirely unchanged

by adding a variable to the model that directly captures COVID-19 policy preferences in terms of a willingness to trade-off freedom in order to save lives (see Section A5.4 in the Appendix). Also note that the models already include controls for age, education, gender, ethnicity and position on the left-right and authoritarian-libertarian value scales, all variables likely to be correlated with an individual's COVID-19 preferences. Therefore, the pattern cannot be simply explained in this way, though pursuing an investigation of this pattern further is both beyond the data available and beyond the scope of this paper.

Robustness Checks

I checked the results under a number of alternative model specifications. The coefficients for the main independent variables under these alternative model specifications can be found in Figure 5.3.

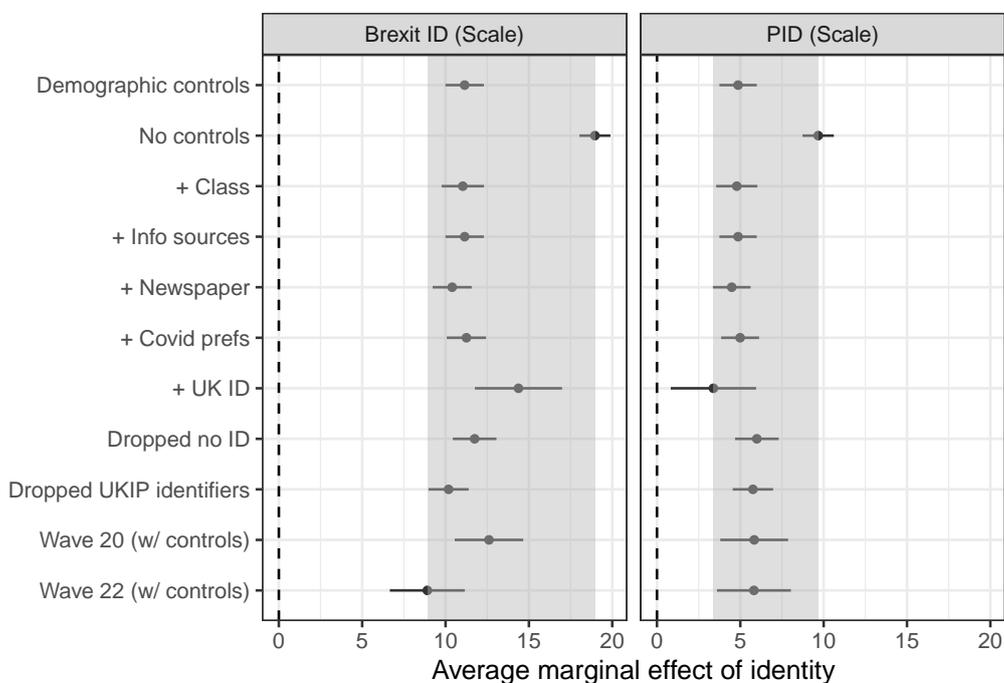


Figure 5.3: The effect of model specification on the coefficients of social identity on the UK's performance, relative to six other countries

Note: For more details of the alternative specifications, see Table A5.7 of the Appendix.

The main finding of the analysis, that those towards the government and Leave ends of the party and Brexit identity scales were more positive about the UK's relative performance on COVID-19, is robust to the inclusion of a range of various controls. These include: (a) class, measured by occupation, (b) exposure to media and other news sources, measured by self-reported daily use of various information sources, (c) the party affiliation of a respondent's preferred daily newspaper, (d) the respondent's preferences with regards to reducing COVID-19 at the expense of national economic prosperity, and (e) the respondent's sense of national identity, measured with a similar social psychology scale in wave 19 to the one used for the two other identity strength measures used in this paper. Furthermore, the findings are unchanged when excluding UKIP / Brexit Party identifiers from the sample, or when removing those individuals who have no partisan identity / no Brexit identity. The results are also replicated when measuring the dependent variable in June 2020 (Wave 20) or in December 2021 (Wave 22).

For more details of the alternative specifications, see Table A5.7 in the Appendix.

Discussion

In this paper, I used British panel data to show that both partisan and Brexit identity exerted independent effects on voters' perceptions of their country's experience of the COVID-19 pandemic. In line with theories of cognitive dissonance and social identity theory, the more an individual felt identified with the government or the Leave side, the more positively they felt about the UK's performance compared to how they felt about the performance of other countries. Equivalently, the more an individual felt identified with the opposition parties or the Remain side, the less positively they felt about the UK's performance. More than five years on from the actual Brexit referendum, Brexit identity exerted a larger effect than partisan identity on these evaluations of the country's performance. Furthermore, whilst the effect of partisan identity related to evaluations of the UK only, Brexit identity was also associated with a more negative perception

of EU and non-EU countries.

To derive these results, I exploited the panel structure of BESIP data by measuring all independent variables before the pandemic, ensuring that COVID-19 related perceptions did not influence the social identities themselves. To mitigate the risk of omitted variable bias, which would mean the results were produced by a confounding variable correlated with both partisan and Brexit identity and performance evaluations, I included relevant control variables like age, education, and ideological position on the left-right and liberal-authoritarian scales. I also ran a number of alternative specifications with additional controls, like preferences about COVID-19 policy, which did not produce substantially different coefficients on the main variables of interest.

The fact that Brexit identity affected perceptions of COVID-19 is an important finding. Though existing research has shown the importance of Brexit for colouring other perceptions, like the UK's economic performance, it is notable that the identity continued to exert a profound effect over one year on from the UK leaving the EU (BBC News 2020a). Indeed, the coefficients on the Brexit identity scale were actually larger than those on the party identity scale. This result makes clear that social identities which quickly emerge in relation to a divisive political issue do not necessarily disappear as soon as that political issue is resolved.

The findings concerning partisan identity might also be considered somewhat surprising. The literature on partisan bias has shown that partisan bias extends to evaluations of health sector performance (Tilley and Hobolt 2011). However, extreme crises can limit partisan bias. For example, the poor economic signals given during the 2008 financial crisis were clear enough that partisans broadly agreed that things were not going well (Parker-Stephen 2013). COVID-19 was an extreme and salient crisis in the health sector, with a number of converging indicators that the UK was not performing well. In this context, it might have been expected that partisans might have converged in their evaluations of the country's performance. The fact that there was not total convergence

highlights once again the robust nature of partisan bias.

The most notable contribution of this paper is the empirical finding that the perceptual bias created by Brexit identity has an international component. Unlike partisan identity, which only affected the way voters evaluated the UK's performance, Brexit identity was consistently and significantly associated with evaluations of other countries too. An individual who was placed towards the Leave end of the identity scale was more negative, on average, about both EU and non-EU countries than someone placed towards the Remain end of the scale.

To my knowledge, no other study has tested for an international component to perceptual bias, but there are important implications of this result. As I made clear in the theory section of this article, voters are not oblivious to global circumstances, and will evaluate their own country's performance in light of this wider context. The potential of social identities to bias perceptions about both the local and the global context provides twice as many opportunities for voters to arrive at misperceptions of how well their government is delivering important outcomes. Essentially, they are able to exaggerate the global context in a direction that makes their own country's comparative performance even better or worse than it appeared in isolation. Accordingly, democratic accountability is far more threatened by social identities that colour international evaluations as well as national evaluations.

One limitation of this paper is that the data did not allow me to unpack exactly how individuals came to form their judgements of the COVID-19 performance of different countries. At one extreme, the results might simply reflect expressive bias, with respondents choosing scores that reflected their prior party and Brexit identity absence much serious thought. Alternatively, respondents might have had firm ideas about the relative performances of different countries before even beginning the survey, which reflected their own research into the experiences of COVID-19 around the world. In either case, it is clear that the identities resulted in perceptual bias, but the exact process by which

this bias was reached is left opaque.

An additional limitation pertains to the choice of using party and Brexit identity scales rather than categorical variables. The scales, developed in the field of social psychology, were chosen because they are expected to better tap the underlying identification with a person's party or Brexit position, rather than related attitudes like Euroscepticism. The downside, however, is that using a continuous measure requires the assumption that both ends of the scale have equal and opposite effects. I was therefore unable to test whether Leave and Remain identification strength, for example, might affect performance evaluations differently. I leave testing for these possible asymmetries as a task for future research, which would need to use categorical measures of identification that are as valid and reliable as the scales used in this paper.

Future work is needed to build on the findings on this paper. In particular, the international component of perceptual bias warrants further study. The competence of a government must be judged in terms of the global conditions faced by the country, and there is some evidence that voters do indeed account for these global conditions when evaluating performance. However, this is the first study to question whether the perceptual bias that affects performance evaluations might also affect evaluations of the international context too. More research is needed to expand this topic and see whether, for other issues, partisan bias also has an international component, as well as more research testing the electoral relevance of these international comparisons.

The idea that voters observe how the country is performing and, if disappointed, 'throw the rascals out', is a cornerstone of democracy. This paper has shown that multiple social identities can simultaneously limit the ability of voters to perform this vital democratic function. As democracies across the world seem increasingly divided in terms of identity as much as they are by ideas, it has never been more important to study the way in which social identities affect how people perceive reality.

Chapter 6

Conclusion

Before discussing the implications and limitations of this thesis, it is worth briefly summarising the methods and findings from the four empirical chapters.

In Chapter 2, I analysed the effectiveness of traditional campaigning methods in the lead-up to the Brexit referendum. Using propensity score matching, I found that the overall impact of traditional campaign methods was remarkably small. While a person's partisan identity had a significant impact on their receptiveness to campaign messaging, their Brexit vote intention did not. The results contribute to the literature on campaign effects in referendums, the literature on voter responsiveness to political messaging.

In Chapter 3, I used a fixed effects model to show that media exposure was significantly associated over time with an individual's Brexit identity strength. In periods where an individual spent more time consuming media sources than they consumed on average, they tended to have a stronger sense of identification with their side of the Brexit divide. Interestingly, there were independent effects on Brexit identity strength of consuming television, reading newspapers and browsing the Internet.

In Chapter 4, I used panel data analysis and a survey experiment to show that partisan identity affected evaluations of the UK's COVID-19 performance, and the degree to which the government was responsible for this outcome. Compared to opposition par-

tisans, government partisans were found to be more positive about the UK's experience of the virus, less likely to blame the government for this experience, and more positive when recalling the government's past handling of the issue.

In Chapter 5, I used panel data analysis to show that both Brexit and partisan identity had independent effects on how respondents rated the UK's pandemic performance compared to other countries. Interestingly, more than a year after the UK had left the European Union, Brexit identity continued to exert a larger effect than partisan identity on these evaluations. Whilst partisanship only affected evaluations of the UK's performance, Brexit identity was also found to impact evaluations of the pandemic performance of other EU and non-EU countries, revealing an international component to the perceptual bias caused by this social identity.

Taken together, the chapters in this thesis therefore answer important questions about contemporary British politics and, specifically, the role of both Brexit and partisan identity in a country that was divided over the issue of EU membership. The findings show that the division over this highly salient question took time to rival partisanship in political impact at the social level. During the referendum campaign itself, it was a person's party identity that determined their receptivity to campaigners more than their intended vote in the referendum. However, by the time of the coronavirus pandemic, Brexit identity was more important than party identity in affecting perceptions of the UK's performance, though party identity still mattered. While average Brexit identity strength itself remained relatively consistent across this time period, media consumption played an important role at the individual level. People who consumed more media in a particular time period tended to feel more identified with their side of the Brexit divide in that period as well.

The implications of these findings may extend beyond Britain. Electorates in other countries are as liable as the UK to see realignment and new social divisions, particularly as partisanship declines across most Western democracies (Dalton 2019), leav-

ing room for the second-dimension of politics to become increasingly salient (Kitschelt 1994; Kriesi 2008; Norris and Inglehart 2018; Stubager et al. 2021).

The case study of post-Brexit Britain gives rise to some expectations regarding such contexts. First, that the other social identities are likely to co-exist with and cut across partisanship, rather than replace it. Second, that they are likely to endure long after the initial sparking event that caused the division in the first place. Third, that media consumption is likely to play a role at reinforcing these divisions, at least at the level of individual voters if not the level of the electorate as a whole.

The research in this thesis is far from the final word on the subject of Brexit identification, let alone social identification in politics more broadly. For a start, there are a number of limitations of the empirical chapters above that future research could help to address. Though the findings have implications beyond Britain, there may be nuances that apply to Britain and limit the applicability of the findings. In other countries with less entrenched partisanship there may, in fact, be more of a replacement effect with regards to partisan identity than seen in Britain. Alternatively, the new social identities may arise and then fade within a relatively short time span, particularly if politicians do not have the electoral incentive to exploit them in the way that was done so successfully in the UK's 2019 General Election with the issue of Brexit (Ford et al. 2021). Ultimately, sociology is a science of explanation not prediction, and only future scholarship can reveal the extent to which the explanations concerning Brexit identity translate well to other contexts.

An additional limitation of this thesis is that the actual internal experiences of respondents in the survey are left somewhat opaque, as is the case with most quantitative research. To take a very specific example, it is left unclear whether the differences by Brexit identity with regards to COVID-19 performance evaluations reflect a process of deliberate cognitive reasoning, albeit directionally motivated, as opposed to emotive or expressive responding. Did respondents selectively call to mind facts the about dif-

ferent countries that supported the conclusion they had a motivation to reach, or did they rate the different countries without much thought and thereby reveal their subconscious motivations? The answer to this question has important implications for the contexts in which we might expect these effects to apply and the measures that can be taken to reduce these biases. If there is some degree of reasoning underpinning the perceptual biases, then issues about which people have a lot of information may be less prone to misperceptions, and providing more information about the topic might help to reduce the bias. Qualitative research, or experiments with a focus on the internal psychology of responses, might help to narrow down the specific mechanisms driving the survey-level findings outlined above.

Limitations aside, this thesis provides important contributions to the theoretical literature on social identity in politics, and to the more empirical literature on contemporary British politics. When voters evaluate real world conditions with bias, serious questions are raised about electoral accountability. Social identities provide one way in which objective perception can be undermined, and they deserve more research that builds upon the findings from this thesis.

Appendix to Chapter 2

A2.1 Coding Strategy

Dependent Variables

Prior voting intention was coded on the basis of the question: **‘If you do vote in the referendum on Britain’s membership of the European Union, how do you think you will vote?’** (wave 7). Answers of ‘Remain’ or ‘Leave’ were included, whilst ‘Don’t know’ and non-voting respondents were coded as missing.

Actual referendum vote choice, which is the dependent variable in the voting models, was coded from the wave 9 question: **‘Which way did you vote in the EU referendum?’**. Again, answers of ‘Remain’ or ‘Leave’ were included, whilst ‘Don’t know’ respondents were classed as missing.

Expectations about the expected impact of Brexit were created from three related questions asked in waves 7 and 8:

Do you think the following would be higher, lower, or about the same if the UK leaves the European Union?

- Unemployment
- International trade
- Immigration to the UK

- The risk of terrorism
- Britain's influence in the world

Responses were 'much lower', 'lower', 'about the same', 'higher', 'much higher', or 'don't know'.

Do you think the following would be better, worse, or about the same if the UK leaves the European Union?

- Working conditions for British workers
- The general economic situation in the UK
- My personal financial situation
- The NHS

Responses were 'much worse', 'worse', 'about the same', 'better', 'much better', or 'don't know'.

If the UK leaves the European Union, how much more likely is it that...

- Big companies would leave the UK
- Scotland would leave the UK

Responses were 'much less likely', 'less likely', 'about as likely', 'more likely', 'much more likely', or 'don't know'.

Finally, because there was no measure in this battery regarding sovereignty I instead relied on answers to the question 'How much do you agree or disagree that the EU has undermined the powers of the UK parliament?'

For my analysis I centered the variables around 0 and then ensured that higher values always meant a more pro-Remain position, which meant reversing some scales. For example, I did not reverse the unemployment scale because believing that Brexit will lead to more unemployment is a pro-Remain position, but I did reverse the trade variable because believing that Brexit will lead to more trade is a pro-Leave position. Overall I therefore reversed trade, leave voice, workers' rights, the economy and finance scales, and the sovereignty scale.

I then combined responses to the above questions into four separate scales. Trade, economy, finance, and big business were combined into an 'economic' measure. There was only one measure of immigration and sovereignty, so each was its own scale. An 'other' index was created from expectations about terrorism, leave voice, workers' rights and Scottish independence. Unfortunately there were no wave 7 responses to expectations about unemployment or the NHS, meaning that these could not be included in the analysis.¹ Where respondents answered 'Don't know' on a measure they were imputed to the mid-point of that measure, provided that they had a response other than 'Don't know' to at least one of the other measures in that scale.

Campaign Contact

The main independent variable used for my analysis was constructed from a series of questions asked about contact by parties and campaigns.

Firstly, respondents were asked:

'Have any of the political parties contacted you during the past four weeks?'

Those who responded 'no' or 'don't know' were asked no further questions. 'Don't know' responses were classed as missing for my analysis, whilst responses of 'no' resulted in

1. Note that the 'other' index was used for matching purposes, but did not feature in the main analysis.

individuals being classed as 'no' for contact by all parties and campaigns.

Those who responded 'yes' were then asked:

'Please indicate all the political parties that have contacted you during the past four weeks.'

Options were given for all the major parties, for the campaigns of 'Stronger in' and 'Vote Leave', as well as 'another organisation campaigning to leave the EU' and 'another organisation campaigning to remain in the EU'.

An answer of yes to any from that list were asked:

'And which (if any) of those parties contacted you to convince you to remain in the European Union?'

and

'And which (if any) of those parties contacted you to convince you to leave the European Union?'

The options here were for all of the parties. An answer of 'yes' to contact by *any* of the parties for a vote for Remain **OR** an answer of 'yes' to any of the Remain campaigns contacting in the previous question resulted in that individual being classed as contacted by Remain.

An answer of 'yes' to contact by *any* of the parties for a vote for Leave **OR** an answer of 'yes' to any of the Leave campaigns contacting in the previous question resulted in that individual being classed as contacted by Leave.

There was also a question asking respondents to indicate the *ways* in which they had been contacted, such as by telephone or by mail. Unfortunately this question was only asked with respect to contact by a political party, but not with regards to contact by

a party or campaign about the EU referendum. However, the responses at least offer some indication as to the types of campaign contact that individuals were reporting - leafleting and mail were the most often selected, while contact at home, on the street and by phone were much less common. Text and 'other forms of contact' were the least often selected. There was no obvious pattern with regards to prior voting intention and the type of campaign contact reported. Note though that, as stated above, these only offer an indication since the questions used in my analysis do not include a question about the type of contact received.

Matching Variables

The post-stratification weights used for the analysis are `wt_full_W7W8`.

Below is a list of the main control variables used in the matching model and how they were coded.

