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Income Shocks, Political Support and Voting Behaviour

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Income Shocks, Political Support and Voting Behaviour*

Abstract

We provide new evidence on the effects of economic shocks on political support, voting behaviour and political opinions over the last 25 years. We exploit a sudden, large and long-lasting shock in the form of job loss and trace out its impact on individual political outcomes for up to 10 years after the event. The availability of detailed information on households before and after the job loss event allows us to reweight a comparison group to closely mimic the job losers in terms of their observable characteristics, pre-existing political support and voting behaviour. We find consistent, long-lasting but quantitatively small effects on support and votes for the incumbent party, and short-lived effects on political engagement. We find limited impact on the support for fringe or populist parties. In the context of Brexit, opposition to the EU was much higher amongst those who lost their jobs, but this was largely due to pre-existing differences which were not exacerbated by the job loss event itself.

Keywords: job loss, political support, voting

JEL classification: C21, C23, D72, J63

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1 Introduction

"It's the economy, stupid." James Carville, Clinton Strategist, 1992.

The idea of the 'Economic voter' — that the economy plays a key part in a government's popularity — is both widely held and also strongly supported by the empirical evidence (Lewis-Beck & Stegmaier 2013). Exactly how the economy impacts on voter behaviour is, however, less clear. Is the voter making a decision based on their own personal circumstances or on a judgement about how the government is managing the economy more generally?

Early theorists (e.g. Downs 1957) emphasised the importance of personal (also called pocketbook or egotropic) considerations. It was thought that when seeking to hold the government to account, individuals should be more certain about, and care more about, changes in their own personal circumstances than the health of the economy as a whole, and so this would therefore be more germane. However, empirical research appeared to show that changes in personal circumstances did little to change voting behaviour with Lewis-Beck & Stegmaier (2000) and Lewis-Beck & Stegmaier (2013) concluding that the evidence for 'pocketbook' voting was slight.

However, many of these early empirical studies were problematic from a methodological perspective. Firstly, some were based on aggregated data, either in terms of voting or in terms of measures of economic well-being. Clearly these did not allow an analysis of how personal economic experiences influenced voting behaviour. Secondly, early studies using individual level information on voting or preferences tended to use cross-sectional data. If an individual's economic outcomes (such as the likelihood of unemployment) are correlated with unobserved characteristics that are also correlated with voting behaviour, then this raises a challenge in establishing a causal link between changes in economic circumstances and individual voting. Thirdly, cross-sectional data does not allow an examination of dynamics — for example, whether changes in personal economic circumstances impact voting behaviour immediately, or for how long those effects persist.

To deal with these issues, in this paper we estimate, at an individual and household level, the effects of sudden and unexpected economic hardship on individual political behaviour over a long time period. We follow a sample of over 5,000 workers who lose their jobs and measure changes in three distinct aspects of individuals' political behaviour: their support for different UK political parties; their voting behaviour in national elections; and their opinions about various political issues, including questions about populism and opposition to the European Union. We track these changes for up to 10 years after the job loss event and compare them to a suit-

ably matched control group of workers who did not experience job loss. Because the timing of job loss differs across individuals, and because the impact of job loss is strongly dependent on the time before and after the event, our methodology relies on the recent innovations in difference-in-differences analysis (see the many recent references in Baker et al. 2022).

In doing this, we provide the first comprehensive study of the effects of job loss on political support, voting and political opinions for the UK, a country which experienced an upsurge in support for populist political outcomes and a decline in support for the two main parties, culminating in the Brexit referendum result in 2016. Our data cover over 25 years and 8 general elections. Further, our data allows us to link those who lose their jobs to their spouses' political support and voting behaviour, which enables us to evaluate the relative importance of egocentric and (narrowly defined) sociotropic effects.

The focus on job loss has two significant advantages for studying economic effects on political support and voting. First, job loss is an event with dramatic consequences for earnings, and welfare more generally, in both the short- and long-term (see Jacobson et al. 1993, and the subsequent literature). Second, job loss is an event which occurs at a particular time, which allows us to more plausibly estimate the causal impact by tracking voting intentions before and after the job loss event.

We use the harmonised British Household Panel Survey (BHPS) and UK Household Longitudinal Survey (UKHLS) datasets (Institute for Social and Economic Research 2019) to follow a panel of UK households from 1991 to the present. Respondents are asked a detailed set of questions about their economic circumstances (including recent experience of job loss), their support for political parties and their voting intentions. In most years of the panel, respondents are also asked about their voting behaviour in the most recent UK general election.

Our results begin by replicating the existing evidence on the cost of job loss. The effect in terms of earnings is sudden, large and long-lasting. Ten years after job loss, treatment group earnings are 20% lower than the control group. However, these costs are far from uniform: while a large fraction of workers who lose their jobs experience spells of non-employment which imply very large losses in income, others experience much smaller losses. We also show that while job loss events are sudden they are preceded by "early warnings" in the form of a reduction in job satisfaction related to increased job insecurity.

We then estimate the effect of these shocks on three main types of political outcomes: support for political parties, voting behaviour in general elections, and political views. We find consistent, long-lasting but quantitatively small effects on support for the incumbent party, and

short-lived effects on political engagement. We find limited impact on the support for fringe or populist parties. Effects on voting behaviour are strikingly similar to those for political support, but are less precisely estimated because voting is less frequently observed than political support. In contrast, political views do not change in predictable ways in response to job loss. Although job loss is highly correlated with certain views (including opposition to the EU), this appears to be a pre-existing relationship which is not itself driven by job loss. Finally, despite the fact that job loss confers large income losses on the household, we find no effect of partner job loss on any of our measures of political support. Our results therefore suggest that even large individual shocks to earnings have moderate, and in most cases short-lived, effects on political attitudes and voting behaviour.

The paper is structured as follows. In Section 2 we discuss the literature on economics shocks and political outcomes and how it relates to our work. In Section 3 we describe the data we use on job loss, political support, voting behaviour and political views. Section 4 explains how we implement an event-study methodology with variable treatment timing. Section 5 reports the impact of job loss on individual and household welfare, and Section 6 reports our main results on the impact of job loss on political outcomes. Various robustness exercises are reported in Section 7 and Section 8 concludes.

2 Literature

There is an extensive literature that seeks to examine how changes in the state of the economy impacts on voting patterns, and in particular how the incumbent party is punished for poor economic performance (Healy & Malhotra 2013). Many of the early papers were aggregate studies that examined how variations in voting between areas correlated with their different economic circumstances. However, by their nature, these studies did little to uncover the mechanisms that drive voters to change their behaviour.

More recently the thrust of empirical research has been to examine how the economy affects an individual's social/political attitudes and voting behaviour. The papers looking at attitudes have focused on support for the welfare state and other indicators of 'left wing' orientation. Electoral studies have focused on four main outcomes: support for the incumbent party; support for parties on the 'left'; support for 'extremist' parties; and withdrawal from the political process.

A number of studies have used individual level data for the dependent variable variable but

continued to use aggregated data for the regressors e.g. district and industry level trade exposure (Dippel et al. 2015), regional import competition (Colantone & Stanig 2018), local house prices (Larsen et al. 2019) and local unemployment (Simonovits et al. 2019). More recently, studies have also considered individual economic circumstances. A key issue when examining the impact of the economy on attitudes and voting behaviour is that changes in economic circumstances are not randomly assigned. That is, a person's situation may be correlated with their characteristics, which may in turn also impact on political attitudes and voting behaviour. If these factors are not adequately controlled for, then the measured impact on political outcomes cannot be considered causal, but will be subject to omitted variable bias. Studies based on cross-sectional data are particular prone to this problem since they typically correlate political outcomes with an individual's current economic status, rather than a change in their circumstances. In a response to this issue, more recent papers have tended to use panel data (Healy & Lenz (2017), Healy et al. (2017), Simonovits et al. (2019)). Such data allows the researcher to control for unobserved individual attributes that are constant over time.

A number of different strategies have been used to identify individual economic 'shocks'. Margalit (2013) and Owens & Pedulla (2013) use panel data to examine the impact of large drops in household income on social attitudes, and observe a leftward shift in policy views. Healy et al. (2017), using panel data combined with administrative data for Sweden concur, and shows that both pocketbook and sociotropic considerations are important in determining voting decisions. Several possible shortcomings remain with these studies however: firstly, changes in household income can originate from many sources, and these need to be adequately controlled for; secondly, whether the observed falls in income are unanticipated is open to question. To overcome these issues, a second approach has examined the impact of truly random unexpected events (lottery wins) on attitudes (Doherty et al. (2006), Powdthavee & Oswald (2016)). Since such wins are by their nature unanticipated, they are therefore plausibly exogenous. However, since they are also inherently unusual, the extent to which the results from such studies are generalisable is moot.

An alternative approach to examine changes in economic circumstance is to use labour market status, and in particular unemployment as a measure of economic hardship. Grafstein (2005) uses cross-sectional data from the US National Election Survey to examine the impact of employment status on voting Democrat. Emmenegger et al. (2015) construct a measure of labour market disadvantage based on whether the individual experiences unemployment, involuntary part-time work, temporary employment or low-wage work and examine how this impacts on political orientation/voting. As with the income literature, they find a deterioration in the individual's circumstances leads to an increase in support for redistributive policies.

More recently the focus of research has shifted to using job loss to capture adverse economic shocks. The focus on job loss has a number of advantages for studying economic effects on attitudes/voting. First, job loss is an event with dramatic consequences for earnings, and welfare more generally, in both the short- and long-term (see Jacobson et al. 1993, and the subsequent literature). Second, job loss is an event which occurs at a particular time, which helps to identify the impact. Thirdly, a substantial proportion of job loss events appear to be unanticipated. The majority of papers that make use of job loss examine how it changes political/social attitudes. Margalit (2013) and Naumann et al. (2015) find that the experience of job loss makes individuals more supportive of welfare assistance. Owens & Pedulla (2013), Martén (2019) and Wiertz & Rodon (2019) have also found statistically significant and sizeable 'leftward shifts' in social attitudes. Braakmann (2017) uses the German Socio-Economic Panel from 2001–2013 to examine the impact of involuntary job loss caused by company closures on party affiliation. He finds that job loss leads to a substantial loss of support for mainstream parties, although not a corresponding increase for parties on the fringe. However, Braakmann's method uses as a control group those who never lose their jobs (including those who are not in employment, and therefore cannot be job losers). The wage and employment effects of job loss that he finds suggest that the treated and controls are quite different before the job loss occurs. The recent econometrics literature has emphasized the critical importance of avoiding 'bad comparisons' when estimating a staggered difference-in-difference treatment model (Goodman-Bacon (2021), Callaway & SantAnna (2021), Sun & Abraham (2021)). In contrast, we use a methodology which "stacks" repeated cohorts of job losers and non-job losers and compares outcomes at the same point in relative and absolute time [1], and we will use inverse probability weighting to ensure that treated and controls are observably similar before the job loss occurs.

The BHPS and UKHLS have been used previously to study political support and voting intentions in the UK. Tilley et al. (2018) use the BHPS to explore how changes to personal finances affect political support, and argue that the "pocketbook" hypothesis is relevant only when voters attribute responsibility for changes in personal finances to government policies. Chrysanthou & Guilló (2023) use the BHPS and UKHLS to explore economic determinants of party support and voting behaviour. Neither of these two papers make use of an event-study approach. Liberini et al. (2017) examine the impact of subjective wellbeing on incumbent support. They also adopt a method similar to that used in this paper to examine the impact of a negative shock, in the form of spousal death. They find that this reduces incumbent support, even if government policies cannot reasonably be blamed for the death.

[1] Cengiz et al. (2019) use a similar approach in their study of minimum wage effects

3 Data

The British Household Panel Survey (BHPS) is an annual panel of approximately 5,000 British households which includes a rich set of information on individuals' employment and work histories, as well as their political views and voting intentions. The panel covers the years 1991 to 2008, at which point it was replaced by the UK Household Longitudinal Study (UKHLS), an expanded panel of around 40,000 households including the majority of respondents in the BHPS (Institute for Social and Economic Research 2019). We use UKHLS data covering the period 2009 to 2019. By linking across the two surveys, we can follow individuals' employment patterns, political views and voting intentions for up to 28 years. More detail on the sample is provided in Appendix A.

3.1 Measures of job loss

Constructing consistent measures of job loss over the entire sample period is complex. Questions about employment events which occurred between interviews are quite different between the two surveys, and, in addition, respondents in the BHPS who were followed into the UKHLS had a significantly longer interval between interviews while the new survey was introduced. Here we describe briefly how the data were constructed; more detail is provided in Appendix B.

We start with the sample of individuals who are employed and interviewed in wave t and who are also interviewed in wave $t+1$. We remove from the sample those in Northern Ireland, since their political support and voting patterns relate to a different set of parties. Both surveys include information on employment events which have occurred between waves t and $t+1$. Individuals who report at $t+1$ that they are with the same employer as at t are coded as having a continuing employment spell. Individuals who report at $t+1$ that they are with a new employer, or who are no longer in employment, are asked for the reason why the spell in progress at t ended, and the date on which it ended.[2] This information is attached to the spell in progress at wave t so that for each employment spell in each wave we have a marker for job loss and the date on which job loss occurred. For those individuals who are interviewed in the last wave of the BHPS and followed into the UKHLS, we are also able to record what happened to the employment spells in progress at the time of the final BHPS interview by examining their first UKHLS interview. Figure 1 plots the proportion of employment spells in each year which end in redundancy, dismissal or the end of a temporary job.

[2] See Table B2 for a complete list of reasons.

