The Political Impact of Job Destruction Announcements: Evidence from the United Kingdom

Costin Ciobanu[*] Riccardo Di Leo[†]

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Abstract

Recent work in political science and economics emphasizes the impact of economic shocks on voting behavior, particularly the rise of populist radical right parties. However, while many studies have used rigorous research designs to establish credible causal relationships, they often overlook how the effects of economic shocks evolve over time. The literature generally captures a static relationship between economic shocks and political outcomes without accounting for potential variation in effects from the immediate aftermath to the longer term. This limits our understanding of the substantive meaning of these effects and the role of time in shaping how shocks influence political behavior. This study offers a novel contribution by theorizing and empirically testing how the effects of contextual economic shocks differ in their immediate aftermath versus the long term. Using the 2014–2020 British Election Study panel data, local job destruction announcements, and two empirical designs (Unexpected-Events-During-Survey-Design and staggered difference-in-differences), we demonstrate that economic factors dominate shortly after a shock, but over time, cultural concerns, particularly around immigration, gain prominence. We explore which political actors benefit from economic shocks, integrating both demand- and supply-side factors and highlight how radical populist parties may struggle initially but gain traction as political competition shifts from economic to cultural issues.

[*]Department of Political Science, Aarhus University, Denmark; Email: costin.ciobanu@ps.au.dk.
[†]Department of Social and Political Sciences, European University Institute, Italy; Email: riccardo.dileo@eui.eu.

1 Introduction

Recent work in political science and economics has shown that contextual economic shocks and, more generally, economic vulnerability have boosted support for the populist radical right, increased the salience of immigration as immigrants become scapegoats for deteriorating economic conditions, and, in the intensely discussed UK case, contributed to the success of the Leave campaign in the Brexit referendum. For instance, using data from 15 Western European democracies over the 1988-2007 period, Colantone and Stanig (2018a) show that a stronger import shock due to Chinese exports triggers greater electoral support for nationalist and isolationist parties. Baccini and Sattler (2024) demonstrate that austerity boosts support for radical-right populism in economically vulnerable European regions. Hopkins, Margalit, and Solodoch (2024) find that economic shocks spur opposition to immigration, while Laaker (2024) finds that an economic shock during young adulthood causes a significant increase in anti-immigration attitudes. Since immigration is central to the populist vote (Margalit, Raviv, and Solodoch, 2024), economic shocks could contribute to the populist surge through this channel as well. Finally, Colantone and Stanig (2018a) argue that the Leave vote share in the Brexit referendum was higher in regions more exposed to the Chinese import shock, while Fetzer (2019) demonstrates that austerity cuts in the UK contributed to the UKIP vote and the outcome of the 2016 referendum.

Given the notorious difficulty in establishing a causal link between contextual economic vulnerability and political attitudes and voting behavior (Guriev and Papaioannou, 2022; Margalit, 2019a), many of these studies opt for rigorous research designs to ensure that the relationships they capture are not spurious. For example, Colantone and Stanig (2018a) and Colantone and Stanig (2018b) use the empirical strategy proposed by Autor, Dorn, and Hanson (2013), which leverages an instrumental variable approach based on exposure to imports from China following the country's accession to the WTO in 2001. Additionally, Dehdari (2022) employs a Bartik instrument to capture the connection between economic distress and support for radical right parties. Similarly, Ballard-Rosa et al. (2021) uses the same shift-share instrument to show that economic shocks led to more authoritarian values.

This laudable methodological rigor comes at a cost, however. In this context, it is reasonable to ask what the carefully identified causal effect of the economic shock on political outcomes actually measures. The issue is that the causally identified estimate of economic shocks captures not just the shock itself but also everything that happens between the shock and the political outcome. This includes how the media portrays the shock, how political parties decide whether and how to address the issue, acting as political entrepreneurs (De Vries and Hobolt, 2020), and whether the shock is connected to other issues salient in the community. In other words, in these observational studies, it is common to observe an economic shock, theorize how it affects a political or electoral outcome, and measure this impact as rigorously as possible without fully considering what this bundled effect incorporates—what its *substantive* meaning is. The longer the time between the shock and the political outcome, the more likely it is that what we estimate encompasses far more than the shock itself. Table 1 briefly illustrates this issue by reviewing a few relevant studies. For example, in Colantone and Stanig (2018a), there is a significant gap between the strength of the Chinese import shock (measured for the 1990-2007 period) and the political outcome of the Brexit referendum in 2016. Similarly, in relation to the UKIP vote and austerity measures (Fetzer, 2019), there can be up to a year between welfare cuts and their electoral consequences. Given the potential and likely politicization of these events, time may be crucial if we consider that the electoral impact of these shocks could vary over time.

Our starting point is that the existing literature, due to data limitations and its primary focus on providing causally credible results, does not account for how the effects of contextual shocks vary over time. Although some studies have mentioned time variation in the effects of economic shocks (see discussion in Margalit (2019b, p. 288)), this has not been systematically addressed for contextual shocks. What we raise here is not just a methodological concern but also a substantive issue—should we expect the effects of shocks to remain constant over time?

We argue that the effects of contextual economic shocks are not constant and that different theoretically meaningful mechanisms are at play immediately after the shock versus in the longer term. In the short-term, we contend that these shocks should lead to *normal politics*. Confronted

Table 1: Examples of relevant studies.

Study	**Outcome variable**	**Main explanatory variable**
Colantone and Stanig (2018a)	Leave vote share (2016)	Strength of the Chinese import shock (1990-2007)
Dehdari (2022)	Change in radical right (SD) vote (2006-2010)	Layoff notices (2007-2010)
Anelli, Colantone, and Stanig (2021)	Radical right vote (1999-2015)	Individual exposure to automation (two years prior to the election)
Fetzer (2019)	UKIP vote (yearly; election-to-election)	Austerity (yearly)
Ballard-Rosa et al. (2021)	Authoritarian values (2017)	Exposure to trade shocks from China's integration into the world economy (1991-2007)

with negative shocks, citizens' economic perceptions are likely to worsen, but this deterioration should not benefit populist right-wing parties. Similarly, these shocks should not increase concerns about immigration. In the longer term, we expect a different situation, which we label *backlash politics*. Negative shocks are anticipated to lead to economic pessimism, heightened concerns about immigration, and a greater likelihood of voting for populist parties. Our focus is on contextual economic shocks, which primarily impact significant numbers of jobs and specific communities. These economic shocks, which could include job loss due to offshoring or delocalization of production, but could also refer to job creation due to the opening of a new factory, are a regular occurrence. For instance, in the United Kingdom (the context we study), between 2002 and 2021, there were 3,252 job announcements, each covered by the media and involving more than 100 jobs. Moreover, mass layoffs have been found to affect voting behavior (Baccini and Weymouth, 2021; Rickard, 2021), and the local context is relevant for the formation of economic perceptions and voting calculations (Bisgaard, Sønderskov, and Dinesen, 2016; Reeves and Gimpel, 2012).

Our key contribution to the extensive literature on the political consequences of economic shocks is that we theorize and empirically test how contextual economic shocks play out differently in their immediate aftermath compared to months or years after the event. We achieve this using UK individual-level panel data and through two empirical designs: a natural experiment and a staggered

difference-in-differences approach. This contribution is also significant because it addresses the broader debate about the cultural versus economic roots of populism (see discussions in Guriev and Papaioannou (2022), Margalit (2019a), Margalit, Raviv, and Solodoch (2024), and Agnolin, Colantone, and Stanig (2024)). With our work, we aim to contribute to the methodological challenge of credibly distinguishing between these two causes through a theoretical lens. We demonstrate that, shortly after a shock, economic considerations dominate, while hardening attitudes towards immigration are absent. While we can differentiate between economic and cultural effects immediately after a shock, this becomes impossible in the longer term, a finding consistent with the existing scholarship.

We also address the puzzle of which political actors benefit from economic shocks—whether opposition parties, left-wing parties, Populist Radical Right (PRR), or radical left parties. As discussed by Margalit (2019b, p. 291), there is a lack of a theoretical framework to explain empirical findings that show economic shocks benefiting a wide range of political actors, including left-wing parties, populist radical right parties, and mainstream opposition parties, without a clear expectation of when one type of party should perform better than another. To "go beyond the investigation of each political response to a shock as a separate phenomenon," we need "a broad framework that considers in tandem the range of possible responses to the shock" (Margalit, 2019b, p. 291). Guriev and Papaioannou (2022, p. 819) raise a similar issue, concluding that, while the link between adverse economic shocks and populism is established, specifying the exact mechanisms remains a challenge, particularly in understanding which types of parties should benefit from such events. Our approach moves beyond demand-side explanations (e.g., public opinion) to incorporate supply-side factors such as party competition and the multidimensionality of political conflict in Europe. By drawing on insights from party competition (De Vries and Hobolt, 2020; Hobolt and De Vries, 2015; Kriesi et al., 2012), we explain how different political actors respond to economic shocks, how this shapes voter behavior, and specifically why radical populist parties may initially suffer electorally when the economy is the main axis of political competition, but rebound in the longer run as immigration becomes more salient for voters.

2 Theoretical argument

In the immediate aftermath of an economic shock, economic concerns are expected to rise to the forefront, leading to a focus on the economy. This is consistent with previous findings in the literature. Margalit (2013) finds that economic hardship affects people's economic policy preferences, with those affected by job loss increasing their support for welfare spending. Cotofan et al. (2023) reports that recessions create cohorts of workers who prioritize income over job meaning. In analyzing the electoral consequences of the financial and economic crisis in Europe, Hernández and Kriesi (2016, p. 205) argue that "in a period of economic turmoil, economic considerations are likely to be more salient for the decision to reward or punish the incumbent." Moreover, compared to good economic times, the economy seems to structure voter behavior more during bad economic times (Lewis-Beck and Paldam, 2000), such as those induced by economic shocks. This increased salience of the economy is expected to activate the logic of economic voting (Lewis-Beck and Stegmaier, 2019), where voters assess parties based on the state of the economy and their perceived economic competence.

A focus on the economy and the logic of economic voting centers attention on the dominant axis of political competition: the economic left–right axis. As De Vries and Hobolt (2020, p. 88) argue, in Western Europe, mainstream dominant parties "have risen to power in political systems structured along a left–right dimension, with political programs that focus on the role of state intervention in the economy. Each party's distinctive brand is thus closely associated with the distinct positions it adopts on left–right issues, and its partisan voters unite behind that core message." Structured this way, political conflict pits left-wing parties that advocate for "state intervention in the free market, a strong welfare state, and redistribution" against right-wing political actors who advocate for an "unregulated market and low taxes" (De Vries and Hobolt, 2020, p. 98). Mainstream parties are typically perceived as owning economic issues (Held, 2023), so they seek to focus the public agenda on economic left–right concerns. Economic shocks naturally center public debate around the (in)competence of handling economic issues. In this way, mainstream parties can avoid issues

that do not align with the left–right dimension and could divide their voters (see discussion in De Vries and Hobolt (2020, p. 89)), such as immigration. Even though competence shocks (Green and Jennings, 2017) can affect the reputation and electoral support of mainstream parties, the scope of the conflict is contained, and no electoral hemorrhage in favor of parties that operate mainly outside the dominant axis of political competition should be observed.

Populist radical right parties struggle to compete on the economic left–right axis of political competition. These parties typically mobilize voters on other issues, primarily opposing immigration and European integration, while portraying mainstream political leaders as a corrupt elite disconnected from the people. Immigration does not easily fit within the dominant economic left–right dimension, making it challenging for center-left and center-right parties to respond effectively to the radical right on this issue (see discussion in De Vries and Hobolt (2020, pp. 31–32)). Crucially, radical right parties often lack clear positions on economic policies and sometimes even present inconsistent stances—for instance, in the 1980s, they embraced neoliberal policies and supported tax cuts, while more recently, they have advocated for greater state intervention in the economy (De Vries and Hobolt, 2020, p. 32). As underperformers on the dominant dimension of contestation, these parties engage in issue entrepreneurship (Hobolt and De Vries, 2015), attempting to shift the focus of political competition. However, this is unlikely to occur immediately after an economic shock, when economic concerns remain highly salient.

Thus, in general, voters perceive mainstream parties as more competent on economic issues, giving them a "competence advantage" (De Vries and Hobolt, 2020, p. 101) in the aftermath of an economic shock. As competence refers to "the degree to which parties are trusted to govern and deliver policies," De Vries and Hobolt (2020, p. 102) argue that "voters are more likely to associate the dominant party brands with competence and trust them to govern in the future, rather than maverick challenger parties without a track record." This is further supported by findings from Di Tella and Rodrik (2020), who show, using experimental data, that support for government intervention rises sharply in response to labor market shocks. The lack of credibility on economic issues for populist radical right parties should put them at an electoral disadvantage following such an event. Even in

the U.S., evidence suggests that priming voters to think about the economic downturn caused by the COVID-19 pandemic reduced support for a populist candidate like Donald Trump (Neundorf and Pardos-Prado, 2022).

In addition to the argument related to party competition, a more general psychological reaction could be at play. Economic shocks trigger a psychological need for stability and uncertainty reduction. Thus, voters are more likely to rally around mainstream parties, seeking competence and stability. As Delis, Matakos, and Xefteris (2020) argue, there is a prevailing preference for political stability, which fosters support for leaders perceived as capable "problem solvers." This pattern has been observed in various contexts, including responses to economic crises, the COVID-19 pandemic, and terrorist attacks. For instance, Ciobanu and Van Spanje (2024) show that when faced with an external economic threat, voters tend to rally behind their government despite worsening economic conditions. Similarly, in response to the COVID-19 pandemic, De Vries et al. (2021) demonstrate that "the Italian lockdown alerted citizens in other European countries about how grave the unfolding crisis was, leading them to increase support for their government." In a meta-analysis, Godefroidt (2023) argue that terrorism is associated with rally-round-the-flag effects. While we do not claim that economic shocks will necessarily benefit incumbents, as our theory is rooted in the economic voting tradition, we posit that, given the uncertainty that localized economic shocks induce, voters are likely to prefer political stability and mainstream political parties. This means they will distance themselves from populist right-wing parties, which are more likely to produce upheaval in the political system and are thus not perceived as a factor of stability.

All in all, in the short term following an economic shock, we argue that the salience of the economy crowds out other issues, such as immigration, which are typically the domain of populist radical right parties. As a result, these parties lose electoral support because voters prioritize economic competence, where the populists are at a disadvantage. This is our first key expectation, which we label as "normal politics".

Whereas in the short term, contextual economic shocks are primarily announcements with effects that are only anticipated, over the longer term, their consequences should become more apparent.

The broader literature shows that, following recessions, areas that experience greater job losses during the downturn face persistent relative declines in employment and population (Hershbein and Stuart, 2024). Thus, over time, the impact of the economic shock becomes more visible. For political actors, this presents an opportunity to redefine political conflict and transform an economic shock into an electoral one (Fieldhouse et al., 2021).

Here, we expect that populist right parties will act as issue entrepreneurs (Hobolt and De Vries, 2015) and attempt to shift the political debate from the economic left-right axis (where they are weaker) to issues they "own," such as immigration. They will, for instance, blame immigrants for the deteriorating economic situation, accuse them of abusing the welfare system, and associate them with any rise in criminality. Populist right parties will actively seek to reframe the political debate, using immigration and anti-immigrant rhetoric to regain political ground and move mainstream political parties away from the dominant economic left-right axis of competition towards a libertarian-authoritarian cultural dimension of political competition (Norris and Inglehart, 2019), that has gained ground in Europe.

Furthermore, populist radical right parties are unlikely to limit themselves to strategically redefining the political conflict. They are expected to also employ anti-establishment rhetoric, attacking the economic credibility of mainstream parties (for an extensive discussion, see: De Vries and Hobolt, 2020, p. 144) and centering the political debate on immigration and elite corruption. This reframing helps them partially neutralize their disadvantage on economic issues by leveraging their perceived competence on immigration.

As the short-term effects of the economic shock fade and parties engage in politicizing the event, immigration once again becomes salient, allowing populist radical right parties to recover lost electoral support. This story fits with what Panunzi, Pavoni, and Tabellini (2024) argue: after a negative shock, economically disappointed voters become risk lovers and are attracted to the riskier candidates–the populists. In the longer term, both economic and immigration issues coexist within the space of party competition, with the outcome depending on how well mainstream parties maintain dominance and how successfully populists innovate their messaging. This evolution

aligns with what Guriev and Papaioannou (2022, p. 792) describe as the "culture-times-economics view" (or the "interactive theory", according to Gidron and Hall, 2017) on the interaction between economic and cultural factors: in this case, "recent economic shocks triggered dissatisfaction with the status quo, leading to the (re)emergence of identity politics alongside preexisting cultural fault lines. Economic and cultural factors reinforce each other." Thus, in the longer term, distinguishing between economic and cultural causes of populism, even when an economic shock serves as the starting point, remains very challenging, as extensively discussed in the literature (Guriev and Papaioannou, 2022; Margalit, 2019a; Margalit, Raviv, and Solodoch, 2024; Agnolin, Colantone, and Stanig, 2024).

To sum up, in the longer term following an economic shock, we should observe the resilience of populism (or "backlash politics," as we call it). Over time, populist radical right parties can recover or even increase their electoral support as immigration issues re-enter the political debate–our second key expectation. The general story that we will next empirically test is that, while populist radical right parties may lose electoral support in the short term, they can regain ground in the longer term as immigration re-enters voters' evaluations after an initial post-shock period in which the economy dominates.

3 Empirical Design

3.1 Data

Information on contextual economic shocks comes from the European Restructuring Monitor (ERM) database, covering more than 25,000 large-scale restructuring events taking place in the EU since 2002. An announcement of forthcoming job openings or losses is included in the ERM database if it is reported in the media and affects 100 or more jobs (or 10% of the workforce, for sites with more than 250 employees).[1] We demonstrate the quality of the data by replicating the

[1]Extant literature has shown how announcements covered by the ERM reflect into subsequent mass layoffs (Braakmann and Vermeulen, 2023).

well-established correlations in the literature between exposure to contextual economic shocks and political outcomes. Specifically, we show that more exposure to negative shocks boosts populist support, deteriorates economic sentiment, and increases the salience of immigration.[2]

We focus on announcements involving the UK for which we can retrieve both the date on which they went public and the Local Authority Districts (LADs) being involved. We focus on salient events, *i.e.*, on announcements involving at least 100 jobs being created/destroyed (the median value in the sample).[3] This leaves us with a sample of 3,252 announcements covering 19 years (May 2002 - February 2021). Taking advantage of the taxonomy proposed in the ERM, we distinguish between two types of announcements: *positive*, *i.e.*, business expansions, and *negative*, *i.e.*, bankruptcies/closures, internal restructurings, offshoring/delocalizations, and relocations.[4] In line with our theorizing, we focus on negative shocks; however, for completeness, we also present results for positive shocks and account for them in the estimations.

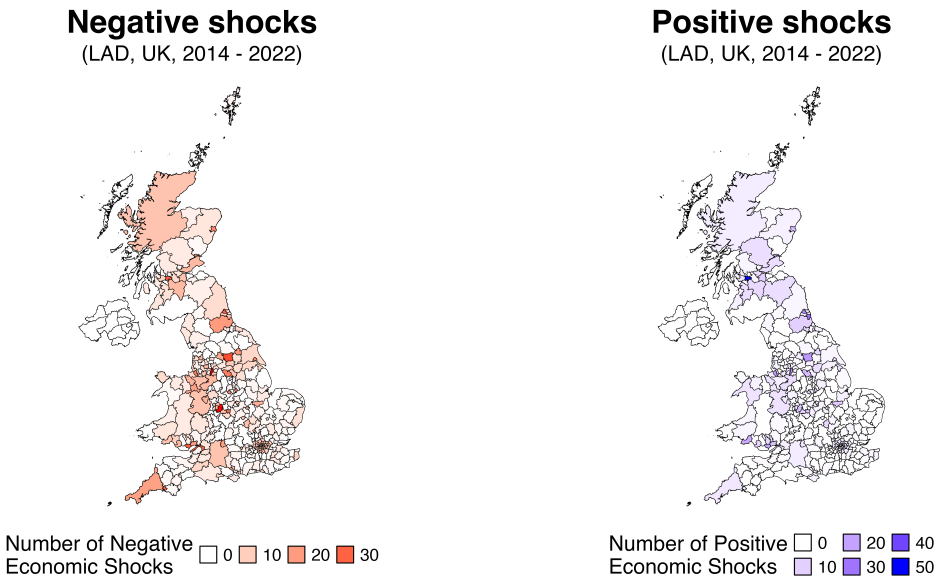
We merge the ERM announcements with individual-level data from the British Election Study (BES), an Internet panel survey with a stratified random probability sample of citizens living in England, Scotland and Wales (Fieldhouse et al., 2022). This survey, which has widely been used in social sciences (see, *e.g.*, Efthyvoulou, Pickard, and Bove, 2024; Pickard, Efthyvoulou, and Bove, 2023), contains questions designed to capture the respondents' political behavior and attitudes on a range of topical issues. Figure 1 shows how 279 (216) of the LADs covered by BES experience at least one negative (positive) ERM announcement over the time frame covered by the survey, whose rollout ranges between February 2014 (wave 1) and June 2022 (wave 24).

[2] See Appendix E for details and full results.

[3] In Appendices C.10 and D.6 we show how results are robust when focusing on events involving, respectively, at least 200 and 300 jobs.

[4] We remove from the sample mergers and acquisitions, as their impact on the local economy is *ex-ante* ambiguous. Following the same rationale, we also drop announcements reporting both *both* jobs being created and destroyed, as well as those that, while classified as negative (positive), involve jobs being created (destroyed). In cases where the announcement involves more than one LAD at a time, we evenly divide the number of reported jobs lost/created across the locations.

Figure 1: Local Authorities in BES impacted by ERM announcements (2014-2022).



Notes: Figures obtained combining data on the Local Authority District of respondents interviewed in Waves 1-24 of the British Election Study (2014-2022) with data from the European Restructuring Monitor (ERM) database (2002-2022).

For our main analysis we focus on two sets of variables: (a) the *feeling thermometer* towards a given party ("How much do you like or dislike each of the following parties?"); and (b) the *probability to vote* for it (e.g., "How likely is it that you would ever vote for Conservatives?"). Both variables are measured on an 11-point scale, with higher values indicating, respectively, a higher proximity with and a higher probability to vote for the party. As per our theorizing, we focus on the support for the populist radical right by combining the respondents' answers on two closely related parties: the UK Independence Party (UKIP) and the Brexit Party.[5] We also capture the impact on the Conservative party, always in power throughout the period under scrutiny, and on the Labour party, representing the mainstream center-left opposition.

To explore the key mechanisms underpinning our main results, we construct several supplementary outcomes from the BES questionnaire asking about their (sociotropic and egotropic) economic evaluations, and attitudes towards immigration. In addition, we and take advantage of the full

[5]We take a similar approach as Dickson et al. (2024). However, we show in Appendices C.13 and D.8 that results are robust when focusing on UKIP only.

range of respondents' socio-demographic attributes being recorded. All the items employed in our analyses are described in detail in Online Supplementary Information (SI) B.