Table A2.1: Coding for Main Matching Variables

Variable	Coding
Party ID	Categorical variable from partyIdW7, coded into 'Conservative', 'Labour', and 'Other or None', the last of which serves as the reference category. Reflects responses to the question: 'Generally speaking, do you think of yourself as Labour, Conservative, Liberal Democrat or what?'
2015 Vote	Categorical variable from profile variable of recalled 2015 turnout, p_turnout_2015, and vote, p_past_vote_2015, coded into 'Conservative', 'Labour', 'Lib Dem', 'Other', and 'Non-voter', with non-voter as the reference category.
Political Attention	Standardised variable based on the polAttentionW7 variable, which reflects responses to the question 'How much attention do you generally pay to politics?'
Age	From age in wave 7, ageW7, coded into four categories: '18 to 29', '30 to 44', '45 to 59', and 'over 60'. Age 18 to 29 is selected as the reference category.
Class	From the national sector analytic categories ns_sec_analyticW6W7W8W9, with smaller categories collapsed to leave five groups in total; 'higher managerial and professional', 'lower professional', 'intermediate', 'small employers and lower supervisory', and 'semi-routine and routine occupations'. Higher managerial and professional is the reference category.
Education	From the simplified education variable, p_edlevelW7, but with undergraduate and post-graduate in a single category, leaving five categories: 'undergrad or higher', 'A-level', 'GCSE', 'Below GCSE', and 'No qualifications'. Under GCSE is the reference category.
Male	Dummy variable coded from gender, with male as 1 and female as 0.
Newspaper	Categorical variable reflecting the Brexit position of an individual's preferred newspaper, coded from profile variable p_paper_readW7 into 'Remain', 'Leave', 'Neutral' or 'No Paper' (the reference category).

A2.2 Loveplots

Table A2.2 shows the results from the logistic regressions used to derive the propensity scores that are used for my analysis.

Figure A2.1 shows the distribution of the Kolmogorov-Smirnov statistics of the difference in means between the variables, and reveals show a similar pattern of successful matching as the absolute standardised means, allowing for confidence that the matching procedure has genuinely balanced the sample with regards to the variables included in the matching model.

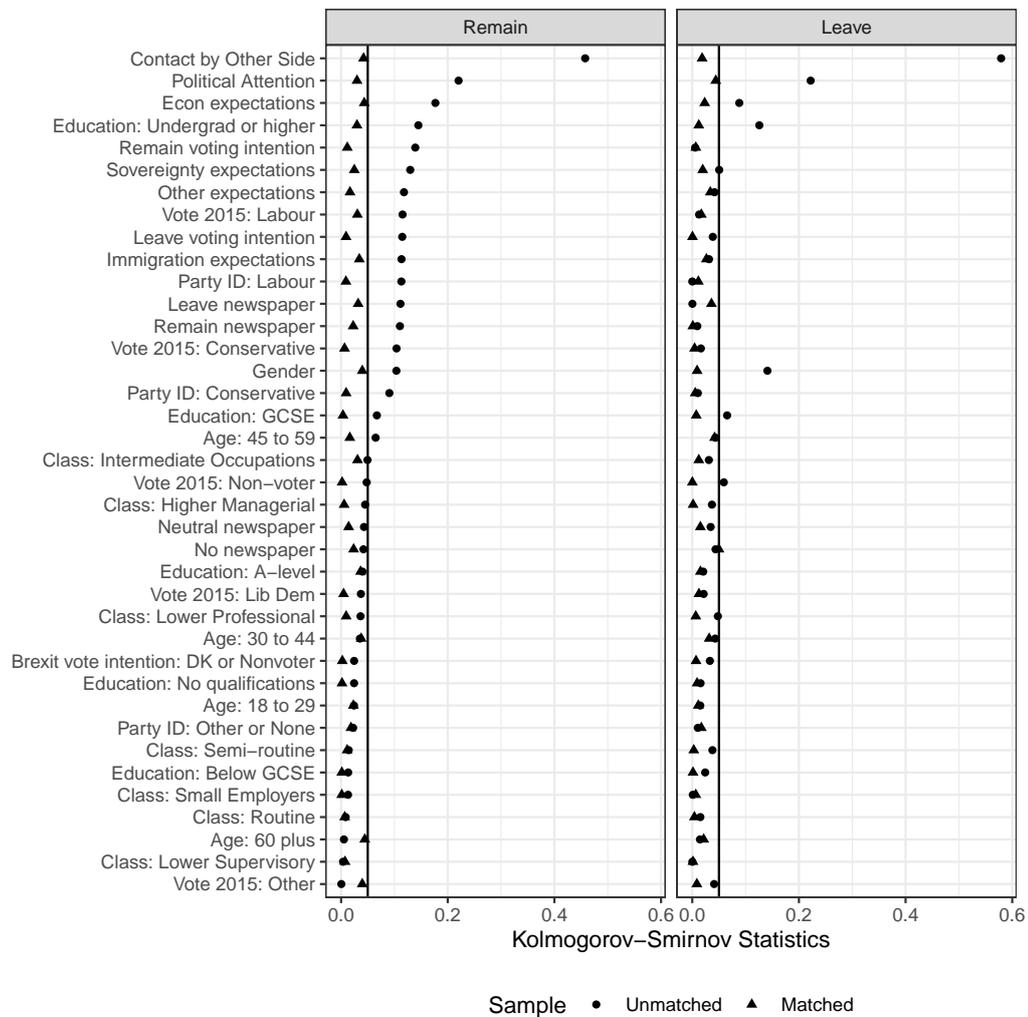


Figure A2.1: Loveplots of Kolmogorov-Smirnov statistics of the unmatched and matched samples

Table A2.2: Table of coefficients from the logistic regressions used to derive propensity scores that were used for matching

	Dependent Variable	
	Remain Contact	Leave Contact
Contact Leave	3.47 (0.09)***	
Contact Remain		3.48 (0.09)***
Age: (ref = Under 18)		
30 to 44	-0.02 (0.13)	0.14 (0.15)
45 to 59	-0.22 (0.13)	0.01 (0.15)
60 plus	0.07 (0.13)	-0.10 (0.15)
Male	0.05 (0.08)	0.35 (0.09)***
Class: (ref = Higher Managerial)		
Lower Professional	-0.02 (0.11)	0.12 (0.12)
Intermediate Occupations	-0.15 (0.12)	0.12 (0.14)
Small Employers	-0.08 (0.16)	0.06 (0.18)
Lower Supervisory	0.23 (0.16)	-0.07 (0.19)
Semi-routine	0.11 (0.14)	-0.20 (0.17)
Routine	0.07 (0.18)	-0.07 (0.20)
Education: (ref = Undergrad or Higher)		
A-level	-0.27 (0.10)**	-0.23 (0.11)*
GCSE	-0.22 (0.10)*	-0.38 (0.12)**
Below GCSE	0.01 (0.18)	-0.71 (0.23)**
No qualifications	-0.35 (0.16)*	-0.30 (0.18)
2015 Vote: (ref = Non-voter)		
Remain newspaper	0.42 (0.10)***	-0.12 (0.12)
Leave newspaper	-0.03 (0.10)	0.16 (0.11)
Neutral newspaper	0.27 (0.15)	0.16 (0.17)
2015 Vote: (ref = Non-voter)		
Labour	0.27 (0.15)	0.05 (0.17)
Lib Dem	0.40 (0.16)*	0.05 (0.18)
Other	0.23 (0.14)	0.15 (0.14)
Non-voter	-0.15 (0.22)	-1.10 (0.30)***
Party ID: (ref = Other or None)		
Conservative	-0.01 (0.13)	-0.09 (0.13)
Labour	0.24 (0.11)*	-0.19 (0.14)
Brexit voting intention: (ref = Remain)		
Leave	0.03 (0.12)	0.13 (0.13)
Don't know or non-voter	-0.00 (0.20)	-0.52 (0.25)*
Political Attention	0.19 (0.02)***	0.13 (0.02)***
Brexit expectations:		
Economic	0.20 (0.05)***	-0.22 (0.06)***
Immigration	0.03 (0.04)	-0.03 (0.05)
Sovereignty	0.16 (0.05)***	-0.08 (0.05)
Other	0.04 (0.04)	-0.01 (0.05)
(Intercept)	-4.63 (0.26)***	-4.61 (0.29)***
McFadden R ²	0.29	0.32
AIC	5726.60	4564.33
BIC	5966.18	4803.90
Log Likelihood	-2831.30	-2250.16
Deviance	5745.88	4610.15
Num. obs.	13184	13184

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

A2.3 Full Tables of Models Used to Generate Dot-Whisker Plots

Table A2.3: The effect of campaign contact by Remain on voting intention and expectations about Brexit's impact for the UK

	Vote	Economic	Immigration	Sovereignty
Remain Contact	0.04 (0.06)	0.07** (0.02)	0.02 (0.03)	0.10** (0.04)
Intercept	0.26*** (0.02)	0.26*** (0.01)	-0.57*** (0.01)	-0.65*** (0.01)
AIC	14131.59			
BIC	14146.42			
Log Likelihood	-7063.80			
Deviance	14947.75			
Num. obs.	12291	13141	12806	12950
R ²		0.00	0.00	0.00
Adj. R ²		0.00	-0.00	0.00

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A2.4: The effect of campaign contact by Leave on voting intention and expectations about Brexit's impact for the UK

	Vote	Economic	Immigration	Sovereignty
Leave Contact	0.01 (0.07)	0.00 (0.03)	0.07* (0.03)	-0.05 (0.04)
Intercept	-0.23*** (0.02)	0.08*** (0.01)	-0.79*** (0.01)	-0.88*** (0.01)
AIC	13937.17			
BIC	13952.01			
Log Likelihood	-6966.59			
Deviance	14682.25			
Num. obs.	12291	13141	12806	12950
R ²		0.00	0.00	0.00
Adj. R ²		-0.00	0.00	0.00

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A2.5: The effect of campaign contact by Remain on voting intention and expectations about Brexit's impact for the UK, with prior voting intention interaction

	Vote	Economic	Immigration	Sovereignty
Leave Intention x Remain Contact	-0.63* (0.27)	0.00 (0.03)	0.09 (0.05)	-0.13* (0.05)
DK or No Vote Intention x Remain Contact	-2.13*** (0.43)	-0.10 (0.09)	-0.32* (0.13)	-0.16 (0.14)
Remain Contact	0.74*** (0.22)	0.05* (0.02)	-0.02 (0.03)	0.14*** (0.04)
Leave Intention	-4.86*** (0.08)	-1.23*** (0.01)	-1.02*** (0.02)	-1.69*** (0.02)
DK or No Vote Intention	-2.16*** (0.11)	-0.69*** (0.02)	-0.37*** (0.04)	-1.25*** (0.04)
(Intercept)	2.56*** (0.05)	0.81*** (0.01)	-0.12*** (0.01)	0.12*** (0.01)
AIC	6055.23			
BIC	6099.73			
Log Likelihood	-3021.62			
Deviance	6341.94			
Num. obs.	12291	13141	12806	12950
R ²		0.53	0.25	0.45
Adj. R ²		0.53	0.25	0.45

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A2.6: The effect of campaign contact by Leave on voting intention and expectations about Brexit's impact for the UK, with prior voting intention interaction

	Vote	Economic	Immigration	Sovereignty
Leave Intention x Leave Contact	0.50 (0.26)	0.04 (0.03)	-0.01 (0.05)	0.07 (0.06)
DK or No Vote Intention x Leave Contact	-0.48 (0.51)	0.13 (0.11)	-0.22 (0.16)	0.01 (0.18)
Leave Contact	-0.28 (0.21)	-0.04 (0.03)	0.07 (0.04)	-0.11* (0.04)
Leave Intention	-5.23*** (0.08)	-1.26*** (0.01)	-0.98*** (0.01)	-1.80*** (0.02)
DK or No Vote Intention	-2.53*** (0.15)	-0.74*** (0.03)	-0.38*** (0.06)	-1.10*** (0.06)
(Intercept)	2.78*** (0.07)	0.81*** (0.01)	-0.23*** (0.01)	0.16*** (0.01)
AIC	5433.72			
BIC	5478.22			
Log Likelihood	-2710.86			
Deviance	5717.35			
Num. obs.	12291	13141	12806	12950
R ²		0.56	0.28	0.49
Adj. R ²		0.56	0.28	0.49

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A2.7: The effect of campaign contact by Remain on voting intention and expectations about Brexit's impact for the UK, with party ID interaction

	Vote	Economic	Immigration	Sovereignty
Conservative Party ID x Remain Contact	0.32 (0.17)	-0.12* (0.06)	0.14 (0.07)	-0.03 (0.09)
Labour Party ID x Remain Contact	-0.30* (0.15)	-0.21*** (0.05)	-0.27*** (0.06)	-0.17* (0.08)
Remain Contact	0.09 (0.11)	0.19*** (0.04)	0.11* (0.05)	0.19*** (0.06)
Conservative Party ID	-1.22*** (0.06)	-0.27*** (0.02)	-0.16*** (0.02)	-0.77*** (0.03)
Labour Party ID	0.83*** (0.05)	0.38*** (0.02)	0.43*** (0.02)	0.35*** (0.03)
(Intercept)	0.21*** (0.04)	0.16*** (0.01)	-0.72*** (0.02)	-0.62*** (0.02)
AIC	12629.98			
BIC	12674.48			
Log Likelihood	-6308.99			
Deviance	13419.35			
Num. obs.	12291	13141	12806	12950
R ²		0.10	0.06	0.12
Adj. R ²		0.10	0.06	0.12

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A2.8: The effect of campaign contact by Leave on voting intention and expectations about Brexit's impact for the UK, with party ID interaction

	Vote	Economic	Immigration	Sovereignty
Conservative Party ID x Leave Contact	0.55** (0.19)	0.36*** (0.06)	0.20** (0.07)	0.23* (0.09)
Labour Party ID x Leave Contact	0.50** (0.17)	0.26*** (0.06)	0.18** (0.07)	0.10 (0.09)
Leave Contact	-0.31** (0.12)	-0.20*** (0.04)	-0.05 (0.05)	-0.16* (0.06)
Conservative Party ID	-1.33*** (0.06)	-0.51*** (0.02)	-0.28*** (0.02)	-0.81*** (0.03)
Labour Party ID	0.35*** (0.05)	0.11*** (0.02)	0.07*** (0.02)	0.20*** (0.03)
(Intercept)	0.04 (0.03)	0.21*** (0.01)	-0.72*** (0.01)	-0.69*** (0.02)
AIC	12802.36			
BIC	12846.86			
Log Likelihood	-6395.18			
Deviance	13533.78			
Num. obs.	12291	13141	12806	12950
R ²		0.10	0.03	0.12
Adj. R ²		0.10	0.03	0.12

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A2.9: The effect of campaign contact by Remain on voting intention and expectations about Brexit's impact for the UK, with political attention interaction

	Vote	Economic	Immigration	Sovereignty
Political Attention x Remain Contact	0.14*** (0.04)	0.03* (0.01)	0.08*** (0.02)	0.03 (0.02)
Remain Contact	-1.13*** (0.31)	-0.18 (0.11)	-0.61*** (0.13)	-0.14 (0.17)
Political Attention	-0.01 (0.01)	0.00 (0.00)	-0.02*** (0.01)	0.03*** (0.01)
Intercept	0.36*** (0.10)	0.24*** (0.04)	-0.41*** (0.04)	-0.91*** (0.06)
AIC	14119.99			
BIC	14149.66			
Log Likelihood	-7055.99			
Deviance	14932.70			
Num. obs.	12291	13141	12806	12950
R ²		0.00	0.00	0.00
Adj. R ²		0.00	0.00	0.00

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A2.10: The effect of campaign contact by Leave on voting intention and expectations about Brexit's impact for the UK, with political attention interaction

	Vote	Economic	Immigration	Sovereignty
Political Attention x Leave Contact	-0.00 (0.04)	-0.02 (0.02)	-0.05** (0.02)	-0.05 (0.02)
Leave Contact	0.04 (0.35)	0.16 (0.13)	0.49*** (0.14)	0.32 (0.20)
Political Attention	-0.01 (0.01)	-0.02*** (0.00)	0.02** (0.01)	0.02** (0.01)
Intercept	-0.10 (0.11)	0.28*** (0.04)	-0.92*** (0.04)	-1.06*** (0.06)
AIC	13939.12			
BIC	13968.78			
Log Likelihood	-6965.56			
Deviance	14680.67			
Num. obs.	12291	13141	12806	12950
R ²		0.00	0.00	0.00
Adj. R ²		0.00	0.00	0.00

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

A2.4 Results from Robustness Check with Political Attention as a Categorical Variable

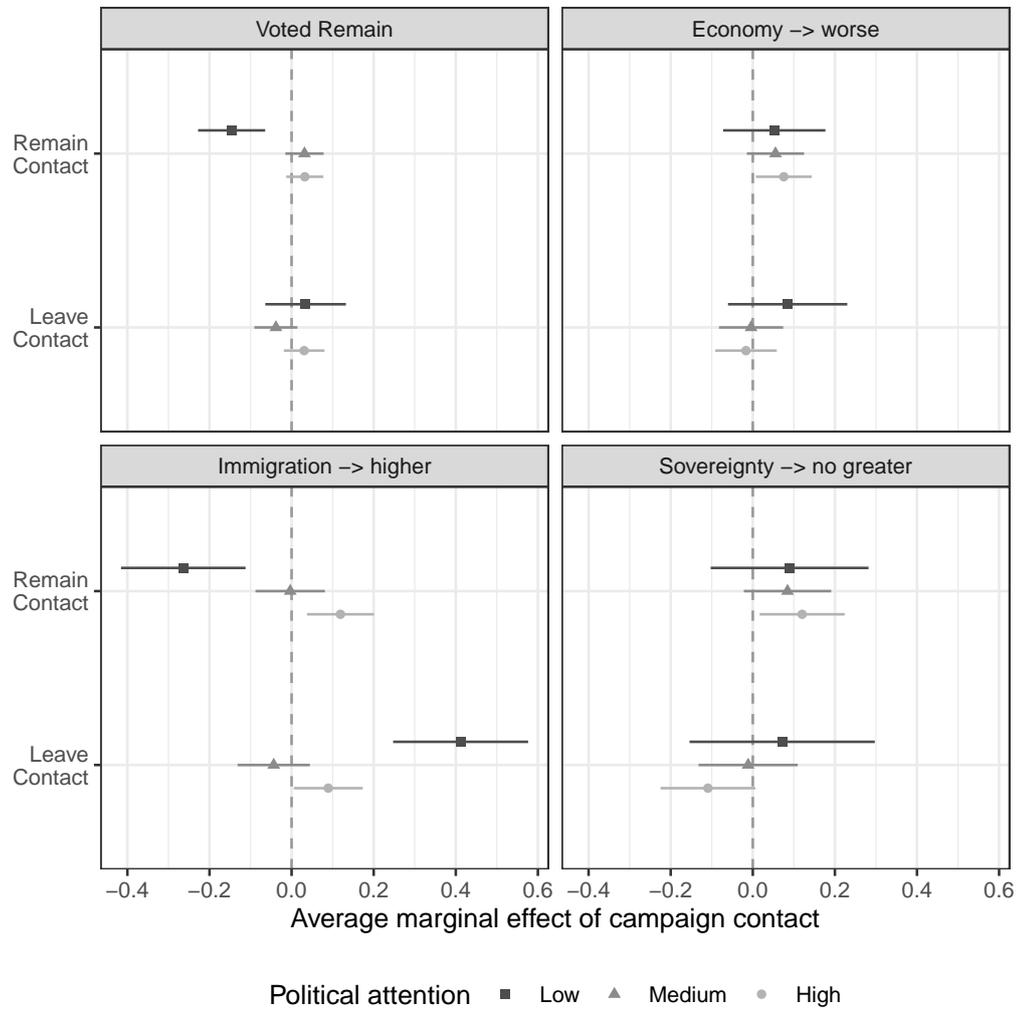


Figure A2.2: The moderating effect of political attention (categorical variable) on Leave and Remain campaign contact effectiveness

