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Income shocks, political support and voting behaviour☆

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ABSTRACT

We provide new evidence on the effects of economic shocks on political support, voting behaviour and political opinions over the last 25 years in the UK. 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 individuals 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 and long-lasting effects on support and votes for the incumbent party, and shorter-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.

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 and Stegmaier, 2013; Guntermann et al., 2021). 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 and Stegmaier (2000, 2013) concluding that the evidence for 'pocketbook' voting was slight.

However, many of these earlier 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, many studies using individual level information on voting or preferences used 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.[1] 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 caused by job loss on individual political behaviour over a long time period.

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[1] Sigelman et al. (1991) sidestep this problem using an experimental approach and conclude that vote choices do indeed depend on economic factors, including pocketbook considerations.

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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 large sample of workers for up to 10 years after they lose their jobs and then measure changes in three distinct aspects of individuals' political behaviour: their support for political parties; their voting behaviour in national elections; and their opinions about various political issues, including opposition to the European Union.

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.

Because the timing of job loss differs across individuals, and because the impact of job loss may vary relative to the event, our methodology relies on recent innovations in 'staggered adoption' difference-in-differences analysis (see the many recent references in Roth et al., 2023). Also, since job loss is not random across individuals, we compare our treatment group to a suitably matched control group of workers who did not experience job loss. The availability of detailed information on individuals 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. A key contribution of the paper is therefore controlling for selection bias stemming from pre-existing differences between job losers and the comparison group. 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 paper is structured as follows. In Section 2 we discuss the literature on economic 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, while Section 6 reports our main results on the impact of job loss on political outcomes and compares our findings with the existing literature. 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 and 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 but continued to use aggregated data for the regressors e.g. district and industry level trade exposure (Dippel et al., 2015), industry level exposure to digitalisation (Gallego et al., 2022), occupational unemployment risk (Abou-Chadi and Kurer, 2021), regional import competition (Colantone and 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 and 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 and 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 show 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 and Oswald, 2016). Such wins are by their nature unanticipated and also 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. Kurer (2020) considers workers in routine occupations (i.e. those threatened by automation). Using individual panel data for three European countries, including the UK, he finds that those who "survive" in routine occupations shift their political support towards right-wing populist parties while those who lose their jobs shift towards traditional left-wing parties or abstain from political support.

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 and Pedulla (2013), Martén (2019) and Wiertz and 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 emphasised the critical importance of avoiding 'bad comparisons' when estimating a staggered difference-in-difference treatment model (Goodman-Bacon, 2021, Callaway and Sant'Anna, 2021, Sun and Abraham, 2021). Therefore, 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 time,[2] and we 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 and Guilló (2023) use the BHPS and UKHLS to explore economic determinants of party support and voting behaviour. However, 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, adopting a method similar to that used in this paper to examine the impact of a negative shock, but in their case they examine the impact of spousal death. They find that this reduces incumbent support, even if government policies cannot reasonably be blamed for the death.

3. Data

The British Household Panel Survey (BHPS) is an annual panel of approximately 5000 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 2021 . 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.[3] 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. Fig. 1 plots the proportion of employment spells in each year which end in redundancy, dismissal or the end of a temporary job.

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. It 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.

Our definition of job loss includes both redundancies and dismissals. We do not include the end of temporary jobs because this is an anticipated event. Redundancies make up 88% of all remaining job loss events in our sample.

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 (UKHLS wave 8).

`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.[4] 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

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

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

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

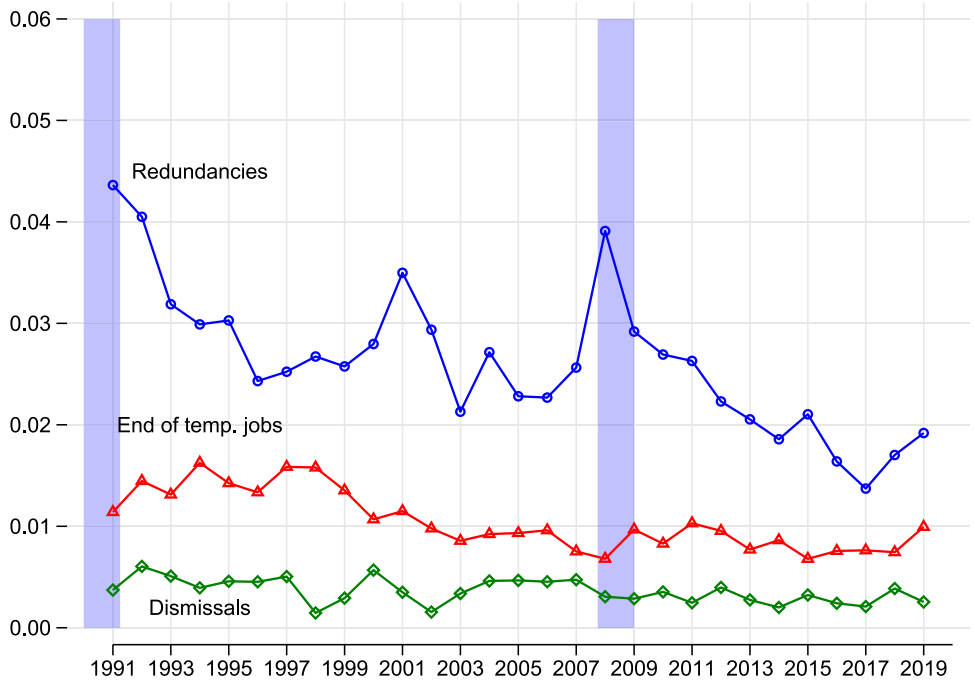


Fig. 1. Proportion of jobs held in year t which end in job loss at the time of the following interview, which is typically 12 months later. Shaded areas indicate recessions. Information from BHPS from 1991–2008 and from UKHLS from 2009–2020. From the final BHPS wave (2008), individuals' subsequent interview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

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 Fig. C.1 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.

3.3. 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.[5] Fig. D.1 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.[6]

Using the sample who say that they voted in the last election, we calculate the predicted vote share. Fig. D.2 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) finds that "voters' preferences regarding welfare policy are strongly affected by their own economic circumstances".[7] Therefore, we will also consider the effects

[5] 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".

[6] This will only bias our estimates of the effect of job loss if recall bias or over-representation of active voters varies between the treatment and control groups, something which is unlikely given that we reweight the two groups to be observably similar in pre-treatment periods.

[7] 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 and Neundorf, 2020) and which themselves determine partisanship in response to political parties' policy goals.

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 the following 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.[8]

`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)

Secondly, 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.[9]

`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 and try to reduce the EU's power (3) leave things as they are (4) stay in and try to increase the EU's power (5) work for the formation of a single European 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)
`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 we will be tracking the answer to the same set of questions between the treated and control groups.

3.5. Some descriptive statistics

In Figs. 2–4 we plot the evolution of the dependent variables we will use in the analysis. Fig. 2(a) demonstrates the typical pattern of a secular decline in incumbent[10] 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. Fig. 3(a) shows that responses to questions about voting in the previous election track quite closely the pattern of political support. Figs. 2(b) and 3(b) 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 at 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 Figs. 2(c) and 3(c). 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, Figs. 2(d) and 3(d) chart the dramatic rise of the UK Independence party (UKIP) leading up to the Brexit referendum in 2016, and then its subsequent decline.

Fig. 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 Fig. 4(a) 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. In Fig. 4(b), 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.

4. Methodology

To examine the effect of job loss on political support and voting behaviour we will use an event-study methodology which compares outcomes before and after a job loss event for a treated and control group. Event-studies (also referred to as "staggered adoption" models

[8] Evidence that our measure of right wing political support captures a coherent underlying concept is provided in Fig. E.1 in Appendix E, which shows an extremely strong, monotonic relationship between the average response and a respondent's stated strength of support for the two main UK political parties.

[9] 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.

[10] Throughout we define incumbency as the national ruling party rather than the local MP.