3.2 Methodology

Our analysis operates on two different time frames, which we broadly define as short- and longer-term. We proceed to illustrate the logic and empirical strategy underlying each of them in this section.

3.2.1 Short-term Analysis

In the short-term setting, we exploit the ERM announcements as "natural experiments." Following an 'Unexpected Event during Survey Design' (UESD) approach (Muñoz, Falcó-Gimeno, and Hernández, 2020), we rely on the hypothesis that ERM announcements occurred independently of the BES survey rollout. Under such assumption, we can identify a "control" and a "treatment" group, composed of individuals being interviewed, *resp.*, *right before* and *right after* the announcement of an economic shock. A comparison between the two groups can then reveal the *causal* effect of exposure to a contextual economic shock on individual attitudes. In order to overcome the risk of estimating compound treatments (Muñoz, Falcó-Gimeno, and Hernández, 2020), we exclude from the analysis respondents from LADs affected by more than one shock during the survey rollout (N=23,777). We estimate the following model:

$$y_{i,d,r,w} = \beta_0 + \beta_1 \text{Post}_{i,d,w} \times \text{Neg}_{d,w} + \beta_2 \text{Post}_{i,d,w} + \beta_3 \text{Neg}_{d,w} + \mathbf{X}_{i,d,w} + \lambda_w + \theta_r + \epsilon_{i,d,r,w} \quad (1)$$

where $y_{i,d,r,w}$ is the outcome variable for respondent i from LAD d and region r, in wave w. $Post_{i,d,w}$ is a binary indicator that takes value 1 if the individual was interviewed after an announcement, else 0; $Neg_{d,w}$ is a binary variable that captures whether the shock hitting the LAD is negative

(0 if positive). $\mathbf{X}_{i,d,w}$ is a vector of covariates that includes age, age squared, and dummies for the following: females, White-British ethnicity, whether the individual has children, highest level of education (below GCSE; GCSE/A-level/Diploma; Bachelor's degree or above), employment status (employed; student/other; retired; unemployed/not working), marital status (single; in a relationship; separated/divorced/widowed), and religious affiliation (no religion; Christianity; Islam; other religions). We also include a control for the number of, *resp.*, positive and negative ERM announcements taking place in the respondent's LAD up until the wave preceding the interview. λ_w and θ_r are wave and region FEs, and $\varepsilon_{i,d,w}$ is the error term, clustered at the LAD-by-wave level.

Our UESD employs three separate sets of bandwidths: 7-day, 15-day, and 30-day. This implies that, in each setting, we compare responses 7/15/30 days before and 7/15/30 days after an announcement, while individuals interviewed on the day of the announcement are excluded from the analysis.[6] In the 30-days window, we cover a sample of 138 major announcements overlapping with the fieldwork of a BES wave, taking place between 2014 and 2020, and involving 73 and 57 LADs affected by, *resp.*, a negative or a positive shock.

The adoption of short time windows before and after the announcement raises the credibility of the quasi-randomness of the treatment assignment, minimizing the likelihood of compounding alternative shocks of other events driving the estimated effects (Bove et al., 2022). Focusing on all announcements that received media coverage and involved more than 100 job openings/closures in the LAD of the respondent, it is reasonable to assume individuals in the treatment group were aware of them.[7] At the same time, this sampling choice ensures that the probability of them being personally involved by the announcement is reasonably low, given that the average LAD has a population of 190,000. Focusing on a more granular unit unit of analysis (*e.g.*, MSOA, postcode, etc.), and/or on less salient shocks would likely endanger one of the two conditions. In light of this discussion, we interpret our short-term treatment as a pure informational update about the economic scenario at the local level.

[6] In *SI* C.5, we show that our results are robust to employing a tighter, 3-day bandwidth, although this choice reduces the sample sizes dramatically, leading to noisier estimates.

[7] We provide support for the salience of the shocks under scrutiny via a battery of robustness tests in *SI* C.

The identification of valid causal estimates hinges on two key assumptions: *excludability*, *i.e.*, differences between treatment and control groups are the sole consequence of the economic shock, and *ignorability*, *i.e.*, the timing of the interview is orthogonal to the one of ERM announcements.

The primary threat to excludability is that our treatment effect can be explained by pre-existing time trends. We perform two tests to address this possibility. First, we follow Dinas, Hartman, and Van Spanje (2016) and look at economic perceptions in the pre- and post-announcement wave, sorting respondents by their treated/control status. The informational update coming from the negative announcement should be common across the two groups by the time of the *follow-up* interview. Hence, we should not retrieve any difference across treated and control individuals in their sociotropic, and, possibly, egotropic, economic assessment. The same should hold true for the interview taking place in the wave *preceding* the shock, if neither members of the treatment or the control group were anticipating the event. Results presented in *SI* C.3 reassure us in two ways: first, recovering similar attitudes in the post-announcement wave supports the validity of our empirical design in dealing with differences in unobservables; second, the lack of an effect on lagged economic perceptions suggests that these shocks were, on average, unexpected. In a second test, we directly test for pre-existing trends by considering placebo treatments at an arbitrary time point to the left of the cutoff points (see: *SI* C.4).

The ignorability assumption may be violated if the rollout of BES ends up over- or under-representing individuals with specific characteristics in the treatment/control group. In Table B.4, we conduct balancing tests comparing individuals interviewed before and after the announcements across the observed characteristics included in $\mathbf{X}_{i,d,w}$. A visual inspection reveals a strong balance across the two groups, for most of the covariates. The statistically significant difference in average age and childminding status is small in magnitude. Nevertheless, in SI C.6 we report estimates before and after including the vector of control $\mathbf{X}_{i,d,w}$, to show how results are virtually unaffected by such differences in observables across the two groups. Also, in *SI* C.7, we confirm that the results hold when we reweight the samples using entropy balancing (Hainmueller, 2012), an exercise ensuring that the distribution of covariates among control units matches the moment conditions of

the treated units.[8]

3.2.2 Long-term analysis

To identify the long(er)-term repercussions of exposure to negative economic announcements on individual attitudes, we exploit the staggered timing of the ERM announcements across different LADs. We implement a staggered Difference-In-Differences design following Callaway and Sant'Anna (2021), who apply a doubly robust DID estimator based on stabilized inverse probability weighting and ordinary least squares (Sant'Anna and Zhao, 2020). We look at changes in individual attitudes for respondents whose LAD has experienced an economic shock between two consecutive BES waves — the treatment group —, and compare them to variations registered among respondents from LADs having *not yet* been involved in one event — the control group.[9] In BES, on average, consecutive interviews occur within 126 days of each other: this allows us to get a snapshot of the individual response approximately every 4 months since the announcement became public.

Importantly, in the long-run, we restrict the sample in two ways. First, we keep only those respondents that are part of a (30-days) *control* group in the short-term analysis. This allows us to showcase the evolution of the estimated short-term effect over a longer time frame for an identical set of respondents while, at the same time, avoiding compounding the response to different shocks.[10] Second, we remove from the sample respondents that would belong to the long-term treatment group, but whose LAD has been impacted by *both* negative and positive shocks since the previous wave, again to avoid compounding effects. We estimate the following equation:

[8]This test can be seen as equivalent to controlling for the lagged dependent variable, *i.e.*, as recorded in the previous wave, when it comes to mitigating concerns of omitted variable bias (Bove, Efthyvoulou, and Pickard, 2022).

[9]Arguably, *not yet treated* LADs provide for a better counterfactual than *never treated* ones. Yet, in SI D.2, we show that results are robust when employing the latter in the staggered DID analysis.

[10]Imagine a *treated* respondent in the short-run (*i.e.*, interviewed right after an ERM announcement involving their LAD) which also enters the long-run sample as a *control* unit (*i.e.*, interviewed in a wave prior to an ERM announcement involving their LAD). This unit would provide an undesirable counterfactual in the long-run, having just been impacted by an announcement.

$$y_{i,d,w} = \delta_0 + \delta_1 \text{Shock}_{d,w} + \gamma_i + \eta_w + u_{i,d,w} \tag{2}$$

where $y_{i,d,w}$ is the outcome for respondent i from LAD d, in wave w. $Shock_{d,w}$ is an indicator equal to 1 if i's LAD is affected by a shock between wave w and the previous one (w-1), while taking value 0 if the respondent comes from a LAD that has *not yet* been impacted by one, at the time of the interview. As a result, both respondents that have been already treated or are never affected by a shock are removed from the sample. Importantly, our treatment variable captures the *first* recorded ERM event occurring in the respondent's LAD between two consecutive waves over the period covered by BES (2014–2022). γ_i and η_w are respondent and wave fixed effects. We employ the default robust and asymptotic standard errors, obtained using Influence Functions, as proposed by Callaway and Sant'Anna (2021).

As for our short-term analysis, let us briefly discuss the assumptions that must hold in order to interpret the coefficients estimated via this identification strategy as reflecting causal links. First, parallel trends must hold: in the absence of a shock, the attitudes of respondents from LADs having just experienced one and of those from LADs not having *yet* done so would have evolved similarly. Finding that the estimated treatment effect is not statistically significant in periods preceding the actual shock would reassure us about the lack of diverging pre-trends across the treatment and control groups. For this reason, we always report the average treatment effect computed in each wave preceding the shock (see, *e.g.*, Figure 3). Second, we expect no sorting in the treatment assignment. In other words, we assume that respondents do not move away from or towards a treated LAD in anticipation of a forthcoming economic ERM announcement. In order to address this concern, in SI D.1, we show that our estimates are robust to removing from the sample those respondents having moved to a different LAD since the previous BES wave, as well as when restricting our analysis to the subset of respondents involved in the short-term analysis *and* reporting living in only one LAD whenever interviewed, *i.e.*, never-movers.

4 Empirical Results

We start by looking at what happens to perceptions towards and voting intentions for political parties in the immediate aftermath of an economic announcement affecting a sizable number of jobs in a Local Authority District (Figure 2). So, what happens to support for populist radical right forces, specifically UKIP and its successor, the Brexit Party? Results in Figure 2 show that, immediately after a negative economic shock is announced, voters tend to shift *away from* UKIP and Brexit Party. Their appreciation for the party declines by about 2-3 p.p. Importantly, this effect translates into a lower probability of voting for the party (between -3 and -5 p.p.).[11]

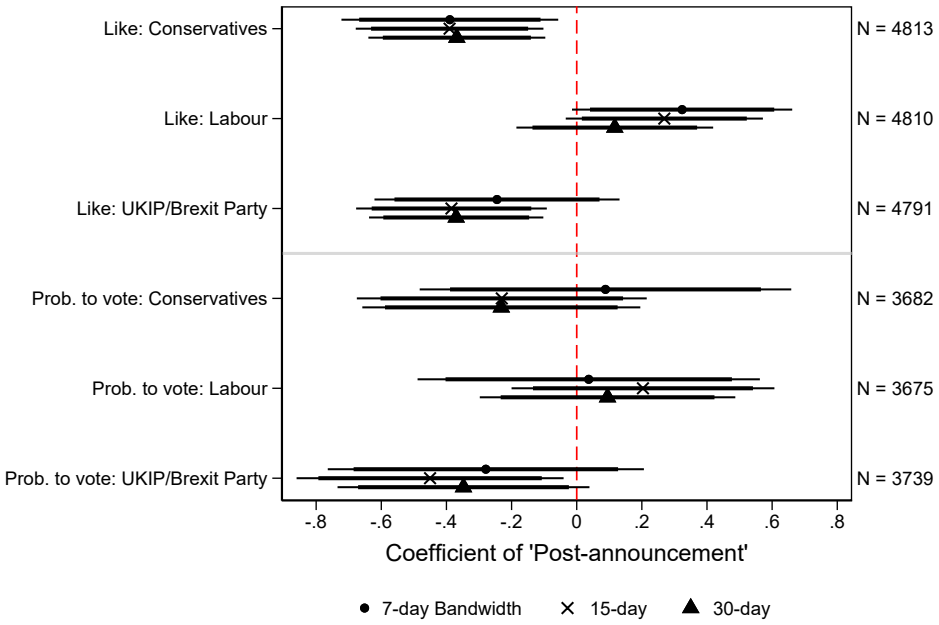
Additionally, experiencing a negative shock decreases support for the incumbent Conservative party, by about 4 percentage points (p.p.), irrespective of the time window under scrutiny (7, 15, or 30 days around the announcement). Yet, the decline in appreciation for the Conservatives does not seem to have immediate repercussions on respondents' probability of voting for the party. Moving to the mainstream opposition, represented by Labour, following the announcement of forthcoming job closures in their district, respondents' view of the party improves by about 3 p.p., yet such boost dissipates shortly, *i.e.*, within 30 days. Again, these shifts in appreciation for Labour among citizens do not translate into changes in its electoral appeal.

Our results show that voters immediately integrate these negative events into their voting calculations, but only for political forces for whom the economy–the primary dimension of political conflict following such an event, we argue–is not a strong point: the populists. As the economy becomes salient due to a restructuring event involving significant job losses, voters navigate between mainstream parties and show a natural propensity for stability–something a populist party, given its low emphasis on the economy and thus reduced perceived competence, cannot offer. These findings are consistent with the *normal politics* logic we put forward.

Our longer-term analysis allows us to compare respondents whose LAD was hit by a shock

[11] The impact is particularly potent for offshoring/delocalization events, as their occurrence decreases support for UKIP/Brexit by 5-6 p.p. (Table A.6, models 10-12).

Figure 2: Negative shocks, political attitudes and voting intentions (short-term).

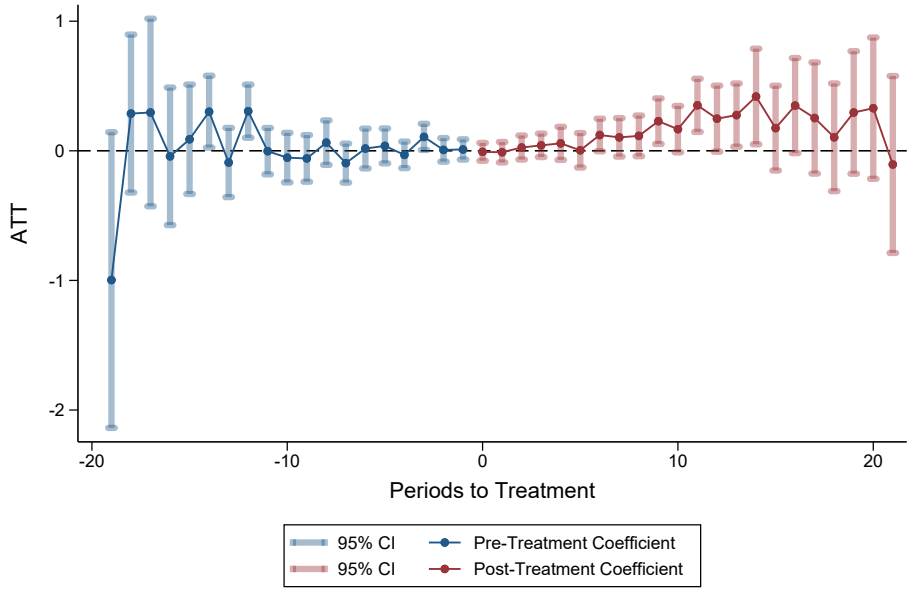


Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the corresponding bandwidth. Full results in Tables A.1-A.6.

between two consecutive BES waves to those who were not *yet* affected, highlighting the longer-term dynamics of citizens' responses among the same set of individuals identified in the short-term exercise. Results in Figure 3 display a substantial and stable increase in the probability to vote for UKIP (or for the Brexit Party), becoming significant only six waves after the original shock took place, *i.e.*, approximately two years later. Specifically, the likelihood of voting for populists increases by 1 percentage point after 6 waves, to 2 percentage points after 9 waves, and to 4 percentage points after 11 waves. As shown in Table A.14 in *SI* A.2, we do not retrieve a comparable effect neither on the Conservatives nor on the Labour party.

Comparing the results of our short- and long-term analyses reveals something about the dynamics through which the political response to economic shocks unfolds. In the immediate aftermath of a shock, support for populist parties declines. Over time, however, voters are progressively drawn back to them, and the populists regain the electoral ground lost shortly after the restructuring event. The question is, then: what could explain the evolution of voters from *normal* to *backlash*

Figure 3: Negative shocks and Probability to Vote UKIP/Brexit Party.



Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. The lines signify the 95% confidence interval. Full regressions reported in Table A.14.

politics? In the following section, we propose and empirically test several plausible explanations, after presenting the robustness tests conducted to confirm the results obtained via each empirical strategy.

4.1 Robustness Tests

We probe the robustness of the short-term results in a number of ways, detailed in *Online Supplementary Information (SI) C*. In sum, we conduct tests to address the possibility of mis-specification error (SI C.2); look at whether the informational update for the control group takes place in the wave following the announcement (SI C.3); test for pre-existing trends to the left of the cut-off points (SI C.4); consider a narrower, 3-day bandwidth (SI C.5); rule out the possibility that results are driven by the inclusion of socio-economic controls (SI C.6); use entropy weighting to optimize covariate balance between treatment and control units (SI C.7); re-estimate our baseline models removing all individuals who reside in each UK region (SI C.8); check the robustness to alternative clustering of

standard errors (SI C.9); focus on more salient shocks, *i.e.*, involving 200 or 300 job losses/creations (SI C.10) and on shocks involving the manufacturing sector (SI C.11).[12] Taken together, the results lend credibility to our causal claims and provide strong support to short-term findings. Also, we do not retrieve any heterogeneity by age, gender and education in the short-term response to negative announcements (SI C.12). Finally, in SI C.13, we show that estimated effect on support for UKIP and the Brexit Party is robust when focusing only on the former.

A second set of tests is devoted to confirming the robustness of the long-term results. First, results are robust when focusing on respondents that have not moved LAD since the previous wave or never at all (SI D.1), and when the control group is composed of respondents from never treated, rather than not yet treated, LADs (SI D.2). Second, as already mentioned, our treatment variable captures the effect of the *first* recorded shock in the respondent's LAD. One potential issue is that, if the respondent's LAD is affected by further ERM announcements occurring after the first "treatment", lagged estimates may compound the reaction to the original shock and to more recent ones. To address this concern, in SI D.3, we re-estimate our coefficients, restricting the sample to those respondents becoming "treated" at the time of the most recent ERM announcement involving their LAD. In spite of the reduced sample size, estimates are consistent in sign and magnitude with those presented in Figure 3. Third, in SI D.4 we find comparable results when re-estimating our coefficients using the improved doubly robust DID estimator based on inverse probability of tilting and weighted least squares (Sant'Anna and Zhao, 2020), and via an inverse probability weighting DiD estimator with stabilized weights (see Callaway and Sant'Anna, 2021). Fourth, we retrieve consistent — though somewhat weaker — estimates effects looking at the feeling thermometer towards UKIP/Brexit Party, rather than at the probability to vote for such parties (Table A.14, Column 2). Also, we do not find any effect on proximity towards the Conservatives and Labour (Table A.14, Columns 3-6). Fifth, we show how the salience of other issues, besides immigration, does not increase as a result of the exposure to a negative shock (SI D.5). As for the short-term

[12]The ERM dataset contains a variable that allows us to identify the sector name. In our dataset, 32% of the shocks (1301 out of a total of 4039 events) are in manufacturing. Of these, 77% (997 events) are negative shocks. See Appendix C.11 for details.

analysis, we verify that our estimates are robust when we focus on a subset or more salient shocks (SI D.6), when we restrict our sample to ERM announcements involving the manufacturing sector (SI D.7), and when coding our outcome variable as voting intentions for UKIP only (SI D.8).

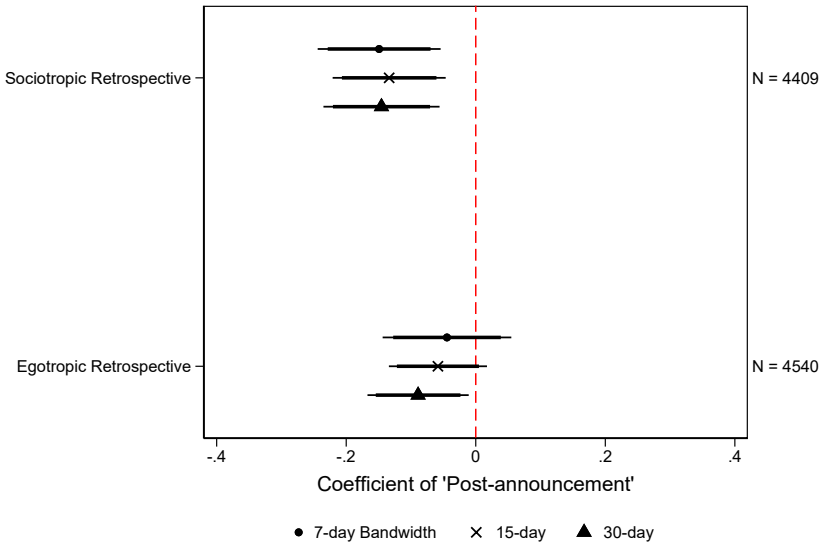
5 Mechanisms

Our argument posits that the link between economic shocks and increased support for populist parties follows a temporal dynamic, as the former are progressively "captured" by political entrepreneurs or become subject to media spin. While there is ample long-term opportunity for political entrepreneurs to frame the issue according to narratives that serve their electoral interests, in the short term, this potential is significantly constrained, as an economic shock automatically directs public attention to the state of the economy. In this section, we explore two channels — economic and immigration-related — that may explain such differences between the short- and the longer-term. They directly align with our theorizing.

First, we aim to verify whether the economic announcements captured by the ERM reflect into an actual worsening of the respondents' sociotropic and/or egotropic economic perceptions, and, if so, on the persistence of such effect. In Figure 4, we provide evidence that, in the short-term, negative shocks worsen the respondents' perceptions about the country's economic scenario, but not about their personal financial situation. This shows that ERM announcements, in their immediate aftermath, are perceived as events affecting the local community, rather than an instantaneous threat to one's personal well-being (Colantone and Stanig, 2018a; Colantone and Stanig, 2018b, find comparable results). However, the impact of ERM announcements on sociotropic economic perceptions fades quickly over time. As shown in Figure 5, within a maximum of four months after the shock, BES respondents do not report any decrease in their retrospective evaluation of the economy.