Table A2.11: The effect of campaign contact by Remain on voting intention and expectations about Brexit's impact for the UK, with political attention categorical variable interaction

	Vote	Economic	Immigration	Sovereignty
Medium Political Attention x Remain Contact	0.73*** (0.20)	0.00 (0.07)	0.26** (0.09)	-0.01 (0.11)
High Political Attention x Remain Contact	0.74*** (0.20)	0.02 (0.07)	0.38*** (0.09)	0.03 (0.11)
Remain Contact	-0.60*** (0.17)	0.05 (0.06)	-0.26*** (0.08)	0.09 (0.10)
Medium Political Attention	-0.22*** (0.06)	0.11*** (0.02)	-0.17*** (0.03)	0.20*** (0.04)
High Political Attention	-0.01 (0.07)	0.12*** (0.02)	-0.12*** (0.03)	0.25*** (0.04)
Intercept	0.36*** (0.06)	0.16*** (0.02)	-0.45*** (0.02)	-0.85*** (0.03)
AIC	14095.18			
BIC	14139.68			
Log Likelihood	-7041.59			
Deviance	14905.87			
Num. obs.	12291	13141	12806	12950
R ²		0.00	0.00	0.01
Adj. R ²		0.00	0.00	0.00

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A2.12: The effect of campaign contact by Leave on voting intention and expectations about Brexit's impact for the UK, with political attention categorical variable interaction

	Vote	Economic	Immigration	Sovereignty
Medium Political Attention x Leave Contact	-0.29 (0.23)	-0.09 (0.08)	-0.46*** (0.10)	-0.08 (0.13)
High Political Attention x Leave Contact	-0.01 (0.23)	-0.10 (0.08)	-0.32*** (0.09)	-0.18 (0.13)
Leave Contact	0.14 (0.20)	0.08 (0.07)	0.41*** (0.08)	0.07 (0.12)
Medium Political Attention	0.16* (0.07)	0.04 (0.02)	0.05 (0.03)	0.12** (0.04)
High Political Attention	-0.03 (0.07)	-0.07** (0.02)	0.04 (0.03)	0.10* (0.04)
Intercept	-0.28*** (0.06)	0.10*** (0.02)	-0.83*** (0.02)	-0.98*** (0.03)
AIC	13923.51			
BIC	13968.01			
Log Likelihood	-6955.76			
Deviance	14660.99			
Num. obs.	12291	13141	12806	12950
R ²		0.00	0.00	0.00
Adj. R ²		0.00	0.00	0.00

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

A2.5 OLS Comparison

As discussed in the Method section of the main paper, I make use of propensity score matching when analysing the BESIP survey data to avoid potential bias due to selective contact. In short, some individuals are more likely to be contacted by the campaigns than others, and they might systematically differ with regards to the dependent variables, meaning that a coefficient appearing to capture a campaign effect may actually just be capturing these differences in likelihood of receiving contact. However, as Tables A2.13 and A2.14 show, there is essentially no difference in the results when using a simple OLS model that controls for all of the variables used in the propensity score matching.

Table A2.13: The effect of campaign contact by Remain on voting intention and expectations about Brexit's impact for the UK - comparison between post-matched regression and logit / OLS including all controls from the propensity score matching (not shown).

	Vote		Economic		Immigration		Sovereignty	
	Matched	Logit	Matched	OLS	Matched	OLS	Matched	OLS
Remain Contact	0.04 (0.06)	0.19 (0.13)	0.07** (0.02)	0.04*** (0.01)	0.02 (0.03)	0.04 (0.02)	0.10** (0.04)	0.06** (0.02)
Intercept	0.26*** (0.02)	1.93*** (0.21)	0.26*** (0.01)	0.22*** (0.02)	-0.57*** (0.01)	-0.48*** (0.04)	-0.65*** (0.01)	-0.65*** (0.04)
AIC	14131.59	6110.34						
BIC	14146.42	6325.42						
Log Likelihood	-7063.80	-3026.17						
Deviance	14947.75	5995.62						
Num. obs.	12291	12291	13141	13141	12806	12806	12950	12950
R ²			0.00	0.74	0.00	0.30	0.00	0.57
Adj. R ²			0.00	0.74	-0.00	0.30	0.00	0.57

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A2.14: The effect of campaign contact by Leave on voting intention and expectations about Brexit's impact for the UK - comparison between post-matched regression and logit / OLS including all controls from the propensity score matching (not shown).

	Vote		Economic		Immigration		Sovereignty	
	Matched	Logit	Matched	OLS	Matched	OLS	Matched	OLS
Leave Contact	0.01 (0.07)	0.09 (0.14)	0.00 (0.03)	0.00 (0.01)	0.07* (0.03)	0.03 (0.03)	-0.05 (0.04)	-0.03 (0.03)
Intercept	-0.23*** (0.02)	1.93*** (0.21)	0.08*** (0.01)	0.22*** (0.02)	-0.79*** (0.01)	-0.48*** (0.04)	-0.88*** (0.01)	-0.65*** (0.04)
AIC	13937.17	6112.22						
BIC	13952.01	6327.30						
Log Likelihood	-6966.59	-3027.11						
Deviance	14682.25	5997.48						
Num. obs.	12291	12291	13141	13141	12806	12806	12950	12950
R ²			0.00	0.74	0.00	0.30	0.00	0.57
Adj. R ²			-0.00	0.74	0.00	0.30	0.00	0.57

***p < 0.001; **p < 0.01; *p < 0.05

A2.6 Predicted Probabilities

Below are predicted probabilities generated from the models used in the main analysis.

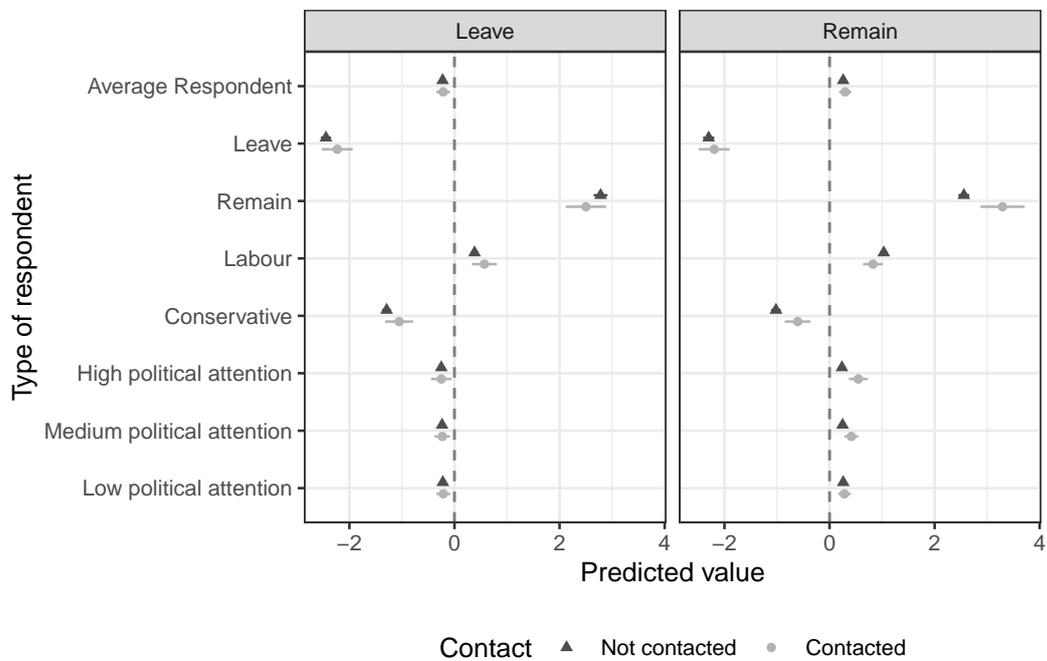


Figure A2.3: Predicted probabilities for vote intention, with whiskers indicating 95% confidence intervals

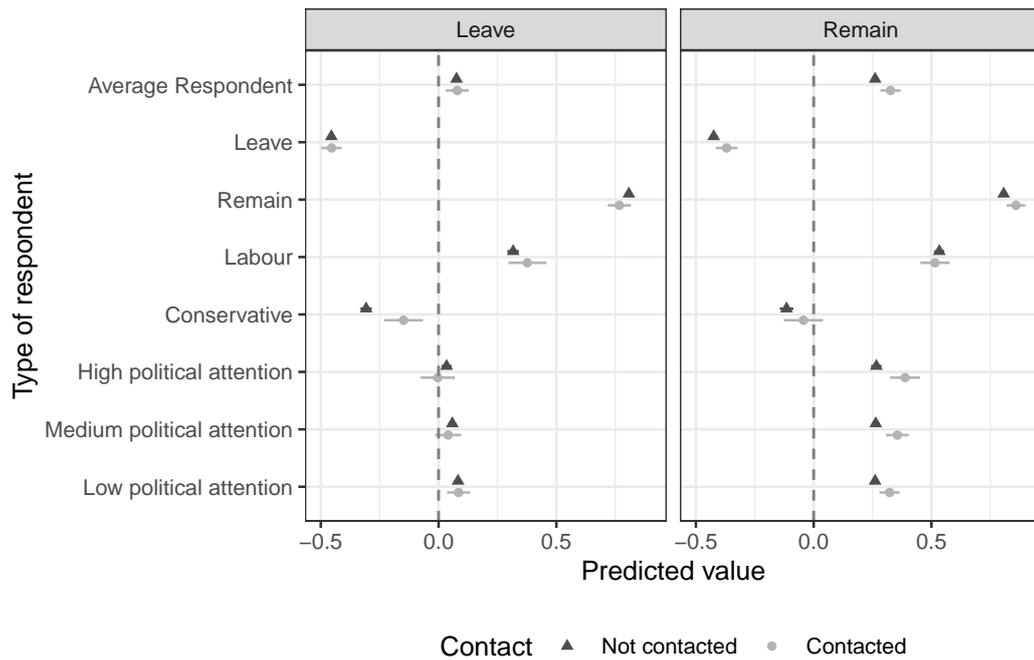


Figure A2.4: Predicted probabilities for economic expectations, with whiskers indicating 95% confidence intervals

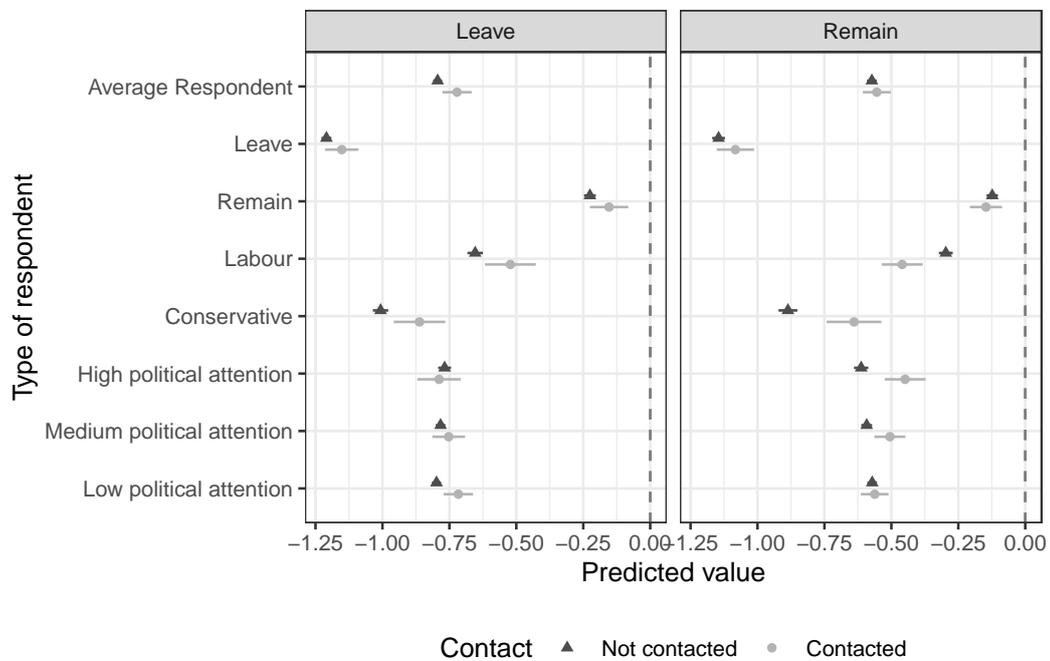


Figure A2.5: Predicted probabilities for immigration expectations, with whiskers indicating 95% confidence intervals

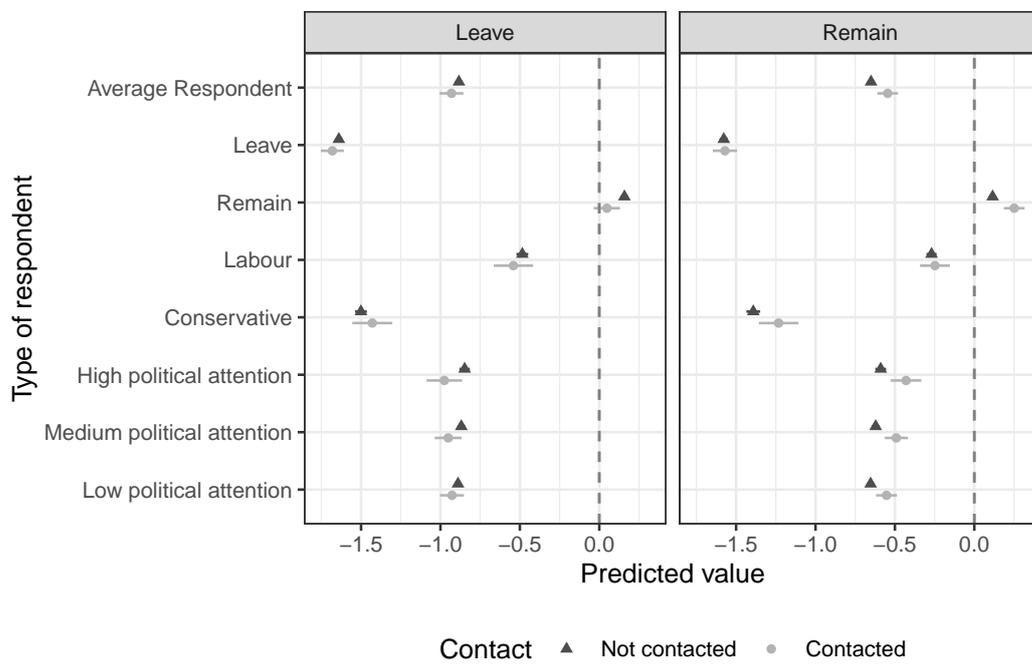


Figure A2.6: Predicted probabilities for sovereignty expectations, with whiskers indicating 95% confidence intervals

A2.7 Results from Robustness Check Using Newspaper Interaction

Table A2.15: The effect of campaign contact by Remain on voting intention and expectations about Brexit's impact for the UK, with newspaper interaction

	Vote	Economic	Immigration	Sovereignty
Remain Newspaper x Remain Contact	-0.65*** (0.18)	-0.23*** (0.06)	-0.36*** (0.07)	-0.22* (0.09)
Leave Newspaper x Remain Contact	0.13 (0.17)	-0.18** (0.06)	0.08 (0.07)	0.24** (0.09)
Neutral Newspaper x Remain Contact	0.38 (0.30)	0.14 (0.08)	0.01 (0.10)	0.63*** (0.12)
Remain Contact	0.16 (0.13)	0.19*** (0.04)	0.11 (0.06)	0.03 (0.07)
Remain Newspaper	0.67*** (0.06)	0.21*** (0.02)	0.40*** (0.02)	0.14*** (0.03)
Leave Newspaper	-1.42*** (0.06)	-0.43*** (0.02)	-0.19*** (0.02)	-1.03*** (0.03)
Neutral Newspaper	0.68*** (0.09)	0.18*** (0.03)	0.18*** (0.03)	-0.25*** (0.04)
Intercept	0.49*** (0.04)	0.32*** (0.01)	-0.65*** (0.02)	-0.32*** (0.02)
AIC	12226.08			
BIC	12285.34			
Log Likelihood	-6105.04			
Deviance	12945.04			
Num. obs.	12192	13034	12699	12844
R ²		0.12	0.06	0.16
Adj. R ²		0.12	0.06	0.16

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A2.16: The effect of campaign contact by Leave on voting intention and expectations about Brexit's impact for the UK, with newspaper interaction

	Vote	Economic	Immigration	Sovereignty
Remain Newspaper x Leave Contact	-0.04 (0.21)	-0.22** (0.07)	0.09 (0.08)	-0.58*** (0.11)
Leave Newspaper x Leave Contact	0.23 (0.19)	0.01 (0.06)	0.09 (0.07)	-0.16 (0.09)
Neutral Newspaper x Leave Contact	0.03 (0.29)	-0.28** (0.09)	0.04 (0.11)	-0.78*** (0.14)
Leave Contact	-0.04 (0.14)	0.07 (0.05)	0.00 (0.06)	0.22** (0.07)
Remain Newspaper	0.30*** (0.06)	0.19*** (0.02)	0.10*** (0.02)	0.40*** (0.03)
Leave Newspaper	-1.43*** (0.05)	-0.53*** (0.02)	-0.21*** (0.02)	-0.65*** (0.03)
Neutral Newspaper	0.82*** (0.09)	0.48*** (0.03)	0.26*** (0.03)	0.76*** (0.04)
Intercept	0.18*** (0.04)	0.22*** (0.01)	-0.75*** (0.02)	-0.76*** (0.02)
AIC	12259.29			
BIC	12318.56			
Log Likelihood	-6121.64			
Deviance	12914.38			
Num. obs.	12192	13034	12699	12844
R ²		0.18	0.03	0.15
Adj. R ²		0.18	0.03	0.15

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Appendix to Chapter 3

A3.1 Variable Coding

EU Identity

EU identity is measured in all waves after wave 7. For waves 7 to 14, the main question, echoing the BES formulation for partisan identity, is:

'In the EU referendum debate, do you think of yourself as closer to either the 'Leave' or 'Remain' side? If yes, which one?'