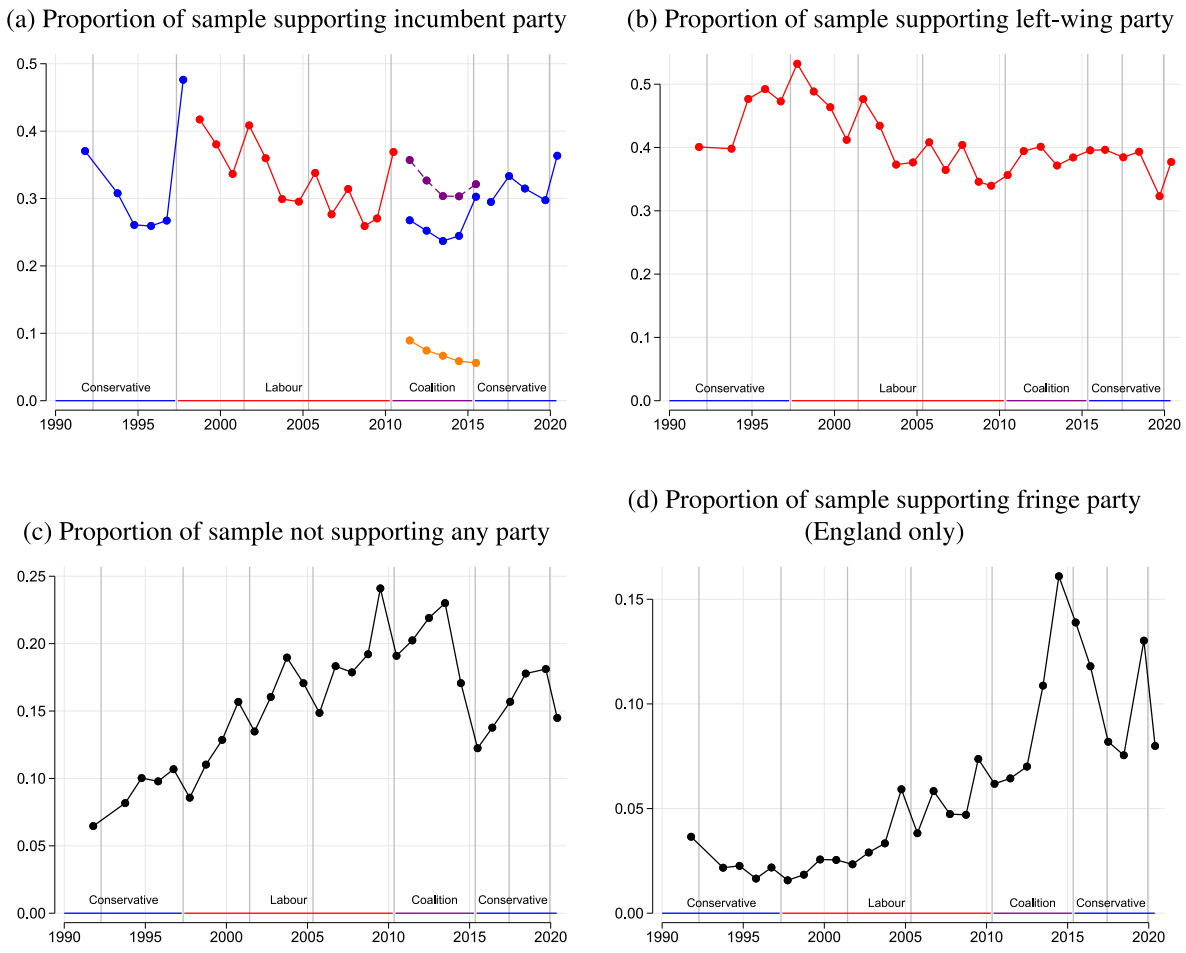


Fig. 2. Political support in the BHPS and UKHLS 1991–2017. Each point represents the average for all interviews in that calendar year. In panel (a), support is split between Conservative and Liberal Democrats during the Coalition Government (2010–2015), with total incumbent support represented by the 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. The vertical lines indicate the date of UK general elections. The colored horizontal lines indicate the governing parties.

by Athey and 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 adequately account for varying treatment time can lead to the "staggered treatment timing problem", when failing to appropriately select the relevant control group for each treatment can result in "bad comparisons" when estimating the model (Goodman-Bacon, 2021, Callaway and Sant'Anna, 2021, Sun and 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.[11] 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.[12] 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, 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 this separately for each job-loss cohort e.g. for the 2000 cohort ($D_i^{2000} = 1$), the estimating equation would be:

$$y_{it} = \alpha + \beta D_i^{2000} + \sum_{s=1991}^{2019} \gamma^s T_t^s + \sum_{s=1991}^{2019} \delta^s (T_t^s D_i^{2000}) + \epsilon_{it} \quad (1)$$

The indicator T_t^s takes the value 1 if $s = t$ and zero otherwise.[13] The coefficients γ^s capture the time-series behaviour of y_{it} for the

[11] 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).

[12] 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.

[13] For the retrospective voting questions s refers to the year in which the election took place, not the year of the survey.

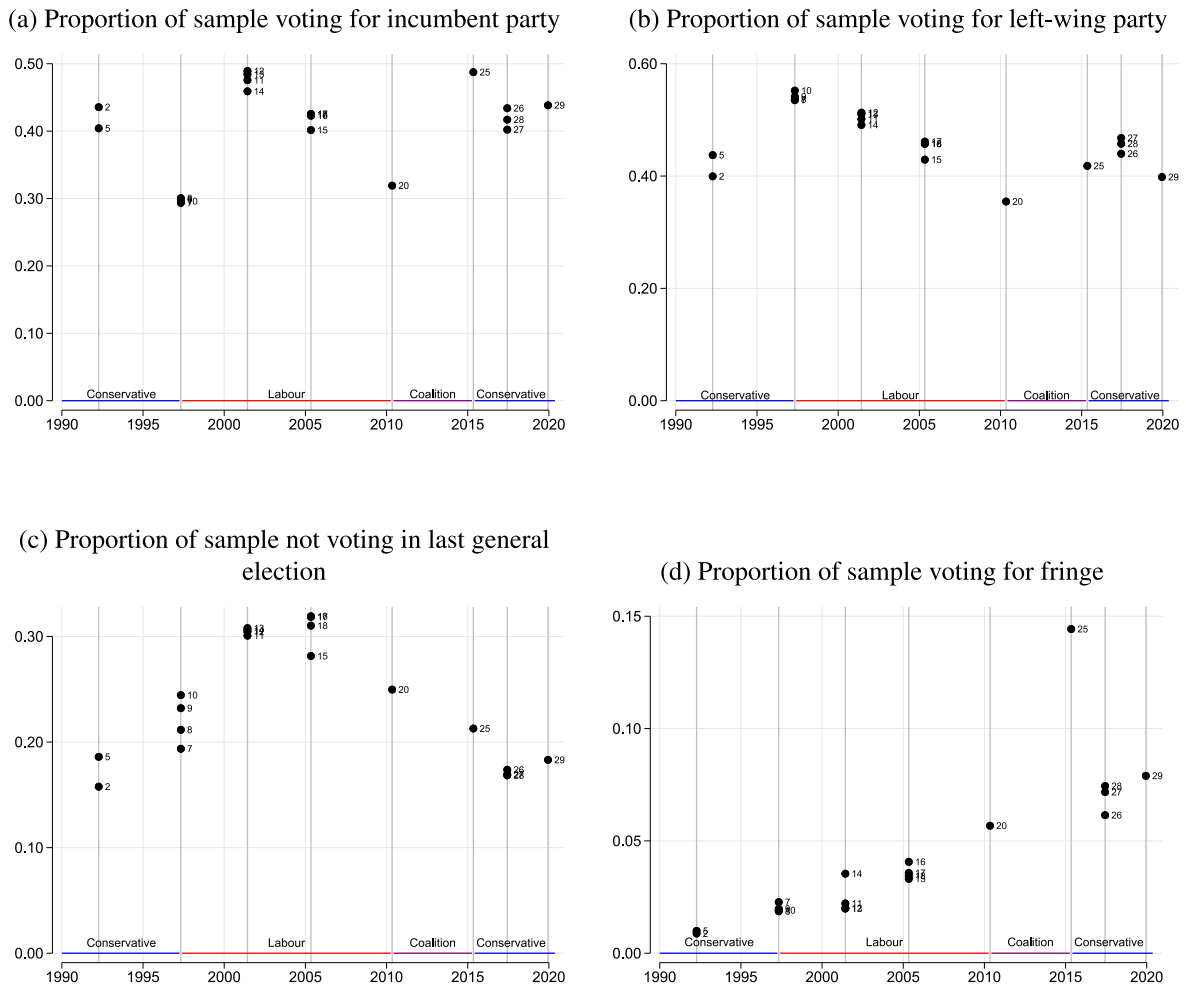


Fig. 3. Voting behaviour in the BHPS and UKHLS 1991–2017. Each point represents the average for all interviews in that calendar year. The number next to each point indicates the wave of the data.