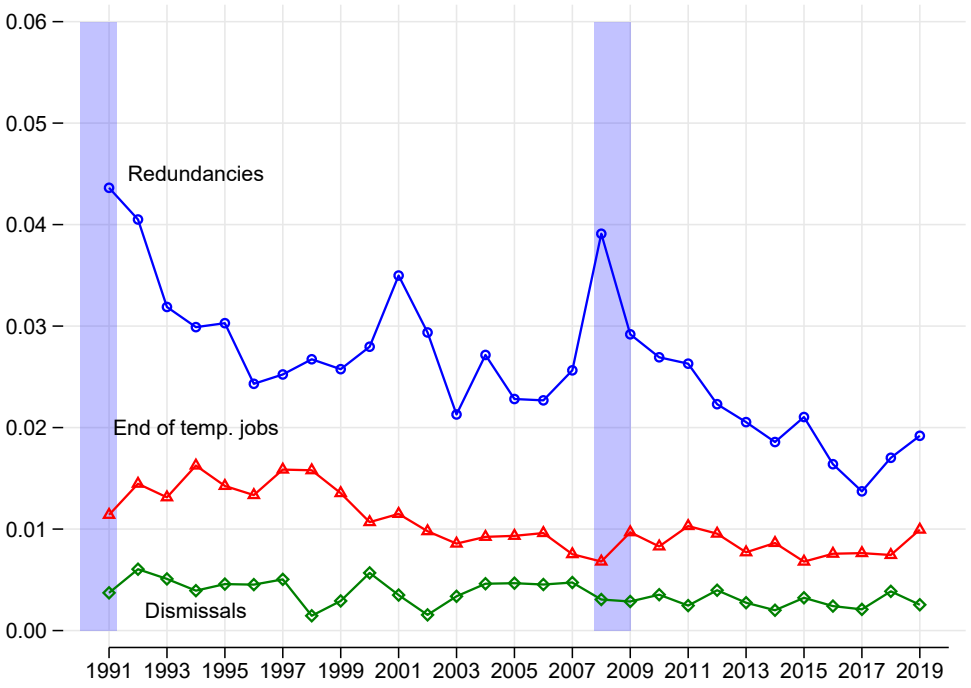


Figure 1: Proportion of jobs held in year t which end in job loss at the time of the following interview, which is typically 12 months later. Shaded areas indicate recessions. Information from BHPS from 1991–2008 and from UKHLS from 2009–2020. From the final BHPS wave (2008), individuals' subsequent interview was approximately 22 months later, and so we only consider job loss events that occur within 12 months of the interview date.

The fraction of jobs which end in redundancy in the next 12 months follows the business cycle, with peaks in the 1991 and 2008 recessions. In contrast, the dismissal rate and the end of temporary jobs is far more stable. Is is striking that after the global financial crisis the redundancy rate continued to fall below the level observed in the 1990s, and was below 2% per year by the end of the sample period.

3.2 Information on political support

Information on respondents' political views and voting intentions are quite consistently recorded across both surveys. There are four questions asked about political support which are available for all respondents in almost every wave:

`support1` "Generally speaking do you think of yourself as a supporter of any one political party?" This question is asked at every wave except wave 26.

`support2` "Do you think of yourself as a little closer to one political party than to the others?" This question is asked only of those who answered "no" to `support1` and is asked in every wave except wave 26.

`support3` "If there were to be a General Election tomorrow, which political party do you think you would be most likely to support?" This question is asked only of those who answered "no" to both `support1` and `support2`.

`support4` "Which party do you regard yourself as being closer to?" This question is asked only of those who answered "yes" to `support1` or `support2`.

Combining `support3` and `support4` together allows us to create a measure of party political support for the great majority of respondents.[3] A concern could be that the partisan respondents do not actually vote for their favoured party in an election. However, in wave 6 an additional question asks "If there were to be a General Election tomorrow, would you vote for the favoured party?". Over 91% responded "Yes" to this question, so it seems reasonable to combine the two into a single measure.

Respondents are also asked about the strength of their support. For those who answered "yes" to `support1` or `support2` and who provide the name of the party they support, they are asked "Would you call yourself a very strong supporter of this party, fairly strong or not very strong?" We create a variable "strength" which takes four values. For those who do not support a party at all, strength=1, while strength=2,3,4 for, respectively, "not very strong", "fairly strong" and "very strong". In Section 7 we examine whether changing political support is more likely for those whose pre-existing support is relatively weak.

The time-series patterns of political support over the sample period are plotted in Figure C1 in Appendix C. Panel (a) shows how the proportion of the sample who said they had "no support" for any political party increased substantially between the early 1990s and the early 2010s, after which there was a sharp increase in the proportion who reported being a supporter of a political party. Panel (b) shows a significant decline in support for the major political parties from the early 1990s to the early 2010s, which again reversed somewhat towards the end of the sample period. Panel (c) shows that the proportion of the sample reporting "strong" support for a political party also declined over the period 1990-2010 but has subsequently recovered to similar levels as at the start of the sample period.

[3] Separate codes for various fringe parties (including the UK Independence Party) only became available in the UKHLS questionnaire.

3.3 Information on voting behaviour

There are two questions asked about voting behaviour:

`vote1` "Did you vote in the <year> general election"

`vote2` "Which political party did you vote for?"

These questions were asked in a subset of waves, indicated in Appendix D. For the elections of 1992, 1997, 2001 and 2005 we have multiple responses (possibly from the same respondents) at different points in time. However, in the UKHLS these questions are only asked of those whose interview date is within about 1 year of the general elections held in 2010, 2015 and 2017. The exact wording depends on the proximity of the survey to the general election.[4] Figure D1 shows that the proportion claiming to have voted in the survey is substantially higher than the actual turnout in every election, sometimes by as much as 10pp.

Using the sample who say that they voted in the last election, we calculate the predicted vote share. Figure D2 shows that the proportion of the sample who said that they voted Conservative is very close to the actual vote share (panel (a)). The proportion of the sample who said that they voted Labour is rather larger than the actual vote share in the three Labour victories in 1997, 2001 and 2005 (panel (b)). The proportion of the sample who said that they voted for other parties is slightly lower than actual votes cast but does capture the upward trend in the vote share of nationalist and non-mainstream parties in 2015.

3.4 Information on political views

It is common to argue (e.g. Goren 2005) that political support and party identification (and therefore voting behaviour) are quite stable, determined by long-run factors such as family background and group identification, and therefore unlikely to be greatly influenced by economic shocks, even if, as in our case, the shocks are large and long-lasting. One response to this is to examine the effects of economic shocks on individuals who do not have strong pre-existing levels of political support; we do this in Section 7. A second response is to note that there is also a counter-argument that political attitudes, particularly towards welfare and redistribution, are partly determined by economic self-interest. For example, Margalit (2013)

[4]For example, in wave 5 (interviews in 1995 and 1996) respondents were asked "Did you vote in the 1992 General Election." If respondents answered "Yes" they were then asked "Which political party did you vote for".

finds that "voters' preferences regarding welfare policy are strongly affected by their own economic circumstances".[5] Therefore, we will also consider the effects of job loss on respondents' agreement with a series of political statements. Unfortunately, neither the BHPS or the UKHLS asks questions specifically about political issues which are close to the issue of job loss (such as whether they agree with the idea that the welfare state should support the unemployed), but there are a number of questions asked which are possible candidates for determining whether job loss affects political views, even if it does not affect voting and party support.

Firstly, we measure agreement with "right wing" political ideas by the responses to these statements. Each item is scaled from 1 to 5, where 5 represents the most right-wing position (for example "strongly agree" for `opsoca` and "strongly disagree" with `opsocb`). An average of all responses is taken, so the result ranges from 1 to 5 for each individual.

`opsoca` Ordinary people get their fair share of the nation's wealth (BHPS 1991–2008)

`opsocb` There is one law for the rich and one for the poor (BHPS 1991–2008)

`opsocc` Private enterprise is the best way to solve Britain's economic problems (BHPS 1991–2008)

`opsocd` Major public services and industries ought to be in state ownership (BHPS 1991–2008)

`opsoce` It is the government's responsibility to provide a job for everyone who wants one (BHPS 1991–2008)

`opsocf` Strong trade unions are needed to protect the working conditions and wages of employees (BHPS 1991–2008)

`opsocg` It is just that those who can afford it obtain better education for their children (BHPS 1992–1993)

`opsoch` It is alright if businessmen make good profits because everybody benefits in the end (BHPS 1992–1993)

`opsoci` It is unjust that rich people are able to buy themselves better health care than poor people (BHPS 1992–1993)

`opsocj` In Britain, people have equal opportunities to get ahead (BHPS 1992–1993)

[5] However, it is worth noting that there is also a literature which considers core political values (such as attitudes towards equality) to be "coherent and stable" (p.1264 Evans & Neundorf 2020) and which themselves determine partisanship in response to political parties' policy goals.

A number of recent papers have argued that economic factors can be an important determinant of populist beliefs (Colantone & Stanig 2016, Dippel et al. 2015). If we characterise populism as having elements of anti-elitism and a defence of the "will of the people" (see for example Mudde 2007, Mudde & Rovira Kaltwasser 2018), then the following statements provide a measure of agreement with populist political ideas. Again, each item is scaled from 1 to 5 so that 5 represents the most "populist" position i.e. someone who disagrees with `oppola`, agrees with `oppolb` and is dissatisfied in response to the `demorient` statement.

`oppola` On the whole, what governments do in Britain reflects the wishes of the people (BHPS 1992–2007)

`oppolb` Ordinary people don't really have a chance to influence what governments do (BHPS 1992-2007)

`demorient` On the whole, are you very satisfied, fairly satisfied, a little dissatisfied or very dissatisfied with the way democracy works in this country? (UKHLS 2010–2019)

Finally, we note that, in the UK, opposition to the European Union has been one of the most important manifestations of discontent with political orthodoxy, and a number of papers have argued that economic deprivation was a key driver of the vote for Brexit (e.g. Becker et al. 2017). The BHPS and the UKHLS have asked a number of questions about views towards the EU which we use to create a summary measure from the following statements. Each item is scaled from 1 to 5 so that 5 represents the most opposed to the EU.[6]

`opeur1` do you think that Britain's membership of the European Union is a good thing, a bad thing, or is it neither good nor bad? (BHPS 1999–2007)

`opeur2` would you say that Britain has on balance benefited or not from being a member of the European Union? (BHPS 1999–2007)

`opeur3` do you think Britain's long-term policy should be (1) Leave the EU (2) stay in cut EU power (3) leave things as they are (4) stay in more EU power (5) form single EU government (BHPS 1999–2007)

`opeur4` If there were a referendum on whether Britain should join the single European currency, the Euro, how do you think you would vote? Would you vote to join the Euro, or not to join the Euro? (BHPS 1999–2007)

[6]The variables `opeur1`, `opeur3` and `opeur4` are in the form of a three-point scale (1,2,3) which is transformed (1,3,5). `opeur2`, `eumem` and `voteeuref` are in the form of a two-point scale (1,2) which are transformed to (1,5). The variable `voteeuint` is collapsed down from a 10 to a 5-point scale.

`eumem` Should UK remain a member of the EU? (UKHLS 2016–2021)

`voteeuref` How did you vote in the EU referendum? (UKHLS 2019–2021)

`voteeuint` On a 0 to 10 scale, where 0 means that the UK should do all it can to unite fully with the European Union and 10 means that the UK should do all it can to protect its independence from the European Union, where would you place yourself on this scale? (UKHLS 2019–2021)

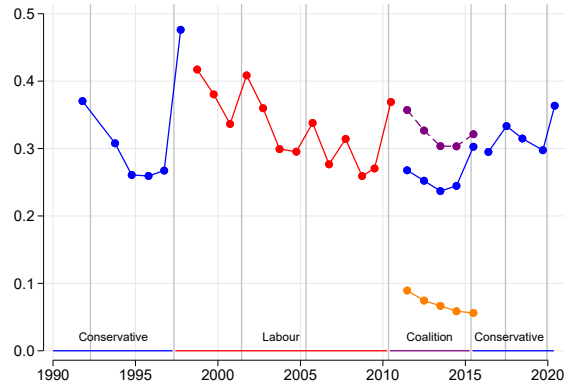
These questions on political views are only available in a subset of waves, and some questions are only asked in the BHPS while others are only asked in the UKHLS. However, our comparison will be between treated and controls in the same year (see Section 4), and so we will be tracking the answer to the same set of questions between the treated and control groups.

3.5 Some descriptive statistics

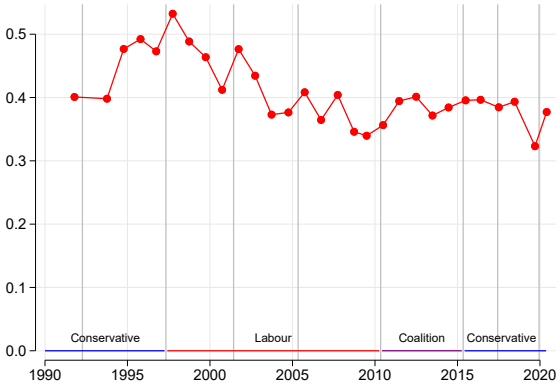
In Figures 2, 3 and 4 we plot the evolution of the dependent variables we will use in the analysis. Figure 2a demonstrates the typical pattern of a secular decline in government popularity following an election win, with some element of recovery as the subsequent election looms. The period covered is unique in recent UK history for containing a Coalition government during the period from 2010-2015. It is notable that the electoral support of the junior partner (Liberal Democrat) fails to recover prior to the 2015 election. Figure 3a shows that responses to questions about voting in the previous election track quite closely the pattern of political support. Figures 2b and 3b show the proportion of the sample supporting and voting for left-wing parties (Labour, Plaid Cymru, Scottish National Party (SNP)). Apart from the surge in support for the Blair led Labour Party for the decade from 1994, this has remained fairly stable around 40%, although the composition of that vote has changed with the rise of the SNP and the consequent decline in support for the Labour Party in Scotland.

Of particular interest in light of the recent discourse on voter disillusionment and disengagement (Jennings et al. 2016, inter alia) are figures 2c and 3c. These show the decline of traditional voter identification with the established political parties and a less pronounced increase in non-voting, although these trends have abated somewhat since 2014. Parties of the extreme right and left have played little part in the electoral makeup of the UK. However, figures 2d and 3d chart the dramatic rise of the UK Independence party (UKIP) leading up to the Brexit referendum in 2016, and then its subsequent decline.