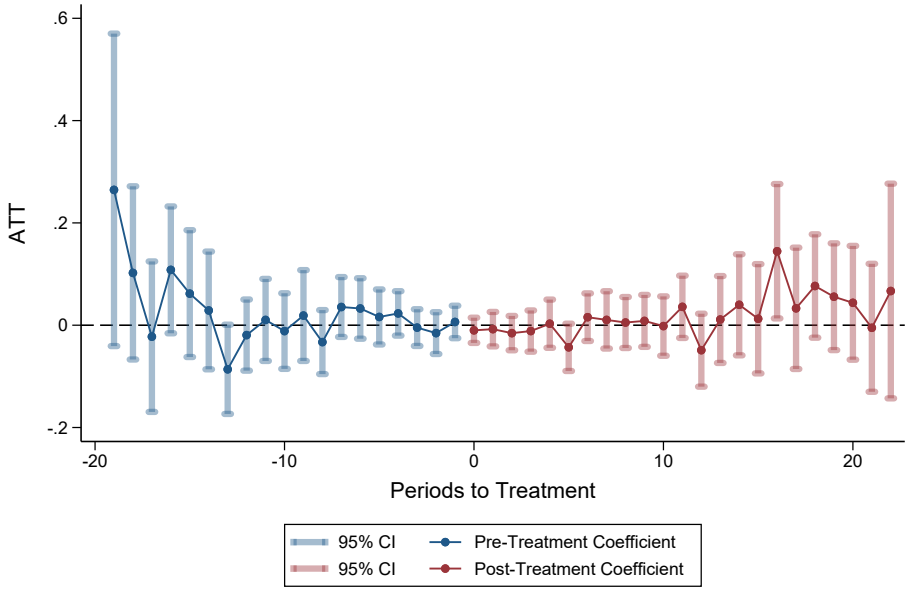
Next, we turn to immigration as a potential catalyst for attention from PRR parties, with UKIP (and the Brexit Party) exemplifying this stance in the UK over the studied period. If UKIP can

Figure 4: Economic shocks, sociotropic and egotropic economic response.



Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. The focus is on negative shocks. Standard errors are clustered at the LAD-by-wave level. Standard errors are clustered at the LAD level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the corresponding bandwidth. Full results in Tables A.7-A.8.

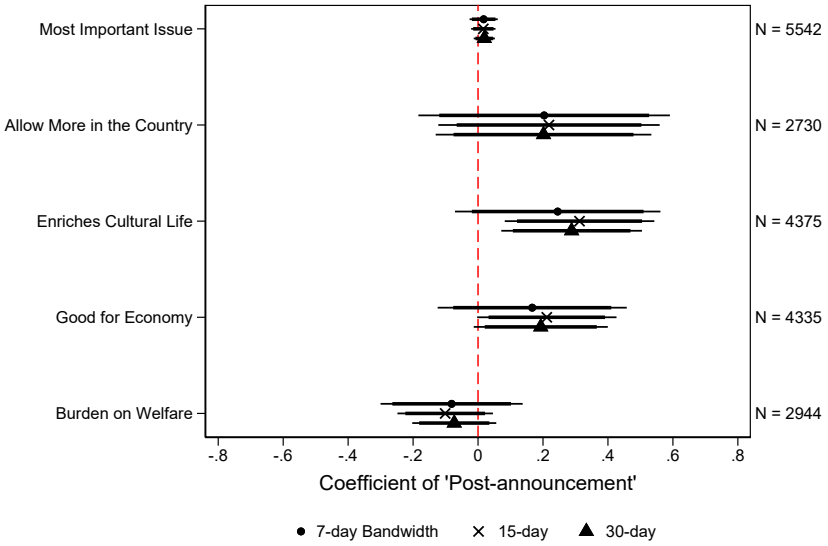
Figure 5: Negative shocks and sociotropic economic evaluations.



Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. The lines signify the 95% confidence interval. Full regressions reported in Table A.16.

convert economic vulnerability into an electoral opportunity, this should manifest in a change in voters' perceptions of immigration. Their ownership of the immigration issue should offset the electoral disadvantage brought about by the shock in the short term. We provide two pieces of suggestive evidence to bolster our claim, looking at immigration-related questions in BES. In the short-term, we find no evidence of an immigration backlash (Figure 6): if anything, respondents are actually more likely to perceive immigration as beneficial for the culture of their country in the aftermath of a negative shock. Over the longer-term, the opposite scenario unfolds (Figure 7): immigration becomes more salient (7a), and voters are more inclined to believe that immigration negatively affects the economy (7b).

Figure 6: Economic shocks and attitudes towards immigration.

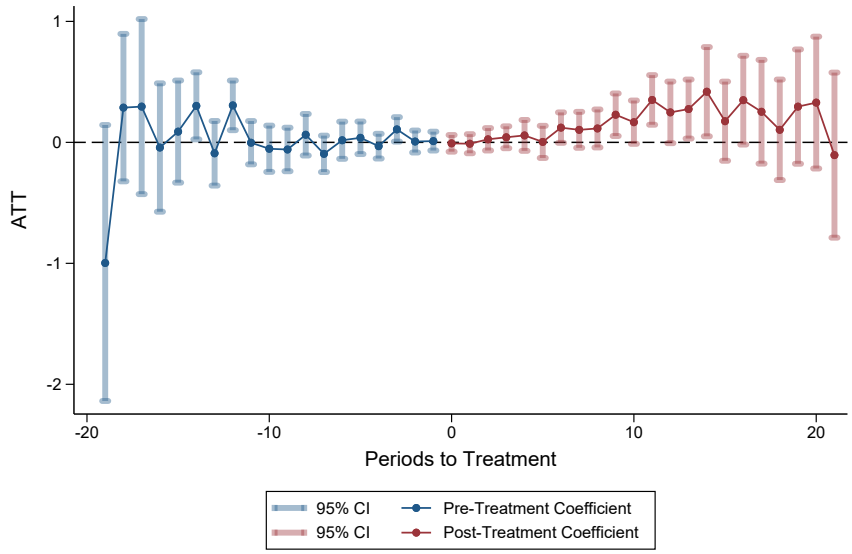


Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. The focus is on negative shocks. Standard errors are clustered at the LAD-by-wave level. Standard errors are clustered at the LAD level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the corresponding bandwidth. Full results in Tables A.9-A.13.

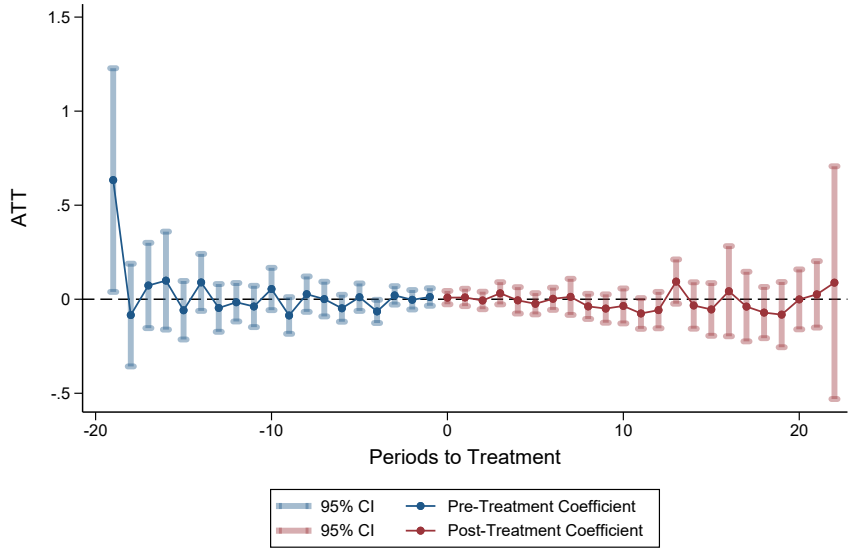
Taken together, our findings suggest that, in the short-term, *economic* shocks are perceived primarily as *economic* events. As a result, at least initially, PRR parties, lacking experience in power and credibility over the first dimension of political conflict – the economy – appear to struggle. In the longer-term, however, these shocks progressively lose their initial connotation, become mediatized and politicized, with non-economic concerns — namely, immigration — entering

Figure 7: Negative shocks and attitudes towards immigration (long-term).

(a) **Most Important Issue: Immigration**



(b) **Immigration: Good for Economy**



Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. The lines signify the 95% confidence interval. Full regressions reported in Table A.15.

voters' considerations. PRR parties then have the opportunity to enter this fray. This scenario underscores the challenge of disentangling the economic from the cultural roots of populism (Guriev and Papaioannou, 2022).

5.1 Alternative Explanations

In SI C.1, we provide further evidence for the difference between short- and long-term results, and rule out alternative explanations.

First, we show that respondents' evaluation of the consequences of Brexit, if interviewed *right after* a negative ERM announcement, worsen significantly on a number of dimensions (Figure C.1).[13] At the same time, these individuals become less nationalist and attached to their English identity (C.2). Both results are consistent with the short-term decline in support for UKIP.

We do not observe any significant change in the most important issue identified by respondents: if anything, the salience of terrorism decreases following the announcement of a negative economic shock (Figure C.3). Also, we show that it is only the respondents' perception about the evolution of the national economy — and not of unrelated matters like inflation, crime levels, and immigration — that worsens in the aftermath of a negative announcement (Figure C.4).

Sticky attributes like the respondents' ideological stance and their position on the authoritarian-libertarian scale do not shift due to receiving information on a forthcoming contextual economic shock. Yet citizens interviewed *just after* a negative announcement is published in the media tend to be more favorable towards redistribution compared to those interviewed *just before* (Figure C.5). We find some evidence of an intensification in the frequency of political discussion in Figure C.6.

We find no effect on affective polarization, measured as the distance in sympathy between each couplet of parties between Conservatives, Labour, and UKIP (Figure C.7). Consistently with the decline in sympathy towards the Conservative party — in charge for most of the time frame under

[13] In this setting, we only keep interviews taking place after the Brexit referendum, *i.e.*, after the 24th of June 2016. This increases the comparability of the responses, as interviews prior to the referendum may be influenced by the uncertainty on the outcome of the (forthcoming) referendum, be it in the form of strategic concerns or desirability bias.

scrutiny — negative announcements reduce support for the UK Government (Figure C.8). Yet, in the same figure, we retrieve no effect on support for the PM and for Members of Parlament (MPs), no significant change in satisfaction with democracy and in a battery of items proxying broadly-defined "populist" views. Finally, we can relate the improved attitudes toward immigration to decreased support for the Conservative party. Respondents interviewed after a negative economic announcement, when asked about whether the Conservative party will allow many fewer/more immigrants in the country, and whether it will succeed in reducing immigration, are less positive on both accounts. We do not retrieve a comparable effect for Labour and UKIP (Figure C.9).

We finally verify the claim in the literature that support for populist parties crucially hinges on a *shared* sense of insecurity and on broadly defined *negative* feelings, like anger (Hochschild, 2018) or nostalgia for an idealized past (Kurer, 2020). If this is the case, we should not find any effect on support for UKIP/Brexit of contextual *positive* shocks, and of *individual* ones. In SI D.9, we show that BES respondents exposed to business expansions, which do not activate any "blaming" dynamic among the electorate, do not turn to the populist right in the longer run.[14] In a similar fashion, populist parties should find it more difficult to build common narratives around personal, economic hits (*i.e.*, becoming unemployed), especially if this is due to haphazard circumstances, rather than systemic shocks. In SI D.10, we estimate the effect of becoming unemployed across two consecutive BES waves, and find no effect on our outcomes of interest, despite a significant, negative impact on retrospective egotropic evaluations in the immediate aftermath of the shock.

6 Conclusions

Though economic shocks are a regular occurrence in people's lives, their impact on political attitudes and voting behavior remains poorly understood. This gap is not due to a lack of effort; scholars have dedicated substantial attention to this issue, using sophisticated research designs to examine the causal effects of contextual economic shocks. Yet, the literature still faces a central challenge: the lack

[14] In Table C.2 of SI C.2 we show this is also not the case in the short-run, analyzing the effect of positive announcements on the probability to vote for UKIP or for the Brexit Party.

of theoretical frameworks addressing how these effects evolve over time. While it is acknowledged that impacts may vary, this variation is not adequately addressed at theoretical or empirical levels. Current work on contextual economic shocks often reveals a significant gap between when the shock is measured and when the political outcome is recorded, raising concerns about what the identified estimates actually capture. As these shocks likely provoke diverse reactions from politicians and the media, it is reasonable to expect that their effects will vary over time–and that the pure effect of a shock can only be observed immediately following the event.

In this paper, we argue that timing is crucial to understanding the political and electoral consequences of economic shocks. We demonstrate that the political effects of these events vary significantly in the short term versus the long term. Immediately following a negative shock in their community, voters lean toward *normal politics*: they reject populists, opting instead for political stability and mainstream solutions to address economic challenges. Over time, however, as economic shocks become politicized and reshape community perspectives, they create an opening for populists, who gain electoral traction by linking these events to issues they dominate–particularly immigration.

What explains this shift? Our findings highlight the key role of immigration. In the short term, we observe a sociotropic economic reaction, with people becoming more concerned about the national economy following a restructuring event. Yet this effect fades over time, while anti-immigration sentiments intensify. Initially, there is no evidence of heightened anti-immigration attitudes; in fact, public sentiment toward immigration is slightly more favorable immediately after a shock. However, as populist political entrepreneurs capitalize on immigration concerns, they work to shift the main axis of political competition away from economic issues, even in the wake of economic shocks–thereby driving *backlash politics*.

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Online Supplementary Information (SI)

The Political Impact of Destruction Announcements: Evidence from the United Kingdom

A Full Regression Tables

A.1 Short-Term Analysis

Table A.1: Short-term impact of shocks: Like/dislike - Conservative Party.

	Like/dislike: Conservative Party											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	-0.389**	-0.388***	-0.365***									
	(0.168)	(0.145)	(0.137)									
Post x Bankruptcy/Closure				-0.357**	-0.395**	-0.259						
				(0.165)	(0.164)	(0.163)						
Post x Restructuring							-0.068	-0.014	-0.159			
							(0.231)	(0.186)	(0.171)			
Post x Offshore/Deloc.										-0.012	0.017	0.031
										(0.260)	(0.275)	(0.293)
Negative Shock	0.142	0.122	0.203**									
	(0.136)	(0.113)	(0.102)									
Bankruptcy/Closure				0.258	0.265**	0.295**						
				(0.160)	(0.129)	(0.127)						
Restructuring							-0.019	-0.044	0.077			
							(0.234)	(0.166)	(0.152)			
Offshore/Deloc.										-0.368	-0.468	-0.488
										(0.286)	(0.299)	(0.303)
Post	0.333**	0.292**	0.278**	0.199*	0.167*	0.134	0.124	0.068	0.109	0.119	0.084	0.087
	(0.130)	(0.114)	(0.107)	(0.108)	(0.094)	(0.092)	(0.108)	(0.092)	(0.085)	(0.100)	(0.086)	(0.082)
Constant	3.777***	4.199***	4.088***	3.781***	4.184***	4.123***	3.893***	4.301***	4.215***	3.911***	4.302***	4.249***
	(0.554)	(0.472)	(0.418)	(0.553)	(0.472)	(0.419)	(0.542)	(0.460)	(0.407)	(0.537)	(0.458)	(0.405)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	96	98	98	96	98	98	96	98	98	96	98	98
R-squared	0.133	0.127	0.125	0.133	0.127	0.125	0.132	0.126	0.125	0.133	0.127	0.125
Observations	4813	7263	8429	4813	7263	8429	4813	7263	8429	4813	7263	8429

Notes. $^{*}p<0.10$,$^{**}p<0.05$,$^{***}p<0.01$. We report here the estimates of the effect of being interviewed in the 7/15/30 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15/30 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.2: Short-term impact of shocks: Probability to vote - Conservative Party.

	Probability to vote: Conservative Party											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	0.090	-0.224	-0.225									
	(0.288)	(0.225)	(0.216)									
Post x Bankruptcy/Closure				0.023	-0.143	-0.131						
				(0.285)	(0.242)	(0.226)						
Post x Restructuring							-0.048	-0.212	-0.251			
							(0.386)	(0.351)	(0.324)			
Post x Offshore/Deloc.										0.367	0.267	0.321
										(0.303)	(0.299)	(0.307)
Negative Shock	0.045	0.220	0.191									
	(0.196)	(0.143)	(0.132)									
Bankruptcy/Closure				0.206	0.217	0.263						
				(0.231)	(0.173)	(0.170)						
Restructuring							-0.084	0.213	0.101			
							(0.304)	(0.214)	(0.187)			
Offshore/Deloc.										-0.324	-0.335	-0.477
										(0.327)	(0.342)	(0.369)
Post	-0.155	0.045	0.005	-0.116	-0.047	-0.096	-0.093	-0.028	-0.071	-0.130	-0.090	-0.129
	(0.236)	(0.177)	(0.175)	(0.174)	(0.148)	(0.147)	(0.149)	(0.129)	(0.123)	(0.150)	(0.132)	(0.125)
Constant	3.837***	3.434***	3.642***	3.759***	3.498***	3.657***	3.888***	3.555***	3.762***	3.887***	3.624***	3.803***
	(0.809)	(0.681)	(0.660)	(0.815)	(0.684)	(0.660)	(0.788)	(0.671)	(0.646)	(0.790)	(0.670)	(0.644)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	96	98	98	96	98	98	96	98	98	96	98	98
R-squared	0.130	0.119	0.116	0.130	0.119	0.116	0.130	0.119	0.116	0.130	0.119	0.116
Observations	3682	5794	6580	3682	5794	6580	3682	5794	6580	3682	5794	6580

Notes. $^{*}p<0.10$,$^{**}p<0.05$,$^{***}p<0.01$. We report here the estimates of the effect of being interviewed in the 7/15/30 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15/30 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.3: Short-term impact of shocks: Like/dislike - Labour Party.

	Like/dislike: Labour Party											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	0.321*	0.263*	0.109									
	(0.170)	(0.153)	(0.153)									
Post x Bankruptcy/Closure				0.099	0.085	0.086						
				(0.225)	(0.210)	(0.197)						
Post x Restructuring							0.265	0.245	0.033			
							(0.200)	(0.170)	(0.164)			
Post x Offshore/Deloc.										0.306	0.265	0.284
										(0.399)	(0.409)	(0.414)
Negative Shock	-0.343**	-0.235**	-0.193*									
	(0.133)	(0.113)	(0.110)									
Bankruptcy/Closure				-0.186	-0.137	-0.190						
				(0.145)	(0.128)	(0.123)						
Restructuring							-0.190	-0.130	-0.048			
							(0.194)	(0.157)	(0.155)			
Offshore/Deloc.										-0.538	-0.427	-0.399
										(0.390)	(0.361)	(0.370)
Post	-0.218*	-0.199*	-0.106	-0.064	-0.071	-0.068	-0.104	-0.112	-0.059	-0.052	-0.058	-0.059
	(0.118)	(0.113)	(0.114)	(0.103)	(0.093)	(0.092)	(0.111)	(0.104)	(0.099)	(0.097)	(0.088)	(0.086)
Constant	5.487***	5.291***	5.138***	5.316***	5.175***	5.076***	5.286***	5.157***	5.013***	5.259***	5.126***	5.008***
	(0.555)	(0.525)	(0.468)	(0.556)	(0.526)	(0.466)	(0.548)	(0.516)	(0.460)	(0.540)	(0.512)	(0.452)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	96	98	98	96	98	98	96	98	98	96	98	98
R-squared	0.112	0.114	0.111	0.111	0.114	0.111	0.111	0.114	0.111	0.112	0.114	0.111
Observations	4810	7259	8427	4810	7259	8427	4810	7259	8427	4810	7259	8427

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7/15/30 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15/30 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.4: Short-term impact of shocks: Probability to vote - Labour Party.

	Probability to vote: Labour Party											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	0.031	0.190	0.077									
	(0.264)	(0.203)	(0.197)									
Post x Bankruptcy/Closure				0.000	0.199	0.022						
				(0.311)	(0.237)	(0.194)						
Post x Restructuring							0.088	0.078	0.081			
							(0.346)	(0.285)	(0.281)			
Post x Offshore/Deloc.										0.185	0.260	0.339
										(0.457)	(0.511)	(0.518)
Negative Shock	-0.131	-0.147	-0.172									
	(0.198)	(0.143)	(0.144)									
Bankruptcy/Closure				-0.290	-0.127	-0.204						
				(0.192)	(0.147)	(0.157)						
Restructuring							0.613*	0.115	0.086			
							(0.334)	(0.205)	(0.221)			
Offshore/Deloc.										-0.603	-0.575	-0.524
										(0.376)	(0.408)	(0.413)
Post	0.070	-0.020	0.004	0.097	0.029	0.050	0.089	0.080	0.033	0.082	0.083	0.028
	(0.195)	(0.148)	(0.156)	(0.165)	(0.136)	(0.140)	(0.150)	(0.124)	(0.116)	(0.146)	(0.115)	(0.107)
Constant	6.147***	5.906***	5.872***	6.201***	5.845***	5.845***	5.917***	5.759***	5.714***	6.064***	5.797***	5.743***
	(0.774)	(0.596)	(0.554)	(0.757)	(0.587)	(0.555)	(0.752)	(0.603)	(0.563)	(0.737)	(0.590)	(0.556)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	96	98	98	96	98	98	96	98	98	96	98	98
R-squared	0.123	0.117	0.107	0.124	0.117	0.107	0.124	0.117	0.107	0.124	0.117	0.107
Observations	3675	5787	6575	3675	5787	6575	3675	5787	6575	3675	5787	6575

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7/15/30 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15/30 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.5: Short-term impact of shocks: Like/dislike - UKIP/Brexit Party.

	Like/dislike: UKIP/Brexit Party											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	-0.190	-0.329**	-0.319**									
	(0.168)	(0.137)	(0.131)									
Post x Bankruptcy/Closure				-0.070	-0.373**	-0.302**						
				(0.172)	(0.151)	(0.134)						
Post x Restructuring							0.057	0.061	-0.031			
							(0.220)	(0.168)	(0.178)			
Post x Offshore/Deloc.										-0.487**	-0.376**	-0.367**
										(0.196)	(0.149)	(0.148)
Negative Shock	0.002	0.040	0.143									
	(0.144)	(0.114)	(0.105)									
Bankruptcy/Closure				0.075	0.177	0.206*						
				(0.143)	(0.113)	(0.115)						
Restructuring							-0.136	-0.271*	-0.069			
							(0.178)	(0.155)	(0.148)			
Offshore/Deloc.										0.051	0.270	0.268
										(0.282)	(0.282)	(0.287)
Post	-0.029	0.147	0.138	-0.118	0.061	0.042	-0.151	-0.066	-0.036	-0.088	-0.016	-0.016
	(0.129)	(0.110)	(0.099)	(0.101)	(0.088)	(0.082)	(0.098)	(0.089)	(0.079)	(0.091)	(0.084)	(0.078)
Constant	1.116**	1.383***	1.381***	1.089**	1.342***	1.408***	1.153**	1.485***	1.506***	1.114**	1.395***	1.474***
	(0.501)	(0.426)	(0.378)	(0.487)	(0.409)	(0.370)	(0.479)	(0.414)	(0.371)	(0.478)	(0.403)	(0.365)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	88	90	90	88	90	90	88	90	90	88	90	90
R-squared	0.129	0.134	0.131	0.128	0.133	0.131	0.128	0.133	0.130	0.129	0.133	0.130
Observations	4383	6767	7929	4383	6767	7929	4383	6767	7929	4383	6767	7929

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7/15/30 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15/30 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.6: Short-term impact of shocks: Probability to vote - UKIP/Brexit Party.