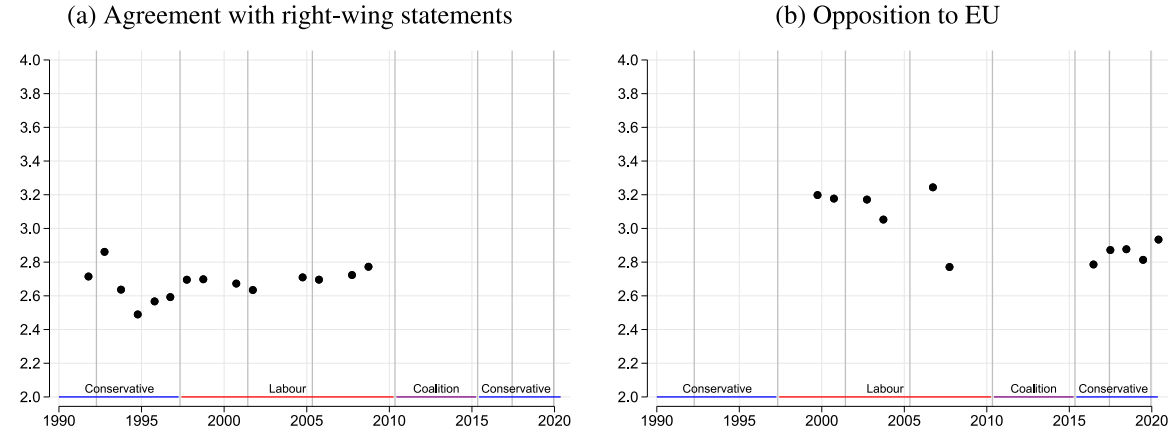


Fig. 4. Political opinions in the BHPS and UKHLS 1991–2017. Scale is 1 "strongly disagree" with right-wing statements to 5 "strongly agree". In panel (b), the statement changes from 2010 onwards. Each point represents the average for all interviews in that calendar year.

control group. The coefficient β 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 $c-1$.[14]

The coefficient δ^s in Eq. (1) is the difference-in-difference estimate of the effect on the outcome y_{it} at time s of a job loss which occurred between 2000 and 2001. For example, δ^{2004} is an estimate of the effect on the outcome in 2004 of a job loss between 2000 and 2001.

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

[14] The choice of base year can be important if pre-job loss differences between treated and controls vary over time.

before the event. In the case of Eq. (1), which measures the effect of job loss in 2000, we observe 9 years before job loss and 19 years after. If job loss is an unexpected and exogenous shock, we would expect that $\delta^s = 0$ before job loss. Rejecting $\delta^s = 0$ for pre-job loss periods amounts to rejecting the common trends assumption.

Note that the "staggered treatment timing problem" is removed if we estimate the impact of job loss separately by job loss cohort. For each cohort there is a single event which occurs between c and $c+1$ and both treated and controls have a well-defined "event time". 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 proceed by stacking together cohorts. We 'align' the cohorts by redefining the difference between the interview date and the job loss (or non-job loss) date in 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.[15] By stacking the data we are imposing the restriction that the effect of job loss relative to the job loss date (δ^s) is the same for each cohort.

In our empirical work 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

$$y_{ict} = \alpha + \beta D_i^c + \sum_{r=-9}^{10} \gamma^r T_t^r + \sum_{r=-9}^{10} \delta^r (T_t^r D_i^c) + \eta_c + \epsilon_{it}. \quad (2)$$

We include cohort fixed-effects η_c and allow the errors ϵ_{it} to be clustered by i across cohorts. The difference-in-difference estimate δ^r measures the average difference in y_{it} between the treatment and control groups in year r.

A number of refinements of Eq. (2) are possible. First, we can replace α with person-cohort fixed effects α_{ic} and estimate using differences or mean deviations (Eq. (3)). This removes the treatment indicator D_i^c and its parameter β.[16]

$$y_{ict} = \alpha_{ic} + \sum_{r=-9}^{10} \gamma^r T_t^r + \sum_{r=-9}^{10} \delta^r (T_t^r D_i^c) + \epsilon_{it}. \quad (3)$$

Second, we can 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:

$$y_{ict} = \alpha_{ic} + \omega_{ic} t + \sum_{r=-9}^{10} \gamma^r T_t^r + \sum_{r=-9}^{10} \delta^r (T_t^r D_i^c) + \epsilon_{it}. \quad (4)$$

Jacobson et al. (1993) note that Eq. (4) can be estimated 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, we 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 may outperform 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 Eq. (3) with IPW reweighting.

We also 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.[17] Define D_j^c to 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 Eq. (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 Fig. 5 plots the average gross wage of job losers 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 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.

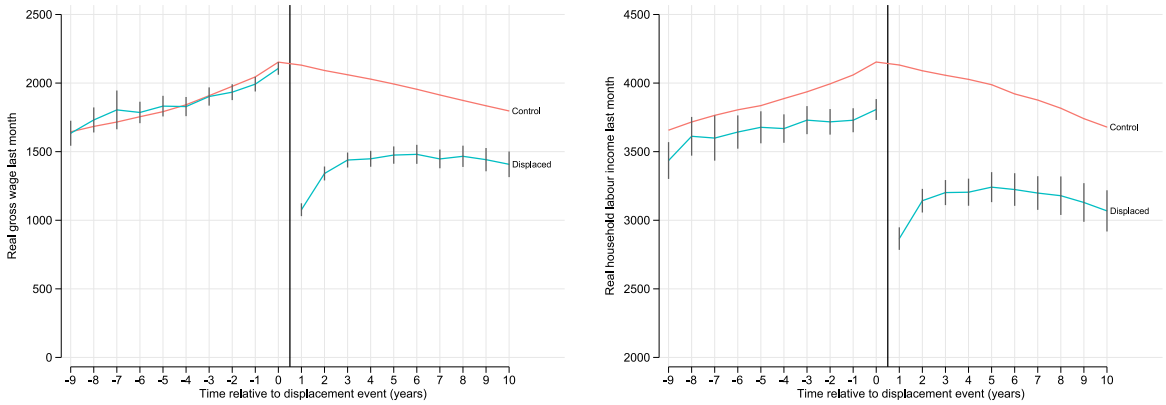
Because of its large effect on wages, job loss also has a significant impact on household income, which suggests that it may have an effect on political support and voting behaviour at the household level. Panel (b) of Fig. 5 plots the effect of job loss on household labour income. It is interesting to note that mean household labour income for our sample is almost exactly twice individual earnings. This is because employed individuals tend to have partners that also 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 6.6.

The large average wage loss shown in Fig. 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 experience a pay increase. This is illustrated in Fig. 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.

[15] Once stacked, each row in the data is identified by i, c and t, because individuals may appear in several cohorts. Butts and 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 approach of Callaway and Sant'Anna (2021) whose method weights by the proportion of treated observations in each cohort.