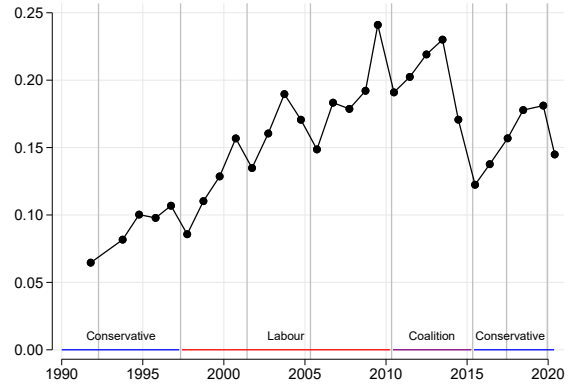
(a) Proportion of sample supporting incumbent party



(b) Proportion of sample supporting left-wing party



(c) Proportion of sample not supporting any party



(d) Proportion of sample supporting fringe party (England only)

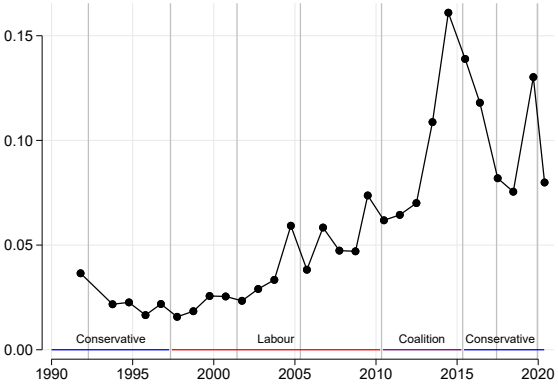
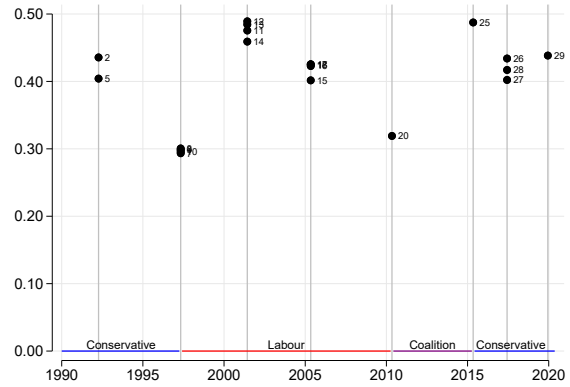
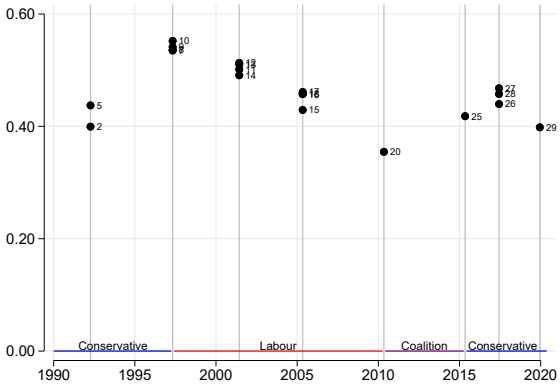


Figure 2: Political support in the BHPS and UKHLS 1991-2017. Each point represents the average for all interviews in that calendar year. In panel (a), support is split between Conservative and Liberal Democrats during the Coalition Government (2010-2015), with total incumbent support represented by the sum (dashed line). In panel (b), left-wing parties are defined as Labour, Green, SNP and Plaid Cymru. In panel (d), fringe parties are defined as all except Conservative, Labour and Liberal Democrat and exclude parties outside England.

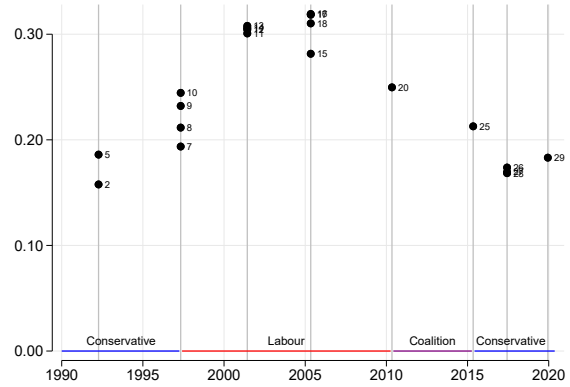
(a) Proportion of sample voting for incumbent party



(b) Proportion of sample voting for left-wing party



(c) Proportion of sample not voting in last general election



(d) Proportion of sample voting for fringe

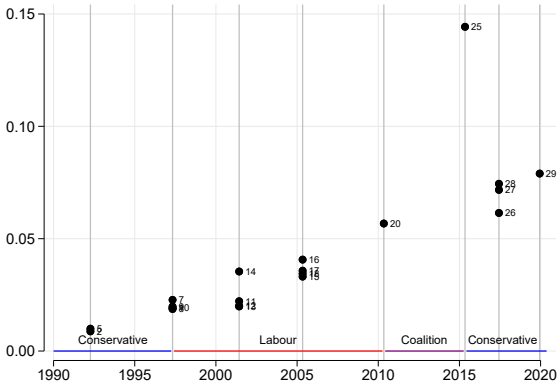
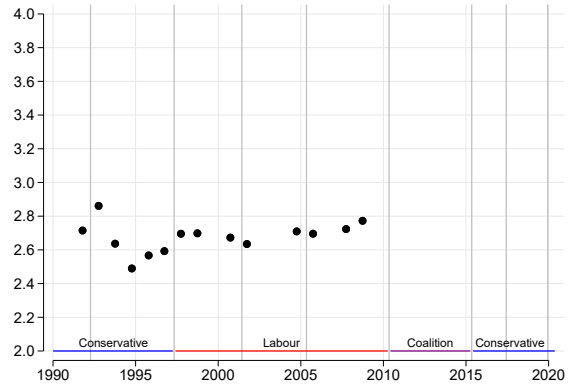


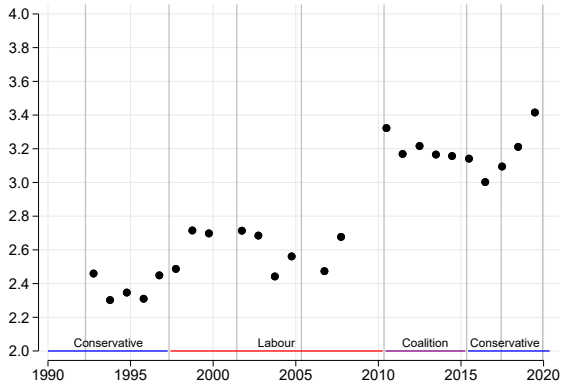
Figure 3: Voting behaviour in the BHPS and UKHLS 1991–2017. Each point represents the average for all interviews in that calendar year.

Figure 4 plots agreement with various political views over the sample period. In contrast to the large swings in support for different parties, agreement with right-wing statements, shown in Figure 4a is quite stable, although note that these statements are only asked in the BHPS and so information on how this tracks after 2010 is not available. Agreement with "populist" statements, shown in Figure 4b, appears to jump significantly after 2010, but this coincides with a change in the question asked: before 2010 the BHPS asks whether governments in Britain reflect the wishes of the people and whether ordinary people can influence what governments do; after 2010 the UKHLS asks whether people are satisfied with the way democracy works. Finally, in Figure 4c, we see that the four questions asked in the BHPS (`opeur1` to `opeur4`) in relation to the EU elicit slightly more negative responses than the questions asked in the UKHLS from 2016 onwards.

(a) Agreement with right-wing statements



(b) Agreement with populist statements



(c) Opposition to EU

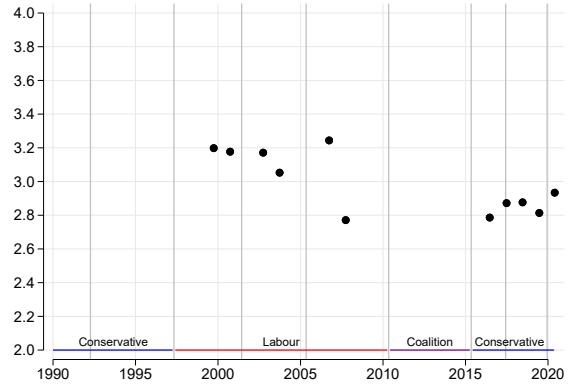


Figure 4: Political opinions in the BHPS and UKHLS 1991–2017. Scale is 1 "strongly disagree" with right-wing statements to 5 "strongly agree". In panels (b) and (c), the statement changes from 2010 onwards. Each point represents the average for all interviews in that calendar year.

4 Methodology

To examine the effect of job loss on political support and voting behaviour we will use an event-study model which compares outcomes before and after a job loss event for a treated and control group. Event-studies (also referred to as "staggered adoption" by Athey & Imbens (2018)) are a particular kind of difference-in-differences model, and have been widely used in the study of job loss since at least Jacobson et al. (1993). The use of an event-study design explicitly allows for variation in treatment effects over time, and it allows for a much cleaner comparison of treated and controls. Failure to account for staggered adoption when selecting the control group can result in 'bad comparisons' when estimating the treatment model (Goodman-Bacon (2021), Callaway & SantAnna (2021), Sun & Abraham (2021)).

Define a series of indicator variables D_i^c, $c = 1991, \ldots, 2019$ which take the value 1 if individual i experiences job loss between wave c and the wave $c+1$ interview, and zero otherwise. Those with $D_i^c = 0$ will include individuals who change job between c and $c+1$ for reasons other than job loss.[7] D_i^c is constant for each individual for a given value of c, but each individual has a separate indicator for each cohort c. We refer to the sample with $D_i^c = 1$ as the cohort c treatment group and those with $D_i^c = 0$ as the cohort c control group. We restrict the sample to all those who are interviewed in wave c and wave $c+1$, who are in employment and aged between 20 and 60 in wave c.[8] We cannot rule out the possibility that workers experience job loss before the sample period begins, but we mitigate the problem by restricting the sample only to workers who do not experience job loss between waves $c-3$ and c.

Define y_{it} to be the outcome of interest for individual i in wave t. These outcomes include political support, retrospective voting decisions and political views (as described in Section 3). y_{it} is typically measured at various points both before $t <= c$ and after $t > c$ the job loss, although not necessarily in every wave. We wish to estimate the impact of D_i^c on y_{it}. The least restrictive method would be to estimate, separately for each job-loss cohort, a standard difference-in-difference (or event study) model:

$$y_{it} = \alpha^c + \beta^c D_i^c + \sum_{s=1992}^{2019} \gamma^{c,s} T_t^s + \sum_{s=c-9}^{c+10} \delta^{c,s} (T_t^s D_i^c) + \varepsilon_{it}^c, \quad c = 1996, \ldots, 2019. \tag{1}$$

[7] Therefore we do not restrict the control group to include only those who continue in employment after wave c. This contrasts with Jacobson et al. (1993), whose control group consists only of those who *remain in the same firm*. Their definition of earnings losses is therefore "the change in expected earnings if ... the worker would be displaced ... rather than being able to keep his or her job indefinitely." (Jacobson et al. 1993, p.691). Instead, our counterfactual is more general, and is intended to measure the political behaviour of job losers had they not lost their jobs. This approach follows Krolikowski (2018).

[8] The job loss indicator D_i^c is potentially correlated with job loss in earlier periods. Stevens (1997) shows that the persistence of earnings losses after job loss can partly be explained by subsequent job loss events.

The indicator T_t^s takes the value 1 if $s = t$ and zero otherwise. The coefficient $\delta^{c,s}$ is a difference-in-difference estimate of the effect of a job loss which occurred between c and $c+1$ on the outcome y_{it} in year s. For example, $\delta^{1996,2000}$ is an estimate of the effect of job loss which occurred between the 1996 and 1997 interviews on y_{it} in 2000. For the retrospective voting questions s refers to the year in which the election took place, not the year of the survey. $\delta^{c,s}$ is also estimated for up to 10 years before job loss $c-9, c-8, \ldots, c$. The extent to which we can estimate effects before and after job loss depend on the cohort: cohorts near the start of the sample period allow us to estimate effects for many years after the event; cohorts near the end of the sample period allow us to estimate effects for many years before the event. If job loss is an unexpected and exogenous shock, we would expect that $\delta^{c,s} = 0$ when $s < c$. Rejecting $\delta^{c,s} = 0$ for pre-job loss periods amounts to rejecting the common trends assumption. The coefficient β^c captures the pre-existing difference in y_{it} between the treated and control groups in the base year. In most of our specifications the base year is $c = -1$.[9] The coefficients $\gamma^{c,s}$ capture the time-series behaviour of y_{it} for the control group.

Eqn. (1) is useful because it shows that the "staggered treatment timing" problem is removed if we estimate separately by job loss cohort. For each cohort there is a single event which occurs between c and $c+1$. Both treated and controls have a well-defined "event time", which occurs between waves c and $c+1$. In addition, we allow the effect of the job loss to vary in an unrestricted way in each year relative to year c, and so we impose no assumptions about treatment effect heterogeneity with respect to time. However, since we observe a relatively small number of job losses in each cohort, we stack together cohorts and impose the restriction that the effect of job loss relative to the job loss date is the same for each cohort. We then estimate the outcome for time relative to the job loss date. Once stacked, each row in the data is identified by i, c and t, because individuals may appear in several cohorts.[10] The difference between the interview date and the job loss (or non-job loss) date, is relative time, denoted r_{ict}. Thus for example $r_{ict} = 0$ in the year immediately preceding the job loss and $r_{ict} = 1$ in the year immediately after. We restrict attention to $-9 \leq r_{ict} \leq 10$ to ensure sufficient numbers of treated and control observations in each year. Our pooled difference-in-difference model is then

$$y_{ict} = \alpha + \beta D_{ic} + \sum_{r=-9}^{10} \gamma^r T_t^r + \sum_{r=-9}^{10} \delta^r (T_t^r D_{ic}) + \eta_c + \varepsilon_{it}. \tag{2}$$

We include cohort fixed-effects η_c and allow the errors ε_{it} to be clustered by i across cohorts.