	Probability to vote: UKIP/Brexit Party											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	-0.303	-0.462**	-0.350*									
	(0.226)	(0.199)	(0.190)									
Post x Bankruptcy/Closure				0.149	-0.142	0.021						
				(0.275)	(0.234)	(0.241)						
Post x Restructuring							-0.330	-0.314	-0.340			
							(0.241)	(0.225)	(0.220)			
Post x Offshore/Deloc.										-0.590**	-0.531**	-0.524**
										(0.248)	(0.224)	(0.227)
Negative Shock	0.040	0.242*	0.244**									
	(0.163)	(0.138)	(0.122)									
Bankruptcy/Closure				0.017	0.150	0.176						
				(0.171)	(0.159)	(0.155)						
Restructuring							-0.010	0.154	0.185			
							(0.219)	(0.182)	(0.152)			
Offshore/Deloc.										-0.017	0.242	0.101
										(0.265)	(0.295)	(0.284)
Post	-0.049	0.132	0.044	-0.273*	-0.090	-0.168	-0.178	-0.074	-0.089	-0.152	-0.077	-0.093
	(0.178)	(0.159)	(0.139)	(0.142)	(0.130)	(0.120)	(0.138)	(0.125)	(0.118)	(0.128)	(0.116)	(0.109)
Constant	1.081*	1.158**	1.200**	1.093*	1.267***	1.289***	1.101*	1.303***	1.340***	1.099*	1.331***	1.375***
	(0.594)	(0.484)	(0.467)	(0.581)	(0.473)	(0.470)	(0.590)	(0.477)	(0.459)	(0.580)	(0.467)	(0.457)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	92	94	94	92	94	94	92	94	94	92	94	94
R-squared	0.128	0.131	0.128	0.127	0.130	0.128	0.127	0.130	0.128	0.128	0.130	0.128
Observations	3605	5724	6513	3605	5724	6513	3605	5724	6513	3605	5724	6513

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7/15/30 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15/30 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.7: Short-term impact of shocks: retrospective sociotropic.

	General economic retrospective evaluation: country											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	-0.149***	-0.134***	-0.145***									
	(0.048)	(0.044)	(0.045)									
Post x Bankruptcy/Closure				-0.099**	-0.092**	-0.072						
				(0.048)	(0.044)	(0.046)						
Post x Restructuring							-0.143**	-0.116*	-0.155***			
							(0.060)	(0.066)	(0.057)			
Post x Offshore/Deloc.										0.032	0.059	0.059
										(0.052)	(0.049)	(0.048)
Negative Shock	0.004	-0.008	0.013									
	(0.041)	(0.034)	(0.035)									
Bankruptcy/Closure				-0.021	-0.034	-0.017						
				(0.044)	(0.037)	(0.039)						
Restructuring							0.023	0.034	0.044			
							(0.064)	(0.046)	(0.043)			
Offshore/Deloc.										-0.063	-0.112	-0.122
										(0.075)	(0.076)	(0.075)
Post	0.081**	0.038	0.044	0.027	-0.006	-0.011	0.020	-0.018	-0.010	-0.004	-0.038	-0.035
	(0.038)	(0.036)	(0.036)	(0.031)	(0.031)	(0.031)	(0.029)	(0.026)	(0.025)	(0.029)	(0.027)	(0.026)
Constant	3.025***	3.197***	3.220***	3.044***	3.209***	3.244***	3.022***	3.174***	3.214***	3.032***	3.187***	3.234***
	(0.186)	(0.177)	(0.156)	(0.186)	(0.178)	(0.155)	(0.188)	(0.178)	(0.155)	(0.186)	(0.178)	(0.154)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	80	82	82	80	82	82	80	82	82	80	82	82
R-squared	0.146	0.153	0.144	0.145	0.153	0.144	0.145	0.152	0.144	0.144	0.152	0.143
Observations	4409	6941	8111	4409	6941	8111	4409	6941	8111	4409	6941	8111

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7/15 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.8: Short-term impact of shocks: retrospective egotropic.

	Personal economic retrospective evaluation: household											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	-0.044	-0.058	-0.089**									
	(0.050)	(0.038)	(0.039)									
Post x Bankruptcy/Closure				-0.041	-0.018	-0.034						
				(0.053)	(0.040)	(0.040)						
Post x Restructuring							0.002	-0.037	-0.061			
							(0.066)	(0.047)	(0.047)			
Post x Offshore/Deloc.										0.000	0.013	0.002
										(0.047)	(0.041)	(0.041)
Negative Shock	-0.001	0.006	0.043									
	(0.035)	(0.026)	(0.029)									
Bankruptcy/Closure				-0.000	-0.021	0.006						
				(0.032)	(0.026)	(0.028)						
Restructuring							0.027	0.063*	0.080**			
							(0.049)	(0.033)	(0.035)			
Offshore/Deloc.										-0.080	-0.111	-0.094
										(0.072)	(0.072)	(0.069)
Post	-0.013	-0.020	0.007	-0.026	-0.045**	-0.029	-0.037	-0.043**	-0.024	-0.035	-0.049**	-0.036*
	(0.038)	(0.031)	(0.033)	(0.027)	(0.023)	(0.023)	(0.025)	(0.021)	(0.021)	(0.026)	(0.021)	(0.021)
Constant	3.514***	3.592***	3.552***	3.514***	3.608***	3.587***	3.507***	3.574***	3.561***	3.515***	3.595***	3.588***
	(0.184)	(0.149)	(0.133)	(0.183)	(0.148)	(0.132)	(0.185)	(0.149)	(0.131)	(0.182)	(0.147)	(0.131)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	80	82	82	80	82	82	80	82	82	80	82	82
R-squared	0.061	0.058	0.060	0.061	0.058	0.060	0.061	0.058	0.060	0.061	0.058	0.060
Observations	4540	7135	8315	4540	7135	8315	4540	7135	8315	4540	7135	8315

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7/15/30 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15/30 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.9: Short-term impact of shocks: immigration most important issue.

	Most Important Issue: Immigration											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	0.018	0.016	0.020									
	(0.022)	(0.019)	(0.017)									
Post x Bankruptcy/Closure				0.004	0.000	0.012						
				(0.017)	(0.017)	(0.016)						
Post x Restructuring							0.029	0.024	0.013			
							(0.021)	(0.017)	(0.016)			
Post x Offshore/Deloc.										-0.019	-0.006	-0.005
										(0.028)	(0.025)	(0.025)
Negative Shock	0.010	-0.005	-0.003									
	(0.014)	(0.011)	(0.010)									
Bankruptcy/Closure				0.023	0.005	0.005						
				(0.015)	(0.014)	(0.014)						
Restructuring							-0.013	-0.017	-0.011			
							(0.022)	(0.014)	(0.012)			
Offshore/Deloc.										0.021	0.014	0.014
										(0.030)	(0.028)	(0.027)
Post	-0.002	0.000	-0.004	0.007	0.009	0.003	0.002	0.004	0.004	0.009	0.009	0.007
	(0.020)	(0.018)	(0.016)	(0.014)	(0.013)	(0.012)	(0.012)	(0.011)	(0.010)	(0.011)	(0.010)	(0.010)
Constant	-0.027	-0.007	0.005	-0.031	-0.014	0.000	-0.016	-0.006	0.006	-0.020	-0.011	0.003
	(0.058)	(0.043)	(0.038)	(0.056)	(0.042)	(0.038)	(0.055)	(0.042)	(0.036)	(0.055)	(0.041)	(0.036)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	104	106	106	104	106	106	104	106	106	104	106	106
R-squared	0.117	0.119	0.120	0.117	0.119	0.120	0.116	0.119	0.120	0.116	0.119	0.120
Observations	5542	9205	10895	5542	9205	10895	5542	9205	10895	5542	9205	10895

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7/15 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.10: Short-term impact of shocks: Immigration good for economy.

	Immigration Bad or Good for the Economy											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	0.166	0.210*	0.190*									
	(0.146)	(0.108)	(0.104)									
Post x Bankruptcy/Closure				0.042	0.152	0.153*						
				(0.130)	(0.094)	(0.087)						
Post x Restructuring							0.057	0.085	0.073			
							(0.140)	(0.118)	(0.113)			
Post x Offshore/Deloc.										0.464*	0.391*	0.357
										(0.255)	(0.231)	(0.228)
Negative Shock	-0.049	-0.065	-0.090									
	(0.078)	(0.059)	(0.058)									
Bankruptcy/Closure				0.055	0.002	0.011						
				(0.073)	(0.061)	(0.066)						
Restructuring							0.064	0.022	-0.053			
							(0.113)	(0.096)	(0.082)			
Offshore/Deloc.										-0.520***	-0.497***	-0.543***
										(0.171)	(0.171)	(0.181)
Post	-0.079	-0.110	-0.082	-0.001	-0.043	-0.030	0.006	-0.005	0.005	-0.017	-0.010	0.010
	(0.124)	(0.093)	(0.093)	(0.090)	(0.071)	(0.071)	(0.079)	(0.061)	(0.061)	(0.069)	(0.053)	(0.053)
Constant	5.542***	5.536***	5.476***	5.472***	5.477***	5.392***	5.490***	5.478***	5.423***	5.527***	5.500***	5.414***
	(0.272)	(0.251)	(0.239)	(0.250)	(0.241)	(0.232)	(0.251)	(0.250)	(0.235)	(0.242)	(0.242)	(0.229)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	71	73	73	71	73	73	71	73	73	71	73	73
R-squared	0.177	0.186	0.190	0.177	0.186	0.190	0.177	0.186	0.190	0.178	0.187	0.191
Observations	4335	6840	7948	4335	6840	7948	4335	6840	7948	4335	6840	7948

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7/15 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.11: Short-term impact of shocks: Immigration enriches culture.

	Immigration Enriches or Undermines Cultural Life											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	0.245	0.311***	0.285**									
	(0.159)	(0.116)	(0.109)									
Post x Bankruptcy/Closure				0.186	0.253**	0.239**						
				(0.172)	(0.117)	(0.111)						
Post x Restructuring							0.004	0.112	0.115			
							(0.190)	(0.151)	(0.140)			
Post x Offshore/Deloc.										0.393*	0.309	0.284
										(0.233)	(0.208)	(0.208)
Negative Shock	-0.011	-0.018	-0.061									
	(0.096)	(0.066)	(0.064)									
Bankruptcy/Closure				-0.028	-0.009	-0.018						
				(0.094)	(0.069)	(0.071)						
Restructuring							0.188	0.076	0.001			
							(0.129)	(0.101)	(0.089)			
Offshore/Deloc.										-0.305	-0.263	-0.341*
										(0.185)	(0.179)	(0.202)
Post	-0.125	-0.148	-0.118	-0.041	-0.052	-0.036	0.017	0.017	0.023	-0.018	0.010	0.028
	(0.123)	(0.098)	(0.095)	(0.095)	(0.077)	(0.074)	(0.089)	(0.068)	(0.066)	(0.082)	(0.063)	(0.059)
Constant	5.442***	5.407***	5.283***	5.444***	5.397***	5.239***	5.391***	5.378***	5.235***	5.455***	5.419***	5.248***
	(0.346)	(0.290)	(0.261)	(0.328)	(0.279)	(0.252)	(0.323)	(0.283)	(0.256)	(0.316)	(0.275)	(0.249)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	71	73	73	71	73	73	71	73	73	71	73	73
R-squared	0.168	0.173	0.170	0.167	0.172	0.170	0.167	0.172	0.170	0.167	0.172	0.170
Observations	4375	6895	8017	4375	6895	8017	4375	6895	8017	4375	6895	8017

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7/15 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.12: Short-term impact of shocks: Allow More Immigrants.

	Self: Allow More or Fewer Immigrants											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	0.202	0.215	0.199									
	(0.193)	(0.170)	(0.165)									
Post x Bankruptcy/Closure				-0.106	-0.001	-0.034						
				(0.263)	(0.218)	(0.201)						
Post x Restructuring							-0.073	-0.062	-0.105			
							(0.271)	(0.246)	(0.236)			
Post x Offshore/Deloc.										0.791*	0.740*	0.793**
										(0.398)	(0.369)	(0.365)
Negative Shock	0.008	0.103	0.105									
	(0.134)	(0.140)	(0.143)									
Bankruptcy/Closure				0.095	0.135	0.142						
				(0.140)	(0.141)	(0.146)						
Restructuring							0.322	0.548*	0.483			
							(0.306)	(0.287)	(0.295)			
Offshore/Deloc.										-0.527*	-0.653**	-0.637**
										(0.300)	(0.311)	(0.306)
Post	-0.129	-0.205**	-0.234**	0.023	-0.067	-0.095	0.020	-0.018	-0.054	-0.096	-0.127	-0.170*
	(0.102)	(0.100)	(0.103)	(0.128)	(0.123)	(0.120)	(0.123)	(0.106)	(0.103)	(0.097)	(0.094)	(0.092)
Constant	6.964***	6.956***	7.097***	6.928***	6.995***	7.122***	6.896***	6.893***	7.046***	7.023***	7.107***	7.228***
	(0.460)	(0.418)	(0.404)	(0.461)	(0.425)	(0.417)	(0.453)	(0.419)	(0.409)	(0.459)	(0.422)	(0.408)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	50	50	50	50	50	50	50	50	50	50	50	50
R-squared	0.177	0.169	0.175	0.177	0.169	0.174	0.177	0.170	0.175	0.178	0.170	0.175
Observations	2730	3639	3855	2730	3639	3855	2730	3639	3855	2730	3639	3855

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7/15 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table A.13: Short-term impact of shocks: Immigration burdens Welfare.

	Immigrants are a Burden on the Welfare State											
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	-0.084	-0.105	-0.077									
	(0.110)	(0.074)	(0.065)									
Post x Bankruptcy/Closure				0.062	0.030	0.028						
				(0.089)	(0.083)	(0.076)						
Post x Restructuring							-0.005	-0.073	-0.060			
							(0.088)	(0.080)	(0.077)			
Post x Offshore/Deloc.										-0.330**	-0.310***	-0.295***
										(0.130)	(0.093)	(0.088)
Negative Shock	-0.027	-0.026	0.013									
	(0.069)	(0.057)	(0.047)									
Bankruptcy/Closure				0.129	0.022	0.015						
				(0.078)	(0.081)	(0.065)						
Restructuring							-0.247***	-0.127	-0.049			
							(0.078)	(0.083)	(0.066)			
Offshore/Deloc.										0.229	0.235	0.290
										(0.176)	(0.174)	(0.184)
Post	-0.008	0.025	0.009	-0.070	-0.041	-0.039	-0.062	-0.028	-0.024	-0.018	-0.006	-0.011
	(0.095)	(0.062)	(0.057)	(0.057)	(0.045)	(0.042)	(0.065)	(0.050)	(0.047)	(0.054)	(0.039)	(0.036)
Constant	2.383***	2.475***	2.565***	2.375***	2.470***	2.582***	2.487***	2.534***	2.607***	2.360***	2.457***	2.572***
	(0.270)	(0.196)	(0.189)	(0.249)	(0.187)	(0.182)	(0.260)	(0.194)	(0.185)	(0.253)	(0.190)	(0.183)
Window	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days	7 Days	15 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
No. Clusters	55	57	57	55	57	57	55	57	57	55	57	57
R-squared	0.198	0.200	0.195	0.199	0.199	0.195	0.200	0.200	0.195	0.199	0.200	0.196
Observations	2944	4880	6048	2944	4880	6048	2944	4880	6048	2944	4880	6048

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7/15 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

A.2 Long-Term Analysis

Table A.14: Longer-term impact of shocks: party proximity and voting intentions.

	Prob. to vote: UKIP/Brexit	Like/dislike: UKIP/Brexit	Prob. to vote: Cons.	Like/dislike: Cons.	Prob. to vote: Labour	Like/dislike: Labour
	(1)	(2)	(3)	(4)	(5)	(6)
ATT (control)	0.008	0.012	-0.050	-0.006	0.042	-0.004
	(0.039)	(0.048)	(0.036)	(0.032)	(0.041)	(0.028)
ATT (treated)	0.161**	0.038	0.021	0.061	-0.021	-0.040
	(0.069)	(0.054)	(0.070)	(0.056)	(0.064)	(0.052)
-19	-0.997*	-0.237	-0.712	-0.349	0.957	-0.140
	(0.582)	(0.915)	(0.527)	(0.596)	(0.697)	(0.340)
-18	0.287	0.093	-0.058	-0.316	-0.621*	-0.008
	(0.311)	(0.303)	(0.349)	(0.226)	(0.374)	(0.290)
-17	0.296	-0.151	-0.273	-0.050	0.139	0.001
	(0.369)	(0.259)	(0.246)	(0.224)	(0.227)	(0.276)
-16	-0.043	0.157	-0.217	0.100	0.006	0.113
	(0.271)	(0.198)	(0.226)	(0.133)	(0.264)	(0.144)
-15	0.089	0.120	0.290	0.035	0.168	-0.017
	(0.215)	(0.164)	(0.212)	(0.122)	(0.170)	(0.131)
-14	0.302**	0.097	-0.165	0.196*	-0.086	0.056
	(0.141)	(0.123)	(0.130)	(0.113)	(0.152)	(0.104)
-13	-0.090	0.065	-0.116	0.234**	0.168	0.072
	(0.136)	(0.096)	(0.129)	(0.098)	(0.132)	(0.101)
-12	0.306***	-0.016	0.116	-0.121*	-0.184*	-0.138
	(0.105)	(0.080)	(0.120)	(0.070)	(0.106)	(0.085)
-11	-0.003	0.085	-0.075	0.159**	0.012	-0.029
	(0.091)	(0.065)	(0.106)	(0.070)	(0.098)	(0.066)
-10	-0.053	0.079	0.063	-0.059	0.099	0.086
	(0.098)	(0.073)	(0.099)	(0.066)	(0.093)	(0.071)
-9	-0.059	-0.023	0.108	-0.003	-0.137	-0.098*
	(0.092)	(0.055)	(0.078)	(0.052)	(0.090)	(0.055)
-8	0.063	0.062	-0.118	-0.054	0.055	0.079
	(0.088)	(0.052)	(0.083)	(0.045)	(0.082)	(0.051)
-7	-0.095	-0.086*	-0.015	0.017	0.163**	-0.058
	(0.077)	(0.049)	(0.075)	(0.047)	(0.075)	(0.049)
-6	0.018	0.054	0.188**	0.047	-0.038	0.084**
	(0.078)	(0.043)	(0.077)	(0.040)	(0.076)	(0.041)
-5	0.038	-0.006	0.101*	0.005	0.063	-0.057
	(0.069)	(0.043)	(0.060)	(0.036)	(0.067)	(0.041)
-4	-0.031	-0.072*	-0.015	0.000	0.045	0.010
	(0.052)	(0.038)	(0.051)	(0.035)	(0.055)	(0.037)
-3	0.107**	0.032	-0.026	0.018	0.004	-0.009
	(0.052)	(0.035)	(0.055)	(0.035)	(0.053)	(0.036)
-2	0.007	-0.016	-0.046	-0.006	-0.006	-0.009
	(0.046)	(0.028)	(0.047)	(0.026)	(0.053)	(0.028)
-1	0.010	-0.009	0.013	0.024	-0.020	-0.017
	(0.040)	(0.025)	(0.040)	(0.023)	(0.044)	(0.025)
Shock	-0.008	0.005	0.075**	-0.076***	-0.028	-0.002
	(0.035)	(0.023)	(0.036)	(0.022)	(0.038)	(0.023)
+1	-0.011	-0.013	-0.024	-0.047*	-0.070	-0.025
	(0.040)	(0.029)	(0.043)	(0.027)	(0.045)	(0.028)
+2	0.025	-0.029	0.086*	-0.024	0.037	-0.029
	(0.047)	(0.032)	(0.046)	(0.029)	(0.051)	(0.030)
+3	0.042	-0.005	0.003	-0.015	-0.003	0.023
	(0.046)	(0.034)	(0.047)	(0.031)	(0.050)	(0.033)
+4	0.057	0.003	0.020	-0.020	0.033	-0.015
	(0.065)	(0.040)	(0.063)	(0.035)	(0.064)	(0.037)
+5	0.004	0.026	-0.065	-0.078**	-0.013	-0.008
	(0.068)	(0.043)	(0.064)	(0.038)	(0.065)	(0.041)
+6	0.122*	0.087*	-0.010	-0.031	-0.031	-0.025
	(0.064)	(0.045)	(0.064)	(0.041)	(0.068)	(0.044)
+7	0.104	0.074	-0.074	-0.051	0.001	-0.048
	(0.076)	(0.050)	(0.078)	(0.046)	(0.073)	(0.047)
+8	0.115	0.056	-0.081	-0.037	0.014	-0.002
	(0.080)	(0.053)	(0.083)	(0.052)	(0.084)	(0.051)
+9	0.228**	0.010	-0.148*	0.035	0.117	-0.026
	(0.090)	(0.059)	(0.085)	(0.056)	(0.088)	(0.056)
+10	0.167*	0.022	-0.004	0.077	-0.093	-0.053
	(0.091)	(0.068)	(0.102)	(0.062)	(0.094)	(0.063)
+11	0.351***	0.092	-0.005	0.051	0.088	0.010
	(0.105)	(0.076)	(0.102)	(0.069)	(0.093)	(0.069)
+12	0.248*	0.160*	0.051	0.073	-0.019	-0.008
	(0.130)	(0.091)	(0.127)	(0.078)	(0.115)	(0.077)
+13	0.275**	0.072	0.068	0.120	-0.027	-0.030
	(0.124)	(0.098)	(0.119)	(0.085)	(0.110)	(0.081)
+14	0.419**	0.064	0.114	0.085	-0.189	-0.103
	(0.188)	(0.116)	(0.181)	(0.096)	(0.165)	(0.093)
+15	0.175	0.131	-0.013	0.129	0.032	0.059
	(0.167)	(0.117)	(0.173)	(0.114)	(0.160)	(0.108)
+16	0.349*	0.113	0.192	0.210*	-0.128	0.230**
	(0.187)	(0.120)	(0.183)	(0.114)	(0.173)	(0.106)
+17	0.253	0.160	0.030	0.173	-0.238	0.138
	(0.219)	(0.148)	(0.219)	(0.130)	(0.212)	(0.122)
+18	0.104	0.221	0.100	0.194	0.022	-0.056
	(0.212)	(0.174)	(0.200)	(0.146)	(0.188)	(0.139)
+19	0.296	0.165	0.104	0.222	0.173	-0.042
	(0.241)	(0.219)	(0.222)	(0.162)	(0.198)	(0.150)
+20	0.329	0.072	-0.093	0.106	0.082	-0.154
	(0.278)	(0.221)	(0.262)	(0.171)	(0.247)	(0.162)
+21	-0.106	-0.646**	0.128	0.138	-0.229	-0.155
	(0.348)	(0.294)	(0.311)	(0.197)	(0.308)	(0.191)
+22				0.174		-0.600*
				(0.329)		(0.342)

Notes. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. Coefficients estimated using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave FEs. Robust and asymptotic standard errors, obtained using Influence Functions. Data sources: *BES* and *ERM*.

Table A.15: Longer-term impact of shocks: salience of and attitudes towards immigration.