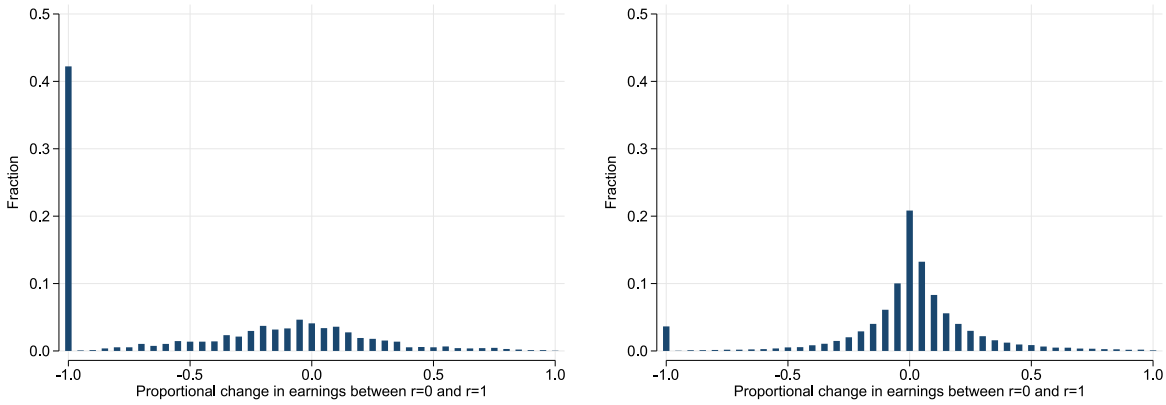
[16] Note that if the panel is balanced then β in Eq. (2) completely captures the pre-job loss difference in α_{ic} between the treatment and control groups, and the FE and DiD estimators are identical. However, in this case the panel is unbalanced and the FE and DiD estimators will differ.

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



(a) Gross wages (b) Household labour income

Fig. 5. Effects of job loss on wages and 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 Eq. (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.



(a) Job loss group (b) Non-job loss group

Fig. 6. Distribution of proportional earnings change between $r = 0$ and $r = 1$. Notes: proportional changes greater than one are not shown.

Job loss also has other measurable impacts on the individual which may have some relevance for political support and voting intentions. In Fig. 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 Fig. 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, job loss. This rationale is supported by panel (a) of Fig. 8 which shows that those who lost jobs were more likely to report being dissatisfied with their job. This suggests that matching job losers to similar individuals in the control group may be important.

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 Fig. 8 which plots 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 account 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 F we report the difference in characteristics between treated and controls before and after applying inverse probability reweighting. Table F.1 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

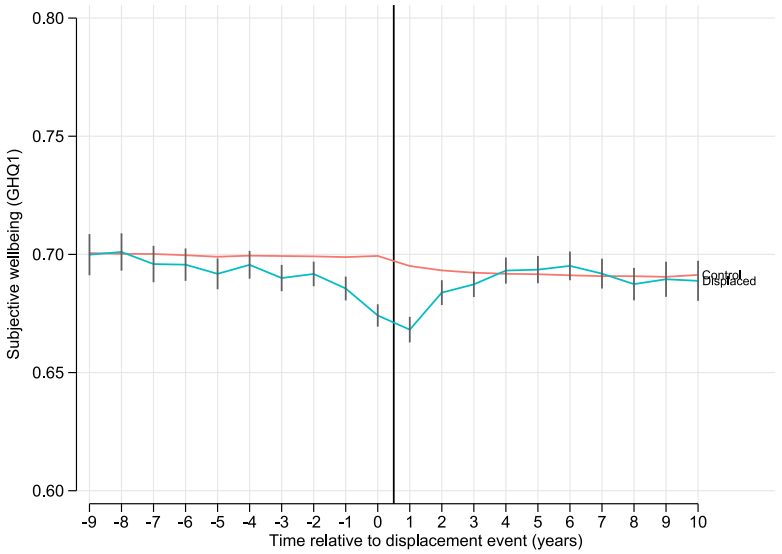
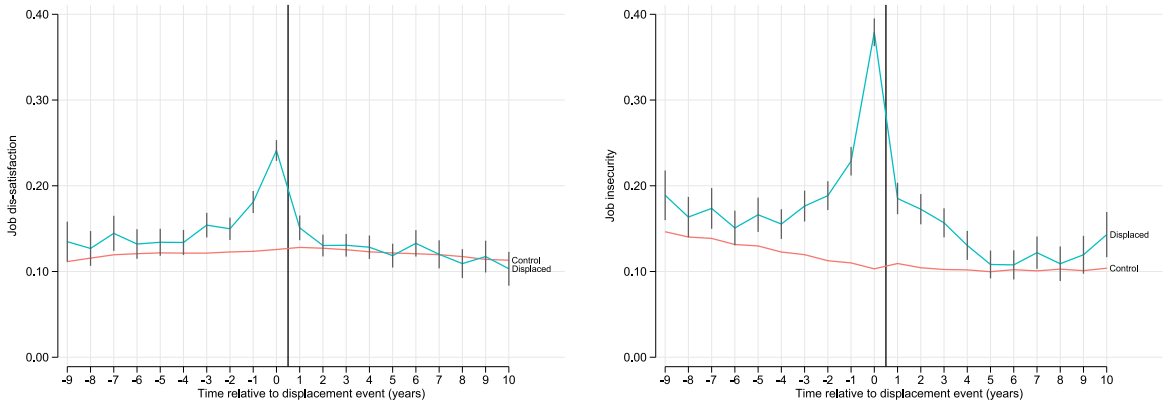


Fig. 7. Job loss and subjective wellbeing.
Notes: information comes from summing answers to 12 questions of the General Health Questionnaire and scaling between 0 (most distressed) and 1 (least distressed). See Institute for Social and Economic Research (2019) for a list of the 12 questions.



(a) Job dissatisfaction (b) Job insecurity

Fig. 8. Job loss events, job dissatisfaction and 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?".

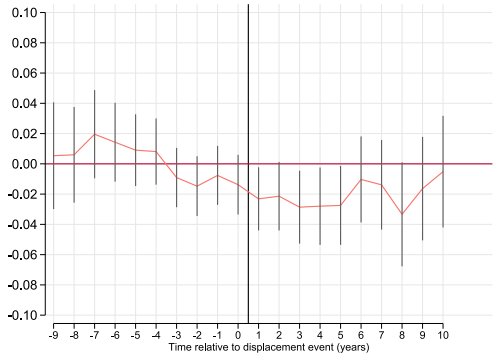
reweighting, we find that about 10% of the control observations are off the common support and so are removed, and the remaining differences in observable characteristics are small and insignificantly different from zero.

6.2. Political support

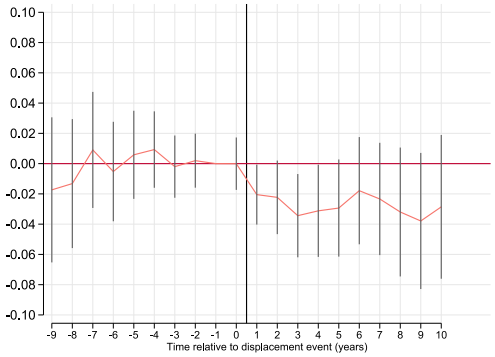
Fig. 9 summarises graphically our results for the effect of job loss on political support, and also shows how the reweighting procedure makes the treated and comparison groups observably similar in the pre-job loss period. In each row of the figure, the left-hand panel shows the raw differences in the outcome and the right-hand panel shows the estimates from Eq. (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. Fig. 9 also provides evidence that job loss reduces support for left-wing parties and for parties in general.