From waves 15 onwards, the question changes to:

'Thinking about the EU referendum, do you think of yourself as closer to either the "Leave" or "Remain" side? If yes, which one?'

There is a standard strength question asked, again echoing the partisan identity strength question, measured on a scale of 'Very strong', 'Fairly strong', 'Not very strong', or 'Don't know'.

'Would you call yourself very strong, fairly strong, or not very strong *euid*?'

This is followed with a similar set of social psychology strength measures to those above:

'You said that you feel closer to the *euid* side. Thinking about this side, how much do you agree with these statements?'

- euID1: When I speak about the *eu*id side, I usually say "we" instead of "they".
- euID2: I am interested in what other people think about the *eu*id side.
- euID3: When people criticize the *eu*id side, it feels like a personal insult.
- euID4: I have a lot in common with other supporters of the *eu*id side.
- euID5: If the *eu*id side does badly in opinion polls, my day is ruined.²
- euID6: When I meet someone who supports the *eu*id side, I feel connected with this person.
- euID7: When people praise the *eu*id side, it makes me feel good.

As Table A3.1 shows, the Cronbach's Alpha for these items is consistently high across all of the waves used for the analysis in this paper.

Table A3.1: Cronbach's Alpha for Remain and Leave identifiers in all waves used for the main analysis

Wave	Cronbach's Alpha for:	
	Remainers	Leavers
Wave 7	0.87	0.88
Wave 8	0.87	0.86
Wave 12	0.89	0.89
Wave 13	0.89	0.88
Wave 15	0.89	0.87
Wave 17	0.89	0.88
Wave 19	0.89	0.88
Wave 20	0.87	0.86

Media Consumption

Created from a series of answers given by respondents to the question: 'During the last seven days, on average how much time (if any) have you spent per day following news?', where the following news sources were television, newspaper (online), radio, internet

². Note that this item was only asked in waves 7 to 9 (pre and just post-referendum).

(excluding newspapers), and talking to other people. Possible answers were ‘No time at all’, ‘Less than 1/2 hour’, ‘1/2 hour to 1 hour’, ‘1 to 2 hours’, ‘More than 2 hours’, or ‘Don’t know’. I use this as a continuous scale, ranging from 0 (no time at all) to 4 (more than 2 hours).

For the analysis of the average media consumption, I impute ‘Don’t knows’ to the mid-point for respondents who have provided a meaningful response to at least one of the media source questions.

Control Variables

Table A3.2: Coding of control variables

Variable	Coding strategy
Left-right economic scale	eco- Created from a battery of items capturing left-right values. These items are measured on a five-party scale: ‘Strongly disagree’, ‘Disagree’, ‘Neither agree nor disagree’, ‘Agree’ and ‘Strongly agree’, with ‘Don’t know’ as an additional option. The statements are: ‘Government should redistribute income from the better off to those who are less well off’, ‘Big business takes advantage of ordinary people’, ‘Ordinary working people do not get their fair share of the nation’s wealth’, ‘There is one law for the rich and one for the poor’ and ‘Management will always try to get the better of employees if it gets the chance’.

Coding of control variables (continued)

Variable	Coding strategy
Liberal-authoritarian scale	Created from a battery of items capturing liberal-authoritarian values. These items are measured on the same five-part scale as above. The statements are: 'Young people today don't have enough respect for traditional British values', 'For some crimes, the death penalty is the most appropriate sentence', 'Schools should teach children to obey authority', 'Censorship of films and magazines is necessary to uphold moral standards' and 'People who break the law should be given stiffer sentences'.
EU Integration scale	In the pre-referendum period, a continuous measure coded from the variable <code>eesEUIntegrationSelf</code> , which captures responses to the question: 'Some say European unification should be pushed further. Others say it has already gone too far. What is your opinion?' Answers are given on a eleven point scale, ranging from 'Unification has already gone too far' to 'Unification should be pushed further'. For analysis of changing identity during the post-referendum period it is coded from responses to the question: 'Some people feel that Britain should do all it can to unite fully with the European Union. Other people feel that Britain should do all it can to protect its independence from the European Union. Where would you place yourself on this scale?'. Answers are again given on a eleven point scale, ranging from 'Unite fully with the European Union', to 'Protect our independence', and I therefore code a continuous measure from the variable <code>EUIntegrationSelf</code> . For both variables 'Don't know' responses are imputed to the mid-point of 5.

Coding of control variables (continued)

Variable	Coding strategy
Party ID	Categorical variable with four categories coded from the variable <code>partyId</code> which captures responses to the question: 'Generally speaking, do you think of yourself as Labour, Conservative, Liberal Democrat or what?'. The categories are 'Conservative or UKIP', 'Labour', 'Pro-Remain' (which covers all other party affiliations), and 'No Party ID' as the reference category. Successors to UKIP, such as the Brexit Party, are included in the 'Conservative or UKIP' category.
Party ID Strength	Continuous variable coded from <code>partyIdStrength</code> which captures responses to the party ID follow-up question: 'Would you call yourself a very strong, fairly strong or not very strong <i>Party ID</i> ?' (only asked to those who express a partisan identity). Individuals with no party ID receive a value of zero for strength.
Age	Categorical variable coded from <code>age</code> with 10 year age brackets. Categories are '25 and under' (the reference category), '26-35', '36-45', '46-55', '56-65', and '65 plus'.
White British	Dummy variable indicating whether the respondent is White British (coded as one), or not (coded as zero). Coded from the BE-SIP profile variable <code>p_ethnicity</code> , with 'prefer not to say' treated as missing values.

Coding of control variables (continued)

Variable	Coding strategy
Class	Categorical variable representing whether an individual's occupation is classed as 'higher managerial' (the reference category), 'lower professional', 'intermediate occupations', 'small employers', 'lower supervisory', 'semi-routine', or 'routine'. Coded from the national sector analytic variable <code>ns_sec_analytic</code> , measured in each wave, with higher managerial and professional collapsed into the single category of 'higher managerial'.
Education	Categorical variable with five categories, representing whether an individual's highest educational attainment is 'Undergrad or higher' (the reference category), 'A-level', 'GCSE', 'Below GCSE', or 'No qualifications'. Coded from the BESIP profile variable <code>p_edlevel</code> , but with undergraduate and post-graduate collapsed into the single category of 'Undergrad or higher'.
Male	Dummy variable coded from gender, with male as one and female as two.

Macro-trends

Average EU Identity Strength Over Time

Figure A3.1 shows the average level of the Brexit strength indicators over time.

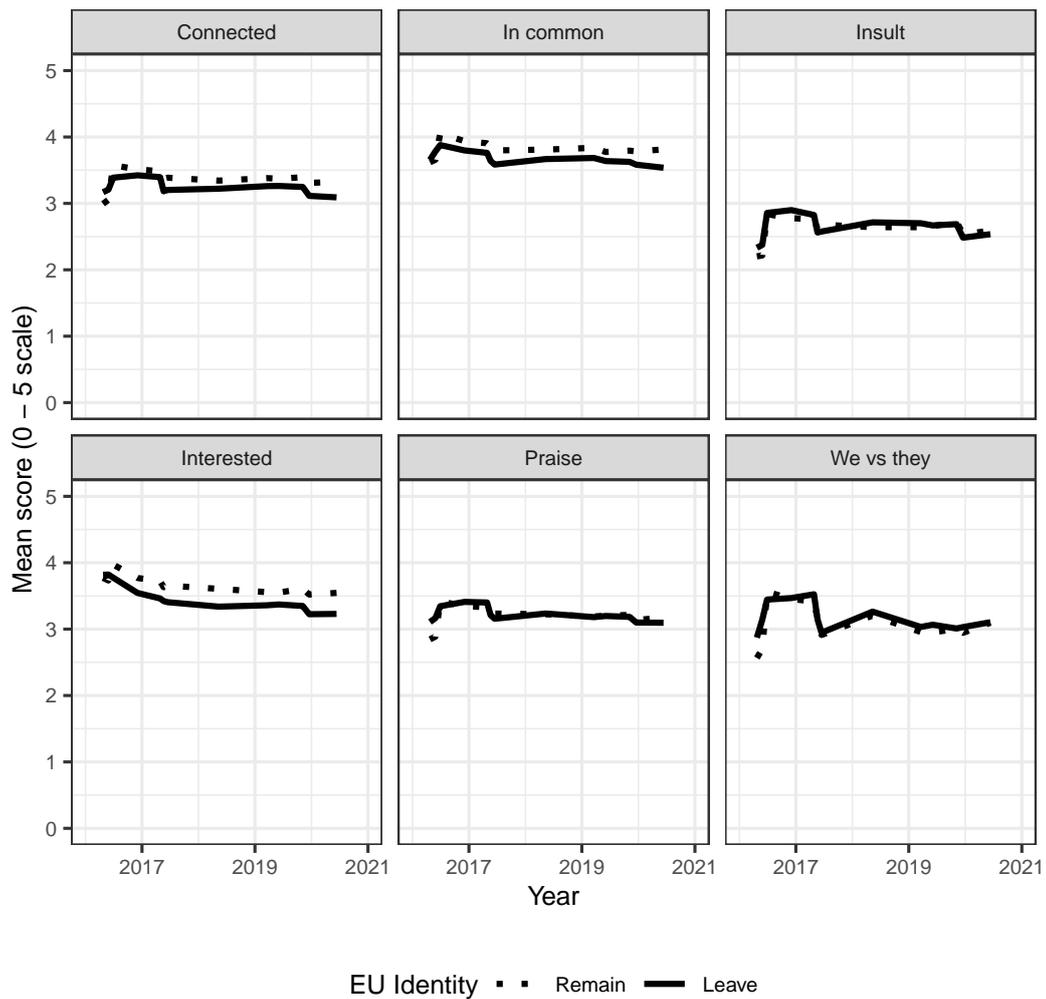


Figure A3.1: Change in separate social psychology indices since the referendum

Note: The full index wordings are as follows: 'We vs they' = 'When I speak about the euid side, I usually say "we" instead of "they"'. 'Interested' = 'I am interested in what other people think about the euid side.' 'Insult' = 'When people criticize the euid side, it feels like a personal insult.' 'In common' = 'I have a lot in common with other supporters of the euid side.' 'Connected' = 'When I meet someone who supports the euid side, I feel connected with this person.' 'Praise' = 'When people praise the euid side, it makes me feel good.'

Average Media Consumption Over Time

Figure A3.2 shows the consistency of these indices over time.

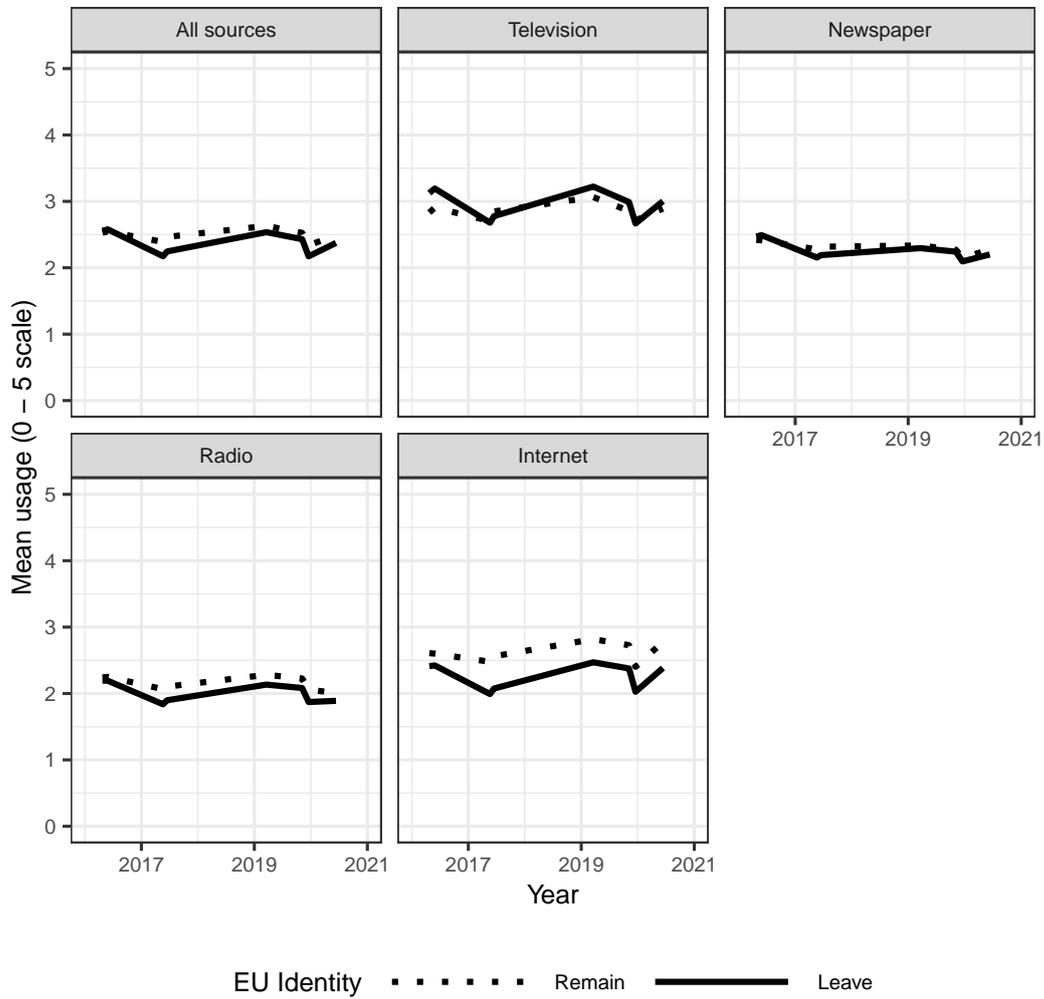


Figure A3.2: Change in average media consumption since the referendum, by Brexit identity

A3.2 Robustness Checks

Without Additional Controls

Table A3.3: The effect of average media exposure on Remain and Leave identities and strengths - results from a series of fixed effects models with no control variables

	<i>Dependent variable:</i>			
	Remain identity	Leave identity	Remain strength	Leave strength
Average media exposure	-0.01*** (0.00)	0.00 (0.00)	0.06*** (0.00)	0.06*** (0.00)
R ²	0.01	0.01	0.04	0.02
Num. obs.	69985	68096	70796	65969

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: Time fixed effects are included but not shown. The Remain identity model includes only those individuals who voted for Remain in the referendum, whilst the Leave identity model includes only respondents who voted for Leave. The strength models only include individuals who hold that identity. The reference category for party ID is 'No party ID'. Nine survey waves are used for the analysis, together spanning the period from April 2016 to June 2020.

Table A3.4: The effect of television, newspaper, radio, and internet exposure on Remain and Leave identities and strengths - results from a series of fixed effects models with no control variables

	<i>Dependent variable:</i>			
	Remain identity	Leave identity	Remain strength	Leave strength
Television	-0.00 (0.00)	0.00 (0.00)	0.02*** (0.00)	0.01*** (0.00)
Newspaper	-0.00 (0.00)	0.00 (0.00)	0.01*** (0.00)	0.02*** (0.00)
Radio	-0.00 (0.00)	0.00 (0.00)	0.01 (0.00)	0.01* (0.00)
Internet	-0.00 (0.00)	0.00 (0.00)	0.02*** (0.00)	0.02*** (0.00)
R ²	0.01	0.01	0.04	0.02
Num. obs.	67706	65689	68704	63928

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: Time fixed effects are included but not shown. The Remain identity model includes only those individuals who voted for Remain in the referendum, whilst the Leave identity model includes only respondents who voted for Leave. The strength models only include individuals who hold that identity. The reference category for party ID is 'No party ID'. Nine survey waves are used for the analysis, together spanning the period from April 2016 to June 2020.

With Lagged Controls

Table A3.5: The effect of average media exposure on Remain and Leave identities and strengths - results from a series of fixed effects models with lagged control variables

	<i>Dependent variable:</i>			
	Remain identity	Leave identity	Remain strength	Leave strength
Media:				
Average media exposure	-0.00 (0.00)	0.00 (0.00)	0.06*** (0.01)	0.05*** (0.01)
Lagged control variables:				
EU Integration scale	-0.01*** (0.00)	0.01*** (0.00)	-0.02*** (0.00)	0.00 (0.00)
PID: Conservative / Anti-EU	0.00 (0.01)	0.02*** (0.01)	0.03 (0.02)	0.02 (0.01)
PID: Labour	0.03*** (0.01)	-0.01 (0.01)	0.02 (0.02)	-0.01 (0.02)
PID: Pro-EU	0.01 (0.01)	0.02* (0.01)	0.04** (0.02)	0.03 (0.02)
R ²	0.01	0.02	0.03	0.02
Num. obs.	42579	44480	42084	42611

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: Time fixed effects are included but not shown. The Remain identity model includes only those individuals who voted for Remain in the referendum, whilst the Leave identity model includes only respondents who voted for Leave. The strength models only include individuals who hold that identity. The reference category for party ID is 'No party ID'. Nine survey waves are used for the analysis, together spanning the period from April 2016 to June 2020.

Table A3.6: The effect of television, newspaper, radio, and internet exposure on Remain and Leave identities and strengths - results from a series of fixed effects models with lagged control variables

	<i>Dependent variable:</i>			
	Remain identity	Leave identity	Remain strength	Leave strength
<i>Media sources:</i>				
Television	-0.00 (0.00)	0.00 (0.00)	0.02*** (0.00)	0.01*** (0.00)
Newspaper	-0.00 (0.00)	0.00 (0.00)	0.01* (0.00)	0.02*** (0.00)
Radio	-0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Internet	-0.00 (0.00)	0.00 (0.00)	0.02*** (0.00)	0.02*** (0.00)
<i>Lagged control variables:</i>				
EU Integration scale	-0.01*** (0.00)	0.01*** (0.00)	-0.01*** (0.00)	0.00 (0.00)
PID: Conservative / Anti-EU	0.00 (0.01)	0.02** (0.01)	0.03 (0.02)	0.02 (0.01)
PID: Labour	0.03** (0.01)	-0.01 (0.01)	0.02 (0.02)	0.00 (0.02)
PID: Pro-EU	0.00 (0.01)	0.02 (0.01)	0.05** (0.02)	0.04 (0.02)
R ²	0.01	0.02	0.03	0.02
Num. obs.	41246	43002	40900	41347

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: Time fixed effects are included but not shown. The Remain identity model includes only those individuals who voted for Remain in the referendum, whilst the Leave identity model includes only respondents who voted for Leave. The strength models only include individuals who hold that identity. The reference category for party ID is 'No party ID'. Nine survey waves are used for the analysis, together spanning the period from April 2016 to June 2020.