	Most Important Issue: Immigration	Would allow more/fewer	Bad/good for the economy	Enriches/undermines cultural life	Burden on the welfare state
	(1)	(2)	(3)	(4)	(5)
ATT (control)	0.001	-0.042	-0.020	0.030*	-0.007
	(0.004)	(0.026)	(0.014)	(0.018)	(0.010)
ATT (treated)	0.021***	61.712	0.017	-0.011	-0.018
	(0.008)	(49.087)	(0.049)	(0.030)	(0.032)
-19	-0.028		-0.115	0.634**	-0.086
	(0.055)		(0.175)	(0.303)	(0.143)
-18	-0.030		-0.104	-0.084	-0.022
	(0.047)		(0.118)	(0.139)	(0.092)
-17	0.037		0.094	0.073	0.040
	(0.046)		(0.105)	(0.116)	(0.085)
-16	0.025		0.108	0.099	-0.074
	(0.034)		(0.121)	(0.133)	(0.086)
-15	0.009		-0.077	-0.059	0.005
	(0.022)		(0.095)	(0.079)	(0.057)
-14	0.001		-0.027	0.090	0.034
	(0.025)		(0.083)	(0.077)	(0.059)
-13	0.024	-0.285	-0.066	-0.046	-0.015
	(0.019)	(0.228)	(0.066)	(0.065)	(0.047)
-12	-0.018	0.046	0.080	-0.016	0.030
	(0.017)	(0.214)	(0.056)	(0.052)	(0.038)
-11	0.012	-0.150	-0.010	-0.038	0.019
	(0.016)	(0.142)	(0.062)	(0.056)	(0.039)
-10	0.005	-0.220*	-0.072	0.054	-0.071*
	(0.014)	(0.116)	(0.062)	(0.057)	(0.040)
-9	-0.008	0.024	-0.047	-0.086*	-0.015
	(0.012)	(0.093)	(0.056)	(0.050)	(0.041)
-8	-0.015	0.005	-0.016	0.027	-0.049
	(0.010)	(0.113)	(0.051)	(0.048)	(0.033)
-7	0.003	0.063	-0.136***	0.001	0.039
	(0.010)	(0.102)	(0.051)	(0.047)	(0.037)
-6	-0.006	-0.036	0.056	-0.048	0.002
	(0.010)	(0.066)	(0.039)	(0.037)	(0.028)
-5	0.021***	0.009	-0.025	0.011	-0.001
	(0.008)	(0.062)	(0.039)	(0.037)	(0.029)
-4	-0.015**	-0.088	-0.029	-0.065**	-0.011
	(0.007)	(0.062)	(0.033)	(0.031)	(0.024)
-3	0.007	0.107**	0.004	0.020	0.020
	(0.007)	(0.051)	(0.026)	(0.025)	(0.021)
-2	-0.005	-0.025	-0.025	-0.003	-0.002
	(0.005)	(0.051)	(0.028)	(0.026)	(0.023)
-1	-0.002	-0.001	0.020	0.012	0.017
	(0.004)	(0.041)	(0.025)	(0.024)	(0.022)
Shock	0.001	16.095	-0.002	0.009	-0.028*
	(0.004)	(12.090)	(0.019)	(0.018)	(0.017)
+1	0.009**	27.758	0.014	0.009	-0.034
	(0.004)	(24.731)	(0.025)	(0.024)	(0.022)
+2	0.007	13.434	0.020	-0.007	-0.012
	(0.005)	(28.057)	(0.025)	(0.024)	(0.025)
+3	0.002	31.279	-0.014	0.032	0.049
	(0.005)	(33.953)	(0.031)	(0.030)	(0.034)
+4	0.008	23.232	-0.091**	-0.006	0.018
	(0.005)	(39.262)	(0.038)	(0.036)	(0.033)
+5	0.014**	-16.334	0.033	-0.024	-0.025
	(0.006)	(39.512)	(0.032)	(0.029)	(0.028)
+6	0.026***	-41.993	0.006	0.002	0.003
	(0.006)	(43.569)	(0.034)	(0.030)	(0.029)
+7	0.025***	-8.117	-0.035	0.013	0.034
	(0.007)	(54.266)	(0.057)	(0.049)	(0.046)
+8	0.021***	66.387	-0.044	-0.038	0.020
	(0.008)	(102.357)	(0.043)	(0.034)	(0.035)
+9	0.024***	-2.192	-0.025	-0.050	-0.009
	(0.008)	(90.001)	(0.044)	(0.038)	(0.039)
+10	0.021**	60.624	-0.018	-0.036	
	(0.009)	(71.446)	(0.051)	(0.047)	
+11	0.014	66.994	0.004	-0.076*	-0.433
	(0.010)	(100.637)	(0.046)	(0.042)	(0.343)
+12	0.036***	192.600	-0.005	-0.058	-0.117
	(0.011)	(117.257)	(0.054)	(0.049)	(0.214)
+13	0.020	101.435	0.110*	0.094	
	(0.012)	(121.098)	(0.061)	(0.060)	
+14	0.036***	619.625	-0.006	-0.033	
	(0.013)	(463.438)	(0.067)	(0.063)	
+15	0.040***	-163.442	0.016	-0.054	0.083
	(0.014)	(297.427)	(0.078)	(0.072)	(0.095)
+16	0.035**		-0.063	0.043	0.074
	(0.014)		(0.132)	(0.122)	(0.083)
+17	0.022		0.034	-0.039	0.043
	(0.016)		(0.125)	(0.094)	(0.098)
+18	0.051***		0.087	-0.071	0.050
	(0.019)		(0.110)	(0.069)	(0.072)
+19	0.029		-0.209	-0.082	
	(0.020)		(0.548)	(0.088)	
+20	0.020		-0.032	-0.001	
	(0.023)		(0.382)	(0.081)	
+21	0.006		0.337	0.026	
	(0.026)		(0.409)	(0.090)	
+22	0.009		0.266	0.088	
	(0.040)		(0.298)	(0.316)	

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. FCoefficients estimated using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave FEs. Robust and asymptotic standard errors, obtained using Influence Functions. Data sources: *BES* and *ERM*.

Table A.16: Longer-term impact of shocks: retrospective economic evaluations.

	Retro Sociotropic Economic Evaluation	Retro Egotropic Economic Evaluation	Good Time to Purchase
	(1)	(2)	(3)
ATT (control)	0.027**	0.006	0.021**
	(0.011)	(0.011)	(0.008)
ATT (treated)	0.018	-0.019	0.013
	(0.021)	(0.017)	(0.012)
-19	0.265*	0.012	0.142
	(0.156)	(0.150)	(0.114)
-18	0.102	0.050	-0.018
	(0.086)	(0.099)	(0.073)
-17	-0.022	0.003	-0.111**
	(0.075)	(0.080)	(0.055)
-16	0.108*	0.042	0.132**
	(0.063)	(0.066)	(0.058)
-15	0.062	0.042	0.082*
	(0.063)	(0.051)	(0.049)
-14	0.029	-0.049	-0.002
	(0.059)	(0.055)	(0.039)
-13	-0.086*	-0.035	0.046
	(0.045)	(0.040)	(0.035)
-12	-0.019	-0.007	-0.050*
	(0.035)	(0.033)	(0.029)
-11	0.010	0.047	0.080***
	(0.041)	(0.037)	(0.030)
-10	-0.011	-0.034	-0.000
	(0.038)	(0.036)	(0.026)
-9	0.019	0.041	0.031
	(0.045)	(0.043)	(0.032)
-8	-0.033	-0.069**	0.028
	(0.032)	(0.030)	(0.023)
-7	0.036	0.032	-0.021
	(0.030)	(0.030)	(0.020)
-6	0.033	0.018	0.030
	(0.030)	(0.030)	(0.023)
-5	0.016	0.008	0.023
	(0.027)	(0.025)	(0.018)
-4	0.023	0.022	-0.026
	(0.022)	(0.021)	(0.016)
-3	-0.005	-0.034**	0.014
	(0.018)	(0.016)	(0.014)
-2	-0.015	-0.004	0.022
	(0.021)	(0.020)	(0.016)
-1	0.006	0.019	-0.004
	(0.016)	(0.015)	(0.013)
Shock	-0.010	-0.025**	0.001
	(0.013)	(0.012)	(0.009)
+1	-0.008	-0.005	-0.009
	(0.017)	(0.017)	(0.012)
+2	-0.015	-0.019	0.011
	(0.017)	(0.017)	(0.013)
+3	-0.011	-0.017	0.001
	(0.021)	(0.020)	(0.015)
+4	0.003	-0.012	-0.011
	(0.024)	(0.023)	(0.013)
+5	-0.043*	-0.015	0.009
	(0.024)	(0.022)	(0.016)
+6	0.016	-0.006	-0.008
	(0.024)	(0.022)	(0.018)
+7	0.010	0.013	-0.003
	(0.029)	(0.025)	(0.016)
+8	0.005	-0.004	0.017
	(0.025)	(0.023)	(0.021)
+9	0.008	-0.025	0.020
	(0.026)	(0.023)	(0.022)
+10	-0.002	-0.019	0.005
	(0.030)	(0.026)	(0.023)
+11	0.036	-0.026	0.079***
	(0.031)	(0.027)	(0.029)
+12	-0.049	-0.069**	-0.006
	(0.036)	(0.032)	(0.025)
+13	0.011	-0.043	-0.023
	(0.043)	(0.036)	(0.025)
+14	0.040	0.072*	0.026
	(0.050)	(0.041)	(0.032)
+15	0.013	-0.017	0.038
	(0.055)	(0.044)	(0.038)
+16	0.145**	-0.066	0.061
	(0.067)	(0.053)	(0.062)
+17	0.033	-0.075	-0.028
	(0.061)	(0.048)	(0.063)
+18	0.077	0.010	0.061
	(0.052)	(0.041)	(0.057)
+19	0.056	0.002	
	(0.053)	(0.044)	
+20	0.044	-0.006	
	(0.057)	(0.049)	
+21	-0.005	0.026	
	(0.064)	(0.057)	
+22	0.067	-0.101	
	(0.107)	(0.096)	

Notes. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Like/dislike Conservatives (0-10). The estimations include individual, survey wave, and LAD fixed effects. Data sources: *BES* and *ERM*.

B Descriptives

- Table B.1 provides examples of events included in the ERM database.
- Table B.2 presents summary statistics and definitions for all outcome variables used in the main analysis.
- Table B.3 presents summary statistics and definitions for all control variables used in the main analysis and included in vector $\mathbf{X}_{i,d,w}$.
- Table B.4 performs balancing tests in observed characteristics across treatment and control units. This shows that there are differences in the mean of some covariates (e.g., age and employment status) across the two groups, but the magnitude of the difference is very small.

Table B.1: Sample of announcements in the ERM database.

Tomlinson's Diaries, a dairy firm based in Wrexham, Wales, has gone into administration, leading to the loss of 331 jobs. The administrators, PwC, have confirmed that all staff will be made redundant, few of the workers, however, will be retained during the administrating procedure. The bankruptcy comes despite one-year efforts to save the company. Legal representatives of some of the workers have announced that they had started an investigation to see whether the company had followed the regulations to undertake the compulsory redundancy consultation. A spokesperson for the Welsh Government said that it would work with staff affected by the closure of the Wrexham site. Tomlinson's Dairies was one of the UK's largest dairy firms, with operations in Wrexham, Chester, Shropshire and across North West England. *Bankruptcy/Closure, Case no. (NID) 99126, 2019*
Scottish owned outsourcing company Ago Outsourcing has announced plans to create 240 jobs when it opens a new call centre in Gateshead. The company added 470 jobs to its Glasgow head office in 2017, and is now continuing its expansion by planning further call centres in other locations. The new site in Gateshead will be the first to open, and reports suggest that the business will also set up in Manchester. The majority of the work at the new premises will be for contracts which Ago holds with Scottish Power. The Gateshead offices will open in October 2018 with an initial staff of 80, which the company will expand to 240 over a period of a few months. *Business expansion, Case no. (NID) 95510, 2018*
Car manufacturer Vauxhall has announced plans to cut 400 jobs from its site at Ellesmere Port in Cheshire. The company has made the decision as it faces falling sales and difficult market conditions. Ellesmere Port manufactures the Astra model and will change its production process to move from two shifts to one in early 2018. A spokesperson for the company stated that the decision had no link to the uncertainty created by Brexit and was instead reflective of changes in patterns of consumption, away from the estates and saloons produced by Vauxhall and towards the Sports Utility Vehicles. Other commentators have suggested that falls in the value of Sterling linked to the Brexit decision have pushed up costs at the plant and may be a factor in this announcement. Unite the union is about to comment the job losses after discussions with shop stewards. *Internal restructuring, Case no. (NID) 92332, 2017*
An optical manufacturer, Polaroid Eyewear, based west of Glasgow in Scotland will close resulting in the loss of 107 jobs. The factory is in the Vale of Leven in Western Scotland and is owned by an Italian parent company, Safilo who has decided to offshore the production to China. The site opened in 1965 and at its height it employed 5,000 people. The site manufactured polarising lenses and also served as the base for the company's European research centre. Official consultation has commenced with staff with the site due to close by Spring 2017. *Offshoring/Delocalization, Case no. (NID) 86486, 2016*

Table B.2: Summary statistics: outcome variables (14-days window).

	Mean	Std. Dev.	Min.	Max.	Obs.	Definition
General econ. retrospective evaluation	2.58	1.03	1.00	5.00	4409	The respondent's answer to the question "How do you think the general economic situation in this country has changed over the last 12 months? Has it:", where answers range from value 1 "Got a lot worse" to value 5 "Got a lot better".
Personal econ. retrospective evaluation	2.76	0.91	1.00	5.00	4540	The respondent's answer to the question "How does the financial situation of your household now compare with what it was 12 months ago? Has it:", where answers range from value 1 "Got a lot worse" to value 5 "Got a lot better".
Government Approval	2.50	1.15	1.00	5.00	3758	The respondent's answer to the question "Do you approve or disapprove of the job that each of the following are doing? UK Government, Scottish Government, Welsh Government", where answers range from value 1 "Strongly disapprove" to value 5 "Strongly approve".
Prob. to Vote Conservatives	3.84	4.04	0.00	10.00	3682	The respondent's answer to the question "How likely is it that you would ever vote for Conservatives?", where answers range from value 0 "Very unlikely" to value 10 "Very likely".
Prob. to Vote Labour	4.48	3.91	0.00	10.00	3675	The respondent's answer to the question "How likely is it that you would ever vote for Labour?", where answers range from value 0 "Very unlikely" to value 10 "Very likely".
Prob. to Vote Lib-Dem	3.31	3.23	0.00	10.00	3659	The respondent's answer to the question "How likely is it that you would ever vote for Liberal Democrats?", where answers range from value 0 "Very unlikely" to value 10 "Very likely".
Prob. to Vote UKIP	2.16	3.32	0.00	10.00	3605	The respondent's answer to the question "How likely is it that you would ever vote for UKIP?", where answers range from value 0 "Very unlikely" to value 10 "Very likely".
Prob. to Vote Greens	3.31	3.27	0.00	10.00	3655	The respondent's answer to the question "How likely is it that you would ever vote for Greens?", where answers range from value 0 "Very unlikely" to value 10 "Very likely".
Prob. to Vote Brexit Party	2.66	3.72	0.00	10.00	1125	The respondent's answer to the question "How likely is it that you would ever vote for Brexit Party/Reform UK?", where answers range from value 0 "Very unlikely" to value 10 "Very likely".
Most important issue: immigration	0.13	0.34	0.00	1.00	5542	The respondent's answer to the question "As far as you're concerned, what is the SINGLE MOST important issue facing the country at the present time?", taking value 1 if the answer is "Immigration", 0 otherwise.
Would allow more/fewer immigrants	3.55	2.76	0.00	10.00	2730	The respondent's answer to the question "Some people think that the UK should allow *many more* immigrants to come to the UK to live and others think that the UK should allow *many fewer* immigrants. Where would you place yourself on this scale?", where answers range from value 1 "Allow many fewer" to value 10 "Allow many more".
Immigration bad/good for the economy	4.27	1.85	1.00	7.00	4335	The respondent's answer to the question "Do you think immigration is good or bad for Britain's economy?", where answers range from value 1 "Bad for the economy" to value 7 "Good for the economy".
Immigration enriches/undermines cultural life	4.05	2.04	1.00	7.00	4375	The respondent's answer to the question "Do you think that immigration undermines or enriches Britain's cultural life?", where answers range from value 1 "Undermines cultural life" to value 7 "Enriches cultural life".
Immigrants are a burden on the welfare state	3.17	1.31	1.00	5.00	2944	The respondent's answer to the question "How much do you agree or disagree with the following statements? Immigrants are a burden on the welfare state.", where answers range from value 1 "Strongly disagree" to value 5 "Strongly agree".

Table B.3: Summary statistics: control variables (14-days window).

	Mean	Std. Dev.	Min.	Max.	Obs.	Definition
Age	50.70	16.34	17.00	91.00	4409	Age of the respondent
Age sqr.	2837.14	1614.68	289.00	8281.00	4409	Age of the respondent (squared)
Female	0.53	0.50	0.00	1.00	4409	=1 if the respondent's gender is female (0 if male)
Employed	0.55	0.50	0.00	1.00	4409	=1 if respondent in full- or part-time employment (else 0)
Student/other	0.07	0.25	0.00	1.00	4409	=1 if the respondent is a student or has "other" labour market status (else 0)
Retired	0.29	0.45	0.00	1.00	4409	=1 if the respondent is retired (else 0)
Unemployed/Not working	0.09	0.29	0.00	1.00	4409	=1 if the respondent is unemployed or not currently working (else 0)
Educ.: Below GCSE	0.16	0.37	0.00	1.00	4409	=1 if the respondent's highest level of education is below GCSEs (else 0)
Educ.: GCSE/A-level/Diploma	0.38	0.49	0.00	1.00	4409	=1 if the respondent's highest level of education is either GCSE, A-level or a Diploma (else 0)
Educ.: Bachelor or higher	0.46	0.50	0.00	1.00	4409	=1 if the respondent's highest level of education is a bachelor degree or above (else 0)
Single	0.21	0.41	0.00	1.00	4409	=1 if the respondent is single (else 0)
In a relationship	0.68	0.47	0.00	1.00	4409	=1 if the respondent is in any type of relationship (else 0)
Separated/Divorced/Widowed	0.11	0.31	0.00	1.00	4409	=1 if the respondents is separated, divorced or widowed (else 0)
1 or more child	0.20	0.40	0.00	1.00	4409	=1 if the respondent has 1 or more children, 0 otherwise
White British	0.93	0.26	0.00	1.00	4409	=1 if the respondent's ethnicity is "White British" (else 0)
No religion	0.52	0.50	0.00	1.00	4409	=1 if the respondent has no religious affiliation (else 0)
Christian	0.42	0.49	0.00	1.00	4409	=1 if the respondent is Christian, 0 otherwise
Islamic	0.01	0.08	0.00	1.00	4409	=1 if the respondent is Muslim (else 0)
Other religion	0.05	0.22	0.00	1.00	4409	=1 if the respondent has a different reported religion (else 0)
N. Good Shocks in LAD (lag)	7.73	9.31	0.00	35.00	4409	Count of "positive" ERM shocks in the respondent's LAD until wave *w-1*
N. Bad Shocks in LAD (lag)	14.81	10.39	0.00	36.00	4409	Count of "negative" ERM shocks in the respondent's LAD until wave *w-1*

Table B.4: Covariate balance: 14-days and 30-days windows.

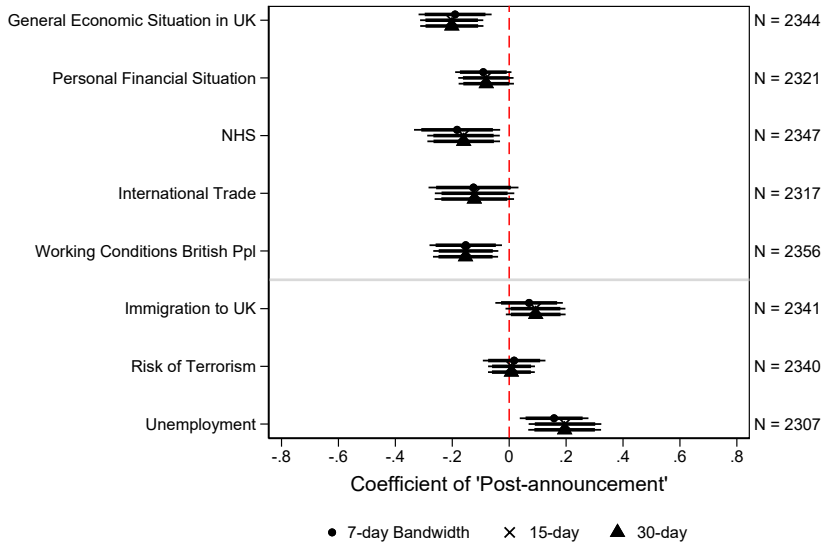
	14-days Window				30-days Window			
	Mean (control)	Mean (treatment)	Diff.	p-value	Mean (control)	Mean (treatment)	Diff.	p-value
Age	51.19	49.96	1.24	0.014	51.85	49.58	2.27	0.000
Age sqr.	2880.55	2771.66	108.89	0.028	2938.06	2737.63	200.43	0.000
Female	0.52	0.54	-0.03	0.057	0.50	0.53	-0.03	0.032
Employed	0.54	0.56	-0.02	0.121	0.54	0.56	-0.02	0.066
Student/other	0.06	0.07	-0.01	0.181	0.06	0.08	-0.02	0.000
Retired	0.30	0.27	0.03	0.028	0.31	0.26	0.05	0.000
Unemployed/Not working	0.10	0.09	0.00	0.715	0.09	0.09	-0.00	0.981
Educ.: Below GCSE	0.17	0.16	0.01	0.479	0.17	0.16	0.01	0.397
Educ.: GCSE/A-level/Diploma	0.38	0.38	-0.00	0.882	0.37	0.39	-0.02	0.104
Educ.: Bachelor or higher	0.45	0.46	-0.01	0.704	0.46	0.45	0.01	0.340
Single	0.22	0.20	0.02	0.157	0.22	0.20	0.01	0.168
In a relationship	0.68	0.69	-0.01	0.405	0.69	0.69	-0.00	0.741
Separated/Divorced/Widowed	0.10	0.11	-0.01	0.537	0.09	0.10	-0.01	0.159
1 child or more	0.18	0.22	-0.04	0.004	0.18	0.23	-0.05	0.000
White British	0.92	0.93	-0.01	0.123	0.93	0.93	-0.00	0.556
No religion	0.52	0.52	-0.00	0.964	0.52	0.52	-0.00	0.805
Christian	0.42	0.42	-0.00	0.863	0.42	0.43	-0.01	0.625
Muslim	0.01	0.01	0.00	0.883	0.01	0.01	0.00	0.971
Other religion	0.05	0.05	0.00	0.656	0.05	0.04	0.01	0.099
N. Good Shocks in LAD (lag)	7.69	7.78	-0.09	0.753	6.77	7.45	-0.68	0.001
N. Bad Shocks in LAD (lag)	14.13	15.85	-1.72	0.000	14.64	15.02	-0.38	0.142
Observations	2651	1758	4409		4609	2332	6941	

Notes: This table shows the mean of covariates across treatment and control units, together with conventional t-tests for differences in means across the two groups for the "bad shocks" sample, using, resp., a 14-days and 30-days window.