Our attempt to estimate effects for each year post job loss is hampered by a lack of statistical precision, with the standard error bars getting wider as we move further away from the job loss event. This is a feature of the stacked cohort data: cohorts who lose their job 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.[18] 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 Fig. 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.[19] Since there is some indication of pre-treatment differences between the control and treatment, our preferred

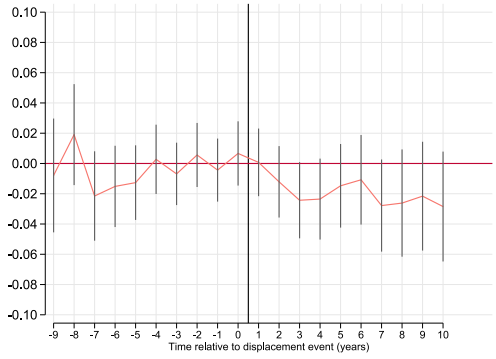
[18] This is why including individual fixed effects in Eq. (3) is important, so that time effects are identified only off within-person comparisons.



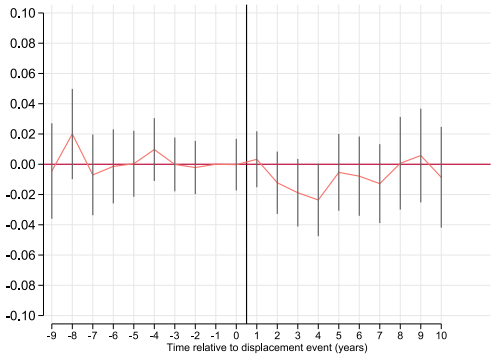
(a) Support for incumbent: raw difference from control



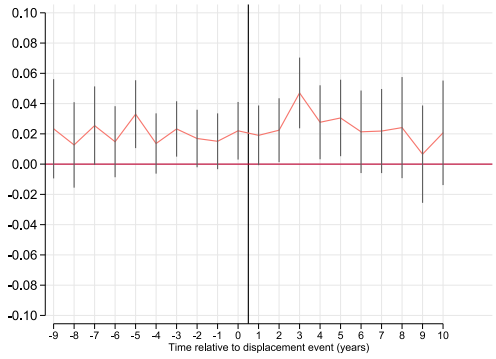
(b) Support for incumbent: FE with IPW



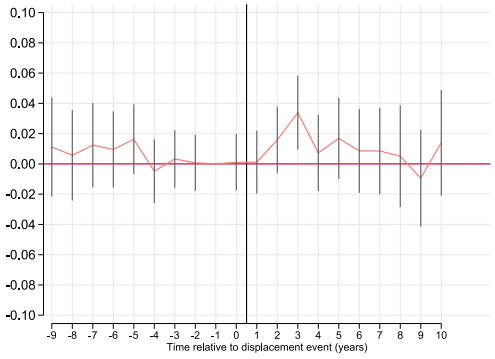
(c) Support for left-wing parties: raw difference from control



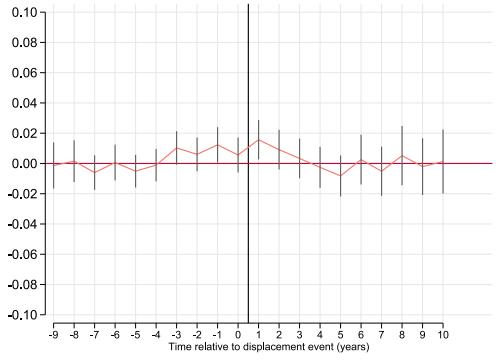
(d) Support for left-wing parties: FE with IPW



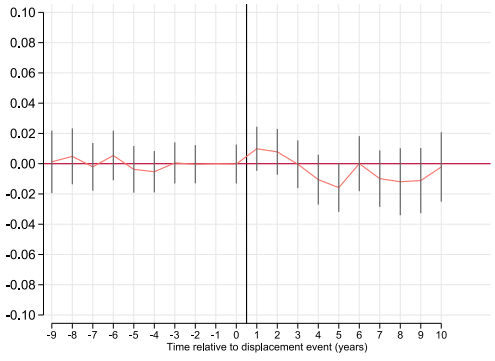
(e) No support for any party: raw difference from control



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



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



(h) Support for fringe parties: FE with IPW

Fig. 9. Effect of job loss on political support.

Table 1
Effect of job loss on political support.

	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

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 Eq. (3) after reweighting using inverse probability weights. Sample excludes Northern Ireland. Measures of political support are constructed as described in Section 3.2; see Fig. C.1 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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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 Fig. 2(d) that there was a substantial increase in support for 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 and so we have smaller sample sizes. 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.

6.4. Political views

Our results thus far point to a consistent 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 two measures to reflect underlying political attitudes: agreement with "right wing" political ideas and 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 individuals less likely to agree with right-wing views, although the size of the effect is small and is not significant 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. However, it is interesting to note that we found that such individuals are less likely to vote for a left-leaning party, and more likely to withdraw

[19] 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.

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

	Vote for incumbent		Vote for left		Did not vote		Vote 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	0.006	−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	—	−0.032*	—	0.044***	—	0.002	—
	(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

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 Eq. (3) after reweighting using inverse probability weights. Sample excludes Northern Ireland. Measures of voting are constructed as described in Section 3.3; see Fig. 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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

their support altogether. 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 that this is largely a pre-existing difference, as shown in column (3). In the year before job loss, those who will lose their jobs are 17pp more likely to agree with anti-EU statements. 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. Thus, opposition to the EU is higher amongst individuals more likely to lose their jobs, but it is not the job loss event that causes this attitude.

6.5. A comparison with existing estimates

How do our estimates compare to the existing literature? It is difficult to make precise comparisons because the literature varies substantially in terms of (a) the measurement or size of the "shock" to income; (b) the political outcome measured and (c) the identification strategy. Nevertheless, if we focus our attention on those papers in the literature which consider the effect of sudden (and possibly exogenous) changes in individual income on incumbent support, we find estimates which are broadly consistent with our own. The closest comparison in the literature is Braakmann (2017), who studies the effect of firm closure on support for mainstream political parties in Germany. His estimated effect size is −6.7pp for men, but close to zero for women, so the average across the whole sample is similar to the loss of support for incumbent parties reported in Tables 1 and 2. Healy et al. (2017), Liberini et al. (2017) and Tilley et al. (2018) consider the relationship between changes in self-reported income and incumbency voting or support. Although it is less clear that changes in self-reported income are exogenous to political preferences, all three papers find a positive short-run relationship between income and incumbency support with effect sizes which are less than 5pp, which are again comparable to our incumbent support and voting effects.[20] We also note that our incumbency effect sizes are larger than the effects on political support of events irrelevant to government performance estimated by Healy et al. (2010).

Turning to the effect of job loss on political views, 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). In contrast, we find no effect on opposition to the EU, and only a small effect on right-wing attitudes. This may be because the political opinions reported in our data are not directly related to social policy and redistribution which directly benefit those who have lost their job,

Table 3
Effect of job loss on political views.

	Agreement with right-wing views		Opposition to EU	
	(1) Raw Diff	(2) IPW FE	(3) Raw Diff	(4) IPW FE
>6 years before	0.007	−0.002	0.113*	0.033
	(0.019)	(0.017)	(0.068)	(0.077)
3–6 years before	−0.007	−0.004	0.118*	0.029
	(0.019)	(0.016)	(0.065)	(0.065)
1–3 years before	−0.027	—	0.082	—
	(0.020)		(0.056)	
0–1 year before	−0.049*	−0.029	0.170**	0.035
	(0.027)	(0.021)	(0.083)	(0.079)
0–1 year after	−0.061**	−0.064***	0.182***	0.041
	(0.030)	(0.023)	(0.070)	(0.064)
1–3 years after	−0.061***	−0.026	0.225***	0.040
	(0.022)	(0.018)	(0.054)	(0.055)
3–6 years after	−0.014	−0.012	0.145***	0.026
	(0.021)	(0.018)	(0.051)	(0.059)
>6 years after	−0.041	−0.060***	0.245***	0.091
	(0.027)	(0.022)	(0.063)	(0.076)
Mean of dep.var.	2.664	2.664	2.861	2.861
Number of obs.	252,164	224,535	215,156	196,923
Number of indiv.	6,828	6,724	16,966	16,626

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 Eq. (3) after reweighting using inverse probability weights. Sample excludes Northern Ireland. Measures of political views are constructed as described in Section 3.4; see Fig. 4 for a graphical description of the three measures. Standard errors are clustered at the individual level. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

[20] A smaller literature considers lottery wins as an exogenous shock to income (Powdthavee and Oswald, 2014; Peterson, 2016), but in both of these papers the outcome is "right-wing support" rather than support for the incumbent.

but rather relate to more abstract concepts on the left–right spectrum and opposition to the EU.