Media Consumption as a Categorical Variable

Table A3.7: The effect of television, newspaper, radio, and internet exposure on Remain and Leave identities and strengths - results from a series of fixed effects models

	<i>Dependent variable:</i>			
	Remain identity	Leave identity	Remain strength	Leave strength
Television (ref: no time):				
Less than 30mins	-0.00 (0.00)	-0.00 (0.00)	0.03** (0.01)	0.02* (0.01)
30mins to 1hr	0.00 (0.01)	0.00 (0.01)	0.04*** (0.01)	0.06*** (0.01)
1hr to 2 hrs	-0.00 (0.01)	0.01 (0.01)	0.06*** (0.01)	0.06*** (0.01)
Over 2hrs	-0.01 (0.01)	0.01 (0.01)	0.07*** (0.01)	0.05*** (0.01)
Newspaper (ref: no time):				
Less than 30mins	0.00 (0.00)	-0.00 (0.00)	0.02** (0.01)	0.03** (0.01)
30mins to 1hr	0.00 (0.00)	-0.00 (0.00)	0.04*** (0.01)	0.05*** (0.01)
1hr to 2 hrs	-0.00 (0.01)	0.00 (0.01)	0.04*** (0.01)	0.05*** (0.01)
Over 2hrs	-0.00 (0.01)	0.01 (0.01)	0.05*** (0.02)	0.07*** (0.02)
Radio (ref: no time):				
Less than 30mins	0.00 (0.00)	0.00 (0.00)	0.01 (0.01)	-0.00 (0.01)
30mins to 1hr	-0.00 (0.01)	-0.00 (0.01)	0.02 (0.01)	0.00 (0.01)
1hr to 2 hrs	-0.01 (0.01)	0.00 (0.01)	0.03* (0.01)	0.03* (0.01)
Over 2hrs	-0.01 (0.01)	0.01 (0.01)	0.01 (0.02)	0.04** (0.02)
Internet (ref: no time):				
Less than 30mins	-0.00 (0.00)	0.01* (0.00)	0.03*** (0.01)	0.03*** (0.01)
30mins to 1hr	-0.00 (0.00)	-0.00 (0.00)	0.06*** (0.01)	0.07*** (0.01)
1hr to 2 hrs	-0.01* (0.01)	0.00 (0.01)	0.08*** (0.01)	0.06*** (0.01)
Over 2hrs	-0.01 (0.01)	0.00 (0.01)	0.07*** (0.01)	0.07*** (0.01)
Control variables:				
EU Integration scale	-0.03*** (0.00)	0.04*** (0.00)	-0.03*** (0.00)	0.03*** (0.00)
PID: Labour	0.03*** (0.01)	-0.02** (0.01)	0.06** (0.02)	-0.06*** (0.02)
PID: Pro-EU	-0.00 (0.01)	-0.01 (0.01)	0.06*** (0.02)	-0.01 (0.02)
R ²	0.04	0.07	0.05	0.03
Num. obs.	67664	65628	68663	63870

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: Time fixed effects are included but not shown. The Remain identity model includes only those individuals who voted for Remain in the referendum, whilst the Leave identity model includes only respondents who voted for Leave. The strength models only include individuals who hold that identity. The reference category for party ID is 'No party ID'. Nine survey waves are used for the analysis, together spanning the period from April 2016 to June 2020.

Appendix to Chapter 4

A4.1 Demographic Coding

Table A4.1: Full list of variable coding used for analysis

Variable	Coding
<i>Demographics:</i>	
Gender	Dummy variable with a value of 1 if respondents identified as female, and 0 otherwise.
Ethnicity	Dummy variable with a value of 1 for respondents who are white British, and 0 otherwise.
Age	Coded as a categorical variable with four groups: '18 to 29', '30 to 44', '45 to 59', and 'over 60'. Age 18 to 29 is the reference category.
Class	Categorical variable based on simplified national sector analytic categories, with higher managerial and professional collapsed into a single category, leaving 'higher managerial', 'lower professional', 'intermediate occupations', 'small employers', 'lower supervisory', 'semi-routine occupations', and 'routine occupations'. Higher managerial is the reference category.
Education	Categorical variable with five groups: 'undergrad or higher', 'A-level', 'GCSE', 'Below GCSE', and 'No qualifications'. 'Undergrad or higher' is the reference category.

A4.2 Experiment Design

After respondents had been randomly allocated to participate in our experiment, they were also randomly assigned to one of three groups; negative treatment, positive treat-

ment, or control.

Respondents were then shown either the negative or positive treatment paragraphs below. Those in the control group were shown no additional text.

Negative Treatment = “Before the rollout of the coronavirus vaccine, the UK had one of the highest coronavirus death tolls per capita in the entire world. Over 125,000 Britons have died after contracting the virus.”

Positive Treatment = “Well over half of the UK adult population have already received their first dose of the coronavirus vaccine, and the UK continues to have one of the best vaccination rates in the entire world.”

On the same page as this initial treatment paragraph, we asked all respondents to answer the following question:

“How well has the UK performed overall in dealing with the coronavirus crisis?”

<5> Very well

<4> Quite well

<3> Neither well nor badly

<2> Quite badly

<1> Very badly

<99> Don't know

Next, respondents were asked:

“To what extent are the following the result of decisions taken by the UK government?”

A randomised grid was presented whereby respondents could choose to allocate responsibility on a scale of 0 to 10 for the following outcomes.

- “The UK’s overall experience of the coronavirus crisis”
- “The UK’s high coronavirus death toll”
- “The fast pace of the UK’s vaccine rollout”

Text in the grid indicated that a score of 0 represented “Not at all due to government decisions”, whilst a score of 10 represented “Entirely due to government decisions”. Respondents could also answer that they did not know.

Finally, we asked respondents:

“Thinking back to **this time last year**, how well do you think the UK Government had handled the coronavirus outbreak in Britain?”

<5> Very well

<4> Quite well

<3> Neither well nor badly

<2> Quite badly

<1> Very badly

<99> Don’t know

A4.3 Sample Composition

Table A4.2: Sample descriptives (wave 21 weights applied)

Variable	Number				Proportion				Standard Deviation			
	All	C	+ve	-ve	All	C	+ve	-ve	All	C	+ve	-ve
Male	3376	1195	1125	1056	0.48	0.49	0.49	0.48	0.50	0.50	0.50	0.50
Female	3586	1254	1188	1144	0.52	0.51	0.51	0.52	0.50	0.50	0.50	0.50
White British	5460	1932	1819	1708	0.87	0.88	0.87	0.86	0.34	0.32	0.34	0.35
Not White British	820	263	269	288	0.13	0.12	0.13	0.14	0.34	0.32	0.34	0.35
Age: 18 to 29	1176	431	404	341	0.17	0.18	0.17	0.16	0.37	0.38	0.38	0.36
Age: 30 to 44	1883	640	642	601	0.27	0.26	0.28	0.27	0.44	0.44	0.45	0.45
Age: 45 to 59	1770	616	556	597	0.25	0.25	0.24	0.27	0.44	0.43	0.43	0.44
Age: 60 plus	2134	763	710	661	0.31	0.31	0.31	0.30	0.46	0.46	0.46	0.46
Class: Higher Managerial	792	282	261	249	0.15	0.15	0.15	0.15	0.36	0.36	0.36	0.35
Class: Lower Professional	1517	536	498	484	0.29	0.29	0.29	0.29	0.45	0.45	0.45	0.45
Class: Intermediate Occupations	1258	424	420	413	0.24	0.23	0.25	0.24	0.43	0.42	0.43	0.43
Class: Small Employers	357	130	111	116	0.07	0.07	0.06	0.07	0.25	0.25	0.25	0.25
Class: Lower Supervisory	348	124	116	108	0.07	0.07	0.07	0.06	0.25	0.25	0.25	0.24
Class: Semi-routine Occupations	603	209	198	196	0.11	0.11	0.12	0.12	0.32	0.32	0.32	0.32
Class: Routine Occupations	391	156	108	126	0.07	0.08	0.06	0.07	0.26	0.28	0.24	0.26
Education: Undergrad or higher	2523	895	835	794	0.42	0.43	0.42	0.42	0.49	0.50	0.49	0.49
Education: A-level	1312	453	449	410	0.22	0.22	0.23	0.22	0.41	0.41	0.42	0.41
Education: GCSE	1431	487	461	483	0.24	0.23	0.23	0.25	0.43	0.42	0.42	0.44
Education: Below GCSE	277	96	90	91	0.05	0.05	0.05	0.05	0.21	0.21	0.21	0.21
Education: No qualifications	416	142	158	116	0.07	0.07	0.08	0.06	0.25	0.25	0.27	0.24
Party Id: Opposition partisan	2482	871	817	795	0.36	0.36	0.35	0.36	0.48	0.48	0.48	0.48
Party Id: Non-partisan	2442	852	812	778	0.35	0.35	0.35	0.35	0.48	0.48	0.48	0.48
Party Id: Government partisan	2037	727	684	626	0.29	0.30	0.30	0.28	0.46	0.46	0.46	0.45

Table A4.3: Mean and standard deviation of main dependent variables (wave 21 weights applied)

Variable	Mean				Standard deviation			
	All	C	+ve	-ve	All	C	+ve	-ve
UK covid performance	3.14	3.11	3.46	2.85	1.30	1.25	1.29	1.30
Hold gov. responsible for death toll	6.49	6.44	6.48	6.54	2.68	2.65	2.70	2.70
Hold gov. responsible for vaccine	7.29	7.33	7.30	7.26	2.47	2.43	2.46	2.52
Hold gov. responsible overall	7.03	7.07	7.04	6.97	2.19	2.13	2.18	2.25
Retrospective handling	2.65	2.69	2.64	2.60	1.31	1.31	1.30	1.31
Change in retrospective handling	0.10	0.13	0.09	0.08	1.05	1.03	1.01	1.11

Table A4.4: Mean and standard deviation of main dependent variables (wave 21 weights applied)

Variable	Respondent number				Response rate			
	All	C	+ve	-ve	All	C	+ve	-ve
UK covid performance	6639	2361	2189	2089	0.96	0.97	0.96	0.97
Hold gov. responsible for death toll	6166	2193	2027	1946	0.90	0.90	0.88	0.90
Hold gov. responsible for vaccine	6282	2242	2060	1980	0.91	0.92	0.90	0.92
Hold gov. responsible overall	6156	2189	2030	1937	0.89	0.90	0.89	0.90
Retrospective handling	6613	2335	2191	2087	0.96	0.96	0.96	0.97
Change in retrospective handling	3603	1300	1206	1097	0.52	0.53	0.53	0.51

Table A4.5: Minimum and maximum values of main dependent variables

Variable	Minimum value				Maximum value			
	All	C	+ve	-ve	All	C	+ve	-ve
UK covid performance	1	1	1	1	5	5	5	5
Hold gov. responsible for death toll	0	0	0	0	10	10	10	10
Hold gov. responsible for vaccine	0	0	0	0	10	10	10	10
Hold gov. responsible overall	0	0	0	0	10	10	10	10
Retrospective handling	1	1	1	1	5	5	5	5
Change in retrospective handling	-4	-3	-4	-4	4	4	4	4

A4.4 Full Models for Main Results

Table A4.6: Results from OLS regressions of all dependent variables on party ID, with full controls added

	<i>Dependent variable:</i>					
	Overall	Overall resp	Death toll	Vaccine	Retro handle	Change in retro handle
Partisanship: (ref = Opposition)						
Government partisan	1.04*** (0.06)	-0.49*** (0.11)	-2.05*** (0.13)	1.34*** (0.12)	1.13*** (0.06)	-0.03 (0.06)
Non-partisan	0.50*** (0.06)	-0.20 (0.11)	-1.02*** (0.13)	0.63*** (0.12)	0.55*** (0.06)	0.07 (0.06)
Treatment: (ref = Control)						
Negative treatment	-0.33*** (0.06)	-0.06 (0.11)	0.16 (0.13)	-0.02 (0.12)	-0.15** (0.06)	-0.20** (0.06)
Positive treatment	0.40*** (0.05)	0.04 (0.11)	0.08 (0.13)	0.04 (0.11)	-0.06 (0.06)	-0.17** (0.06)
Male	-0.09 (0.05)	-0.12 (0.09)	-0.18 (0.11)	-0.27** (0.10)	-0.08 (0.05)	0.04 (0.05)
White British	0.06 (0.08)	-0.18 (0.16)	-0.30 (0.19)	0.14 (0.17)	0.01 (0.08)	-0.38*** (0.09)
Age: (ref = Under 18)						
30 to 44	0.28* (0.11)	-0.09 (0.22)	0.08 (0.27)	0.56* (0.24)	0.14 (0.12)	-0.07 (0.12)
45 to 59	0.42*** (0.11)	0.12 (0.22)	-0.03 (0.26)	0.84*** (0.24)	0.22 (0.12)	-0.03 (0.12)
60 plus	0.71*** (0.11)	0.14 (0.21)	-0.35 (0.26)	1.40*** (0.23)	0.53*** (0.11)	0.41*** (0.12)
Class: (ref = Higher Managerial)						
Intermediate occupations	0.18* (0.08)	-0.01 (0.15)	-0.08 (0.18)	0.30 (0.16)	0.16* (0.08)	0.02 (0.09)
Small employers	0.13 (0.11)	0.22 (0.20)	-0.03 (0.25)	0.11 (0.22)	0.14 (0.11)	-0.04 (0.12)
Lower supervisory	0.23* (0.11)	-0.20 (0.21)	-0.63* (0.26)	0.10 (0.23)	0.28* (0.11)	-0.12 (0.12)
Semi-routine occupations	0.18 (0.09)	0.15 (0.18)	-0.26 (0.22)	0.56** (0.19)	0.30** (0.10)	0.10 (0.10)
Routine occupations	0.04 (0.11)	-0.57** (0.21)	-0.29 (0.26)	0.42 (0.23)	-0.16 (0.11)	-0.12 (0.12)
Education: (ref = Undergrad or Higher)						
A-level	0.19** (0.07)	-0.29* (0.13)	-0.33* (0.15)	0.41** (0.14)	0.15* (0.07)	-0.08 (0.07)
GCSE	0.26*** (0.06)	-0.41*** (0.12)	-0.39** (0.15)	0.42** (0.13)	0.31*** (0.07)	0.03 (0.07)
Below GCSE	0.32** (0.10)	-0.13 (0.21)	-0.21 (0.25)	0.47* (0.22)	0.49*** (0.11)	0.22 (0.12)
No qualifications	0.46*** (0.09)	-0.34 (0.18)	-0.68** (0.22)	0.50** (0.19)	0.63*** (0.10)	0.08 (0.10)
Intercept	2.00*** (0.14)	7.56*** (0.28)	8.02*** (0.34)	5.44*** (0.30)	1.74*** (0.15)	0.40* (0.16)
R ²	0.27	0.03	0.15	0.13	0.24	0.07
Adj. R ²	0.26	0.02	0.14	0.13	0.23	0.06
Num. obs.	2399	2230	2234	2284	2385	1543

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A4.7: Models behind line graphs showing the relationship between performance, specific responsibility attribution and generalised attribution

	1	2	3
Government partisan (ref = opposition)	-2.40*** (0.21)	1.54*** (0.16)	-3.44*** (0.26)
Current UK Covid-19 performance	-0.53*** (0.03)		
Current UK Covid-19 performance * government partisan	0.62*** (0.06)		
Death toll gov responsibility		0.49*** (0.02)	
Death toll gov responsibility * government partisan		-0.20*** (0.02)	
Vaccine gov responsibility			0.04* (0.02)
Vaccine gov responsibility* government partisan			0.32*** (0.03)
Intercept	8.76*** (0.09)	3.71*** (0.13)	7.16*** (0.12)
R ²	0.08	0.24	0.06
Adj. R ²	0.08	0.23	0.06
Num. obs.	4506	4454	4498

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A4.8: Association between evaluations of the UK's current Covid-19 performance and evaluations of the performance one year ago

	1	2
Current UK Covid-19 performance	0.50*** (0.02)	0.42*** (0.03)
Covid-19 handling one year ago	0.35*** (0.02)	0.39*** (0.03)
Partisanship: (ref = Opposition)		
Government partisan		-0.02 (0.16)
Non-partisan		-0.22 (0.12)
Current UK Covid-19 performance * government partisan		0.20*** (0.05)
Current UK Covid-19 performance * non-partisan		0.14** (0.05)
Covid-19 handling one year ago * government partisan		-0.16** (0.05)
Covid-19 handling one year ago * non-partisan		-0.05 (0.05)
Treatment: (ref = Control)		
Positive treatment	-0.23*** (0.04)	-0.24*** (0.05)
Negative treatment	0.00 (0.04)	-0.00 (0.05)
Male	0.02 (0.03)	0.02 (0.04)
White British	0.05 (0.05)	-0.16* (0.06)
Age: (ref = Under 18)		
30 to 44	0.13 (0.07)	0.04 (0.09)
45 to 59	0.18* (0.07)	0.03 (0.09)
60 plus	0.35*** (0.07)	0.18* (0.09)
Class: (ref = Higher Managerial)		
Intermediate occupations	-0.02 (0.06)	0.04 (0.07)
Small employers	-0.09 (0.07)	-0.03 (0.09)
Lower supervisory	-0.01 (0.08)	-0.03 (0.09)
Semi-routine occupations	0.04 (0.07)	0.11 (0.08)
Routine occupations	-0.18* (0.07)	-0.14 (0.09)
Education: (ref = Undergrad or Higher)		
A-level	0.04 (0.05)	0.01 (0.06)
GCSE	0.10* (0.04)	0.11* (0.05)
Below GCSE	0.22** (0.07)	0.27** (0.09)
No qualifications	0.13 (0.07)	0.16* (0.08)
Intercept	-0.04 (0.09)	0.31* (0.13)
R ²	0.64	0.67
Adj. R ²	0.64	0.66
Num. obs.	2464	1645

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A4.9: Results from OLS regressions of all dependent variables on treatment status interacted with party ID, with full controls added