C Short-Term: Robustness Tests and Further Insights

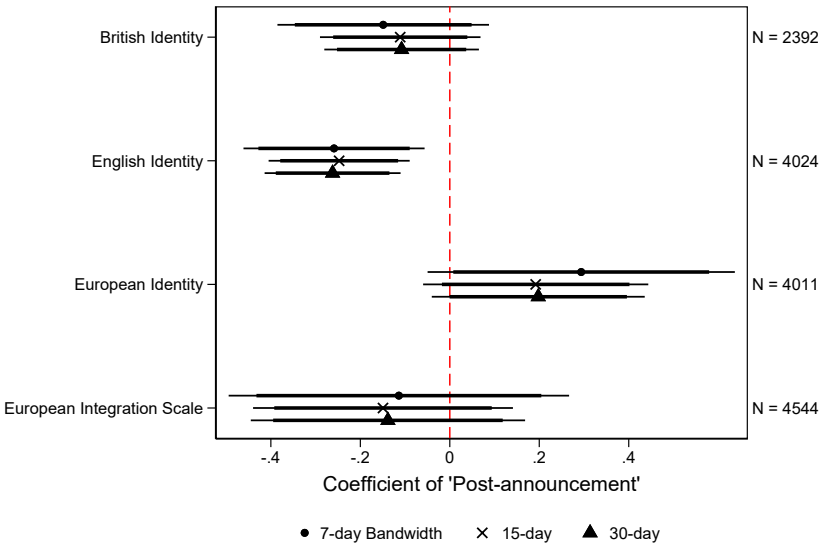
C.1 Mechanisms

Figure C.1: Economic shocks and attitudes towards Brexit.



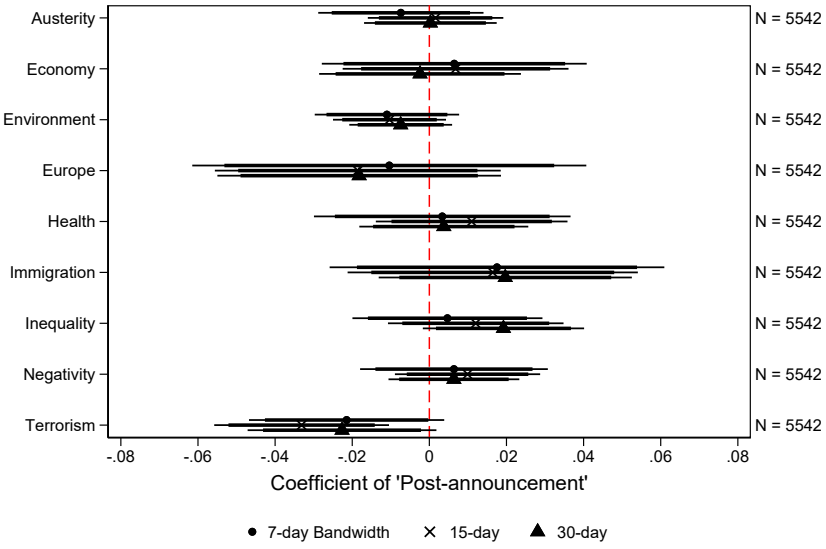
Notes: The dependent variable under scrutiny is reported in each row. All specifications include respondent, LAD and wave fixed effects. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth.

Figure C.2: Economic shocks and national identity.



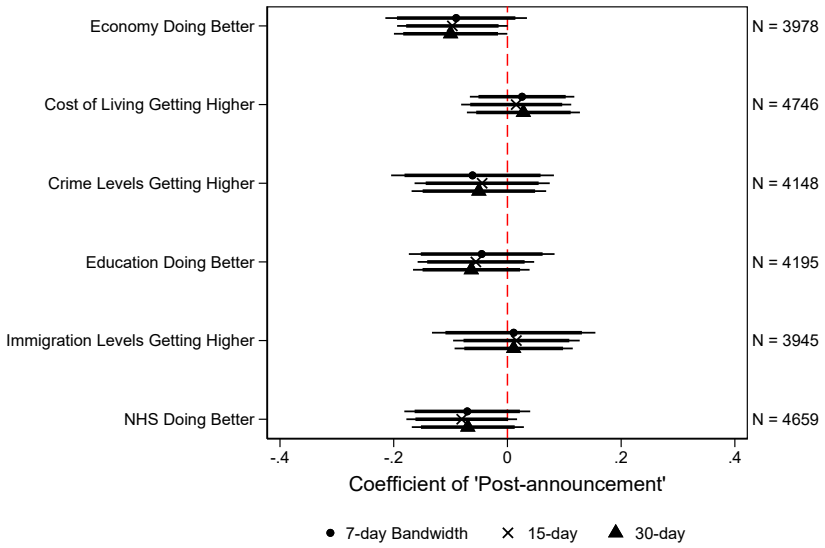
Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth.

Figure C.3: Economic shocks and most important issue faced by the country.



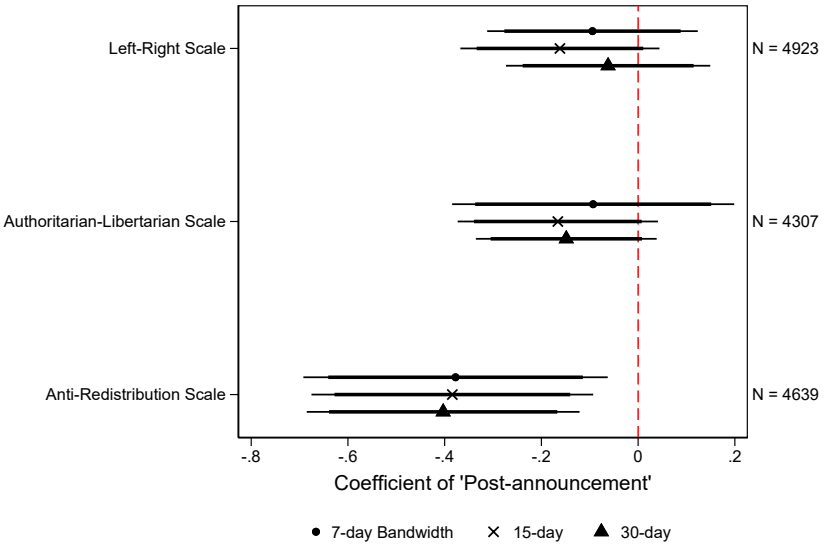
Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth.

Figure C.4: Economic shocks and perceived change in socio-economic indicators.



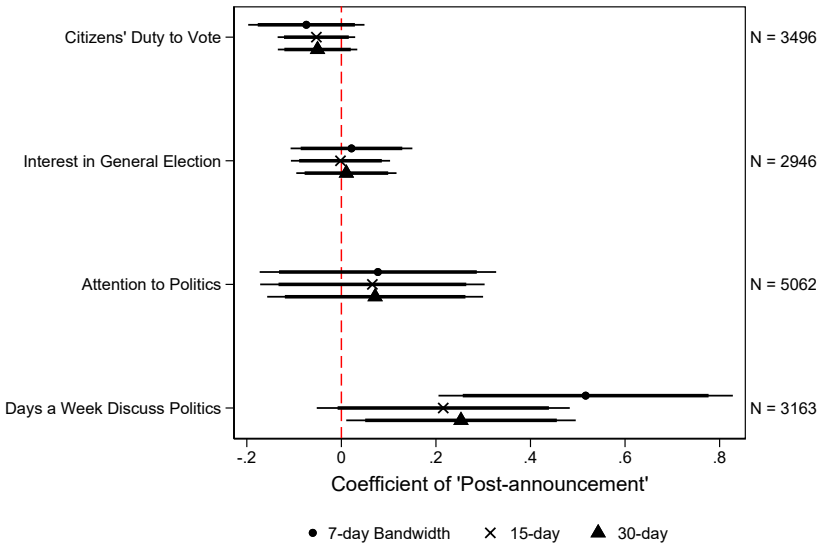
Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth.

Figure C.5: Economic shocks, ideology, authoritarianism, and redistribution.



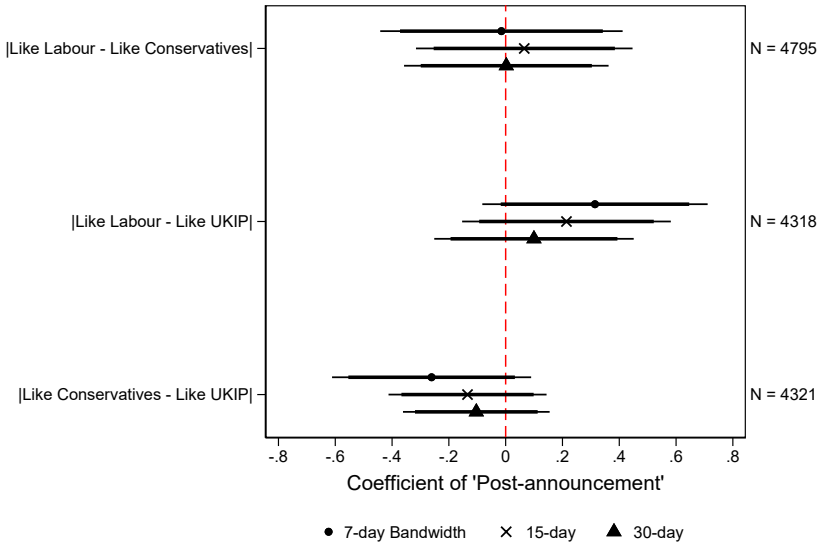
Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth.

Figure C.6: Economic shocks and political attention.



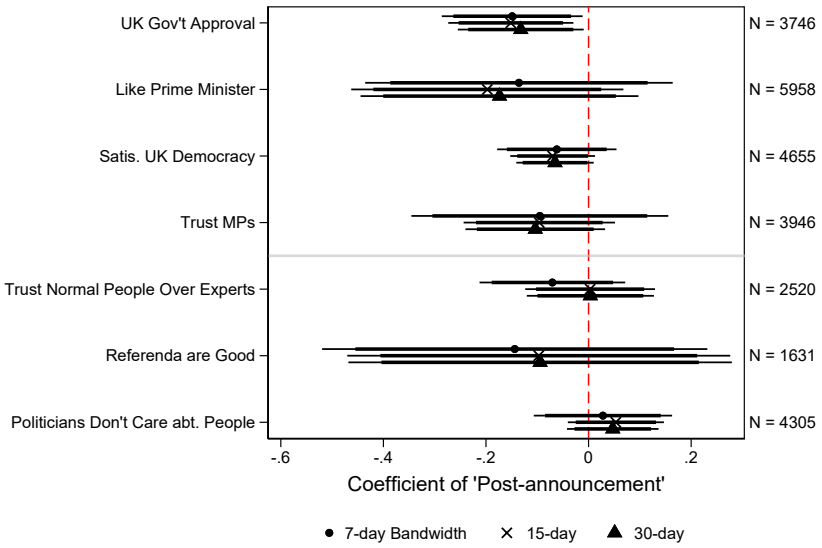
Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth.

Figure C.7: Economic shocks and affective polarization.



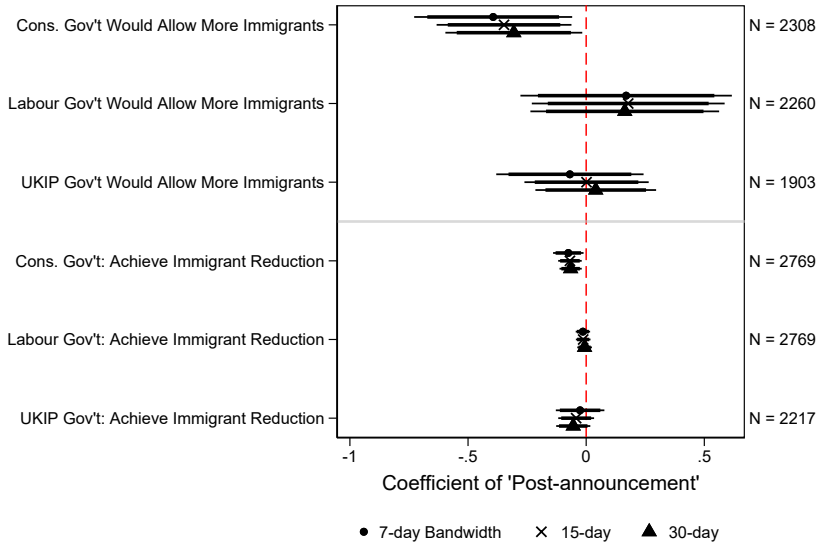
Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth.

Figure C.8: Economic shocks and attitudes towards democracy/institutions.



Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth.

Figure C.9: Economic shocks and evaluation of parties on immigration.



Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth.

C.2 Testing for mis-specification error

In our main analysis, we pool together respondents interviewed in the proximity of an announcement in the ERM dataset, be it positive or negative, and study the interaction between *Post* and a variable capturing the type of event. In this section, we test for heterogeneous effects across the two types of announcements using separate regressions, estimating a reduced version of Equation 1 for each subset of events. Estimates obtained through this strategy, presented in Table C.1, are remarkably similar to the marginal effects calculated in Tables A.7 and A.8.

Table C.1: Shocks and economic perceptions: announcements separately by type.

	General economic retrospective evaluation: country					
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.070*	-0.068**	-0.048	0.063*	0.067*	0.060
	(0.035)	(0.031)	(0.056)	(0.032)	(0.036)	(0.048)
Lagged Outcome (Wave-1)			0.612***			0.634***
			(0.037)			(0.021)
Constant	2.519***	3.111***	1.519***	2.640***	3.081***	1.450***
	(0.019)	(0.281)	(0.328)	(0.016)	(0.254)	(0.250)
Negative Shocks	Yes	Yes	Yes	No	No	No
Positive Shocks	No	No	No	Yes	Yes	Yes
LAD × Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
No. of LADs	41	41	27	39	39	28
R-squared	0.143	0.172	0.513	0.089	0.122	0.474
Observations	2446	2329	933	2176	2080	1266

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

Table C.2: Shocks and voting intentions: announcements separately by type.

	Probability to Vote: UKIP/Brexit Party					
	(1)	(2)	(3)	(4)	(5)	(6)
Post	-0.430**	-0.410**	0.240	-0.046	0.084	0.128
	(0.182)	(0.177)	(0.345)	(0.207)	(0.194)	(0.239)
Lagged Outcome (Wave-1)			0.910***			0.785***
			(0.056)			(0.043)
Constant	2.624***	1.482	-3.164	2.497***	1.983**	2.033
	(0.092)	(0.936)	(2.222)	(0.105)	(0.836)	(2.327)
Negative Shocks	Yes	Yes	Yes	No	No	No
Positive Shocks	No	No	No	Yes	Yes	Yes
LAD × Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	Yes	No	Yes	Yes
No. of LADs	52	52	16	44	44	17
R-squared	0.078	0.145	0.880	0.063	0.154	0.752
Observations	2130	2038	99	1773	1701	221

Notes. $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

C.3 Information Set Update: Leads and Lags

Our short-term analysis captures the effect of an announcement updating the information set of BES respondents interviewed *right after* an announcement (*i.e.*, the treatment group), rather than *right before* (*i.e.*, the control group). In the wave preceding and following the (unanticipated) shock, both groups should exhibit an equivalent information set, as neither/both are aware of its existence (Dinas, Hartman, and Van Spanje, 2016). As a result, we should not retrieve any effect of being interviewed right after an announcement on attitudes measured, resp., in the wave preceding and in the wave following the one involving the shock. This prediction is confirmed by estimates in Table C.3, all statistically insignifcant at conventional levels.

Table C.3: Negative shocks and economic perceptions: lead and lag.

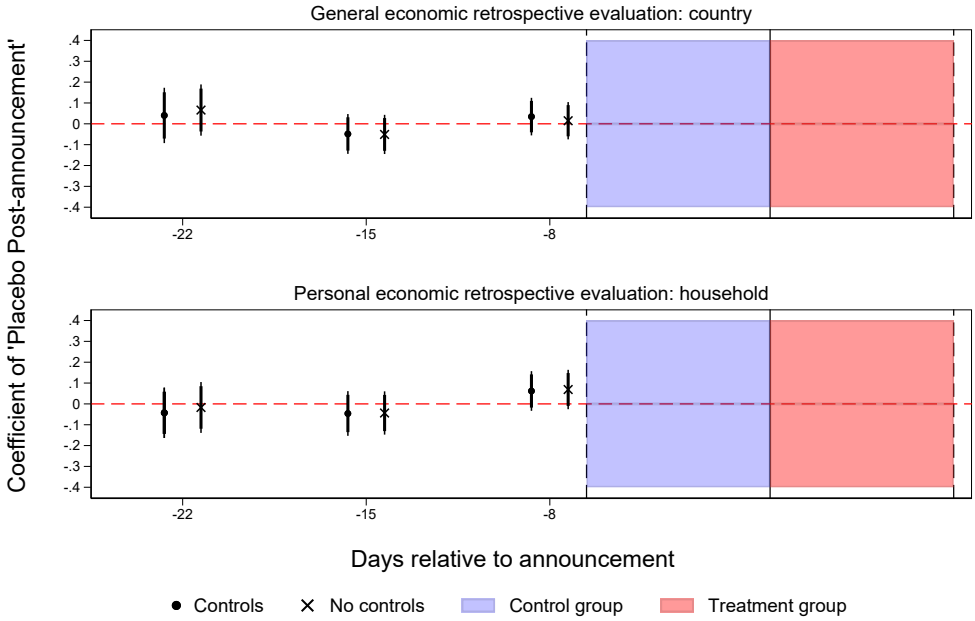
	General economic retrospective evaluation: country		Personal economic retrospective evaluation: household	
	(1)	(2)	(3)	(4)
	Lag (-1 Wave)	Lead (+1 Wave)	Lag (-1 Wave)	Lead (+1 Wave)
Post x Negative Shock	0.006	0.001	0.006	0.003
	(0.080)	(0.094)	(0.064)	(0.083)
Negative Shock	0.056	0.006	0.046	-0.043
	(0.051)	(0.073)	(0.039)	(0.043)
Post	0.032	-0.027	-0.038	0.043
	(0.052)	(0.068)	(0.027)	(0.065)
Constant	2.880***	2.849***	3.501***	3.502***
	(0.161)	(0.223)	(0.180)	(0.226)
Region FEs	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
No. Clusters	75	70	75	70
R-squared	0.103	0.151	0.079	0.047
Observations	3145	2472	3229	2537

Notes. $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$. We report here the estimates of the effect of being interviewed in the 7 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7 days before. The outcome variable is measured in the wave preceding (Columns 1 and 3), or following (Columns 2 and 4) the shock under scrutiny. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

C.4 Testing for Pre-Existing Trends

In this section, we test for pre-existing trends in economic perceptions among BES respondents interviewed in the proximity of negative announcements affecting their LADs. We construct placebo treatments, *i.e.*, announcements, at an arbitrary time point in the period preceding the actual announcement date, as recommended by Muñoz, Falcó-Gimeno, and Hernández, 2020. We begin by defining the 'placebo control' group as individuals interviewed from 8 to 15 days before the actual announcement, and the 'placebo treatment' group as individuals interviewed from 7 to 1 days before it. We iterate this exercise backwards in time, creating a series of placebo tests based on such 7-day bandwidths. For each placebo announcement, we re-run the main short-run regression set-up (Equation 1). Results reported in Figure C.10 show that, in all cases, the placebo treatments have no significant effect on egotropic and sociotropic economic perceptions, confirming the lack of pre-existing trends.

Figure C.10: Negative shocks and economic perceptions: placebo announcements.

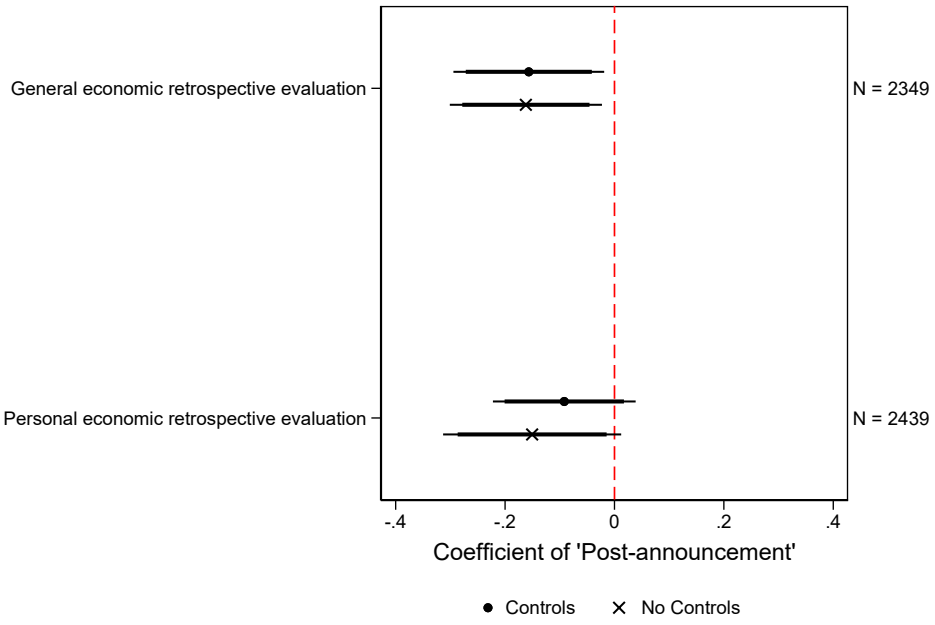


Notes: The dependent variable in each sub-figure is reported in its title. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. The shaded areas around the solid vertical line denote the true control group (to the left) and the true treatment group (to the right). Thick (thin) lines signify the 90% (95%) confidence interval.

C.5 Narrower Bandwidth (3-Days)

In this section, we explore the sensitivity of our results to using a 3-day bandwidth around a negative announcement. In other words, we restrict the sample of control and treated units to include individuals interviewed within 3 days before and 3 days after a negative announcement affecting their LAD. As shown in Figure C.11, the estimated treatment effects are almost identical to those obtained in our baseline analysis (based on a 7-day bandwidth) – although they are less precisely estimated due to the reduced statistical power, mechanically resulting from the narrower bandwidths (Muñoz, Falcó-Gimeno, and Hernández, 2020). These estimates suggest that respondents do not react in strikingly different ways in the immediate aftermath of an announcement.

Figure C.11: Negative shocks and economic perceptions: 3-day bandwidth.



Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 3-day bandwidth.

C.6 Robustness to Excluding/Including Covariates

Table C.4: Negative shocks and economic perceptions: including (or not) a vector of covariates.