[9] The choice of base year can be important if there are pre-job loss differences between treated and controls. In this case, however, after balancing these differences are unimportant.

[10] Butts & Gardner (2021) explains that "this estimator identifies an average of group-specific average treatment effects, weighted by the relative sizes of the group-specific datasets and the variance of treatment status within those dataset." This differs from the the approach of Callaway & SantAnna (2021) whose method weights by the proportion of treated observations in each cohort.

The difference-in-difference estimate δ^r controls for the average difference in y_{it} between the treatment and control groups in the base year $t = -1$.

A number of refinements of Eqn. (2) are possible. First, we can replace the α with person-cohort fixed effects α_{ic} and estimate using differences or mean deviations. This removes the treatment indicator D_{ic} and its parameter β. Note that if the panel is balanced then β in Eqn. (2) completely captures the pre-job loss difference in α_{ic} between the treatment and control groups, and the FE and DiD estimators are identical. However, in this case the panel is unbalanced and the FE and DiD estimators will differ.

$$y_{ict} = \alpha_{ic} + \sum_{r=-9}^{10} \gamma^r T_t^r + \sum_{r=-4}^{10} \delta^r (T_t^r D_{ic}) + \varepsilon_{it}. \tag{3}$$

Second, we could allow for differences in pre-existing trends in political support or voting behaviour between the treatment and control groups. This leads to the estimating equation

$$y_{ict} = \alpha_{ic} + \omega_{ic} t + \sum_{r=-9}^{10} \gamma^r T_t^r + \sum_{r=-4}^{10} \delta^r (T_t^r D_{ic}) + \varepsilon_{it}. \tag{4}$$

Jacobson et al. (1993) note that one can estimate Eqn. (4) by deviating each variable from the person-specific time-trend (as opposed to the person-specific mean in the FE model) and estimating by OLS. Alternatively, one can difference the data and then estimate using a within-estimator (Wooldridge 2010, p.375).

Third, one can control for differences in observable characteristics between the treatment and control groups during the pre-job loss period. There are a number of possible methods to do this, including propensity score matching and reweighting based on the propensity score. We use inverse probability reweighting (IPW), which Busso et al. (2014) suggests outperforms matching estimators. If the reweighting procedure makes the treatment and control groups sufficiently similar before treatment, then allowing for differences in pre-existing levels and trends can be rendered unnecessary. This is the case here, so our main results are based on Eqn. (3) with IPW reweighting.

Finally, we extend our analysis to the effect of partner job loss on political support and voting outcomes. To do this, for each wave we create a dataset of every individual who has a partner or spouse, together with information on whether their partner or spouse experienced job loss or not in that wave. To be in the partner sample, an individual must be: interviewed in wave c, aged between 20 and 60; and have a partner at risk of job loss in that wave.[11] Define D_j^c to

[11] That is, their partner must be interviewed in waves c and $c+1$, and be in employment and be aged between

be the series of job loss indicator variables for partner $j = J(i)$ of individual i in each wave. We then stack together cohorts as before and define relative time r_{jct} in relation to the partner's job loss date. So our comparison is between a treated individual whose partner lost their job r years ago against a control individual whose partner did not lose their job r years ago (but had a partner r years ago who could have experienced job loss). This is achieved by estimating Eqn. (3) with r and D redefined in this way.

5 The costs of job loss

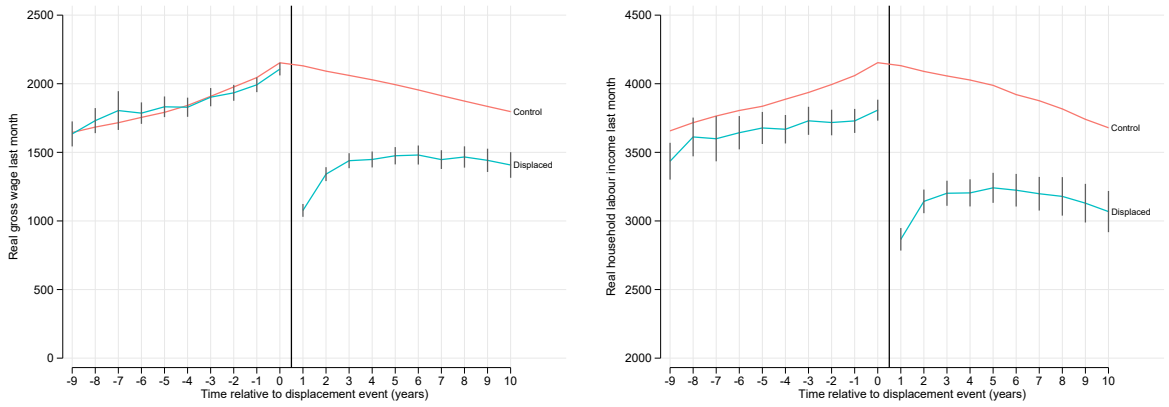
Our hypothesis is that job loss impacts political support because it has large and long-lasting consequences on individual and household economic outcomes. Therefore, before we estimate the impact of job loss on political views and behaviour we examine the cost of job loss in terms of earnings and self-reported wellbeing. Panel (a) of Figure 5 plots the average gross wage of a job loser around the time of the job loss ($r = 0$) relative to a control group who did not lose their job at $r = 0$. The patterns observed here are quite similar to other estimates of the cost of job loss see (see for example Schmieder et al. 2022, Figure 1a) which are based on administrative data. Note that this is a simple comparison of means without any covariate adjustment. Nevertheless, the pre-job loss pattern of wages is quite similar with slightly slower wage growth in the treated group. Even 10 years after the job loss, the treated group earnings are more than 20% lower than the control group.

Because of its large effect on wages, job loss also has a significant impact on household income, which suggests that it may have effect on political support and voting behaviour at the household level. Panel (b) of Figure 5 plots the effect of job loss on household labour income. It is interesting to note that mean household labour income for our sample is almost exactly twice individual earnings. This is because employed individuals tend to also have partners that work. It appears however that on average it is lower income households that experience a job loss. It is clear that the impact of job loss on family income is drastic, opening the possibility that job loss within the household may have an effect on political outcomes, which we will explore by considering the impact of partner job loss in Section 6.5.

The large average wage loss shown in Figure 5 disguises the fact that the wage change following job loss has a wide distribution. Some workers who lose their jobs will face a period of unemployment and zero wages, while others immediately move to another job and may even

20 and 60 in wave c. Note that for i to be in the partner sample does not require that they are themselves at risk of job loss, but instead that they have a partner who is at risk of job loss.

Figure 5: Effects of job loss on wages and household labour income



(a) Gross wages

(b) Household labour income

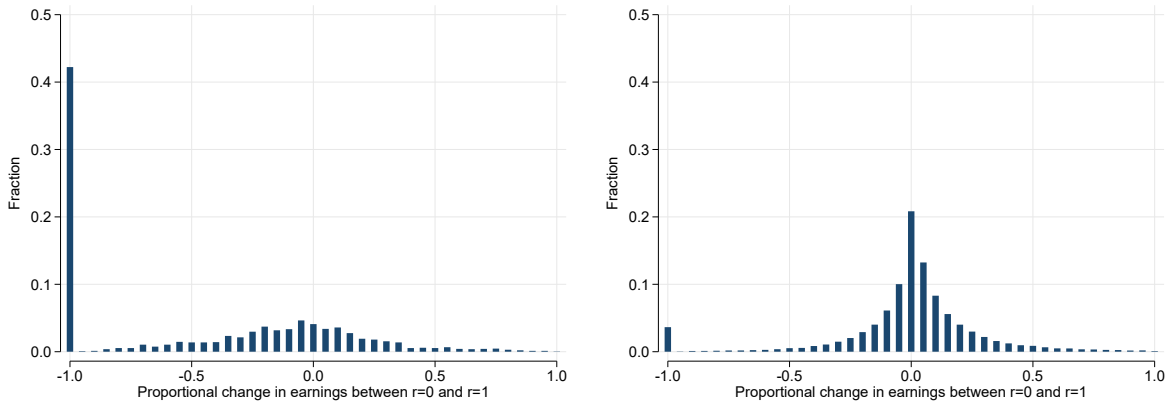
Notes: wages are measured as gross pay from last pay period. The control group mean is an estimate of γ^r from Eqn. (2). The treated group mean is an estimate of $\gamma^r + \delta^r$. The vertical lines represent the 95% confidence interval around estimates of δ^r.

experience a pay increase. This is illustrated in Figure 6. Panel (a) shows that, a year after job loss, more than 40% of job losers have yet to find a job and so have zero earnings. A smaller fraction of job losers have found a new job but experienced a decrease in real wages. The distribution of wage changes for the control group, shown in panel (b) is far more symmetric, but still includes some who exit employment and have zero wages in wave $c+1$. This pattern of increased risk of unemployment and reduction of in-work income persist in future years. In Section 7 we test whether the change in political support is driven by the size of the loss.

Job loss also has other measurable impacts on the individual which may have some relevance for political support and voting intentions. In Figure 7 we plot the mean values for subjective wellbeing (GHQ12 scores) before and after job loss. This is of interest because it has been suggested (Liberini et al. 2017) that subjective wellbeing can influence voting intentions. There are significant falls in GHQ12 around the time of the job loss event, although it is notable that the pattern does not mirror the sharp decline in wages which occurs between $r = 0$ and $r = 1$, shown in Figure 5. Instead, there appears to be a slow decline in self-reported wellbeing which accelerates at $r = -1$ and $r = 0$. Wellbeing is lowest just after job loss, but recovers far more quickly than wages, and catches up with the control group within four years.

The fact that GHQ declines prior to job loss may be explained in a number of ways. Firstly, it is possible that whilst the job loss event itself is unexpected, those who are job losers have declining GHQ scores prior to the event and either select, or are selected into, redundancy. This rationale is supported by panel(a) of Figure 8 which shows that those who lost jobs were more

Figure 6: Distribution of proportional earnings change between $r = 0$ and $r = 1$

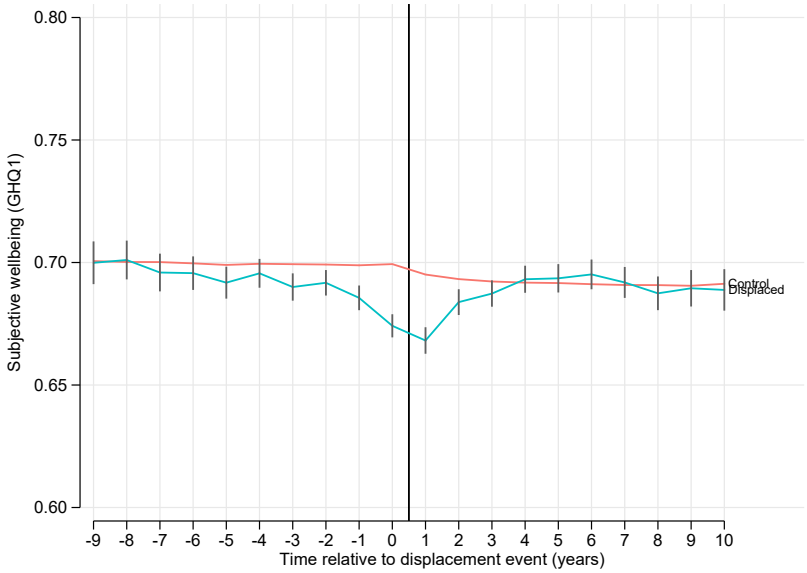


(a) Job loss group

(b) Non-job loss group

Notes: proportional changes greater than one are not shown.

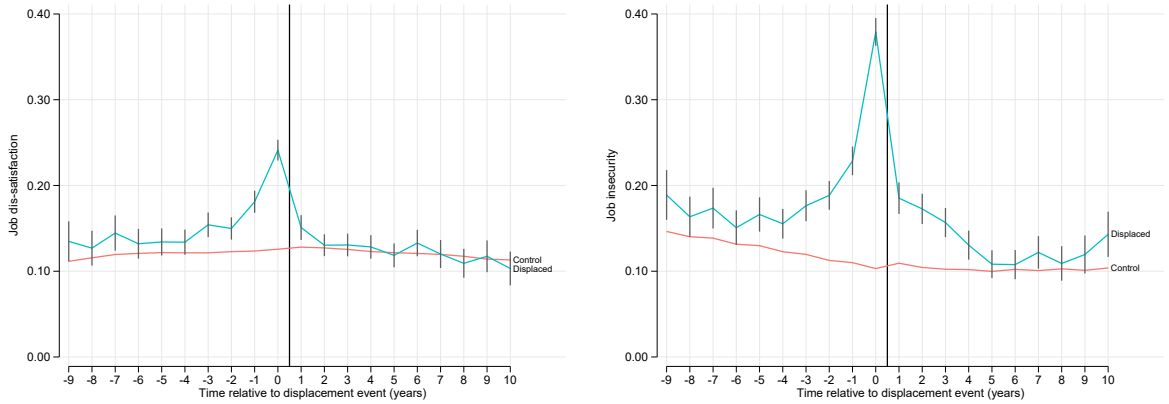
Figure 7: Job loss and subjective wellbeing



Notes: information comes from summing answers to 12 questions of the General Health Questionnaire and scaling between 0 (most distressed) and 1 (least distressed). See Institute for Social and Economic Research (2019) for a list of the 12 questions.

likely to report being dissatisfied with their job. This suggests that 'matching' job losers to similar individuals in the control group may be important.

Figure 8: Job loss events, job dissatisfaction and job insecurity



(a) Job dissatisfaction

(b) Job insecurity

Notes: the measure of job dissatisfaction comes from the same question in both surveys which is asked in every wave: "Which number best describes how satisfied or dissatisfied you are with your present job overall?" where codes of 3 (Somewhat dissatisfied) 2 (Mostly dissatisfied) and 1 (Completely dissatisfied) are used to indicate dissatisfaction. the measure of job security comes from two different questions. In the BHPS (waves 1-18) the question is "how satisfied are you with the job security in your present job" while in the UKHLS (waves 18,20,22,24,26,28) the question is "how likely do you think it is that you will lose your job during the next 12 months?".