	<i>Dependent variable:</i>					
	Overall	Overall resp	Death toll	Vaccine	Retro handle	Change in retro handle
Treatment: (ref = Control)						
Government partisan	1.39*** (0.07)	-0.49*** (0.14)	-2.52*** (0.16)	1.89*** (0.15)	1.53*** (0.07)	0.16 (0.09)
Non-partisan	0.61*** (0.07)	-0.55*** (0.14)	-1.15*** (0.16)	0.63*** (0.14)	0.76*** (0.07)	0.10 (0.08)
Partisanship: (ref = Opposition)						
Negative treatment	-0.26*** (0.07)	0.11 (0.14)	0.06 (0.16)	0.08 (0.14)	-0.01 (0.07)	-0.08 (0.09)
Positive treatment	0.56*** (0.07)	-0.16 (0.14)	0.02 (0.16)	-0.15 (0.14)	0.13 (0.07)	0.10 (0.09)
Negative treatment * government partisan	-0.02 (0.10)	-0.43* (0.20)	-0.01 (0.23)	-0.07 (0.21)	-0.07 (0.10)	-0.11 (0.13)
Positive treatment * government partisan	-0.33** (0.10)	0.13 (0.20)	0.19 (0.23)	0.03 (0.20)	-0.23* (0.10)	-0.36** (0.12)
Negative treatment * non-partisan	-0.05 (0.10)	0.01 (0.20)	0.11 (0.23)	-0.21 (0.21)	-0.23* (0.10)	-0.02 (0.12)
Positive treatment * non-partisan	-0.16 (0.10)	0.30 (0.20)	-0.04 (0.23)	0.32 (0.21)	-0.29** (0.10)	-0.28* (0.12)
Male	-0.12*** (0.04)	-0.14* (0.07)	-0.06 (0.08)	-0.34*** (0.07)	-0.15*** (0.04)	0.00 (0.04)
White British	-0.13* (0.06)	-0.13 (0.11)	-0.31* (0.13)	-0.04 (0.12)	-0.08 (0.06)	-0.14* (0.07)
Age: (ref = Under 18)						
30 to 44	0.32*** (0.07)	0.01 (0.13)	-0.51*** (0.15)	0.27 (0.14)	0.17* (0.07)	0.05 (0.09)
45 to 59	0.41*** (0.07)	0.08 (0.13)	-0.46** (0.15)	0.65*** (0.14)	0.27*** (0.07)	0.13 (0.09)
60 plus	0.59*** (0.07)	0.23 (0.13)	-0.63*** (0.15)	0.98*** (0.14)	0.49*** (0.07)	0.47*** (0.09)
Class: (ref = Higher Managerial)						
Intermediate occupations	0.14* (0.06)	0.16 (0.11)	-0.10 (0.13)	0.13 (0.12)	0.11 (0.06)	0.02 (0.07)
Small employers	0.17* (0.08)	0.49** (0.16)	0.05 (0.19)	0.17 (0.17)	0.16 (0.08)	-0.04 (0.10)
Lower supervisory	0.04 (0.08)	0.01 (0.17)	-0.52** (0.19)	-0.23 (0.17)	0.18* (0.08)	-0.02 (0.10)
Semi-routine occupations	0.05 (0.07)	0.30* (0.14)	0.04 (0.16)	0.12 (0.14)	0.18* (0.07)	0.09 (0.08)
Routine occupations	0.13 (0.08)	-0.25 (0.16)	-0.32 (0.19)	0.09 (0.17)	0.04 (0.08)	-0.26** (0.10)
Education: (ref = Undergrad or Higher)						
A-level	0.18*** (0.05)	-0.20* (0.10)	-0.06 (0.11)	0.33*** (0.10)	0.15** (0.05)	-0.06 (0.06)
GCSE	0.26*** (0.05)	-0.33*** (0.09)	-0.29** (0.11)	0.44*** (0.10)	0.30*** (0.05)	-0.01 (0.06)
Below GCSE	0.30*** (0.08)	-0.28 (0.16)	-0.24 (0.18)	0.24 (0.16)	0.38*** (0.08)	0.10 (0.09)
No qualifications	0.40*** (0.07)	-0.26 (0.15)	-0.58*** (0.17)	0.49** (0.15)	0.50*** (0.07)	0.09 (0.09)
Intercept	2.12*** (0.10)	7.38*** (0.19)	8.49*** (0.22)	5.94*** (0.20)	1.64*** (0.10)	-0.02 (0.12)
R ²	0.30	0.04	0.19	0.18	0.30	0.05
Adj. R ²	0.30	0.03	0.19	0.18	0.30	0.04
Num. obs.	4055	3774	3787	3867	4039	2308

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

A4.5 Robustness Checks

Below are figures showing the effect of alternative model specifications on the marginal effect of treatment for government and opposition partisans.

The base model includes full demographic controls for gender, ethnicity, age, class (simplified national sector analytic categories) and educational attainment.

The '+ weights for pol attention' uses the additional weights that we calculated to make the sample representative of the political attention distribution found in the British Election Study face-to-face survey.

'+ remove vaccine hesitant' then removes from the sample all those individuals who indicated in the main BESIP survey that they would not take a coronavirus vaccine because they believe it is unsafe (not including those who have received official advice that it is unsafe for them personally), or because they are against vaccines in general.

'+ remove by party' removes respondents who identify with the Brexit Party or some 'Other' party, or who were present in the pre-experiment survey and identified with the Conservative Party but then by the time of the experiment identified with an opposition party, or who identified with an opposition party in the pre-experiment survey and identified with the Conservative party by the time of the experiment.

Note that these are all cumulative. For example, '+ remove vaccine hesitant' also makes use of the weights for political attention and includes full demographic controls.

The results presented in the main paper are from this final model, '+ remove by party'.

Checking the Effect of Model Specification on Main Panel Data Models

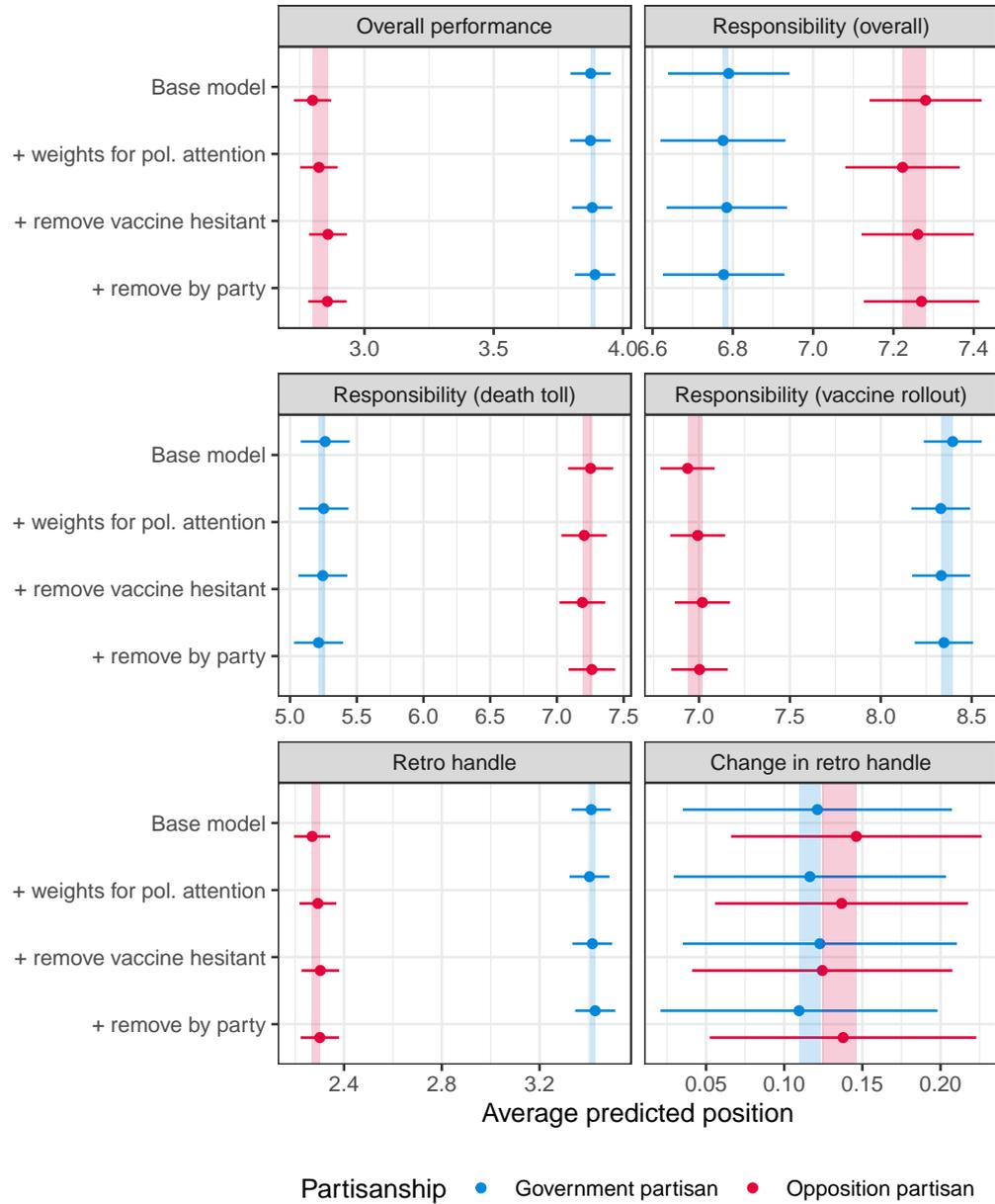


Figure A4.1: Robustness check for effect of model specification on the predicted values for government and opposition partisans

Checking the Effect of Model Specification on Party ID Interaction

Models

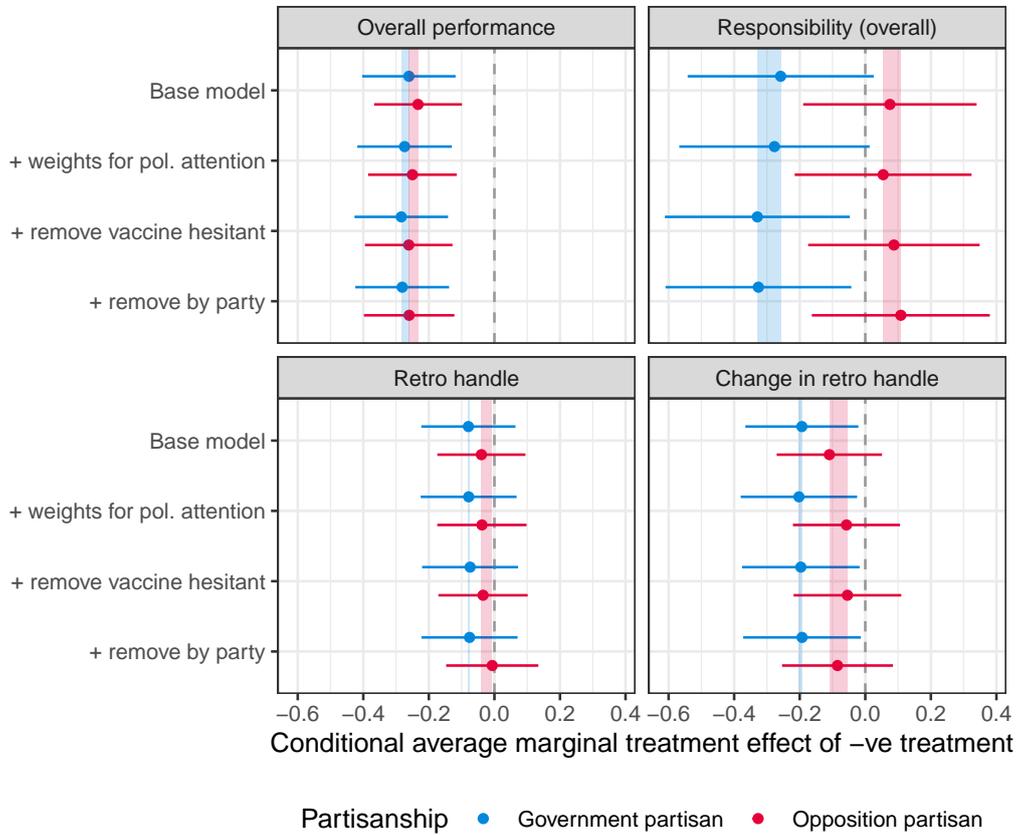


Figure A4.2: Robustness check for effect of model specification on the negative treatment effect for government and opposition partisans

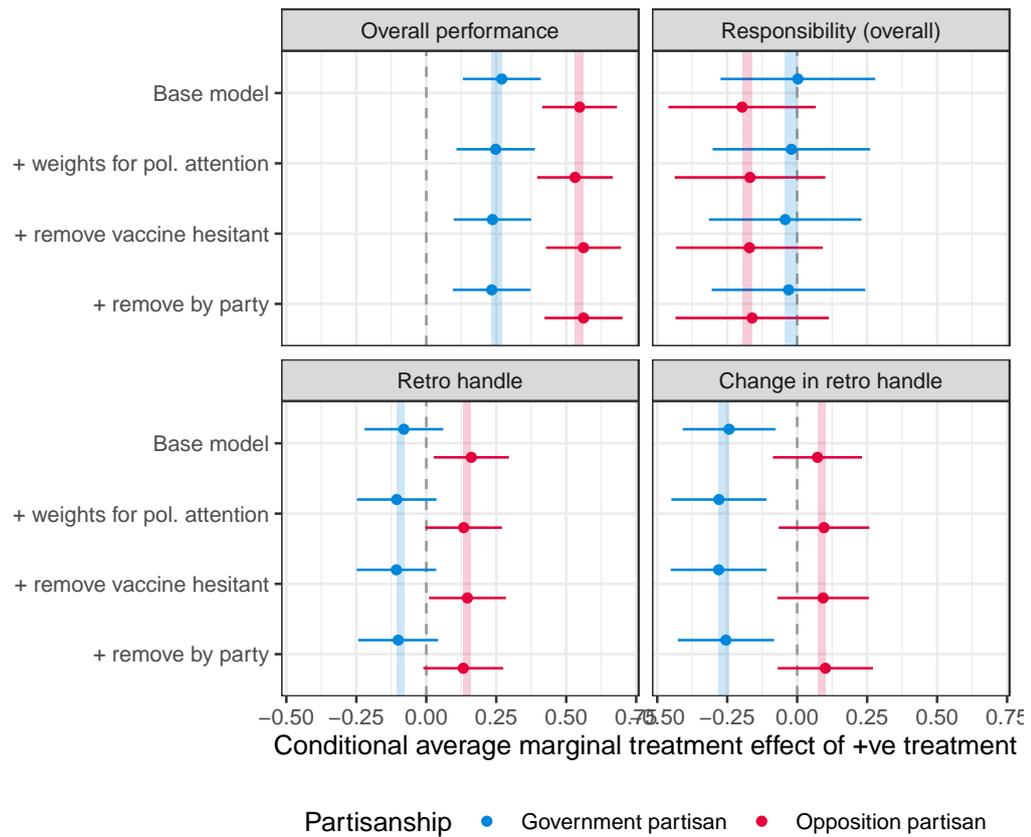


Figure A4.3: Robustness check for effect of model specification on the positive treatment effect for government and opposition partisans

Using Ordered Logit Models for Dependent Variables Measured on a Five-Point Scale

Table A4.10: Results from ordered logit regressions of handling variables on party ID, with full controls added

	<i>Overall performance</i>		<i>Retro handling</i>	
	OLS	Ord. Log	OLS	Ord. Log
Partisanship: (ref = Opposition)				
Government partisan	1.03*** (0.06)	1.61*** (0.10)	1.12*** (0.06)	1.75*** (0.10)
Non-partisan	0.50*** (0.06)	0.73*** (0.10)	0.55*** (0.06)	0.88*** (0.10)
Conditionals				
Very badly Quite badly		-0.36 (0.25)		0.29 (0.24)
Quite badly Neither well nor badly		1.02*** (0.25)		1.47*** (0.24)
Neither well nor badly Quite well		1.81*** (0.25)		2.29*** (0.25)
Quite well Very well		3.82*** (0.26)		4.60*** (0.26)
Male	-0.11* (0.05)	-0.18* (0.08)	-0.07 (0.05)	-0.12 (0.08)
White British	0.10 (0.08)	0.20 (0.14)	0.01 (0.08)	0.00 (0.14)
Age: (ref = Under 18)				
30 to 44	0.25* (0.12)	0.38* (0.19)	0.14 (0.12)	0.17 (0.19)
45 to 59	0.32** (0.11)	0.48* (0.19)	0.20 (0.12)	0.30 (0.19)
60 plus	0.64*** (0.11)	1.04*** (0.19)	0.52*** (0.11)	0.82*** (0.19)
Class: (ref = Higher Managerial)				
Lower professional	0.10 (0.08)	0.18 (0.13)	0.05 (0.08)	0.09 (0.13)
Intermediate occupations	0.19* (0.08)	0.31* (0.14)	0.16* (0.08)	0.25 (0.14)
Small employers	0.11 (0.11)	0.17 (0.19)	0.14 (0.11)	0.26 (0.19)
Lower supervisory	0.30** (0.11)	0.52** (0.19)	0.29** (0.11)	0.44* (0.19)
Semi-routine occupations	0.20* (0.10)	0.27 (0.16)	0.30** (0.10)	0.48** (0.16)
Routine occupations	0.01 (0.11)	0.01 (0.19)	-0.16 (0.11)	-0.29 (0.19)
Education: (ref = Undergrad or Higher)				
A-level	0.22** (0.07)	0.31** (0.11)	0.15* (0.07)	0.27* (0.11)
GCSE	0.28*** (0.06)	0.42*** (0.11)	0.31*** (0.07)	0.49*** (0.11)
Below GCSE	0.32** (0.11)	0.50** (0.18)	0.49*** (0.11)	0.75*** (0.18)
No qualifications	0.53*** (0.10)	0.90*** (0.17)	0.63*** (0.10)	1.01*** (0.17)
Intercept	2.04*** (0.15)		1.67*** (0.15)	
R ²	0.21		0.23	
Adj. R ²	0.21		0.23	
Num. obs.	2399	2187	2385	2184
AIC		6188.93		6148.94
BIC		6308.43		6268.41
Log Likelihood		-3073.47		-3053.47
Deviance		6146.93		6106.94

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Table A4.11: Results from ordered logit regressions of handling variables on treatment status interacted with party ID, with full controls added

	Overall performance		Retro handling	
	OLS	Ord. Log	OLS	Ord. Log
Partisanship: (ref = Opposition)				
Government partisan	1.39*** (0.07)	2.26*** (0.13)	1.53*** (0.07)	2.53*** (0.14)
Non-partisan	0.61*** (0.07)	0.91*** (0.12)	0.76*** (0.07)	1.22*** (0.13)
Treatment: (ref = Control)				
Negative treatment	-0.26*** (0.07)	-0.50*** (0.12)	-0.01 (0.07)	-0.01 (0.13)
Positive treatment	0.56*** (0.07)	0.85*** (0.13)	0.13 (0.07)	0.22 (0.13)
Negative treatment * government partisan	-0.02 (0.10)	-0.08 (0.18)	-0.07 (0.10)	-0.18 (0.18)
Positive treatment * government partisan	-0.33** (0.10)	-0.20 (0.18)	-0.23* (0.10)	-0.49** (0.18)
Negative treatment * non-partisan	-0.05 (0.10)	0.02 (0.17)	-0.23* (0.10)	-0.34 (0.18)
Positive treatment * non-partisan	-0.16 (0.10)	-0.17 (0.18)	-0.29** (0.10)	-0.46** (0.18)
Conditionals				
Very badly Quite badly		-0.59*** (0.17)		0.25 (0.17)
Quite badly Neither well nor badly		0.92*** (0.17)		1.57*** (0.17)
Neither well nor badly Quite well		1.79*** (0.17)		2.46*** (0.18)
Quite well Very well		3.97*** (0.18)		4.84*** (0.19)
Male	-0.12*** (0.04)	-0.19** (0.06)	-0.15*** (0.04)	-0.26*** (0.06)
White British	-0.13* (0.06)	-0.17 (0.10)	-0.08 (0.06)	-0.18 (0.10)
Age: (ref = Under 18)				
30 to 44	0.32*** (0.07)	0.51*** (0.11)	0.17* (0.07)	0.28* (0.12)
45 to 59	0.41*** (0.07)	0.64*** (0.12)	0.27*** (0.07)	0.40*** (0.12)
60 plus	0.59*** (0.07)	1.01*** (0.12)	0.49*** (0.07)	0.81*** (0.12)
Lower professional	-0.02 (0.06)	0.01 (0.10)	-0.04 (0.06)	-0.05 (0.10)
Class: (ref = Higher Managerial)				
Intermediate occupations	0.14* (0.06)	0.27* (0.11)	0.11 (0.06)	0.21* (0.10)
Small employers	0.17* (0.08)	0.30* (0.15)	0.16 (0.08)	0.26 (0.15)
Lower supervisory	0.04 (0.08)	0.08 (0.15)	0.18* (0.08)	0.33* (0.14)
Semi-routine occupations	0.05 (0.07)	0.08 (0.12)	0.18* (0.07)	0.31* (0.12)
Routine occupations	0.13 (0.08)	0.26 (0.14)	0.04 (0.08)	0.07 (0.14)
Education: (ref = Undergrad or Higher)				
A-level	0.18*** (0.05)	0.29*** (0.09)	0.15** (0.05)	0.27** (0.09)
GCSE	0.26*** (0.05)	0.41*** (0.08)	0.30*** (0.05)	0.50*** (0.08)
Below GCSE	0.30*** (0.08)	0.53*** (0.14)	0.38*** (0.08)	0.63*** (0.14)
No qualifications	0.40*** (0.07)	0.72*** (0.13)	0.50*** (0.07)	0.85*** (0.13)
Intercept	2.12*** (0.10)		1.64*** (0.10)	
R ²	0.30		0.30	
Adj. R ²	0.30		0.30	
Num. obs.	4055	3797	4039	3789
AIC		10308.21		10339.45
BIC		10476.74		10507.93
Log Likelihood		-5127.10		-5142.73
Deviance		10254.21		10285.45

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$

Appendix to Chapter 5

A5.1 Distribution of Relative Country Scores Over Time

Figure A5.1 shows that there are clear differences in the relative ranking of countries over time. Respondents seem to have updated their assessments of the UK's relative performance over time, with Germany and South Korea in particular praised as being relatively more successful at dealing with the COVID-19 pandemic in June 2020, but more equally by May 2021 and again in December 2021.