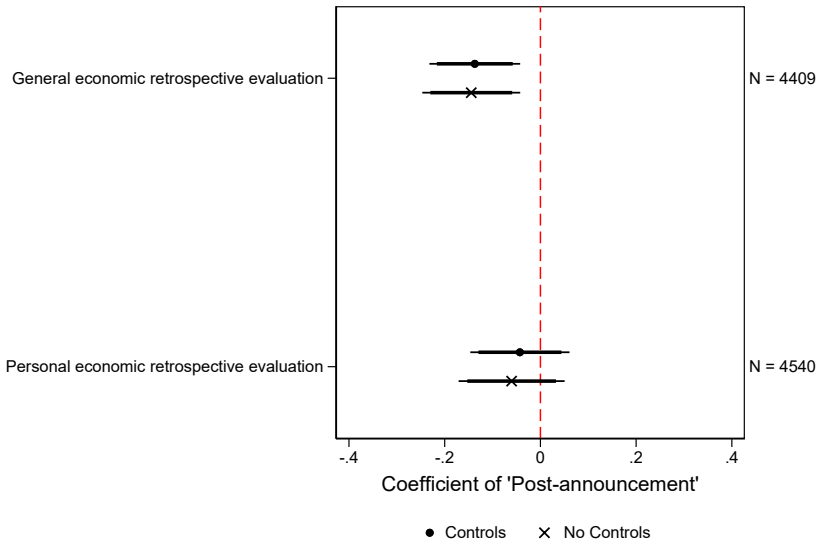
	Personal economic retrospective evaluation: household						General economic retrospective evaluation: country					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post x Negative Shock	-0.087	-0.044	-0.087**	-0.058	-0.129***	-0.089**	-0.147***	-0.149***	-0.138***	-0.134***	-0.163***	-0.146***
	(0.053)	(0.050)	(0.039)	(0.038)	(0.038)	(0.039)	(0.048)	(0.048)	(0.043)	(0.044)	(0.043)	(0.045)
Negative Shock	0.008	-0.002	0.007	0.006	0.029	0.043	0.014	0.004	0.019	-0.008	0.028	0.014
	(0.030)	(0.035)	(0.024)	(0.026)	(0.029)	(0.029)	(0.043)	(0.041)	(0.032)	(0.034)	(0.034)	(0.034)
Post	0.023	-0.013	0.007	-0.020	0.053*	0.008	0.075**	0.081**	0.031	0.038	0.045	0.044
	(0.038)	(0.038)	(0.031)	(0.031)	(0.030)	(0.033)	(0.033)	(0.038)	(0.034)	(0.036)	(0.033)	(0.036)
Constant	2.757***	3.515***	2.754***	3.591***	2.720***	3.551***	2.567***	3.024***	2.628***	3.196***	2.648***	3.218***
	(0.020)	(0.184)	(0.015)	(0.148)	(0.020)	(0.133)	(0.028)	(0.186)	(0.023)	(0.177)	(0.022)	(0.156)
Window	30 Days	30 Days	30 Days	30 Days	30 Days	30 Days	30 Days	30 Days	30 Days	30 Days	30 Days	30 Days
Region FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
No. Clusters	80	80	82	82	82	82	80	80	82	82	82	82
R-squared	0.010	0.061	0.012	0.058	0.012	0.060	0.118	0.146	0.117	0.153	0.107	0.144
Observations	4759	4540	7481	7135	8715	8315	4622	4409	7277	6941	8497	8111

Notes. $^{*}p<0.10$,$^{**}p<0.05$,$^{***}p<0.01$. We report here the estimates of the effect of being interviewed in the 7/15/30 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7/15/30 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

C.7 Dealing with Imbalance in Covariates

In this section, we re-weight the sample affected by a negative shock in a 7-day window using entropy balancing. This procedure imposes that the distribution of covariates among control units matches the moment conditions (mean, variance and skewness) of the treated units, preventing results from being driven by imbalances in observables between the two groups (for a discussion, see: Hainmueller, 2012). Estimates in Figure C.12 remain similar to those in the main paper, although becoming statistically insignificant for the egotropic outcome (second row).

Figure C.12: Negative shocks and economic perceptions: entropy balancing.

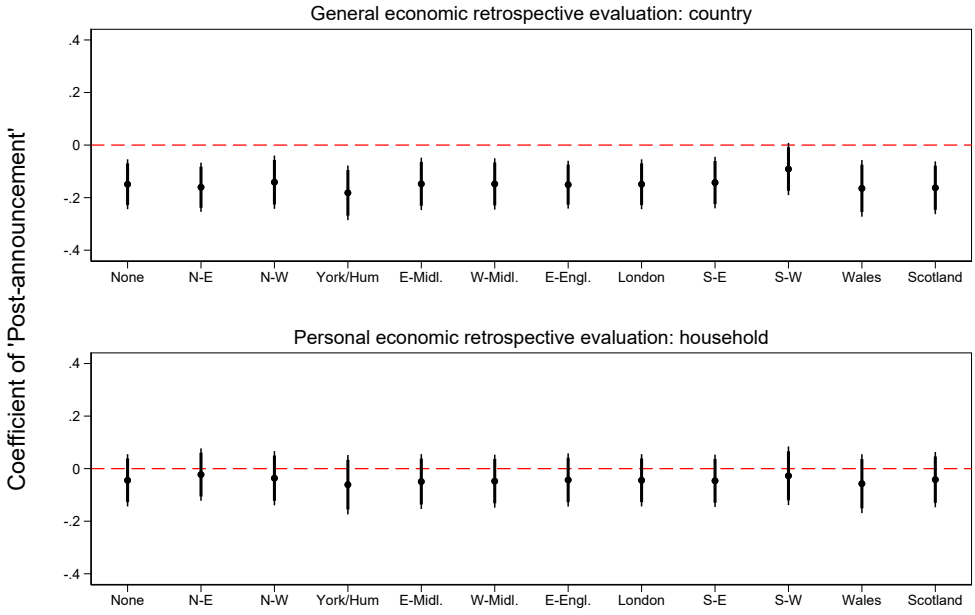


Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. N indicates the n. of observations using the 7-day bandwidth. Thick (thin) lines signify the 90% (95%) confidence interval.

C.8 Excluding regions

In Figure C.13, we estimate Equation 1 across different sub-samples, each time removing respondents residing in one of the eleven administrative regions defined in the UK. Our estimates confirm that, regardless of which region is dropped from the sample, the post-announcement estimates are negative and relatively stable in size when looking at sociotropic economic evaluations, while remaining statistically insignificant at conventional levels for egotropic ones.

Figure C.13: Negative shocks and economic perceptions: excluding regions.

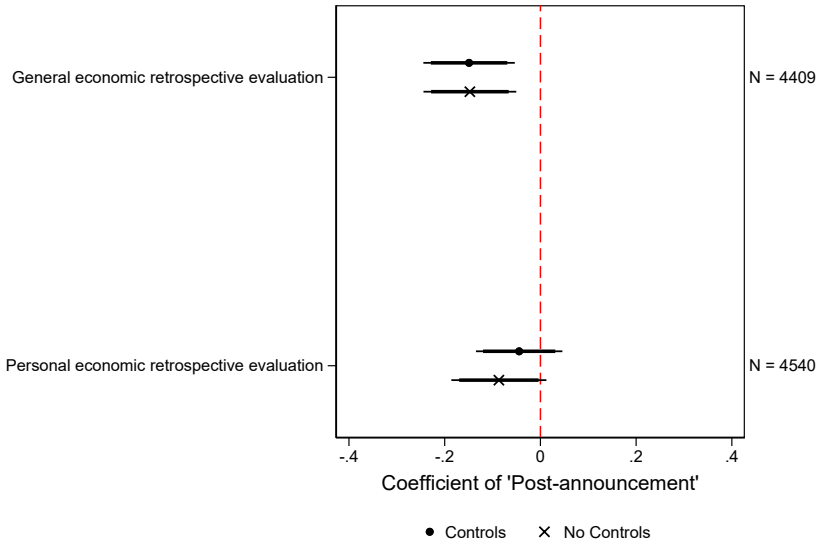


Notes: The dependent variable under scrutiny is reported in each row. The text on the horizontal axis denotes the excluded government office region. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval.

C.9 Alternative Clustering of Standard Errors

When clustering standard errors in our estimation of Equation 1 at the Local Authority District (LAD) level, rather than at the LAD-by-wave one, our results remain virtually unchanged (Figure C.14).

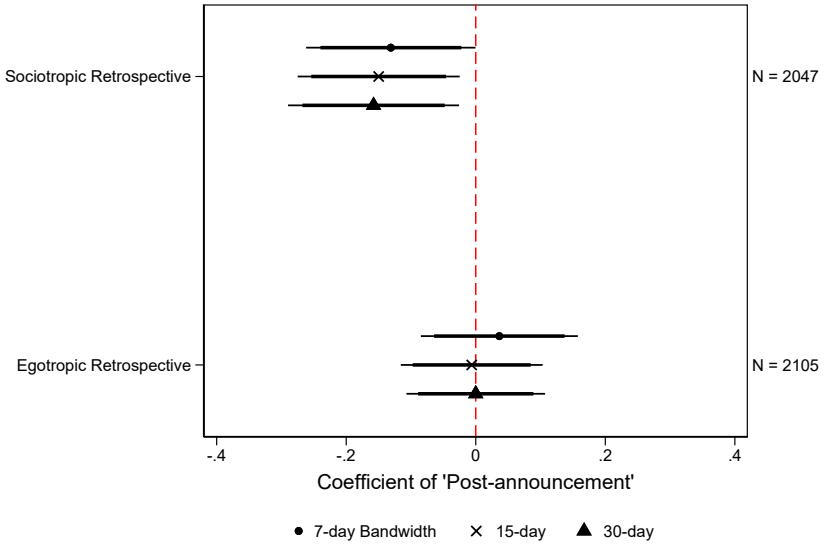
Figure C.14: Negative shocks and economic perceptions: alternative SE clustering.



Notes: The dependent variable under scrutiny is reported in each row. The text on the horizontal axis denotes the excluded government office region. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD level. N indicates the n. of observations using the 7-day bandwidth. Thick (thin) lines signify the 90% (95%) confidence interval.

C.10 Focusing on More Salient Shocks

Figure C.15: Economic shocks, sociotropic and egotropic economic response. 200 jobs lost.



Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. The focus is on negative shocks. Standard errors are clustered at the LAD-by-wave level. Standard errors are clustered at the LAD level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth.

C.11 Manufacturing Shocks Only

Figure C.16: Number of restructuring events by sector (UK, 2002-2021)

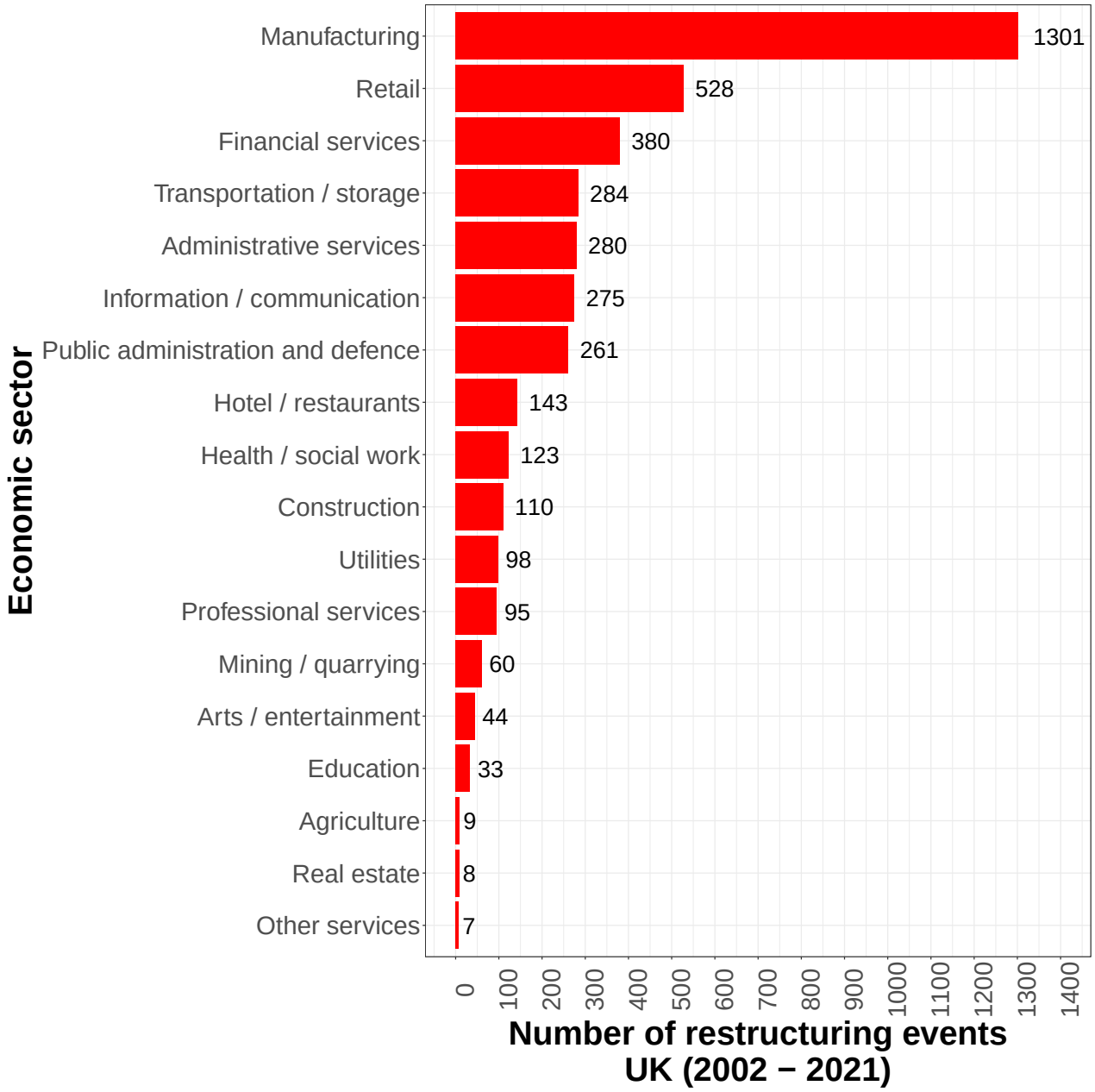


Figure C.17: Number of manufacturing restructuring events by year (UK, 2002-2021)

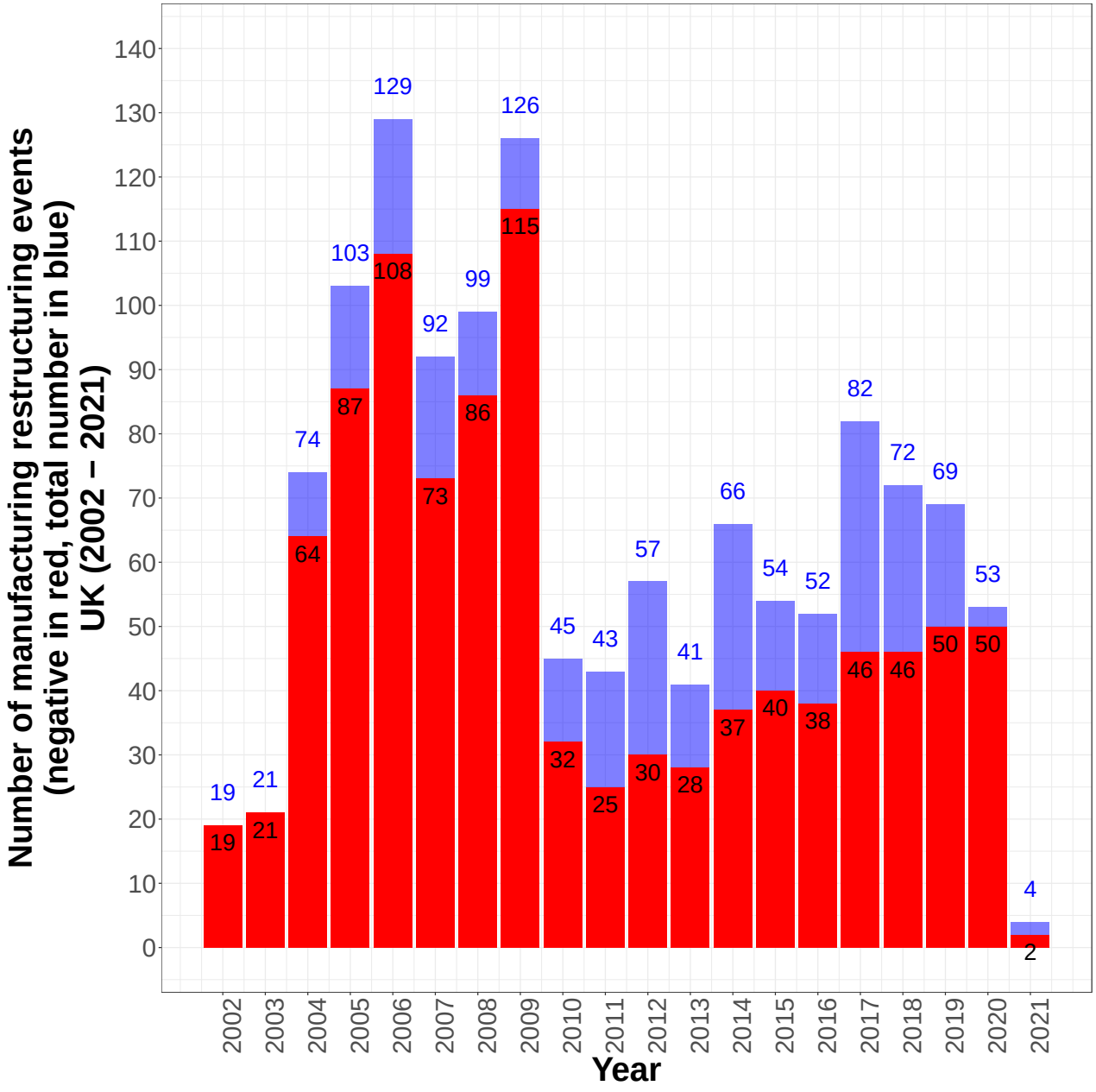
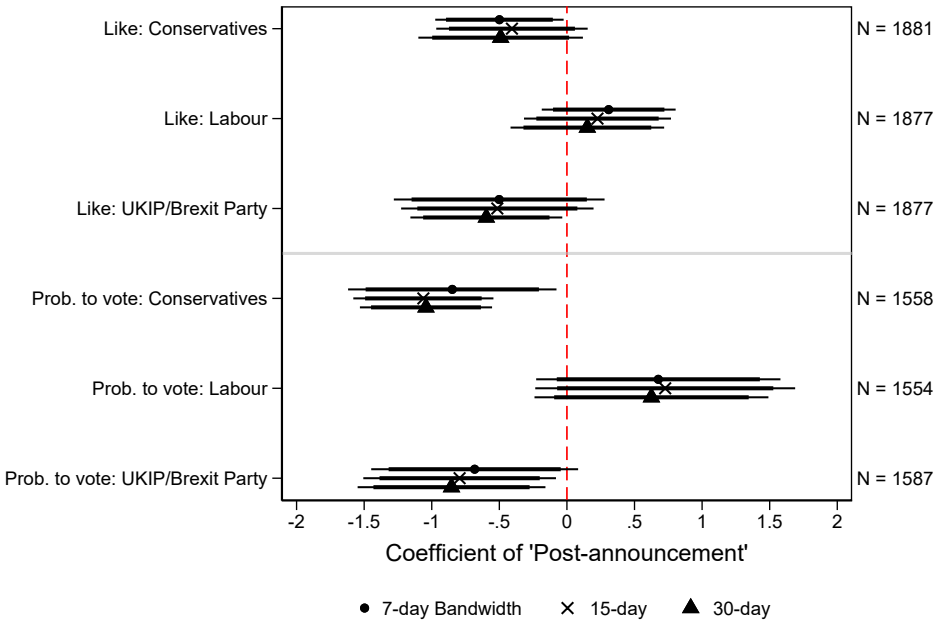


Figure C.18: Negative shocks, attitudes and voting: manufacturing only.



Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth. We include only LADs affected by ERM announcements involving the manufacturing sector.

C.12 Heterogeneity by Individual Characteristics

In this section, we explore potential heterogeneities between exposure to negative announcements and three individual-level mediators: age, gender and education. We expand Equation 1 with an interaction term between *Post*, *Negative* and binary indicators taking value 1 if the respondent: (i) is aged 55 or above; (ii) identifies as a woman; (iii) has completed a bachelor degree or above. Results in Table C.5 show that the effect of exposure to local negative shocks does not affect individuals differently based on their age, gender or education.

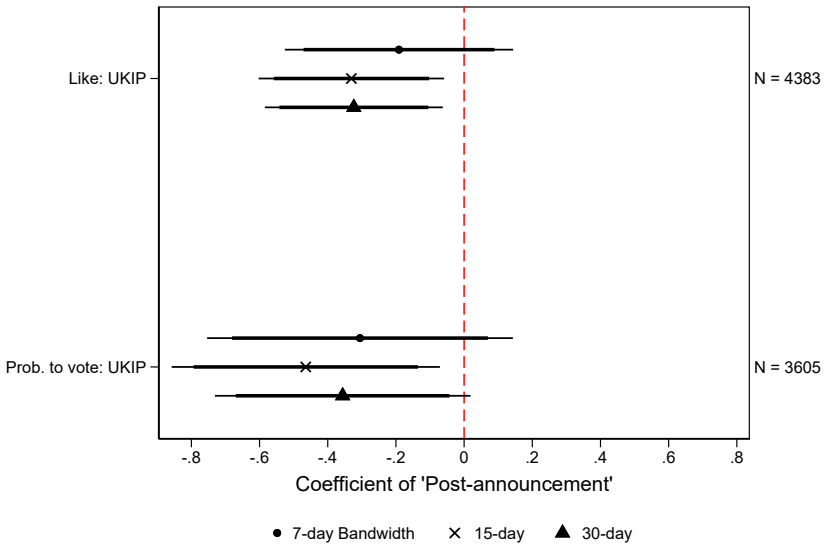
Table C.5: Negative shocks and economic perceptions: individual heterogeneities.

	(1)	(2)	(3)
	General economic retrospective evaluation: country		
Post × Negative Shock × Older	0.053		
	(0.061)		
Post × Negative Shock × Female		-0.003	
		(0.075)	
Post × Negative Shock × High education			-0.036
			(0.064)
Post x Negative Shock	-0.184***	-0.150**	-0.121**
	(0.055)	(0.061)	(0.054)
Post	0.093**	0.081**	0.068*
	(0.041)	(0.039)	(0.038)
Negative Shock	0.019	0.013	0.002
	(0.041)	(0.040)	(0.039)
Constant	2.396***	3.003***	3.023***
	(0.100)	(0.226)	(0.222)
Region FEs	Yes	Yes	Yes
Wave FEs	Yes	Yes	Yes
Controls	Yes	Yes	Yes
No. Clusters	68	68	68
R-squared	0.142	0.146	0.146
Observations	4411	4409	4480

Notes. $^{*}p < 0.10,^{**}p < 0.05,^{***}p < 0.01$. We report here the estimates of the effect of being interviewed in the 7 days following a restructuring announcement recorded by the ERM and involving the respondent's LAD, rather than in the 7 days before. Coefficients are estimated using an OLS regression with wave and region fixed-effects and individual controls: age(^{2}), gender, employment status, education, marital status, any children, ethnicity, religion, (lag) N. of "positive" and "negative" events in the respondent's LAD. SE clustered at the LAD-by-wave level. Data sources: *BES* and *ERM*.