6.6. Partner effects

It is clear from the above results that the loss of a job can have a significant impact on the political support and voting patterns of individuals in the following years. Is this the result of income loss or some other psychological factor such as a loss of self esteem? A large literature (see e.g. Brand, 2015) shows that job loss causes declines in psychological well-being, and another literature has established that subjective well-being has a causal effect on support for the governing party (e.g. Ward, 2020). We can use the large losses in household income which arise from partner job loss (see panel (b) of Fig. 5) to address this question. If the results in Sections 6.2 and 6.3 are primarily driven by income loss, we would expect to find similar (although possibly attenuated) effects from partner job loss. On the other hand, if the loss of social status or self esteem is the driver then the measured effects should be much less from partner job loss. To measure the effects of partner job loss, we restrict the sample to those individuals who have a spouse or partner in employment at $r = 0$ (and are therefore at risk of job loss). 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. As before, in the even-numbered columns we report results from a fixed-effect model after re-weighting using inverse probability weighting. For all four outcomes, none of the coefficients are significantly different from zero. This may be partly because our reduced sample size means that the estimates are less precise than the equivalent own effects. Nevertheless, in almost every case the estimates are smaller in absolute size than the corresponding own effects in Table 1.

In particular, in column (2) we see that none of the estimates are negative, in contrast to the own effects. There is therefore no evidence from these results to support the hypothesis that large falls in household income, as a result of partner job loss, cause an individual to reduce support for the incumbent or make changes in other aspects of political support (columns (4), (6) and (8)). This is consistent with the hypothesis that, rather than income loss driving political support, it is being driven by other factors such as the loss of social status and self esteem caused by own job loss.

7. Heterogeneity and robustness

In this section we investigate the heterogeneity of our results across the sample, and the robustness of our results to a number of methodological and sampling decisions that we have made. Column (1) of Table 5 repeats our preferred model,[21] which gives us a baseline for job loss events.

First, we examine whether the size of the income loss matters. Whilst mean income falls substantially following job loss, we showed in Fig. 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 income loss between $r = 0$ and $r = 1$ was greater than the median income loss of the entire treatment group (−42%). Our comparison is therefore between job losers who experience large income 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 have no effect on political support.

Second, following (for example) Tilley et al. (2018), we investigate the hypothesis that political responses to job loss are greater if voters hold the government responsible for that loss. Our prior is that individuals are less likely to blame the government for a job loss which occurs in a boom, and so in column (3) we remove job losses which occur during a recession.[22] Comparing with the base model in column (1) we see that the sample restriction has a negligible impact on the effect.

Third, we investigate whether the response is greater for those who are not strong partisans. 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 (4) 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 (5) and (6) 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 (5) 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 subjective well-being, job dissatisfaction and job insecurity shown in Figs. 7 and 8 suggested that the job loss event was preceded by a decline in job security and wellbeing. If this decline in job security causes a reduction in support for the incumbent party before $r = 0$, our method will lead to an underestimate of the actual effect on incumbent support. Therefore in column (6) we balance observable characteristics, including political support, during an earlier period ($r = -3$, $r = -4$ and $r = -5$), thus allowing for pre-job loss differences in outcomes at $r = -2$, $r = -1$ and $r = 0$. This change has little impact. The effects of job loss on the four outcomes remain very similar, 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 compare job losers with an reweighted control group which is observably similar in the pre-job-loss period. We measure effects on three important aspects of political support and

[21] The FE-IPW results in Table 1.

[22] In our estimation sample the recession of 2008-9 is the only downturn. We cannot use the early 1990s recession because, although the data start in 1991, we require three years of pre-job-loss information to ensure that control and treatment groups are comparable. This means that of the 2481 job loss events we observe in the regression sample, only 102 occur in this recession window.

Table 4
Effect of partner job loss on political support.

	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	0.008	−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	—	0.007	—	0.012	—	−0.001	—
	(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

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 Eq. (3) after reweighting using inverse probability weights. Sample excludes Northern Ireland. Measures of political support are constructed as described in Section 3.2; see Fig. C.1 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. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

behaviour: party support, voting and political views. We use staggered difference-in-differences models with flexible relative time effects. In contrast to the existing literature, we exploit a precisely-timed, 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. In doing so we provide new evidence from the UK on the political consequences of economic shocks.

We find that job loss causes a 2–4 percentage point reduction in support and voting for the incumbent party, a smaller reduction in support for left-wing parties, and a decrease in political engagement. We find little evidence that economic shocks increase support for fringe political parties. This could be because of the stable political environment in the UK, which is dominated by two parties with relatively similar legislative agendas over this time period. Job loss reduces support for our combination measure of right-wing political views, but the effect is smaller than the effect on incumbent support or voting. Although opposition to the EU is strongly correlated with job loss risk, job loss has no significant effect on opposition after the event.

Our methodology provides a clearer view of the dynamics of income shocks and political preferences because we have a precisely-timed shock and can follow individuals for a relatively long period. We find that the reduction in support for the incumbent lasts for at least an electoral cycle, but the effects on other outcomes are more short-lived. We find little effect on support for fringe or populist parties in the short- or long-term. The majority of the empirical literature does not provide comparable estimates at different points in time after the income shock, partly because in the existing literature the timing of the shock is not precisely measured. In most cases, dynamic effects are estimated by comparing those who experienced unemployment but subsequently returned to work (e.g. Margalit, 2013; Naumann et al., 2015), whereas we show that the effects on incumbent support and voting are quite persistent, beyond an electoral cycle.

Finally, we 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).

Declaration of competing interest

Upward's work on this project was supported by the *VW Stiftung* grant 94750 "The rise of populist parties in Europe: the dark side of globalization and technological change". We have no potential competing interests to declare.

Appendix A. Sample information

We use all 18 waves of the BHPS and the first 11 waves of the UKHLS. 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.

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.[23]

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

Table 5
Robustness of results to methodological decisions.