A second possibility is that job loss is not unexpected, and the individual's mental health deteriorates in anticipation of the event. We investigate this possibility in panel (b) of Figure 8 which plots information information on job security. It is clear that those who will lose their jobs at $r = 0$ have a lower level of perceived job security for five or more years before the job loss event, and that this feeling of job insecurity increases significantly in the lead up to the event itself. A possible explanation for this is that job loss is the culmination of a cumulative decline in job security (which translates into an increase in job dissatisfaction in the years leading up to job loss). Again this suggests the need to 'match' the job losers to individuals with similar work experiences.

6 Results

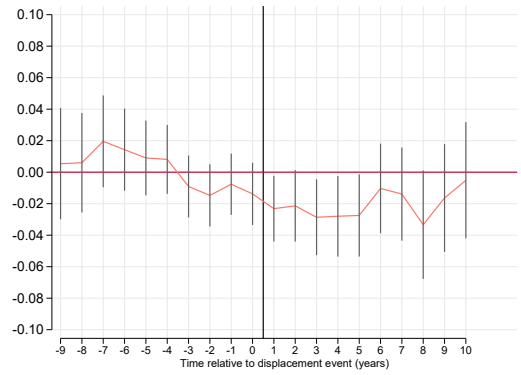
6.1 Inverse Probability Reweighting

For each outcome, we estimate the probability of job loss as a function of observable characteristics measured at $c-3, c-2, \ldots, c$ using a Logit model separately by cohort, thus ensuring that the weights accounts for aggregate labour market conditions. We then reweight the comparison sample using the estimated probability of job loss so that the distribution of characteristics in the control group is balanced to those of the treated. This procedure also removes observations off the common support. The use of characteristics measured at different points in time before job loss ensures that we achieve balance not just on levels but also on possible changes which occur before the job loss. In Appendix E we report the difference in characteristics between treated and controls before and after applying inverse probability reweighting. Table E1 shows that, before reweighting, job losers are significantly less likely to be in employment at $r = -1, \ldots, -3$, are more likely to be men, have lower wages, lower tenure, more likely to work in the private sector, work in smaller firms and are less educated. After reweighting, we find that about 10% of the control observations are off the common support and so are removed, and the remaining differences in observable characteristics are small and insignificantly different from zero.

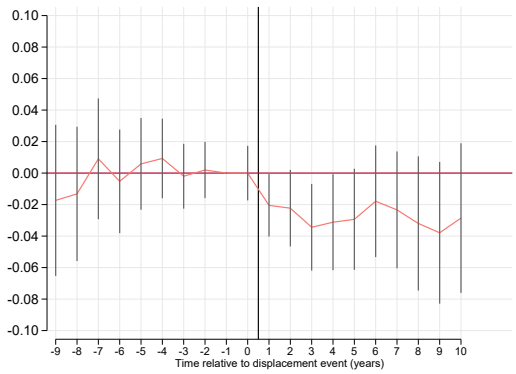
6.2 Political support

Figure 9 graphically summarises our results for the effect of job loss on political support, and also shows how the reweighting procedure makes the treated and comparison groups observably similar in the pre-job loss period. In each row of the figure, the left-hand panel shows the raw differences in the outcome and the right-hand panel shows the estimates from Eqn. (3) after reweighting. For incumbency support, panel (a) shows some evidence of declining support before the job loss event. After reweighting, panel (b) shows not only similar trends but also similar levels of incumbency support before job loss, with a distinct fall in the periods after job loss.

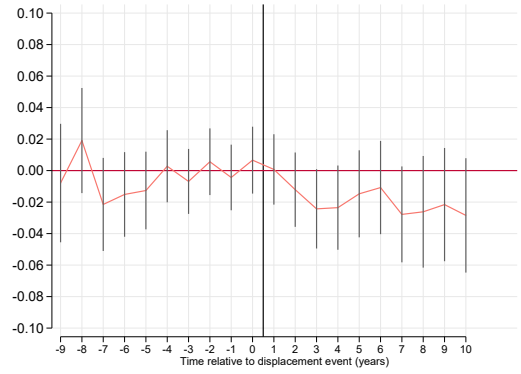
Figure 9 provides evidence that job loss reduces support for the incumbent, for left-wing parties and for any political party, but our attempt to estimate effects for each year is hampered by a lack of statistical precision with standard error bars getting wider as we move further away from the job loss event. This is a feature of our stacked cohort data: cohorts who lose their job



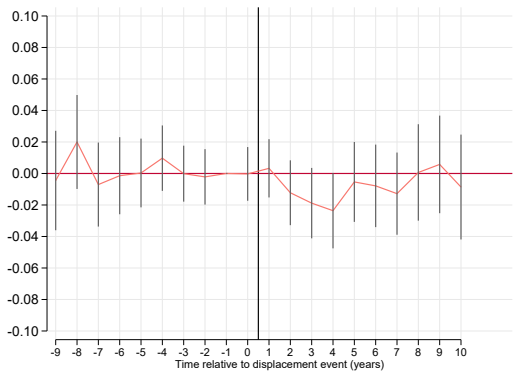
(a) Support for incumbent: raw difference from control



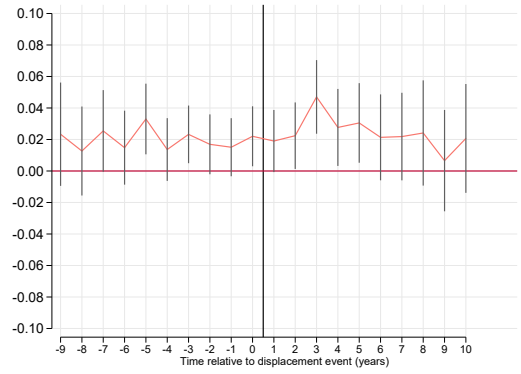
(b) Support for incumbent: FE with IPW



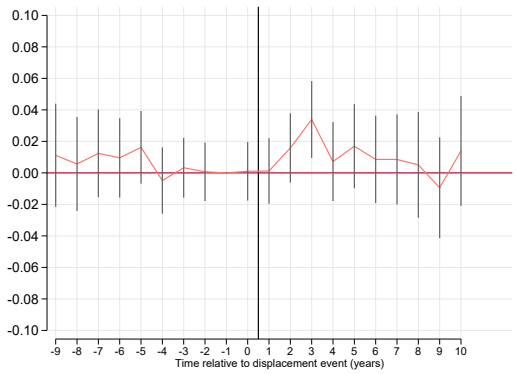
(c) Support for left-wing parties: raw difference from control



(d) Support for left-wing parties: FE with IPW

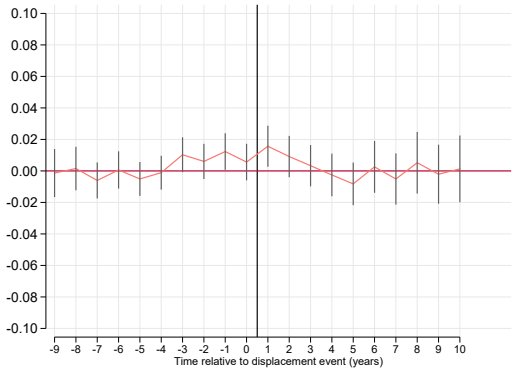


(e) No support for any party: raw difference from control

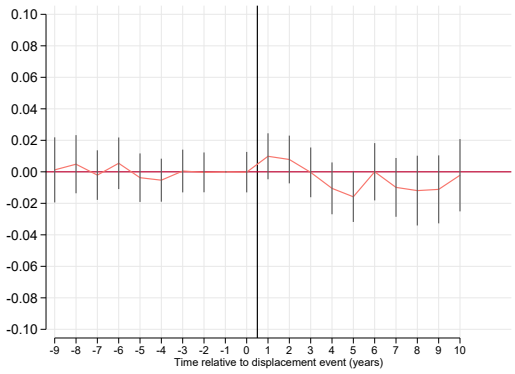


(f) No support for any party: FE with IPW

Figure 9: Effect of job loss on political support



(g) Support for fringe parties: raw difference from control



(h) Support for fringe parties: FE with IPW

Figure 9: Effect of job loss on political support (cont'd)

towards the end of the sample period do not have observations for many years after the event. Thus, estimates for long-run effects rely on earlier cohorts and smaller sample sizes.[12]

Therefore, in Table 1 we group relative time into four periods before job loss and four periods after job loss. To reflect the smaller number of observations, periods get wider as we move further away from the job loss event. For each outcome we report the same two models as in Figure 9, namely the simple difference model, without any control variables apart from time and cohort dummies, and the fixed-effect model weighted by inverse probability weights to ensure that the treated and controls are observably similar in the pre-treatment period. For the fixed-effect models the base group is the period 1-2 years before job loss.[13] Since there is some indication of a pre-treatment differences between the control and treatment, our preferred estimates are the fixed effects with inverse probability weights. As noted previously, this procedure eliminates pre-job loss differences in both mean and trend.

Turning first to the support for the incumbent (column 2), our estimates show post job loss effects that accord with our priors — individuals are less likely to support the incumbent by about 2 percentage points, and this effect continues for up to 10 years after the job loss event. Job losers are less likely to support a party of the left (column 4), but this effect is smaller in size and dissipates entirely after 3 years. We also find evidence consistent with the hypothesis that job loss causes a withdrawal of any political support (column 6), but this is also a relatively short-lived effect. Finally, we find no evidence that job loss increases support for fringe parties in England (column 8). We saw in Figure 2d that there was a substantial increase in support for

[12] This is why including individual fixed effects in Eqn. (3) is important, so that time effects are identified only off within-person comparisons.

[13] Because the samples are balanced, the choice of base group is not critical since, after applying inverse probability weights, the differences in the outcome before job loss are all insignificantly different from zero.

	Support for incumbent		Support for left		No support		Support for fringe	
	(1) Raw Diff	(2) IPW FE	(3) Raw Diff	(4) IPW FE	(5) Raw Diff	(6) IPW FE	(7) Raw Diff	(8) IPW FE
>6 years before	0.012	−0.006	−0.008	0.002	0.019*	0.009	−0.001	0.003
	(0.011)	(0.015)	(0.012)	(0.010)	(0.010)	(0.010)	(0.004)	(0.006)
3-6 years before	0.001	0.003	−0.005	0.004	0.023***	0.004	0.002	−0.002
	(0.008)	(0.008)	(0.009)	(0.007)	(0.008)	(0.007)	(0.004)	(0.005)
1-3 years before	−0.011	—	0.001	—	0.016**	—	0.009*	—
	(0.009)		(0.010)		(0.008)		(0.005)	
0-1 year before	−0.014	−0.001	0.007	0.001	0.022**	0.001	0.006	0.000
	(0.010)	(0.008)	(0.011)	(0.008)	(0.010)	(0.009)	(0.006)	(0.006)
0-1 year after	−0.023**	−0.021**	0.001	0.004	0.019*	0.001	0.016**	0.010
	(0.010)	(0.009)	(0.011)	(0.008)	(0.010)	(0.009)	(0.007)	(0.007)
1-3 years after	−0.025**	−0.029***	−0.018*	−0.014*	0.034***	0.024***	0.006	0.004
	(0.010)	(0.011)	(0.011)	(0.008)	(0.009)	(0.009)	(0.005)	(0.006)
3-6 years after	−0.023**	−0.028**	−0.017	−0.012	0.027***	0.011	−0.003	−0.009
	(0.011)	(0.014)	(0.012)	(0.010)	(0.010)	(0.010)	(0.005)	(0.006)
>6 years after	−0.017	−0.031*	−0.026*	−0.004	0.019	0.004	−0.001	−0.009
	(0.013)	(0.018)	(0.014)	(0.011)	(0.012)	(0.011)	(0.006)	(0.007)
Mean of dep.var.	0.320	0.320	0.391	0.391	0.242	0.242	0.064	0.064
Number of obs.	1,123,383	974,428	1,123,383	978,502	1,123,383	979,444	957,660	818,488
Number of indiv.	19,452	18,806	19,452	18,793	19,452	18,791	17,880	17,131

Table 1: Effect of job loss on political support

Notes: the odd-numbered columns reports the raw difference in means between job losers and non-job losers for each point in time relative to the job loss event. The even-numbered columns report estimates of Eqn. (3) after reweighting using Inverse Probability Weights. Sample excludes Northern Ireland. Measures of political support are constructed as described in Section 3.2; see Figure C1 for a graphical description of the three measures. The outcome "Support for fringe" is only estimated on respondents from England (thus excluding nationalist parties in Wales and Scotland). Standard errors are clustered at the individual level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

fringe parties in England after 2010, but there is no significant association of this increase with job loss.

6.3 Voting behaviour

In this section we examine how job loss affects actual voting patterns. Although we would expect political support and voting to be highly correlated, they may diverge for a number of reasons. Firstly, individuals may vote strategically if their preferred party is unlikely to win; Secondly, support may change rapidly in the run up to an election as views are crystallised in the election campaign. Table 2 presents the results in the same way as Table 1, with relative time effects grouped into bands which reflect the number of observations in each group. As before, for each voting outcome we report both the simple difference in means as well as the results of the IPW fixed effects model.

An important difference in the estimation of the models summarised in Section 4 arises because the question on voting behaviour relates to the last election, which may have taken place some time before the current interview. When considering whether voting behaviour changes after a job loss event, we therefore need to define relative time r_{ict} in terms of election dates rather than interview dates. Thus, $r_{ict} = 0$ if the last election took place in the year immediately before the job loss event. Although elections take place only at intervals of 4-5 years, the varying timing of job loss events means we still have observations which fall within each value of r.