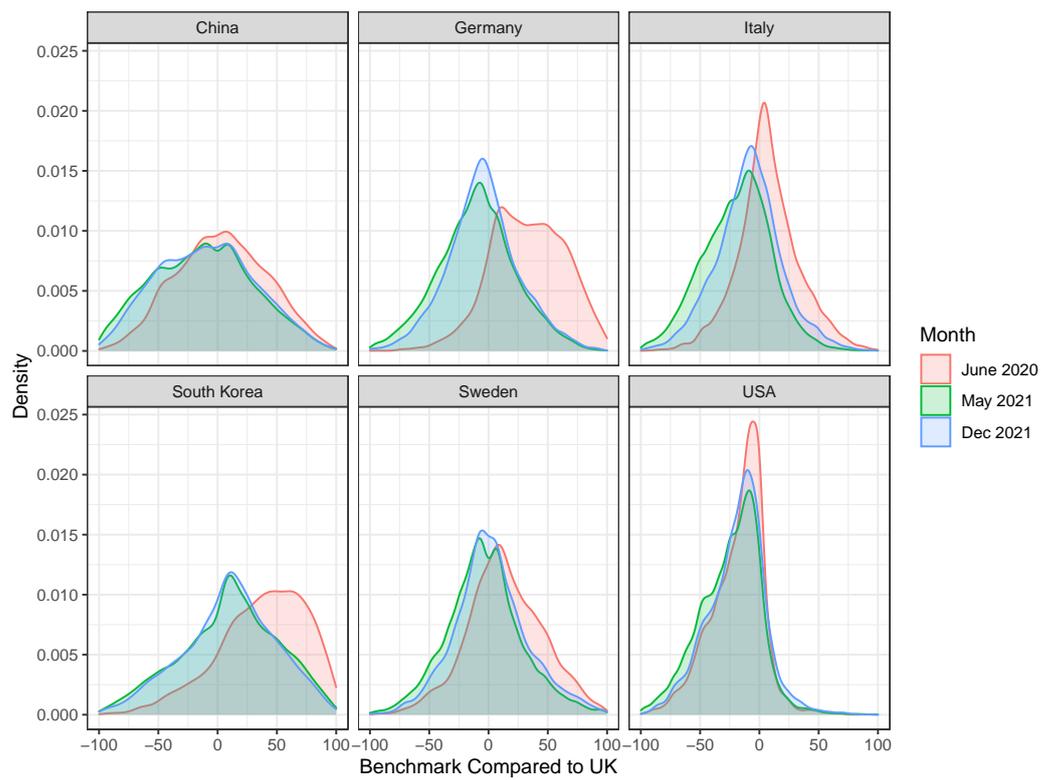


Figure A5.1: Distribution of scores relative to UK by country by wave

Note: Positive values indicate more positivity about a given country compared to the UK. Each of the waves is measured cross-sectionally, i.e. though there is some overlap between the different survey waves, the sample for each wave is slightly different.

A5.2 Variable Coding

Table A5.1: Coding methods for demographic variables

Variable	Description of coding method
Age	From the age profile variable (<code>age</code>), coded into 14 categories, most of which span five years: 'under 25' (reference category), '25-29', '30-34', '36-39', '40-44', '45-49', '50-54', '55-59', '60-64', '65-69', '70-74', '75-79', '80-84', 'over 85'.
White British	Dummy variable coded from the profile ethnicity variable (<code>p_ethnicity</code>) with a value of 1 indicating that the respondent is White British.
Class	From the national sector analytic categories (<code>ns_sec_analytic</code>), with smaller categories collapsed to leave five groups in total; 'higher managerial and professional' (reference category), 'lower professional', 'intermediate', 'small employers and lower supervisory', and 'semi-routine and routine occupations'.
Male	Dummy variable coded from (<code>gender</code>), with male as 1 and female as 0.
Education	From the simplified education profile variable (<code>p_edlevel</code>) but with undergraduate and post-graduate in a single category, leaving five categories: 'undergrad or higher' (reference category), 'A-level', 'GCSE', 'Below GCSE', and 'No qualifications'.
Left / right scale	From the variable <code>lr_scale</code> which captures a respondent's economic values, where higher values indicate more right-wing values. See the BES codebook for detail on the separate items that are added and scaled to make this composite scale.
Lib / auth scale	From the variable <code>al_scale</code> which captures a respondent's values on the second dimension, where higher values indicate more authoritarian preferences. See the BES codebook for detail on the separate items that are added and scaled to make this composite scale.

A5.3 Full Model Tables

Table A5.2: Results from an OLS regression of UK relative performance scores on partisan and Brexit identity scales

	<i>Dependent variable: UK relative score</i>		
	1	2	3
Partisan ID (scale)	7.86*** (0.56)		4.87*** (0.57)
Brexit ID (scale)		12.52*** (0.57)	11.14*** (0.59)
Age	10.05*** (0.51)	9.56*** (0.50)	9.22*** (0.50)
White British	3.37*** (0.77)	2.51*** (0.76)	2.18** (0.76)
Male	-1.84*** (0.48)	-2.42*** (0.47)	-2.40*** (0.47)
Education: (ref = Undergrad or Higher)			
A-level	3.34*** (0.64)	2.09** (0.64)	2.19*** (0.63)
GCSE	4.48*** (0.64)	2.52*** (0.64)	2.78*** (0.64)
Below GCSE	4.80*** (1.19)	2.81* (1.18)	3.05** (1.17)
No qualifications	10.42*** (1.02)	7.58*** (1.01)	8.06*** (1.00)
Left / right scale	7.44*** (0.53)	8.20*** (0.49)	6.62*** (0.52)
Lib / auth scale	14.21*** (0.55)	11.32*** (0.57)	10.72*** (0.57)
(Intercept)	6.59*** (0.79)	8.51*** (0.78)	8.66*** (0.78)
R ²	0.29	0.31	0.31
Adj. R ²	0.29	0.31	0.31
Num. obs	9423	9423	9423

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: All continuous independent variables have been standardised (divided by two times the standard deviation) to make coefficients comparable. The dependent variable was measured in May 2021, and all independent variables in November 2019. Higher values on the partisan identity scale equate to pro-government (Conservative) partisan strength, low values to pro-opposition (Labour / other) partisan strength. Higher values on the Brexit identity scale relate to stronger Leave identity, and lower values to stronger Remain identity. Non-identifiers were assigned to the midpoint of the respective scale.

Table A5.3: Results from an OLS of performance scores for the UK, EU and non-EU countries regressed on partisan and Brexit identity scales

	<i>Dependent variable:</i>		
	UK	EU average	Non-EU average
Partisan ID (scale)	5.42*** (0.55)	-0.39 (0.36)	0.72 (0.38)
Brexit ID (scale)	9.14*** (0.57)	-1.70*** (0.37)	-1.70*** (0.40)
Age	10.25*** (0.49)	-0.33 (0.32)	2.31*** (0.34)
White British	2.25** (0.73)	0.67 (0.49)	-1.09* (0.51)
Male	-1.85*** (0.45)	-2.89*** (0.30)	4.17*** (0.32)
Education: (ref = Undergrad or Higher)			
A-level	1.79** (0.61)	0.63 (0.41)	-1.32** (0.43)
GCSE	2.96*** (0.62)	1.71*** (0.41)	-1.35** (0.43)
Below GCSE	3.38** (1.13)	1.42 (0.76)	-2.29** (0.79)
No qualifications	6.62*** (0.96)	1.60* (0.64)	-3.96*** (0.68)
Left / right scale	8.38*** (0.50)	1.65*** (0.33)	2.21*** (0.35)
Lib / auth scale	10.43*** (0.55)	2.04*** (0.36)	-2.49*** (0.38)
(Intercept)	52.09*** (0.76)	43.25*** (0.50)	43.56*** (0.53)
R ²	0.32	0.02	0.04
Adj. R ²	0.32	0.02	0.04
Num. obs	9722	9053	9400

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: All continuous independent variables have been standardised (divided by two times the standard deviation) to make coefficients comparable. The dependent variables were measured in May 2021, and all independent variables in November 2019. Higher values on the partisan identity scale equate to pro-government (Conservative) partisan strength, low values to pro-opposition (Labour / other) partisan strength. Higher values on the Brexit identity scale relate to stronger Leave identity, and lower values to stronger Remain identity. Non-identifiers were assigned to the midpoint of the respective scale.

Table A5.4: Results from an OLS regression of performance scores of four countries by partisan and Brexit identity scales

	<i>Raw country score</i>			
	UK	China	Germany	Italy
Partisan ID (ref = None / DK)				
Partisan ID (scale)	5.42*** (0.55)	-1.02 (0.76)	-0.39 (0.51)	0.47 (0.48)
Brexit ID (scale)	9.14*** (0.57)	-7.74*** (0.78)	-6.47*** (0.52)	-2.20*** (0.50)
Brexit ID (ref = None / DK)				
Age	10.25*** (0.49)	-0.46 (0.68)	-0.89* (0.45)	0.36 (0.43)
White British	2.25** (0.73)	-4.05*** (1.01)	1.40* (0.68)	0.63 (0.65)
Male	-1.85*** (0.45)	3.88*** (0.63)	-1.11** (0.42)	-4.82*** (0.40)
A-level	1.79** (0.61)	-1.46 (0.85)	-0.26 (0.57)	0.12 (0.54)
GCSE	2.96*** (0.62)	-0.67 (0.86)	0.14 (0.57)	1.77** (0.54)
Education: (ref = Undergrad or Higher)				
Below GCSE	3.38** (1.13)	0.05 (1.58)	-0.58 (1.06)	1.30 (1.01)
No qualifications	6.62*** (0.96)	-3.27* (1.34)	-1.05 (0.89)	2.24** (0.85)
Left / right scale	8.38*** (0.50)	-0.39 (0.70)	0.26 (0.47)	2.03*** (0.44)
Lib / auth scale	10.43*** (0.55)	-4.66*** (0.76)	-1.04* (0.51)	-0.32 (0.48)
(Intercept)	52.09*** (0.76)	43.18*** (1.04)	43.29*** (0.70)	35.64*** (0.66)
R ²	0.32	0.05	0.04	0.02
Adj. R ²	0.32	0.05	0.04	0.02
Num. obs	9722	8212	8466	8597

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: All continuous independent variables have been standardised (divided by two times the standard deviation) to make coefficients comparable. The dependent variable was measured in May 2021, and all independent variables in November 2019. Higher values on the partisan identity scale equate to pro-government (Conservative) partisan strength, low values to pro-opposition (Labour / other) partisan strength. Higher values on the Brexit identity scale relate to stronger Leave identity, and lower values to stronger Remain identity. Non-identifiers were assigned to the midpoint of the respective scale.

Table A5.5: Results from an OLS regression of performance scores of three countries by partisan and Brexit identity scales

	<i>Raw country score</i>		
	Korea	Sweden	USA
Partisan ID (ref = None / DK)			
Partisan ID (scale)	1.97** (0.72)	-1.21 (0.64)	1.79*** (0.54)
Brexit ID (scale)	-4.99*** (0.75)	3.55*** (0.66)	5.18*** (0.56)
Brexit ID (ref = None / DK)			
Age	2.81*** (0.65)	-0.14 (0.57)	4.24*** (0.48)
White British	0.57 (0.95)	0.22 (0.86)	0.90 (0.72)
Male	8.96*** (0.61)	-2.88*** (0.53)	1.42** (0.44)
A-level	-3.54*** (0.82)	2.10** (0.72)	0.24 (0.60)
GCSE	-5.83*** (0.83)	3.52*** (0.73)	1.20* (0.60)
Education: (ref = Undergrad or Higher)			
Below GCSE	-7.49*** (1.54)	3.97** (1.34)	-1.03 (1.11)
No qualifications	-14.42*** (1.26)	3.91*** (1.13)	2.57** (0.95)
Left / right scale	-0.47 (0.67)	3.01*** (0.59)	7.46*** (0.49)
Lib / auth scale	-7.95*** (0.73)	8.53*** (0.65)	3.44*** (0.54)
(Intercept)	60.35*** (0.98)	49.39*** (0.88)	27.55*** (0.74)
R ²	0.11	0.07	0.11
Adj. R ²	0.10	0.07	0.11
Num. obs	7037	7891	9235

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: All continuous independent variables have been standardised (divided by two times the standard deviation) to make coefficients comparable. The dependent variable was measured in May 2021, and all independent variables in November 2019. Higher values on the partisan identity scale equate to pro-government (Conservative) partisan strength, low values to pro-opposition (Labour / other) partisan strength. Higher values on the Brexit identity scale relate to stronger Leave identity, and lower values to stronger Remain identity. Non-identifiers were assigned to the midpoint of the respective scale.

A5.4 Alternative Model Specifications

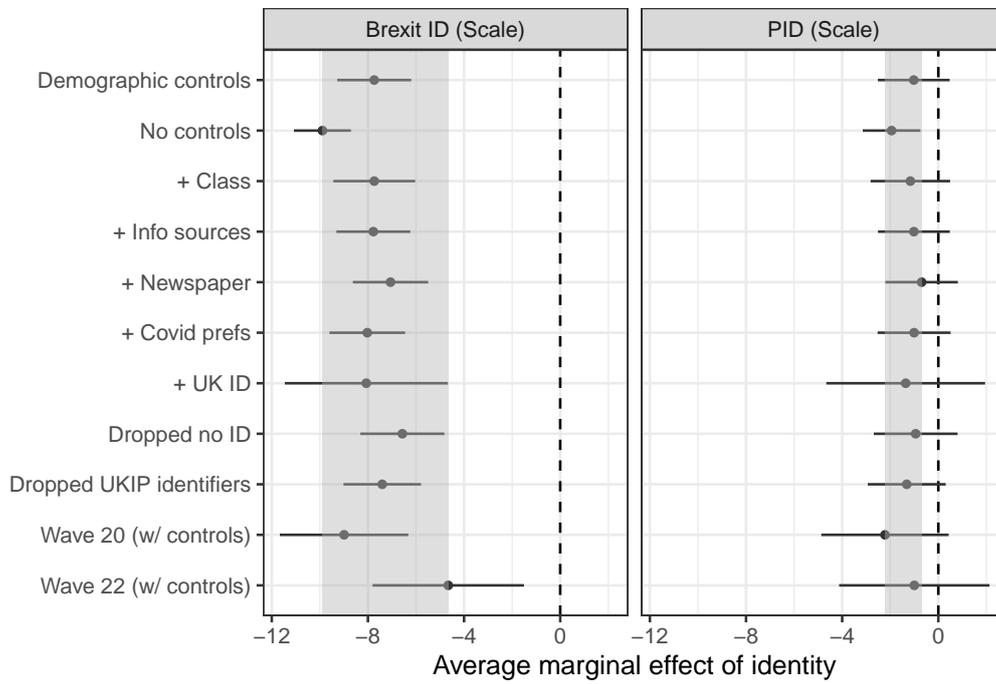


Figure A5.2: The effect of model specification on the coefficients of social identity strength on evaluations of China's performance

Note: For more details of the alternative specifications, see Table A5.7 below.

Table A5.6: Results from an OLS regression of UK relative performance scores on partisan and Brexit identity

	<i>Dependent variable: UK relative score</i>		
	1	2	3
Partisan ID (ref = None / DK)			
Government	5.57*** (0.63)		4.73*** (0.63)
Opposition	-2.67*** (0.56)		-1.12* (0.56)
Brexit ID (ref = None / DK)			
Remain		-8.88*** (0.81)	-8.74*** (0.82)
Leave		2.47** (0.80)	1.59 (0.81)
Age	10.17*** (0.49)	10.20*** (0.49)	9.61*** (0.49)
White British	3.32*** (0.74)	2.84*** (0.73)	2.48*** (0.73)
Male	-2.03*** (0.46)	-2.38*** (0.45)	-2.43*** (0.45)
Education: (ref = Undergrad or Higher)			
A-level	3.72*** (0.62)	2.59*** (0.61)	2.64*** (0.61)
GCSE	4.30*** (0.61)	2.35*** (0.61)	2.61*** (0.61)
Below GCSE	4.31*** (1.12)	2.36* (1.11)	2.57* (1.10)
No qualifications	9.66*** (0.95)	7.12*** (0.95)	7.60*** (0.94)
Left / right scale	7.26*** (0.51)	8.23*** (0.47)	6.45*** (0.50)
Lib / auth scale	14.24*** (0.52)	11.96*** (0.53)	11.26*** (0.53)
(Intercept)	6.78*** (0.83)	11.71*** (1.02)	11.24*** (1.04)
R ²	0.27	0.28	0.29
Adj. R ²	0.26	0.28	0.29
Num. obs	10455	10455	10455

*** $p < 0.001$; ** $p < 0.01$; * $p < 0.05$.

Note: The dependent variable was measured in May 2021, and all independent variables in November 2019. Government partisans are those individuals with a Conservative partisan identity, whilst opposition partisans are those who identify with any other UK political party.