	(1) Base model	(2) Large income loss	(3) No recession	(4) Weak pre-existing support	(5) Pre-job loss sample restriction	(6) Reweight on earlier period
(a) Support for incumbent						
0–1 year after	−0.021**	−0.028**	−0.021**	−0.027**	−0.025***	−0.011
	(0.009)	(0.013)	(0.010)	(0.012)	(0.008)	(0.014)
1–3 years after	−0.029***	−0.027*	−0.028**	−0.035***	−0.028***	−0.037**
	(0.011)	(0.016)	(0.011)	(0.013)	(0.009)	(0.014)
3–6 years after	−0.028**	−0.031	−0.024*	−0.035**	−0.019*	−0.033*
	(0.014)	(0.021)	(0.014)	(0.016)	(0.011)	(0.018)
Number of obs.	974,428	835,328	934,582	658,835	1,506,616	711,011
Number of indiv.	18,806	18,336	18,717	15,108	28,788	14,068
(b) Support for left-wing parties						
0–1 year after	0.004	0.007	0.004	−0.001	0.001	0.009
	(0.008)	(0.012)	(0.009)	(0.011)	(0.007)	(0.011)
1–3 years after	−0.014*	−0.007	−0.016*	−0.015	−0.011*	−0.012
	(0.008)	(0.012)	(0.008)	(0.011)	(0.006)	(0.010)
3–6 years after	−0.012	−0.012	−0.016*	−0.011	−0.006	−0.013
	(0.010)	(0.014)	(0.010)	(0.012)	(0.007)	(0.012)
Number of obs.	978,502	837,223	934,582	666,844	1,507,787	711,307
Number of indiv.	18,793	18,326	18,717	15,136	28,770	14,063
(c) No support for any party						
0–1 year after	0.001	−0.012	0.003	0.003	0.009	−0.007
	(0.009)	(0.013)	(0.010)	(0.013)	(0.007)	(0.012)
1–3 years after	0.024***	0.021	0.024***	0.028**	0.020***	0.032***
	(0.009)	(0.013)	(0.009)	(0.012)	(0.007)	(0.011)
3–6 years after	0.011	0.019	0.012	0.009	0.006	0.013
	(0.010)	(0.014)	(0.010)	(0.013)	(0.007)	(0.012)
Number of obs.	979,444	832,849	934,582	666,041	1,510,422	708,688
Number of indiv.	18,791	18,268	18,717	15,124	28,768	14,026
(d) Support for fringe parties						
0–1 year after	0.010	0.016*	0.008	0.018**	0.007	0.018**
	(0.007)	(0.009)	(0.007)	(0.009)	(0.005)	(0.008)
1–3 years after	0.004	0.003	0.002	0.009	0.007	−0.002
	(0.006)	(0.009)	(0.006)	(0.008)	(0.005)	(0.006)
3–6 years after	−0.009	−0.012	−0.008	−0.002	−0.005	−0.002
	(0.006)	(0.009)	(0.006)	(0.008)	(0.005)	(0.007)
Number of obs.	818,488	707,265	797,250	532,499	1,275,620	605,693
Number of indiv.	17,131	16,662	17,208	13,732	25,968	13,078

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 treatment group to those whose job loss event did not coincide with a recession. In column (4) 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. In column (5) we relax the restriction that the sample must be interviewed in the three waves preceding job loss. In column (6) we balance observable characteristics at $r = -3$, $r = -4$ and $r = -5$ to allow for the possibility of pre-job-loss effects.

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 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.[24] 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".

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

Table A1
BHPS and UKHLS sample sizes.

	First interview date	Last interview date	BHPS only	UKHLS only	BHPS and UKHLS
1	01 09 91	31 12 91	6132	0	3777
2	02 09 92	24 04 93	5600	0	3858
3	02 09 93	28 04 94	5059	0	3964
4	01 09 94	09 05 95	4931	0	4128
5	04 09 95	20 05 96	4582	0	4244
6	29 08 96	17 04 97	4675	0	4461
7	30 08 97	08 05 98	4558	0	4559
8	31 08 98	30 04 99	4270	0	4669
9	01 09 99	31 05 00	4037	0	4782
10	01 09 00	31 05 01	3804	0	4897
11	01 09 01	30 04 02	3557	0	5033
12	01 09 02	21 05 03	3271	0	5111
13	01 09 03	10 05 04	3036	0	5227
14	01 09 04	11 05 05	2748	0	5331
15	01 09 05	04 04 06	2489	0	5495
16	01 09 06	03 04 07	2285	0	5661
17	01 09 07	13 03 08	1931	0	5824
18	01 09 08	02 04 09	1606	0	5894
19	08 01 09	10 03 11	0	39,044	0
20	12 01 10	27 03 12	0	32,764	5950
21	13 01 11	12 05 13	0	29,604	5468
22	20 01 12	19 06 14	0	28,198	4968
23	09 01 13	02 06 15	0	26,985	4708
24	08 01 14	11 05 16	0	24,605	4366
25	15 01 15	16 05 17	0	23,810	4125
26	05 01 16	03 05 18	0	22,879	3986
27	05 01 17	21 05 19	0	21,550	3785
28	09 01 18	15 05 20	0	20,856	3589
29	04 01 19	13 05 21	0	19,724	3400

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.

Table B1
Sample selection.

	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

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

Table B2
Reasons for employment spell ending (BHPS).

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

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 (wave 2 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 6681 employment spells in progress at the wave 18 interview. Of these, 5115 are 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.

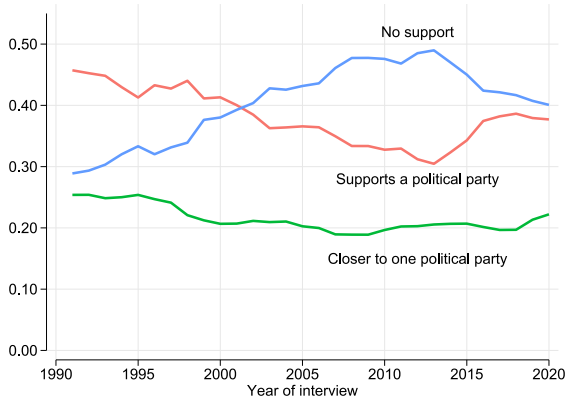
Table B3 summarises the resulting job loss information, which is shown graphically in Fig. 1. Note that in 2008 (which is usually the last interview date from the BHPS) the proportion of continuing jobs is significantly lower than either 2007 or 2009. This is because the median length of time between 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.

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.

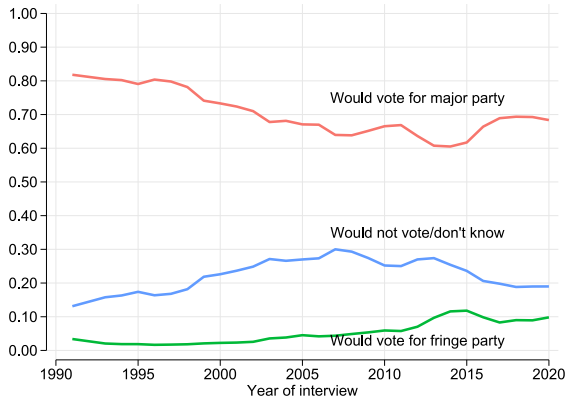
	(1) In sample at wave t and wave $t+1$	(2) In employment	(3) Job loss	(4) Job ended for other reasons	(5) Job continued
1991	8424	4034	0.059	0.115	0.825
1992	8039	3802	0.063	0.121	0.816
1993	7940	3732	0.051	0.141	0.808
1994	8033	3813	0.052	0.149	0.799
1995	8045	3929	0.049	0.144	0.807
1996	8502	4195	0.042	0.158	0.800
1997	8190	4161	0.047	0.162	0.790
1998	8104	4115	0.045	0.170	0.784
1999	7375	3766	0.043	0.168	0.789
2000	8465	4398	0.046	0.175	0.779
2001	7712	4002	0.051	0.164	0.785
2002	7504	3881	0.041	0.166	0.793
2003	7475	3851	0.034	0.168	0.798
2004	7189	3682	0.041	0.162	0.797
2005	7422	3857	0.038	0.144	0.818
2006	7258	3747	0.038	0.165	0.798
2007	7023	3588	0.039	0.139	0.822
2008	5774	2941	0.071	0.132	0.797
2009	15,477	7331	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	9512	4325	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

Appendix C. Time-series patterns of political support

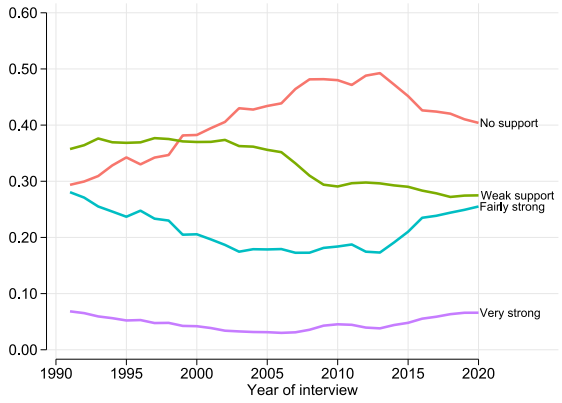
The time-series patterns of political support over the sample period are plotted in Fig. C.1.



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



(b) Party support or voting intention



(c) Strength of support for political party

Fig. C.1. 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. Fig. D.1 compares the proportion of the sample who reported voting in the most recent general election with the actual turnout, and Fig. D.2 compares voting recall with actual vote shares by political party.