The results for the effect of job loss on voting are strikingly similar to those for political support, except that they are less precisely estimated because information on voting is not available in every year. In column (2) of Table 2 we see that job losers are less likely to vote for the incumbent party with approximately the same effect size as for political support, between 2 and 4 percentage points in the 10 years following job loss. The effect on voting for a left-leaning party in column (4) are consistently negative and slightly larger in size than the corresponding effects on support for left-leaning parties shown in Table 1, albeit with larger standard errors. Column (6) shows that job losers are more likely to report not voting in the last election, once again slightly larger than the effect on political support. Finally, in column (8) we find absolutely no effect on the probability of voting for a fringe (English) political party, just as we found no effect on the effect of expressing support for a fringe party.

	Vote for incumbent		Vote for left		Did not vote		Vote for fringe	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Raw Diff	IPW FE	Raw Diff	IPW FE	Raw Diff	IPW FE	Raw Diff	IPW FE
>6 years before	0.006	−0.006	0.006	0.013	0.026*	−0.005	0.002	−0.000
	(0.015)	(0.025)	(0.016)	(0.016)	(0.014)	(0.016)	(0.003)	(0.005)
3-6 years before	−0.001	−0.012	−0.009	−0.004	0.039***	−0.006	0.004	0.002
	(0.014)	(0.025)	(0.015)	(0.015)	(0.013)	(0.016)	(0.003)	(0.005)
1-3 years before	−0.011	0.000	−0.032*	0.000	0.044***	0.000	0.002	0.000
	(0.017)	(.)	(0.018)	(.)	(0.017)	(.)	(0.004)	(.)
0-1 year before	−0.021	−0.023	−0.027	0.003	0.031	−0.032	0.004	0.005
	(0.022)	(0.034)	(0.023)	(0.021)	(0.021)	(0.022)	(0.006)	(0.009)
0-1 year after	−0.035*	−0.038	−0.042*	−0.015	0.036*	0.006	0.005	0.006
	(0.021)	(0.032)	(0.022)	(0.021)	(0.021)	(0.022)	(0.006)	(0.008)
1-3 years after	−0.050***	−0.040*	−0.037**	−0.026*	0.071***	0.034**	0.003	−0.001
	(0.015)	(0.024)	(0.017)	(0.015)	(0.016)	(0.015)	(0.004)	(0.005)
3-6 years after	−0.040**	−0.039	−0.041**	−0.013	0.057***	0.017	0.002	−0.000
	(0.016)	(0.029)	(0.017)	(0.017)	(0.016)	(0.018)	(0.004)	(0.005)
>6 years after	−0.026	−0.021	−0.057***	−0.020	0.043**	−0.002	0.009	0.002
	(0.019)	(0.030)	(0.019)	(0.019)	(0.018)	(0.019)	(0.006)	(0.007)
Mean of dep.var.	0.296	0.296	0.379	0.379	0.248	0.248	0.017	0.017
Number of obs.	604,209	516,766	604,209	510,214	618,258	528,753	590,582	511,747
Number of indiv.	17,607	17,180	17,607	17,164	17,814	17,347	16,620	15,853

Table 2: Effect of job loss on self-reported voting

Notes: the odd-numbered columns reports the raw difference in means between job losers and non-job losers for each point in time relative to the job loss event. Relative time here refers to the time between the job loss event and the election, rather than the interview. The even-numbered columns report estimates of Eqn. (3) after reweighting using Inverse Probability Weights. Sample excludes Northern Ireland. Measures of voting are constructed as described in Section 3.3; see Figure 3 for a graphical description of the three measures. The outcome "vote for fringe" is only estimated on respondents from England (thus excluding nationalist parties in Wales and Scotland). Standard errors are clustered at the individual level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

6.4 Political views

Our results thus far point to a consistent but small effect of job loss on political support and self-reported voting behaviour. As noted in Section 3.4, political support and party identification may be quite stable over time. For this reason, we also consider how job loss affects underlying political beliefs which, it has been suggested, may be more responsive to economic shocks and self-interest. As described in Section 3.4, we have constructed a number of measures to reflect a number of underlying political attitudes: agreement with "right wing" political ideas; agreement with "populist" political ideas; opposition to the European Union. Each variable is scaled from 1 to 5.

Our results are reported in Table 3, again with a comparison of the raw difference with our preferred FE specification with reweighting. In column (2), we find some evidence that job loss makes the individuals less likely to agree with right-wing views, although the size of the effect is not consistent across all four post-job loss periods. This accords with previous findings in the literature that those made unemployed are more inclined to favour government intervention. It is interesting to note that we have previously found that such individuals are less likely to vote for a left-leaning party, and more likely to withdraw their support altogether. In column (4), we see that the coefficients on "Agreement with populist views" are positive 1-3 years after job loss, and insignificantly different from zero in other periods. In terms of opposition to the EU, we see that those who lose their jobs are much more likely to agree with anti-EU statements, but this is largely a pre-existing difference, as shown in column (5). In the year before job loss, those who will lose their jobs are 17pp more likely to agree with anti-EU statement. Once we reweight to make the treated and controls similar on observable characteristics, this pre-existing difference largely disappears and the post-job loss effect becomes much smaller and is insignificant. Nevertheless, estimates in all four post-job loss periods are positive. Opposition to the EU is higher amongst individuals more likely to lost their jobs, but it is not the act of job loss that causes this attitude.

6.5 Partner effects

It is clear from the above results that the unexpected loss of a job can have a significant impact on the political support and voting patterns of individuals in the years following redundancy. The mechanism for this is however in need of investigation. As shown in panel (b) of Figure 5, own job loss has very large and sudden effects on household income as well as on own income, so this could be the cause. However, the change in behaviour might be due to other egocentric

	Agreement with right-wing views		Agreement with populist views		Opposition to EU	
	(1) Raw Diff	(2) IPW FE	(3) Raw Diff	(4) IPW FE	(5) Raw Diff	(6) IPW FE
>6 years before	0.007	−0.002	−0.051**	−0.024	0.113*	0.033
	(0.019)	(0.017)	(0.024)	(0.030)	(0.068)	(0.077)
3-6 years before	−0.007	−0.004	−0.006	0.023	0.118*	0.029
	(0.019)	(0.016)	(0.022)	(0.026)	(0.065)	(0.065)
1-3 years before	−0.027	0.000	0.004	0.000	0.082	0.000
	(0.020)	(.)	(0.024)	(.)	(0.056)	(.)
0-1 year before	−0.049*	−0.029	0.015	0.028	0.170**	0.035
	(0.027)	(0.021)	(0.035)	(0.037)	(0.083)	(0.079)
0-1 year after	−0.061**	−0.064***	−0.077**	−0.031	0.182***	0.041
	(0.030)	(0.023)	(0.039)	(0.037)	(0.070)	(0.064)
1-3 years after	−0.061***	−0.026	0.022	0.067**	0.225***	0.040
	(0.022)	(0.018)	(0.029)	(0.032)	(0.054)	(0.055)
3-6 years after	−0.014	−0.012	−0.026	0.022	0.145***	0.026
	(0.021)	(0.018)	(0.029)	(0.034)	(0.051)	(0.059)
>6 years after	−0.041	−0.060***	−0.060*	0.049	0.245***	0.091
	(0.027)	(0.022)	(0.036)	(0.043)	(0.063)	(0.076)
Mean of dep.var.	2.664	2.664	2.833	2.833	2.861	2.861
Number of obs.	252,164	224,535	422,510	383,814	215,156	196,923
Number of indiv.	6,828	6,724	19,351	18,950	16,966	16,626

Table 3: Effect of job loss on political views

Notes: the odd-numbered columns reports the raw difference in means between job losers and non-job losers for each point in time relative to the job loss event. The even-numbered columns report estimates of Eqn. (3) after reweighting using Inverse Probability Weights. Sample excludes Northern Ireland. Measures of political views are constructed as described in Section 3.4; see Figure 4 for a graphical description of the three measures. Standard errors are clustered at the individual level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

factors such as loss of social/self esteem. To investigate this further we examine the impact of partner job loss on voting behaviour. If the results in Sections 6.2 and 6.3 are primarily driven by family income loss, we would expect to find similar (although possibly attenuated) effects from partner job loss. If the loss of social/self esteem is the driver then the measured effects should be much less. In this analysis, the sample is restricted to those individuals who have a spouse or partner in employment at $r = 0$. The treatment group are those whose partners experience job loss between $r = 0$ and $r = 1$, while the control are those whose partners do not experience a job loss between $r = 0$ and $r = 1$.

Our results are reported in Table 4, and are striking in that they show absolutely no effect for any of the four political support outcomes.[14] All estimates are small and insignificantly different from zero. This strongly suggests that a loss in household income caused by partner job loss has no effect on political support, in contrast to the loss in income from own job loss. In turn, this suggests that it is loss of social/self esteem caused by one's own job loss that causes a change in political support.

[14] As before, in the even-numbered columns we report results from a fixed-effect model after re-weighting using inverse probability weighting.

	Support for incumbent		Support for left		No support		Support for fringe	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Raw Diff	IPW FE	Raw Diff	IPW FE	Raw Diff	IPW FE	Raw Diff	IPW FE
>6 years before	0.008	−0.002	0.004	−0.002	0.021*	0.009	−0.008*	0.003
	(0.014)	(0.022)	(0.015)	(0.014)	(0.012)	(0.014)	(0.005)	(0.008)
3-6 years before	0.009	0.002	−0.005	−0.008	0.017*	0.002	0.004	0.004
	(0.011)	(0.012)	(0.012)	(0.010)	(0.010)	(0.010)	(0.005)	(0.006)
1-3 years before	−0.002	0.000	0.007	0.000	0.012	0.000	−0.001	0.000
	(0.011)	(.)	(0.012)	(.)	(0.010)	(.)	(0.006)	(.)
0-1 year before	−0.006	−0.001	0.003	−0.001	0.012	0.004	0.002	0.001
	(0.013)	(0.012)	(0.013)	(0.011)	(0.012)	(0.012)	(0.007)	(0.008)
0-1 year after	−0.023*	0.001	0.003	−0.005	0.024*	−0.002	0.004	0.007
	(0.013)	(0.015)	(0.014)	(0.013)	(0.013)	(0.014)	(0.008)	(0.009)
1-3 years after	0.004	0.022	0.001	−0.010	0.015	−0.004	−0.004	−0.000
	(0.013)	(0.017)	(0.013)	(0.012)	(0.011)	(0.013)	(0.006)	(0.008)
3-6 years after	−0.022	0.015	0.005	−0.003	0.005	−0.016	−0.005	−0.003
	(0.014)	(0.022)	(0.015)	(0.014)	(0.012)	(0.014)	(0.006)	(0.008)
>6 years after	−0.014	0.009	−0.000	−0.004	0.008	−0.017	−0.003	−0.004
	(0.016)	(0.026)	(0.017)	(0.017)	(0.014)	(0.016)	(0.008)	(0.010)
Mean of dep.var.	0.331	0.331	0.393	0.393	0.230	0.230	0.060	0.060
Number of obs.	724,336	444,480	724,336	438,837	724,336	439,667	622,006	363,012
Number of indiv.	12,921	10,138	12,921	10,139	12,921	10,152	11,972	9,149

Table 4: Effect of partner job loss on political support

Notes: the odd-numbered columns reports the raw difference in means between those whose partners lost their job and those whose partners did not lose their job for each point in time relative to the partner's job loss event. The even-numbered columns report estimates of Eqn. (3) after reweighting using Inverse Probability Weights. Sample excludes Northern Ireland. Measures of political support are constructed as described in Section 3.2; see Figure C1 for a graphical description of the three measures. The outcome "Support for fringe" is only estimated on respondents from England (thus excluding nationalist parties in Wales and Scotland). Standard errors are clustered at the individual level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

7 Robustness to methodological decisions

In this section we investigate the robustness of our results on political support/voting to a number of methodological and sampling decisions that we have made. Column (1) of Table 5 repeats our preferred model,[15] which gives us a baseline for job loss events. Whilst the mean family income falls substantially following job loss, we showed in Figure 6 that a significant fraction of job-losers do not suffer large income losses because they find a good job within one year of the event. In column (2) we therefore focus on those whose wage loss between $r = 0$ and $r = 1$ was greater than the median wage loss of the entire treatment group (-42%). Our comparison is therefore between job losers who experience large wage losses with non-job losers. Comparing column (2) with column (1) we find, in fact, only small differences in the results, with the exception of panel (d), in which there is now a small, but significant effect on support for fringe parties. Overall, however, there is limited evidence that our results are driven by the monetary cost of the job loss event. This is consistent with our finding in Table 4 that partner job loss events had no effect on political support.

From Figure C1 we know that the strength of pre-existing support varies across the sample. It has been suggested that those with strong pre-existing support for a political party are "attached" to that party and are unlikely to change their support even if they experience an economic shock. We therefore removed from the sample those who answered that they had "very strong" or "fairly strong" support for a political party in the four years leading up to the job loss event. In column (3) panel (a), we see that there is a greater decrease in the support for the incumbent in all time periods. In each case, the fall in incumbent support is about 20% greater for this sub-sample. However, there is no clear-cut increase in effect size in panel (b) or (c). We find some evidence for a larger positive effect on support for fringe parties immediately after the job-loss event, shown in panel (d), but this effect is not sustained over time.