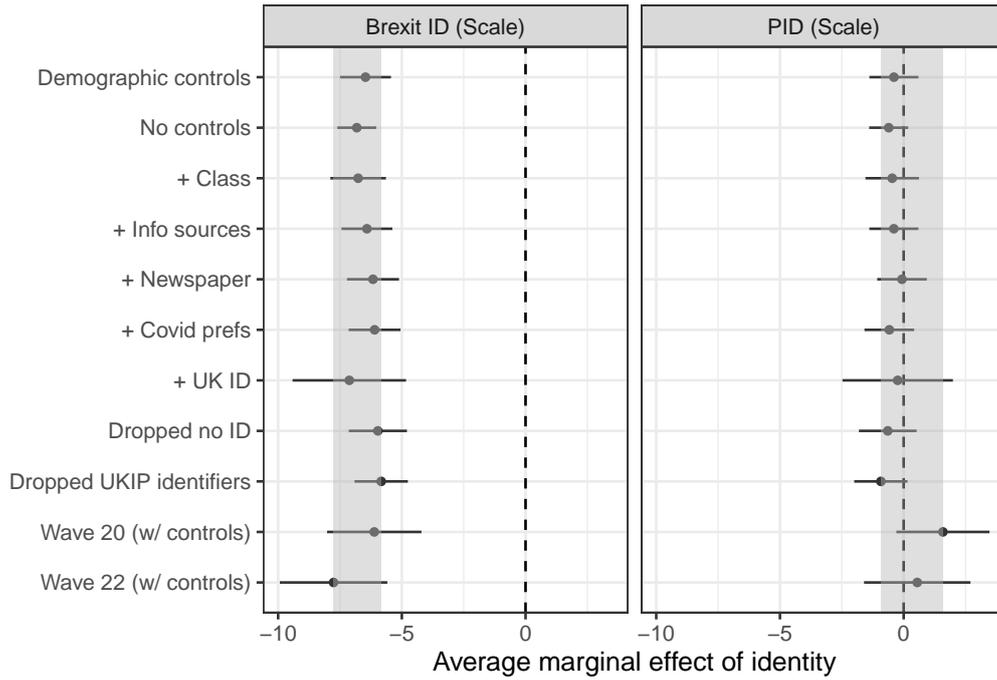


Figure A5.3: The effect of model specification on the coefficients of social identity strength on evaluations of Germany’s performance

Note: For more details of the alternative specifications, see Table A5.7 below.

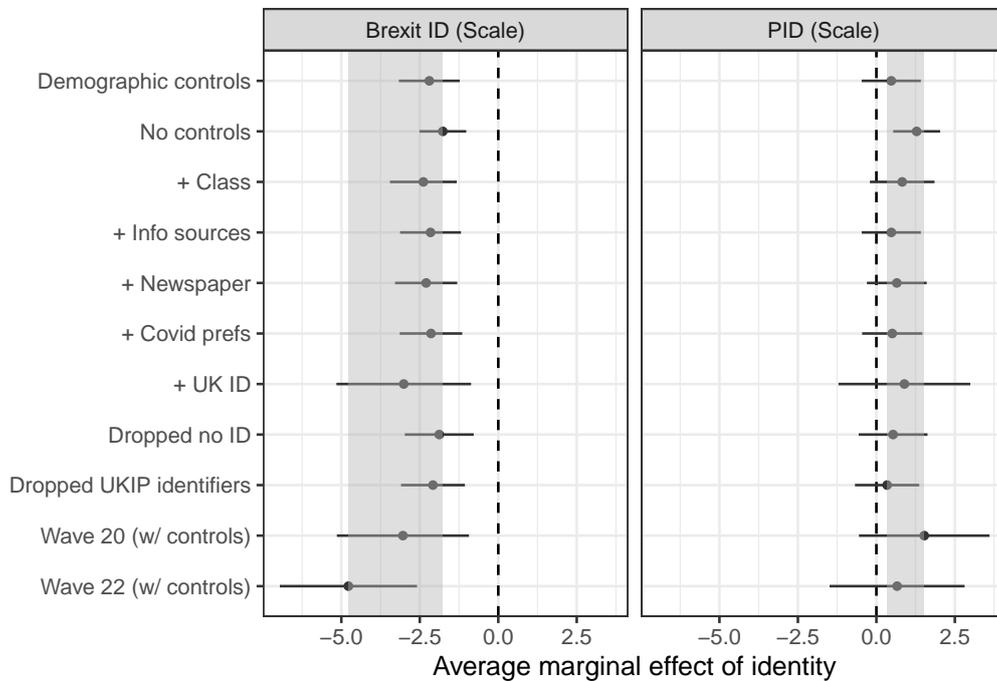


Figure A5.4: The effect of model specification on the coefficients of social identity strength on evaluations of Italy’s performance

Note: For more details of the alternative specifications, see Table A5.7 below.

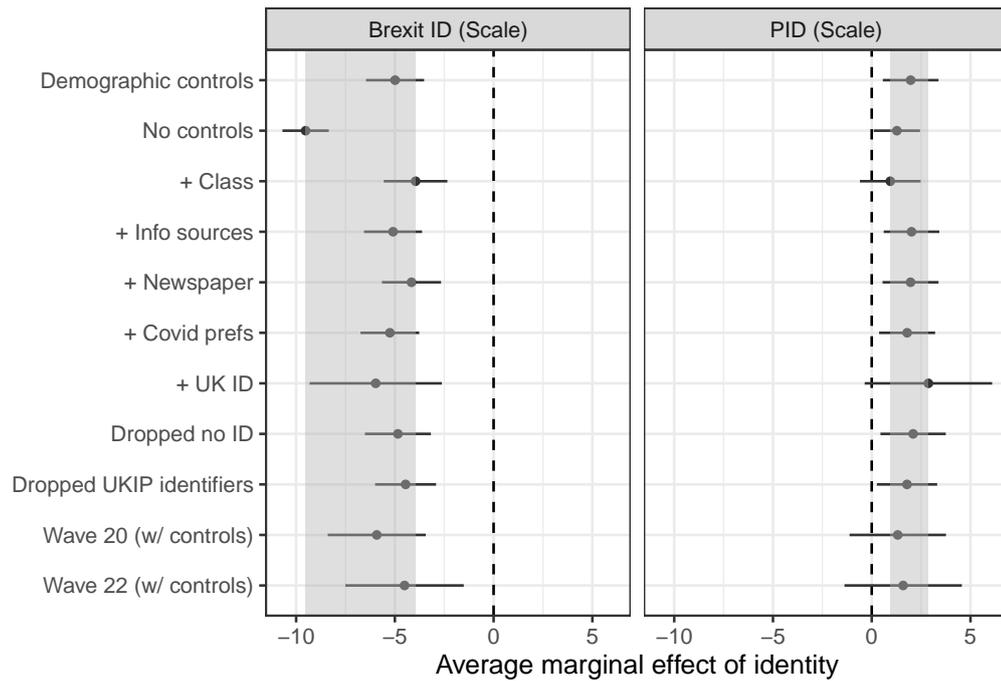


Figure A5.5: The effect of model specification on the coefficients of social identity strength on evaluations of South Korea’s performance

Note: For more details of the alternative specifications, see Table A5.7 below.

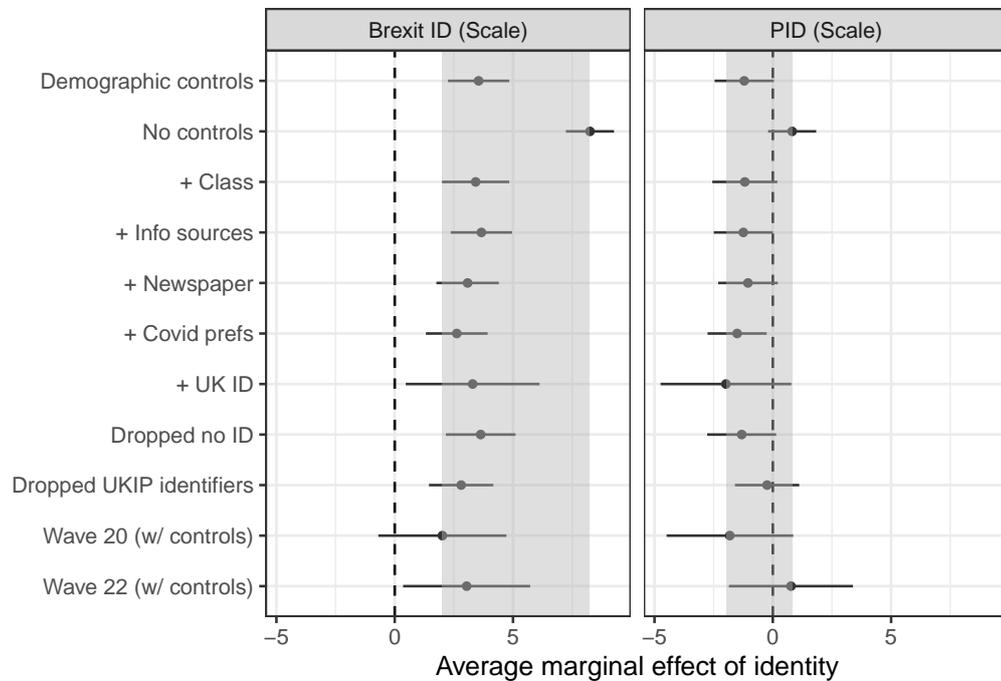


Figure A5.6: The effect of model specification on the coefficients of social identity strength on evaluations of Sweden’s performance

Note: For more details of the alternative specifications, see Table A5.7 below.

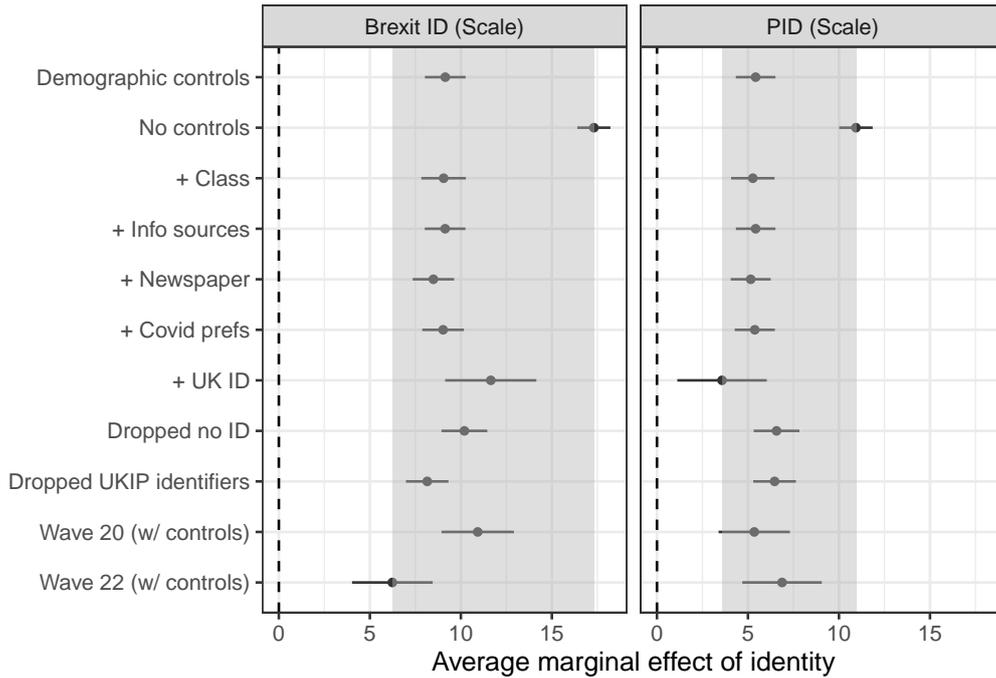


Figure A5.7: The effect of model specification on the coefficients of social identity strength on evaluations of the UK’s performance

Note: For more details of the alternative specifications, see Table A5.7 below.

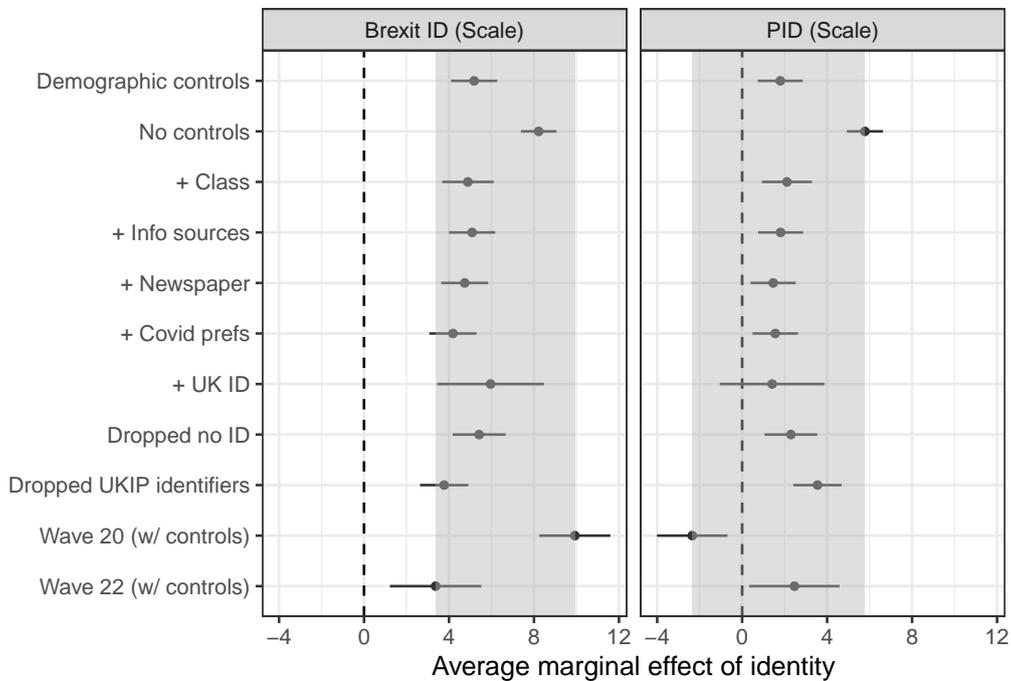


Figure A5.8: The effect of model specification on the coefficients of social identity strength on evaluations of the USA’s performance

Note: For more details of the alternative specifications, see Table A5.7 below.

Table A5.7: Details of alternative specifications

Shorthand	Full specification
Demographic controls	The model displayed in the main paper, with the relative scores measured in wave 21 and controls for age, ethnicity, gender, education, left-right position and liberal-authoritarian position all included (measured in wave 17).
No controls	None of the control variables (age, education etc.) are included in the model. The only independent variables are the identity variables.
+ Class	The display model with an additional variable capturing the respondent's class, measured with a categorical variable representing whether an individual's occupation is 'higher managerial' (the reference category), 'lower professional', 'intermediate occupations', 'small employers', 'lower supervisory', 'semi-routine', or 'routine'. Coded from the national sector analytic variable <code>ns_sec_analytic</code> , with higher managerial and professional collapsed into the single category of 'higher managerial'.

Details of alternative specifications (continued)

Shorthand	Full specification
+ Info sources	<p>The display model with an additional variable included capturing a scale that is coded from a series of answers given by respondents to the question: ‘During the last seven days, on average how much time (if any) have you spent per day following news?’, where the following news sources were television, newspaper (online), radio, internet (excluding newspapers), and talking to other people. Possible answers were ‘No time at all’, ‘Less than 1/2 hour’, ‘1/2 hour to 1 hour’, ‘1 to 2 hours’, ‘More than 2 hours’, or ‘Don’t know’. I take the mean of these different answers, with ‘Don’t knows’ imputed to the midpoint. Individuals who have no responses to any of these, or answered ‘Don’t know’ to every single item are coded as missing.</p>
+ Newspaper	<p>The display model with an additional categorical variable representing whether a respondent’s preferred daily newspaper officially endorsed the Conservatives, Labour, a different party or no party, during the 2019 General Election, with respondents who do not read a daily newspaper acting as the reference category. Preferred newspaper is coded from the BESIP profile variable <code>p_paper_read</code>, measured in wave 17. Table A5.8 details the newspaper affiliations, which are based on the categorisation found at https://en.wikipedia.org/wiki/Endorsements_in_the_2019_United_Kingdom_general_election.</p>

Details of alternative specifications (continued)

Shorthand	Full specification
+ Covid prefs	<p>The display model with an additional variable capturing the degree to which the respondent's preferences concerning lock-down even if it carries an economist cost. Specifically, I use the eleven-point scale, treated as continuous, coded from the variable <code>cvEconSelf</code>, measured in wave 21, which captures responses to the question: 'Some people think that the government should do everything it can to reduce the number of coronavirus infections, even if it greatly damages the economy. Others think that the government should try to save the economy, even if increases the number of coronavirus infections. Where would you place yourself on this scale?' The emphasis is present in the actual BESIP question shown to respondents. Zero represents 'Reduce infections even if it damages the economy' whilst ten represents 'Save the economy even if it increases infections'. 'Don't know' responses were treated as missing.</p>
+ UK ID	<p>The display model with an additional variable capturing the respondent's UK national identity. The variable is derived from the <code>ukAttach</code> grid which provides a similar set of indicators to the partisan and Brexit identity strength scales used for the main analysis - see the BES codebook for full details. I code them in the same manner as the identity strength scales, though there is no direction since there is no way for respondents to indicate that they have an 'opposite' national identity. The variable is only available in wave 19, so the sample size is reduced considerably when it is included.</p>

Details of alternative specifications (continued)

Shorthand	Full specification
Dropped 'No PID'	Individuals with no partisan identity are dropped from the sample, rather than being assigned to the mid-point of the scale.
Dropped UKIP identifiers	Individuals with a UKIP, BNP or Brexit Party identity are dropped from the sample, rather than being coded as opposition partisans.
Wave 20 (w/ controls)	The display model except that the dependent variables are measured in Wave 20 of the BESIP panel, fielded between June 3 rd and 21 th 2020.
Wave 22 (w/ controls)	The display model except that the dependent variables are measured in Wave 22 of the BESIP panel, fielded between 26 November and 15 December 2021.

Table A5.8: Newspaper affiliations

Conservative	Labour	Other	No Affiliation
The Express	The Mirror	The Scotsman	The Daily Star
The Daily Mail	Daily Record	The Herald (Glasgow)	The Daily Star of Scotland
The Scottish Daily Mail	The Guardian	The Western Mail	The Financial Times
The Sun		Other local daily morning newspaper	The Independent
The Telegraph		Other newspaper	
The Times			

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