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

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

Appendix E. Right-wing statements and political support

Fig. E.1 shows the relationship between the average agreement with right-wing statements and the strength of support for the two main UK political parties.

Appendix F. Individual characteristics and reweighting

Tables Table F.1 and Table F.2 show mean comparison tests before and after reweighting.

Data availability

Data are publicly available from https://ukdataservice.ac.uk/. The authors will make all code available to replicate their analysis.

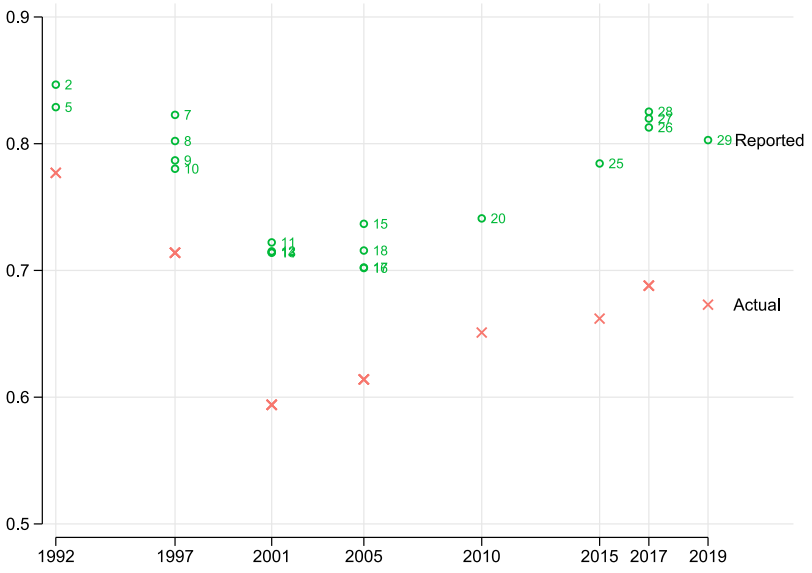


Fig. D.1. Proportion of eligible sample who reported voting in most recent general election. Weighted by cross-section weights. The eligible sample excludes those who report that they are not able to vote. The number next to each point indicates the wave of the data.

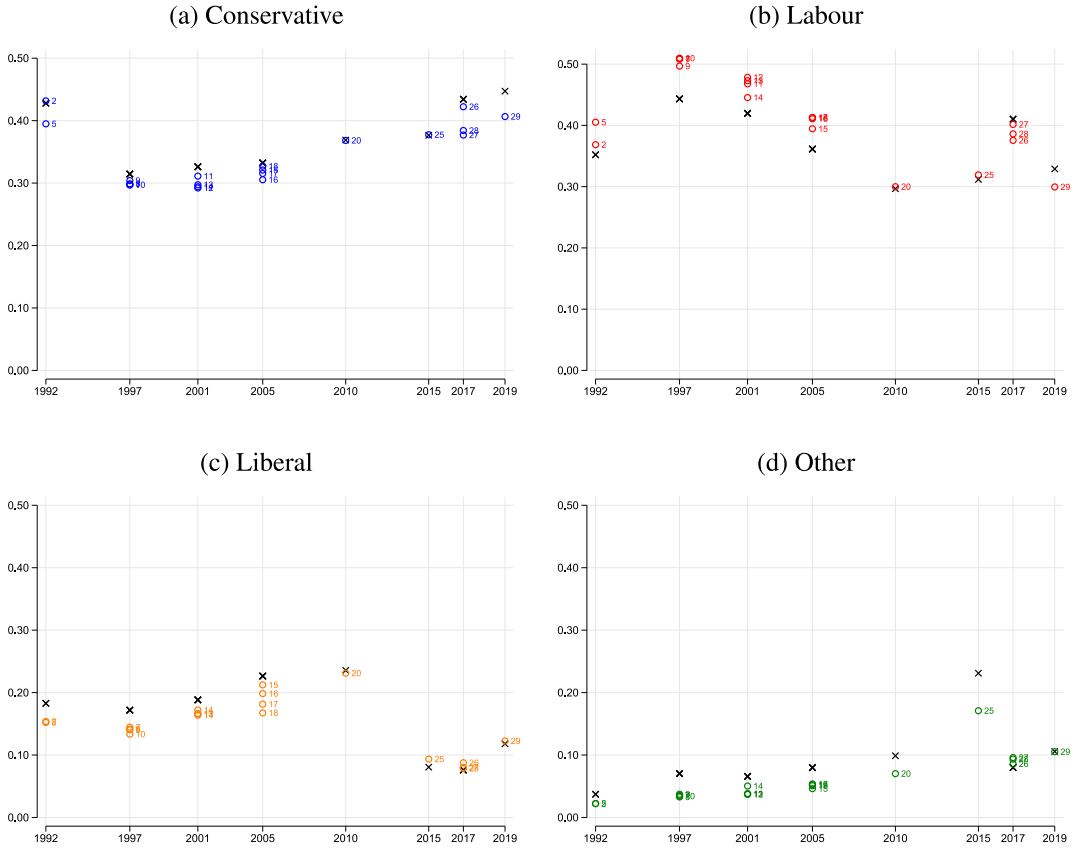


Fig. D.2. Comparison of voting recall with actual vote shares. Weighted by cross-section weights. The number next to each point indicates the wave of the data.

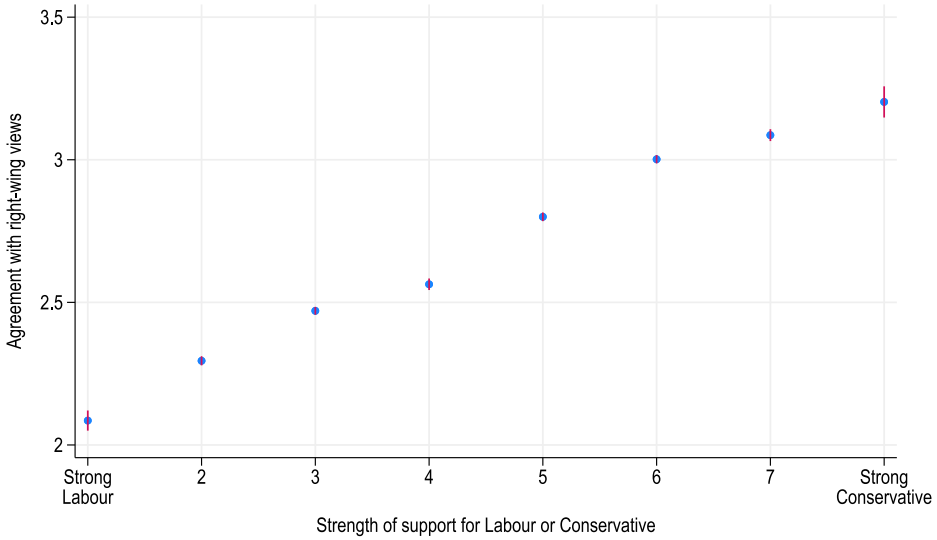


Fig. E.1. Horizontal axis measures strength of support for the two main UK political parties, where "1" represents strong support for Labour and "8" represents strong support for Conservative. Vertical axis measures average agreement with the right-wing statements described on Section 3.4. 95% confidence intervals are shown, but are very small.

Table F.1
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.
=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
	2481	97,372		2,349	85,320	

Table F.2
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.

	Unbalanced			Balanced		
	Job loss	Non-job loss	Diff.	Job loss	Non-job loss	Diff.
=1 voted for incumbent at last election $r = 0$	0.281	0.297	−0.015	0.280	0.280	0.001
=1 voted for incumbent at last election $r = -1$	0.283	0.293	−0.010	0.278	0.278	−0.000
=1 voted for incumbent at last election $r = -2$	0.291	0.293	−0.002	0.285	0.286	−0.001
=1 voted for incumbent at last election $r = -3$	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
	2481	97,372		2359	87,178	

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