In columns (4) and (5) we examine the implications of changing our underlying econometric specification. In our base specification we "clean" the sample by removing any observations that have additional job loss events at $r = -1, -2, -3$. We also remove any individuals who are not interviewed in these three waves. In column (4) we relax these restrictions, which allows us to use a much larger sample, albeit one in which the history of job loss events is less precisely defined. Reassuringly, the results for all four outcomes remain very similar, but are estimated slightly more precisely due to the larger sample. Finally, we consider the effect of balancing on pre-job loss outcomes. In our base model we compare treated and untreated who are balanced in terms of their political support in the four years leading up to $r = 0$. However, the patterns of

[15] The FE-IPW results in Table 1

	(1) Base model	(2) Large income loss	(3) Weak pre-existing support	(4) Pre-job loss sample restriction	(5) Reweight on earlier period
(a) Support for incumbent					
0-1 year after	-0.021^{**}	-0.028^{**}	-0.027^{**}	-0.025^{***}	-0.011
	(0.009)	(0.013)	(0.012)	(0.008)	(0.014)
1-3 years after	-0.029^{***}	-0.027^{*}	-0.035^{***}	-0.028^{***}	-0.037^{**}
	(0.011)	(0.016)	(0.013)	(0.009)	(0.014)
3-6 years after	-0.028^{**}	-0.031	-0.035^{**}	-0.019^{*}	-0.033^{*}
	(0.014)	(0.021)	(0.016)	(0.011)	(0.018)
Number of obs.	974,428	835,328	658,835	1,506,616	711,011
Number of indiv.	18,806	18,336	15,108	28,788	14,068
(b) Support for left-wing parties					
0-1 year after	0.004	0.007	-0.001	0.001	0.009
	(0.008)	(0.012)	(0.011)	(0.007)	(0.011)
1-3 years after	-0.014^{*}	-0.007	-0.015	-0.011^{*}	-0.012
	(0.008)	(0.012)	(0.011)	(0.006)	(0.010)
3-6 years after	-0.012	-0.012	-0.011	-0.006	-0.013
	(0.010)	(0.014)	(0.012)	(0.007)	(0.012)
Number of obs.	978,502	837,223	666,844	1,507,787	711,307
Number of indiv.	18,793	18,326	15,136	28,770	14,063
(c) No support for any party					
0-1 year after	0.001	-0.012	0.003	0.009	-0.007
	(0.009)	(0.013)	(0.013)	(0.007)	(0.012)
1-3 years after	0.024^{***}	0.021	0.028^{**}	0.020^{***}	0.032^{***}
	(0.009)	(0.013)	(0.012)	(0.007)	(0.011)
3-6 years after	0.011	0.019	0.009	0.006	0.013
	(0.010)	(0.014)	(0.013)	(0.007)	(0.012)
Number of obs.	979,444	832,849	666,041	1,510,422	708,688
Number of indiv.	18,791	18,268	15,124	28,768	14,026
(d) Support for fringe parties					
0-1 year after	0.010	0.016^{*}	0.018^{**}	0.007	0.018^{**}
	(0.007)	(0.009)	(0.009)	(0.005)	(0.008)
1-3 years after	0.004	0.003	0.009	0.007	-0.002
	(0.006)	(0.009)	(0.008)	(0.005)	(0.006)
3-6 years after	-0.009	-0.012	-0.002	-0.005	-0.002
	(0.006)	(0.009)	(0.008)	(0.005)	(0.007)
Number of obs.	818,488	707,265	532,499	1,275,620	605,693
Number of indiv.	17,131	16,662	13,732	25,968	13,078

Table 5: Robustness of results to methodological decisions

Notes: column (1) repeats the results from the even-numbered columns in Table 1. In column (2) we restrict the treatment group to those whose loss is greater than the median wage loss (-42%) In column (3) we restrict the entire sample to those who, in the four years leading up to the job loss event, had little or weak support for any political party.

subjective well-being, job dissatisfaction and job insecurity shown in Figures 7 and 8 suggested that the job loss event was preceded by a decline in job security and wellbeing. If this causes changes in political support before $r = 0$, matching on these years immediately before $r = 0$ will diminish the actual effect. Therefore in column (5) we report the result of reweighting to balance observable characteristics, including political support, in the period 3–6 years before the job loss event, thus allowing pre-job loss differences in outcomes. The effects of job loss on the four outcomes are still quite consistent, with a negative effect on support for the incumbent, a smaller and insignificant effect on support for left-wing parties, some increase in no support for any party, and a temporary increase in support for fringe parties.

8 Conclusions

In this paper we examine the importance of individual economic factors on political outcomes. Using detailed information on the precise timing of a sudden and consequential economic shock — job loss — we can trace out effects over a long period of time and use a rich set of pre-job loss characteristics to match job losers with an observably similar control group. We measure effects on three important aspects of political support and behaviour: party support, voting and political views. We use staggered difference-in-differences models with flexible relative time effects.

Our results confirm the existence of the economic voter, but the effects are quantitatively small and in most cases, quite short-lived. We find short-term effects of a 1–2 percentage point reduction in support for the incumbent party, support for left-wing parties and support for any political party. But after 6 years only the reduction in support for the incumbent remains. We find little effect on support for fringe or populist parties in the short-or long-term. Voting behaviour follows a similar pattern in the same direction, with significant effects up to 3 years after the job loss and little long-term effect. Perhaps more surprisingly, we find little evidence that job loss shifts political views on populism or opposition to the EU in a consistent direction. In particular, although opposition to the EU is strongly correlated with job loss risk, job loss has no significant effect on opposition after the event.

We also provide two new results which suggest that it is not the earnings loss *per se* which shifts political support and voting behaviour, but the event itself. First, effects for those who have greater earnings losses are not significantly larger than those with smaller earnings losses. Second, the effects do not spillover to other members of the household, despite large household-level earnings losses. These findings are consistent with the argument that political attitudes are

shifted by well-being as well as economic factors (Liberini et al. 2017). In our setting, job loss causes a temporary fall in self-reported well-being, which is consistent with temporary effects on political attitudes.

In contrast to our findings, a series of papers find that job loss events can cause quite large shifts in support towards welfare provision and redistribution (see Margalit 2019, and references therein). This may be because the political opinions reported in our data are not directly related to social policy and redistribution, but rather relate to more abstract concepts on the left-right spectrum, populism and opposition to the EU. Our results suggest instead that political responses in the UK are quite resilient to personal economic shocks. This could be because of the stable political environment, dominated by two parties with relatively similar legislative agendas over this time period. An alternative explanation is that voters hold governments accountable for economic shocks only when they think that those shocks are the result of government policy as suggested by Tilley et al. (2018), or that voters consider more general societal effects which are outside the scope of this study.

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Appendix A Sample information

We use the first eleven waves of the UKHLS. We refer to them as waves 19–29 to distinguish them from waves 1–18 in the BHPS. The final wave of BHPS interviews took place between September 2008 and April 2009, although the great majority (97%) were completed in 2008. The first wave of UKHLS interviews took place from January 2009 to March 2011. However, BHPS sample members were not interviewed again until wave 20, interviews for which took place between January 2010 and April 2011. We keep only full interview outcomes (i.e. proxy responses are not included) and we drop the boost samples for Scotland, Wales, Northern Ireland and the European Community Household Panel. We also drop the ethnic minority boost sample (IEMB) from waves 24 and 25 because these respondents have no within-person variation in voting intentions. The size and structure of the sample is shown in Table A1, together with the dates on which interviews occurred for each wave.

	First interview date	Last interview date	BHPS only	UKHLS only	BHPS and UKHLS
1	01 09 91	31 12 91	6,132	0	3,777
2	02 09 92	24 04 93	5,600	0	3,858
3	02 09 93	28 04 94	5,059	0	3,964
4	01 09 94	09 05 95	4,931	0	4,128
5	04 09 95	20 05 96	4,582	0	4,244
6	29 08 96	17 04 97	4,675	0	4,461
7	30 08 97	08 05 98	4,558	0	4,559
8	31 08 98	30 04 99	4,270	0	4,669
9	01 09 99	31 05 00	4,037	0	4,782
10	01 09 00	31 05 01	3,804	0	4,897
11	01 09 01	30 04 02	3,557	0	5,033
12	01 09 02	21 05 03	3,271	0	5,111
13	01 09 03	10 05 04	3,036	0	5,227
14	01 09 04	11 05 05	2,748	0	5,331
15	01 09 05	04 04 06	2,489	0	5,495
16	01 09 06	03 04 07	2,285	0	5,661
17	01 09 07	13 03 08	1,931	0	5,824
18	01 09 08	02 04 09	1,606	0	5,894
19	08 01 09	10 03 11	0	39,044	0
20	12 01 10	27 03 12	0	32,764	5,950
21	13 01 11	12 05 13	0	29,604	5,468
22	20 01 12	19 06 14	0	28,198	4,968
23	09 01 13	02 06 15	0	26,985	4,708
24	08 01 14	11 05 16	0	24,605	4,366
25	15 01 15	16 05 17	0	23,810	4,125
26	05 01 16	03 05 18	0	22,879	3,986
27	05 01 17	21 05 19	0	21,550	3,785
28	09 01 18	15 05 20	0	20,856	3,589
29	04 01 19	13 05 21	0	19,724	3,400

Table A1: BHPS and UKHLS sample sizes

Notes: sample comprises individuals with full interview outcomes who come from the original Great Britain sample. We also exclude the small number who are from the original GB sample but who live in Northern Ireland.

Appendix B job loss information

To measure job loss, we consider a sample who are in employment in wave t and who are interviewed in wave $t+1$. For this sample, information is available in both the BHPS and the UKHLS about whether the job held in wave t has ended, and, if so, the reason why it ended. Table B1 describes the sample that we use. The way in which job loss information is recorded differs between the two surveys. In this Appendix we describe in more detail how job loss codes are created.[16]

	BHPS Waves 1–18	UKHLS Waves 1-11
Full sample (person-years)	238,992	476,187
Full interview outcome	227,367	430,219
Original sample members	155,486	334,364
Interviewed in following wave	144,095	275,869
In employment at interview	74,487	129,544
Valid job loss information	69,533	129,106

Table B1: Sample selection

Notes: the UKHLS sample excludes an Ethnic Minority Boost sample which does not contain information on voting.

B.1 BHPS

In the BHPS, Information on the reason for the end of employment spells is available from a respondent's employment history data. If the spell in progress at the time of the interview starts after 1st September in the previous interview year then the employment history data contains recall information on all spells going back until a spell start date occurs before 1st September in the previous year. Respondents are asked "which of the statements on the card best describes why you stopped doing that job?", shown in Table B2.

The broadest definition of job loss includes those spells which are reported to end in (3) "made redundant", (4) "dismissed or sacked" or (5) "temporary job ended". However, many of the jobs to which this job loss information refers were not in progress at the time of the last interview, because they were short-term employment spells which started after the previous interview. We therefore restrict the sample to those spells which were in progress at the time of the last interview. Unsurprisingly, the majority of temporary jobs which ended were not in progress at the time of the last interview, which makes sense since these will tend to be shorter spells. There are some discrepancies between the information in the employment history data and the contemporaneous data. The earliest spell in the employment history data should be the spell which was in progress during the last interview. We keep only records from the

[16] Stata code which constructs the data as described is available from the authors on request.

1.	Promoted
2.	Left for a better job
3.	Made redundant
4.	Dismissed/sacked
5.	Temporary job ended
6.	Took retirement
7.	Health reasons
8.	Left to have a baby
9.	Look after family
10.	Look after another person
11.	Other reason

Table B2: Reasons for employment spell ending (BHPS)

employment history data which are consistent in this sense. Finally, we attach the information on job loss to the previous interview, so that for each spell in progress at the time of interview we have information on how that spell ended (if it ended before the next interview). We also take information on the date when that spell ended from the employment history files.

B.2 UKHLS

The question route in UKHLS means that individuals are only asked for the reason why jobs end if they were interviewed previously in the UKHLS.[17] Individuals who report that they are working for the same employer as at the last interview are coded as a "continuing spell". This includes those who are in a new job, but who have remained with the same employer. Individuals who are not working for the same employer are asked why the employment spell in progress at the last interview ended, and the date on which that employment spell ended. The reasons why a job ended available in the UKHLS are almost identical to those available in the BHPS listed in Table B2, with the addition of one more reason for those whose job ended because they "moved area".

B.3 Linking the BHPS and the UKHLS

For those interviewed in wave 18 of the BHPS, the job loss reason and spell end date are missing because there is no subsequent interview in the BHPS. But we can fill in this information by using responses to questions in wave 20 of the UKHLS. This is because BHPS sample members who are interviewed in wave 18 are regarded as having had a full interview in the previous wave. Of the 10,879 interviews in wave 20 which are in the BHPS sample, 10,224 have a previous wave interview outcome in wave 18, which means that they are asked questions about their employment history in the period from wave 18 to wave 20.

There are 6,681 employment spells in progress at the wave 18 interview. Of these, 5,115 are

[17] An important exception is for those who were previously in the BHPS. See Section B.3.

also interviewed in wave 20 and therefore can potentially be linked. Of these 5,115 interviews, 4451 are continuing employment spells, 597 are new employment spells and 67 have missing information.

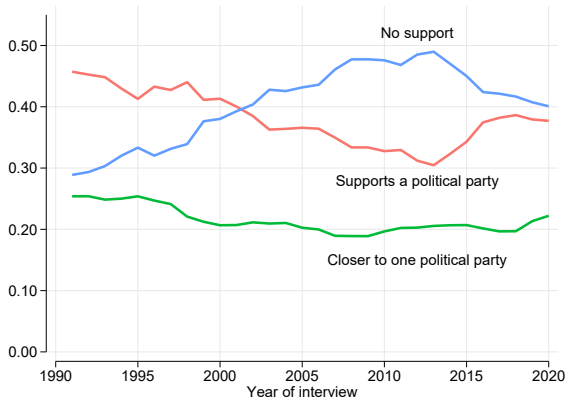
For individuals in the same job in wave 20 as in wave 18, we code the spell at wave 18 as a "continuing job". For individuals with a new employer or who are no longer in employment, the UKHLS tells us why the spell in progress during the last wave of the BHPS ended, and the date on which it ended.

Table B3 summarises the resulting job loss information, which is shown graphically in Figure 1. Note that in 2008 (which is usually the last interview date from the BHPS) the proportion of continuing jobs is significantly lower than either 2007 or 2009. This is because the median length of time between the last BHPS interview and the first UKHLS interview is nearly two years, and so jobs are less likely to continue. For the same reason, the job loss rate is significantly higher in 2008 than in other years.

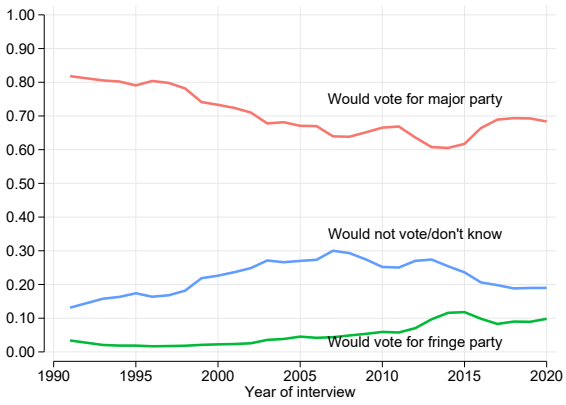
	(1) In sample at wave t and wave $t+1$	(2) In employment	(3) Job loss	(4) Job ended for other reasons	(5) Job continued
1991	8,424	4,034	0.059	0.115	0.825
1992	8,039	3,802	0.063	0.121	0.816
1993	7,940	3,732	0.051	0.141	0.808
1994	8,033	3,813	0.052	0.149	0.799
1995	8,045	3,929	0.049	0.144	0.807
1996	8,502	4,195	0.042	0.158	0.800
1997	8,190	4,161	0.047	0.162	0.790
1998	8,104	4,115	0.045	0.170	0.784
1999	7,375	3,766	0.043	0.168	0.789
2000	8,465	4,398	0.046	0.175	0.779
2001	7,712	4,002	0.051	0.164	0.785
2002	7,504	3,881	0.041	0.166	0.793
2003	7,475	3,851	0.034	0.168	0.798
2004	7,189	3,682	0.041	0.162	0.797
2005	7,422	3,857	0.038	0.144	0.818
2006	7,258	3,747	0.038	0.165	0.798
2007	7,023	3,588	0.039	0.139	0.822
2008	5,774	2,941	0.071	0.132	0.797
2009	15,477	7,331	0.048	0.108	0.844
2010	33,153	15,560	0.042	0.106	0.852
2011	32,896	15,438	0.042	0.107	0.851
2012	30,638	14,345	0.039	0.108	0.853
2013	28,718	13,486	0.034	0.116	0.851
2014	27,542	12,973	0.033	0.131	0.836
2015	25,774	12,072	0.033	0.116	0.851
2016	25,235	12,018	0.029	0.123	0.848
2017	23,540	11,007	0.025	0.116	0.859
2018	22,462	10,339	0.029	0.115	0.856
2019	9,512	4,325	0.034	0.105	0.861
2020	509	251	0.080	0.072	0.849
All years	413,930	198,639	0.040	0.128	0.833

Table B3: job loss data. Column (2) is the total sample of individuals who are at risk of job loss and for whom we can measure job loss. Columns (3)-(5) report fractions of that sample.

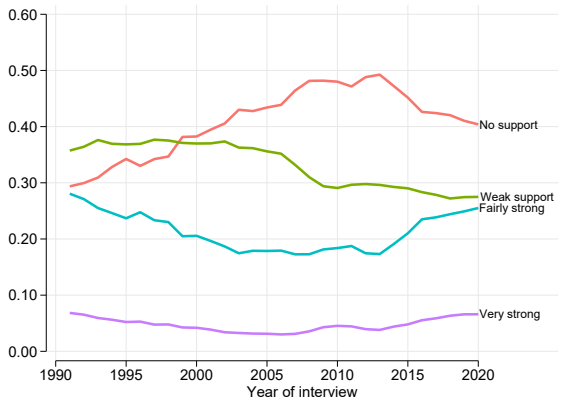
Appendix C Time-series patterns of political support



(a) Proportion of sample supporting, or closer to, a political party



(b) Party support or voting intention



(c) Strength of support for political party

Figure C1: Political support 1991-2019. Smoothed using a 3-period moving-average. The major parties are Conservative, Labour and Liberal Democrat. Fringe parties are all other parties except Plaid Cymru and SNP.

Appendix D Information on voting behaviour

Table D1 shows the availability of information on voting behaviour. In some cases the same individual is asked in repeated waves about their voting behaviour in the same election. In the UKHLS questions on voting behaviour are only asked of a subset of the sample, usually those interviewed in the first year of the survey in that wave.

Survey	Wave	Interview dates	General election
BHPS	2	September 1992–April 1993	April 1992
BHPS	5	September 1995–May 1996	April 1992
BHPS	7	August 1997–May 1998	May 1997
BHPS	8	August 1998–April 1999	May 1997
BHPS	9	September 1999–May 2000	May 1997
BHPS	10	September 2000–May 2001	May 1997
BHPS	11	September 2001–May 2002	June 2001
BHPS	12	September 2002–May 2003	June 2001
BHPS	13	September 2003–May 2004	June 2001
BHPS	14	September 2004–April 2005	June 2001
BHPS	15	September 2005–March 2006	May 2005
BHPS	16	September 2006–April 2007	May 2005
BHPS	17	September 2007–April 2008	May 2005
BHPS	18	September 2008–April 2009	May 2005
UKHLS	20	May 2010–May 2011	May 2010
UKHLS	25	May 2015–March 2016	May 2015
UKHLS	26	June 2017–May 2018	June 2017
UKHLS	27	June 2017–November 2018	June 2017
UKHLS	28	January 2018–October 2018	June 2017
UKHLS	29	December 2019–December 2020	December 2019

Table D1: Questions on voting in General Elections the BHPS and UKHLS

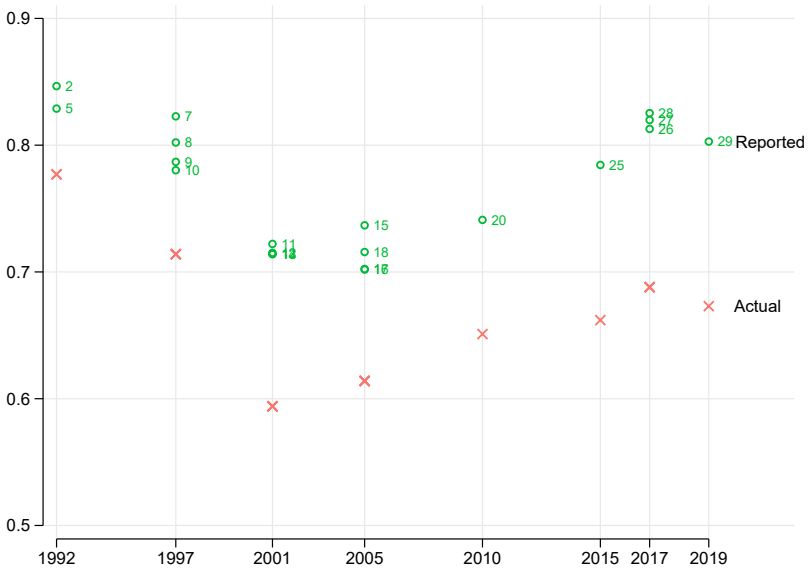


Figure D1: Proportion of eligible sample who reported voting in most recent general election. Weighted by cross-section weights. The eligible sample excludes those who report that they are not able to vote.

(a) Conservative

(b) Labour

(c) Liberal

(d) Other

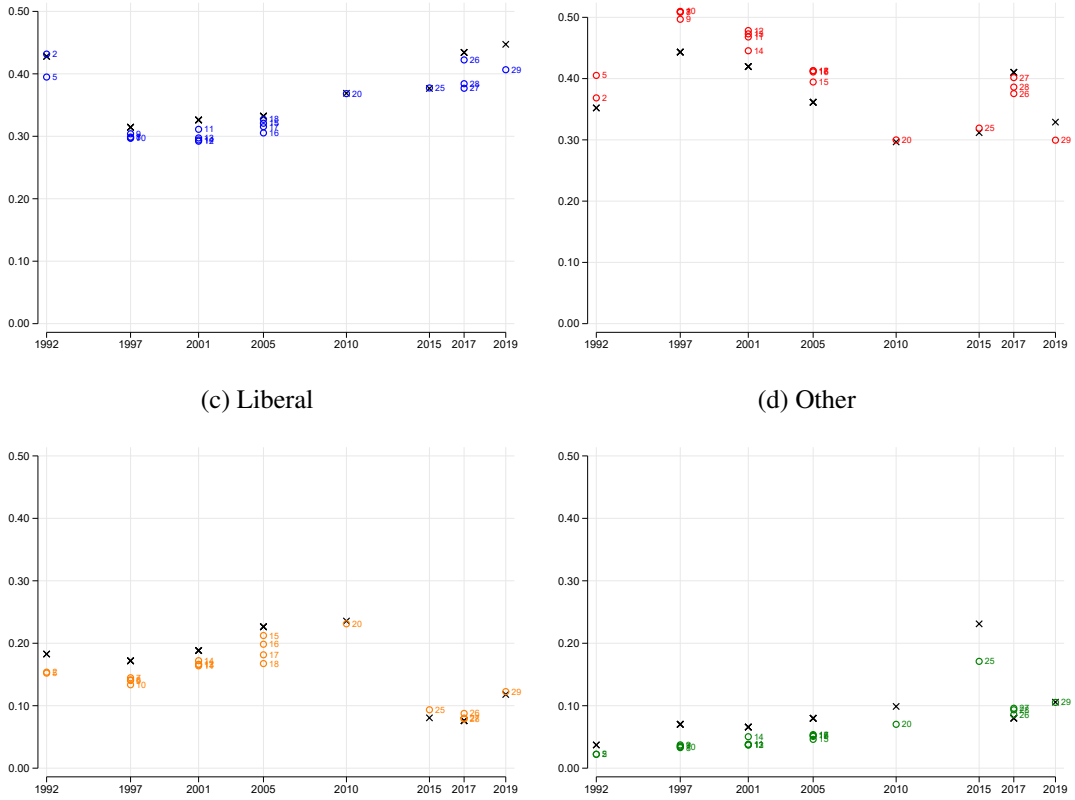


Figure D2: Comparison of voting recall with actual vote shares. Weighted by cross-section weights.

Appendix E Individual characteristics and reweighting

	Unbalanced			Balanced		
	Job loss	Non-job loss	Diff.	Job loss	Non-job loss	Diff.
=1 supports incumbent at $r=0$	0.302	0.314	-0.012	0.303	0.302	0.001
=1 supports incumbent at $r=-1$	0.310	0.317	-0.006	0.310	0.309	0.001
=1 supports incumbent at $r=-2$	0.308	0.323	-0.015	0.308	0.306	0.001
=1 supports incumbent at $r=-3$	0.322	0.326	-0.004	0.323	0.323	-0.000
=1 in employment at $r=0$	1.000	1.000	0.000	1.000	1.000	0.000
=1 in employment at $r=-1$	0.928	0.952	-0.024^{***}	0.928	0.929	-0.001
=1 in employment at $r=-2$	0.898	0.927	-0.028^{***}	0.899	0.899	-0.001
=1 in employment at $r=-3$	0.865	0.898	-0.033^{***}	0.865	0.864	0.000
age at date of interview	41.649	41.679	-0.030	41.599	41.632	-0.034
=1 female	0.464	0.556	-0.092^{***}	0.466	0.466	-0.000
Real monthly wage, last payment	2131.359	2185.201	-53.843^{*}	2125.464	2124.250	1.214
Tenure (years)	5.451	5.837	-0.386^{***}	5.450	5.450	-0.000
=1 works in public sector	0.152	0.374	-0.222^{***}	0.153	0.153	-0.000
Firm size<25	0.378	0.302	0.076^{***}	0.381	0.381	0.000
Firm size 25-99	0.242	0.263	-0.021^{**}	0.242	0.242	-0.000
Firm size 100-999	0.295	0.300	-0.004	0.292	0.292	-0.000
Firm size >999	0.085	0.136	-0.051^{***}	0.085	0.085	-0.000
age left full-time education	18.469	19.028	-0.560^{***}	18.505	18.513	-0.008
	2,481	97,372		2,349	85,320	

Table E1: Mean comparison test of treated and controls for the outcome "support incumbent". Balancing is achieved by reweighting on the estimated probability of treatment as a saturated non-parametric function of the set of lagged dependent variable and a set of observable covariates excluding observations off the common support. The reweighting procedure also includes one-digit industry, one-digit occupation and region dummies. A similar reweighting procedure is used for each outcome variable. Results are available on request.

	Unbalanced			Balanced		
	Job loss	Non-job loss	Diff.	Job loss	Non-job loss	Diff.
Voted for incumbent at last election	0.281	0.297	−0.015	0.280	0.280	0.001
	0.283	0.293	−0.010	0.278	0.278	−0.000
	0.291	0.293	−0.002	0.285	0.286	−0.001
	0.290	0.288	0.001	0.283	0.282	0.000
=1 in employment at $r = 0$	1.000	1.000	0.000	1.000	1.000	0.000
=1 in employment at $r = -1$	0.928	0.952	−0.024***	0.929	0.929	−0.001
=1 in employment at $r = -2$	0.898	0.927	−0.028***	0.900	0.901	−0.001
=1 in employment at $r = -3$	0.865	0.898	−0.033***	0.866	0.865	0.001
age at date of interview	41.649	41.679	−0.030	41.650	41.647	0.003
=1 female	0.464	0.556	−0.092***	0.465	0.465	−0.000
Real monthly wage, last payment	2131.359	2185.201	−53.843*	2128.439	2127.216	1.223
Tenure (years)	5.451	5.837	−0.386***	5.454	5.459	−0.005
=1 works in public sector	0.152	0.374	−0.222***	0.153	0.153	0.000
Firm size<25	0.378	0.302	0.076***	0.382	0.381	0.000
Firm size 25-99	0.242	0.263	−0.021**	0.241	0.241	0.000
Firm size 100-999	0.295	0.300	−0.004	0.292	0.292	0.001
Firm size >999	0.085	0.136	−0.051***	0.085	0.085	−0.001
age left full-time education	18.469	19.028	−0.560***	18.508	18.514	−0.005
	2,481	97,372		2,359	87,178	

Table E2: Mean comparison test of treated and controls for the outcome "voted for incumbent at last election". Balancing is achieved by reweighting on the estimated probability of treatment as a saturated non-parametric function of the set of lagged dependent variable and a set of observable covariates excluding observations off the common support. The reweighting procedure also includes one-digit industry, one-digit occupation and region dummies.

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