C.13 UKIP Only

Figure C.19: Negative shocks and probability to vote UKIP (no Brexit Party).

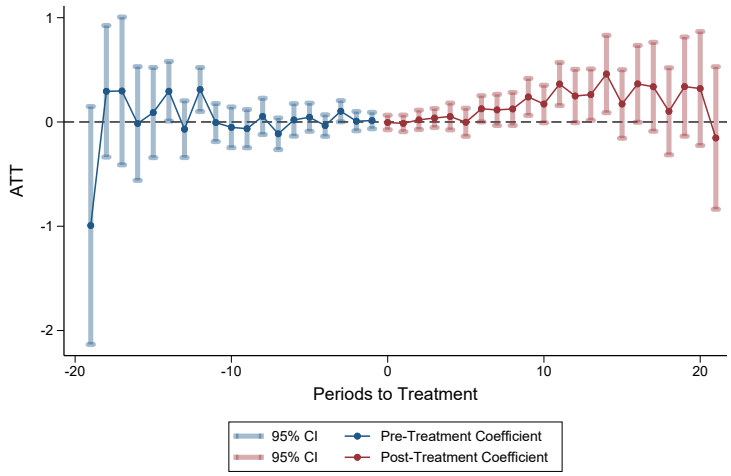


Notes: The dependent variable under scrutiny is reported in each row. All specifications include region and wave fixed effects. Standard errors are clustered at the LAD-by-wave level. Thick (thin) lines signify the 90% (95%) confidence interval. N indicates the n. of observations using the 7-day bandwidth.

D Long-Term: Robustness Tests and Further Insights

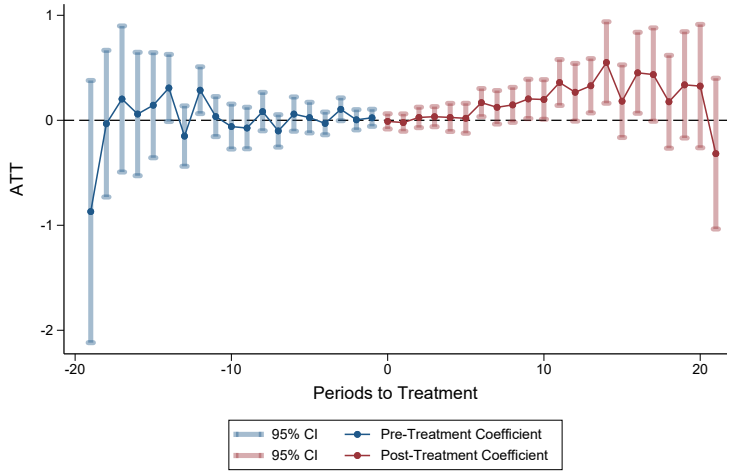
D.1 Accounting for Selection into Treated LADs

Figure D.1: Negative shocks shocks and Probability to vote UKIP/Brexit Party: Non-movers.



Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. Thick (thin) lines signify the 90% (95%) confidence interval.

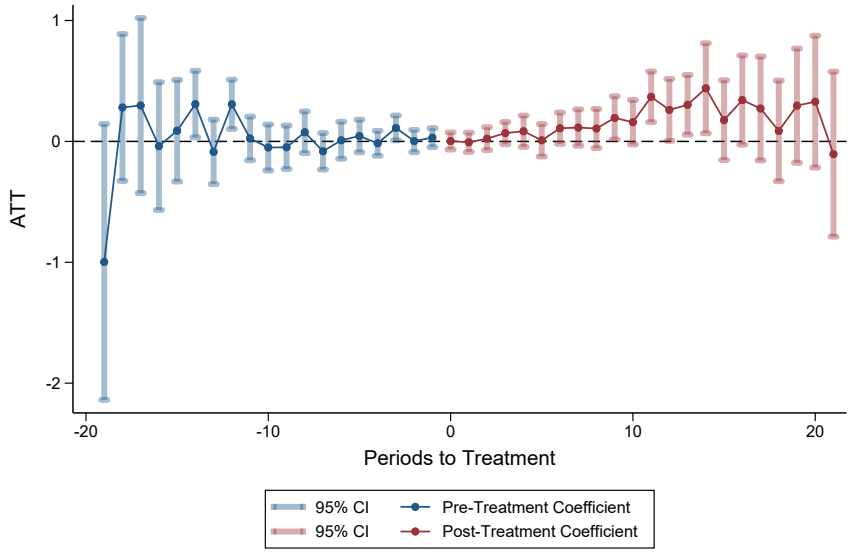
Figure D.2: Negative shocks and Probability to vote UKIP/Brexit Party: Never-movers.



Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. Thick (thin) lines signify the 90% (95%) confidence interval.

D.2 Employing Never Treated Units for the Control Group

Figure D.3: Negative shocks and Probability to vote UKIP/Brexit: never-treated as control.

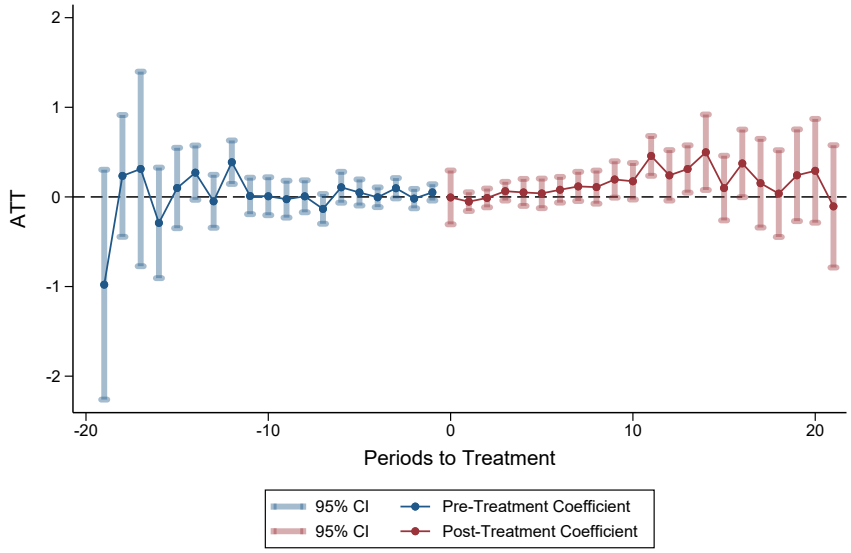


Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. Thick (thin) lines signify the 90% (95%) confidence interval. The control group is composed of never-treated units.

D.3 Focusing on the Most Recent ERM Announcement

In this section, we re-estimate Equation 2 focusing on the subset of respondents becoming "treated" at the time of the most recent ERM announcement involving their LAD. This shrewdness reduces the risk of compounding several shocks, in case the respondent's LAD is affected by further ERM announcements after the first "treatment". As shown in Figure D.4, the estimated treatment effects, despite the reduced sample size, are consistent in sign and magnitude with those presented in Figure 3.

Figure D.4: Negative shocks and Probability to vote UKIP/Brexit: most recent event.



Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. Thick (thin) lines signify the 90% (95%) confidence interval. The sample is restricted to include only BES respondents whose LAD is affected by the most recent ERM announcement.

D.4 Alternative Estimation Strategies

In Table D.1 we re-estimate Equation 2 using the improved doubly robust DID estimator based on inverse probability of tilting and weighted least squares (Sant'Anna and Zhao, 2020) — odd columns — and via an inverse probability weighting DiD estimator with stabilized weights (see: Callaway and Sant'Anna, 2021) — even columns in Table D.1. We obtain comparable results as those obtained in the main body of the paper.

Table D.1: Long-term impact of shocks: alternative estimation methods.

	Prob. to vote: UKIP	Prob. to vote: UKIP	Most Important Issue: Immigration	Most Important Issue: Immigration
	(1)	(2)	(3)	(4)
ATT (control)	0.008	0.008	0.001	0.001
	(0.039)	(0.039)	(0.004)	(0.004)
ATT (treated)	0.161**	0.161**	0.021***	0.021***
	(0.069)	(0.069)	(0.008)	(0.008)
-19	-0.997*	-0.997*	-0.028	-0.028
	(0.582)	(0.582)	(0.055)	(0.055)
-18	0.287	0.287	-0.030	-0.030
	(0.311)	(0.311)	(0.047)	(0.047)
-17	0.296	0.296	0.037	0.037
	(0.369)	(0.369)	(0.046)	(0.046)
-16	-0.043	-0.043	0.025	0.025
	(0.271)	(0.271)	(0.034)	(0.034)
-15	0.089	0.089	0.009	0.009
	(0.215)	(0.215)	(0.022)	(0.022)
-14	0.302**	0.302**	0.001	0.001
	(0.141)	(0.141)	(0.025)	(0.025)
-13	-0.090	-0.090	0.024	0.024
	(0.136)	(0.136)	(0.019)	(0.019)
-12	0.306***	0.306***	-0.018	-0.018
	(0.105)	(0.105)	(0.017)	(0.017)
-11	-0.003	-0.003	0.012	0.012
	(0.091)	(0.091)	(0.016)	(0.016)
-10	-0.053	-0.053	0.005	0.005
	(0.098)	(0.098)	(0.014)	(0.014)
-9	-0.059	-0.059	-0.008	-0.008
	(0.092)	(0.092)	(0.012)	(0.012)
-8	0.063	0.063	-0.015	-0.015
	(0.088)	(0.088)	(0.010)	(0.010)
-7	-0.095	-0.095	0.003	0.003
	(0.077)	(0.077)	(0.010)	(0.010)
-6	0.018	0.018	-0.006	-0.006
	(0.078)	(0.078)	(0.010)	(0.010)
-5	0.038	0.038	0.021***	0.021***
	(0.069)	(0.069)	(0.008)	(0.008)
-4	-0.031	-0.031	-0.015**	-0.015**
	(0.052)	(0.052)	(0.007)	(0.007)
-3	0.107**	0.107**	0.007	0.007
	(0.052)	(0.052)	(0.007)	(0.007)
-2	0.007	0.007	-0.005	-0.005
	(0.046)	(0.046)	(0.005)	(0.005)
-1	0.010	0.010	-0.002	-0.002
	(0.040)	(0.040)	(0.004)	(0.004)
Shock	-0.008	-0.008	0.001	0.001
	(0.035)	(0.035)	(0.004)	(0.004)
+1	-0.011	-0.011	0.009**	0.009**
	(0.040)	(0.040)	(0.004)	(0.004)
+2	0.025	0.025	0.007	0.007
	(0.047)	(0.047)	(0.005)	(0.005)
+3	0.042	0.042	0.002	0.002
	(0.046)	(0.046)	(0.005)	(0.005)
+4	0.057	0.057	0.008	0.008
	(0.065)	(0.065)	(0.005)	(0.005)
+5	0.004	0.004	0.014**	0.014**
	(0.068)	(0.068)	(0.006)	(0.006)
+6	0.122*	0.122*	0.026***	0.026***
	(0.064)	(0.064)	(0.006)	(0.006)
+7	0.104	0.104	0.025***	0.025***
	(0.076)	(0.076)	(0.007)	(0.007)
+8	0.115	0.115	0.021***	0.021***
	(0.080)	(0.080)	(0.008)	(0.008)
+9	0.228**	0.228**	0.024***	0.024***
	(0.090)	(0.090)	(0.008)	(0.008)
+10	0.167*	0.167*	0.021**	0.021**
	(0.091)	(0.091)	(0.009)	(0.009)
+11	0.351***	0.351***	0.014	0.014
	(0.105)	(0.105)	(0.010)	(0.010)
+12	0.248*	0.248*	0.036***	0.036***
	(0.130)	(0.130)	(0.011)	(0.011)
+13	0.275**	0.275**	0.020	0.020
	(0.124)	(0.124)	(0.012)	(0.012)
+14	0.419**	0.419**	0.036***	0.036***
	(0.188)	(0.188)	(0.013)	(0.013)
+15	0.175	0.175	0.040***	0.040***
	(0.167)	(0.167)	(0.014)	(0.014)
+16	0.349*	0.349*	0.035**	0.035**
	(0.187)	(0.187)	(0.014)	(0.014)
+17	0.253	0.253	0.022	0.022
	(0.219)	(0.219)	(0.016)	(0.016)
+18	0.104	0.104	0.051***	0.051***
	(0.212)	(0.212)	(0.019)	(0.019)
+19	0.296	0.296	0.029	0.029
	(0.241)	(0.241)	(0.020)	(0.020)
+20	0.329	0.329	0.020	0.020
	(0.278)	(0.278)	(0.023)	(0.023)
+21	-0.106	-0.106	0.006	0.006
	(0.348)	(0.348)	(0.026)	(0.026)
+22			0.009	0.009
			(0.040)	(0.040)

Notes. $^{*}p < 0.10$,$^{**}p < 0.05$,$^{***}p < 0.01$. Coefficients estimated using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). Estimation in odd Columns performed using the improved doubly robust DID estimator based on inverse probability of tilting and weighted least squares (Sant'Anna and Zhao, 2020). In even Columns: inverse probability weighting DiD estimator with stabilized weights (see: Callaway and Sant'Anna, 2021). All specifications include respondent and wave FEs. Robust and asymptotic standard errors, obtained using Influence Functions. Data sources: *BES* and *ERM*.

D.5 Salience of Alternative Issues

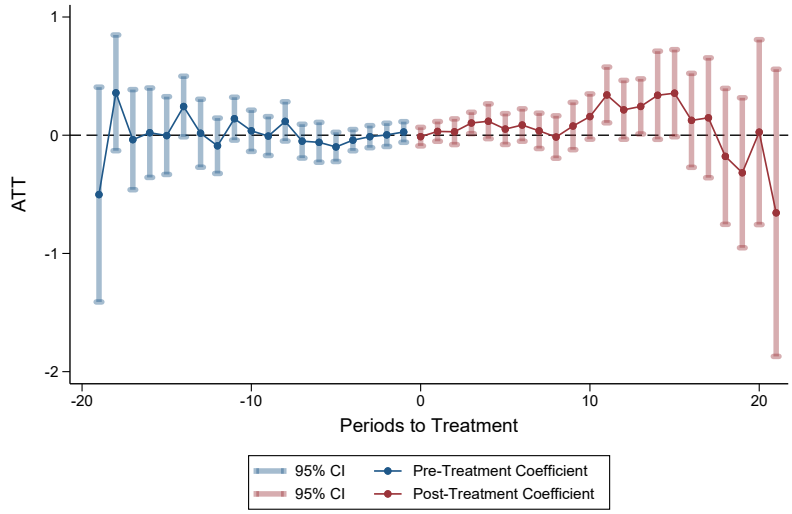
Table D.2: Long-term impact of shocks: most important issue faced by the country.

	Most Important Issue: Austerity	Most Important Issue: Economy	Most Important Issue: Europe	Most Important Issue: Inequality	Most Important Issue: Terrorism
	(1)	(2)	(3)	(4)	(5)
ATT (control)	-0.001	0.007	-0.001	-0.001	-0.002
	(0.004)	(0.005)	(0.002)	(0.002)	(0.002)
ATT (treated)	-0.008*	-0.007	-0.001	0.004	0.001
	(0.004)	(0.009)	(0.005)	(0.004)	(0.003)
-19	0.048	0.081	0.008	-0.004	-0.001
	(0.066)	(0.086)	(0.038)	(0.006)	(0.002)
-18	-0.015	0.036	0.013	-0.026	-0.019
	(0.026)	(0.043)	(0.031)	(0.032)	(0.021)
-17	-0.048**	-0.009	0.013	0.010	0.008
	(0.022)	(0.051)	(0.032)	(0.017)	(0.026)
-16	-0.005	-0.020	-0.060**	0.008	-0.007
	(0.018)	(0.037)	(0.028)	(0.021)	(0.023)
-15	0.003	-0.002	0.029**	-0.026	-0.005
	(0.013)	(0.023)	(0.013)	(0.018)	(0.011)
-14	-0.026*	0.008	-0.022*	0.019	0.006
	(0.014)	(0.023)	(0.012)	(0.014)	(0.013)
-13	-0.005	0.029	-0.006	-0.010	-0.012
	(0.011)	(0.020)	(0.013)	(0.012)	(0.010)
-12	0.011	-0.009	-0.000	0.002	0.009
	(0.009)	(0.016)	(0.011)	(0.008)	(0.009)
-11	-0.010	-0.006	0.017	-0.000	-0.012
	(0.008)	(0.015)	(0.014)	(0.008)	(0.010)
-10	0.007	-0.002	-0.018	-0.002	-0.007
	(0.008)	(0.015)	(0.012)	(0.008)	(0.009)
-9	0.009	0.002	-0.000	0.005	0.001
	(0.007)	(0.013)	(0.011)	(0.007)	(0.006)
-8	-0.002	0.006	-0.001	0.004	0.007
	(0.007)	(0.011)	(0.010)	(0.006)	(0.005)
-7	-0.004	-0.014	0.012	0.003	-0.004
	(0.006)	(0.011)	(0.011)	(0.006)	(0.007)
-6	-0.002	0.010	-0.009	0.001	0.003
	(0.006)	(0.010)	(0.011)	(0.006)	(0.007)
-5	-0.004	0.006	-0.011	0.003	0.000
	(0.005)	(0.009)	(0.008)	(0.005)	(0.004)
-4	0.005	0.006	0.016**	-0.001	-0.002
	(0.005)	(0.008)	(0.008)	(0.005)	(0.005)
-3	0.009**	0.001	-0.002	-0.003	0.006
	(0.004)	(0.007)	(0.008)	(0.004)	(0.005)
-2	-0.000	0.007	-0.004	-0.001	-0.001
	(0.004)	(0.006)	(0.006)	(0.004)	(0.004)
-1	0.002	-0.005	0.005	-0.001	0.000
	(0.003)	(0.005)	(0.006)	(0.003)	(0.003)
Shock	0.004	-0.003	0.006	0.000	0.001
	(0.003)	(0.005)	(0.005)	(0.003)	(0.002)
+1	-0.002	0.002	0.004	0.000	-0.003
	(0.003)	(0.005)	(0.006)	(0.003)	(0.003)
+2	-0.004	-0.002	-0.006	0.004	0.001
	(0.003)	(0.005)	(0.006)	(0.003)	(0.003)
+3	-0.001	-0.001	0.013*	-0.004	-0.001
	(0.003)	(0.006)	(0.007)	(0.003)	(0.003)
+4	0.001	-0.010	0.010	0.001	0.003
	(0.004)	(0.006)	(0.007)	(0.004)	(0.003)
+5	-0.001	0.008	0.001	-0.004	-0.001
	(0.004)	(0.007)	(0.009)	(0.004)	(0.004)
+6	-0.005	-0.011	0.010	-0.007*	0.002
	(0.004)	(0.007)	(0.009)	(0.004)	(0.004)
+7	-0.003	-0.012	0.008	-0.007	0.003
	(0.004)	(0.008)	(0.010)	(0.004)	(0.004)
+8	-0.004	-0.017**	-0.001	-0.003	0.003
	(0.004)	(0.008)	(0.010)	(0.005)	(0.004)
+9	-0.007*	0.002	0.009	-0.009*	-0.002
	(0.004)	(0.009)	(0.011)	(0.005)	(0.005)
+10	-0.004	-0.010	0.004	0.002	0.004
	(0.005)	(0.010)	(0.011)	(0.005)	(0.005)
+11	-0.005	-0.001	0.013	-0.002	0.006
	(0.005)	(0.011)	(0.011)	(0.006)	(0.005)
+12	-0.007	-0.011	-0.003	0.004	0.006
	(0.006)	(0.012)	(0.013)	(0.006)	(0.004)
+13	-0.015**	-0.009	0.012	0.013**	-0.002
	(0.006)	(0.014)	(0.012)	(0.007)	(0.005)
+14	-0.005	0.007	0.015	-0.010	0.002
	(0.007)	(0.014)	(0.012)	(0.007)	(0.005)
+15	-0.005	-0.001	0.013	-0.005	-0.001
	(0.008)	(0.015)	(0.013)	(0.008)	(0.006)
+16	-0.010	-0.023	0.008	0.005	-0.004
	(0.008)	(0.017)	(0.012)	(0.008)	(0.005)
+17	-0.012	-0.007	-0.021*	0.001	0.004
	(0.009)	(0.019)	(0.013)	(0.009)	(0.006)
+18	-0.016*	-0.010	-0.012	0.012	0.004
	(0.010)	(0.023)	(0.010)	(0.011)	(0.008)
+19	-0.004	0.020	-0.022**	0.012	-0.005
	(0.010)	(0.025)	(0.011)	(0.011)	(0.009)
+20	-0.015	-0.013	-0.011	0.019	-0.004
	(0.011)	(0.028)	(0.013)	(0.012)	(0.008)
+21	-0.020	-0.055	-0.018	0.037**	0.002
	(0.014)	(0.036)	(0.012)	(0.015)	(0.003)
+22	-0.040	-0.006	-0.044**	0.033	0.003
	(0.026)	(0.058)	(0.018)	(0.022)	(0.003)

Notes. * $p < 0.10$,** $p < 0.05$,*** $p < 0.01$. Coefficients estimated using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave FEs. Robust and asymptotic standard errors, obtained using Influence Functions. Data sources: *BES* and *ERM*.

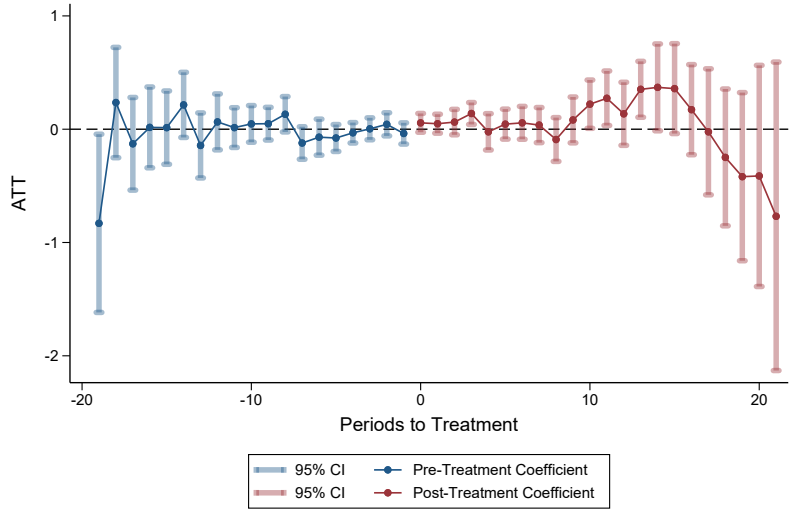
D.6 Focusing on More Salient Shocks

Figure D.5: Negative shocks and Probability to vote UKIP/Brexit Party. 200 jobs lost.



Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. Thick (thin) lines signify the 90% (95%) confidence interval.

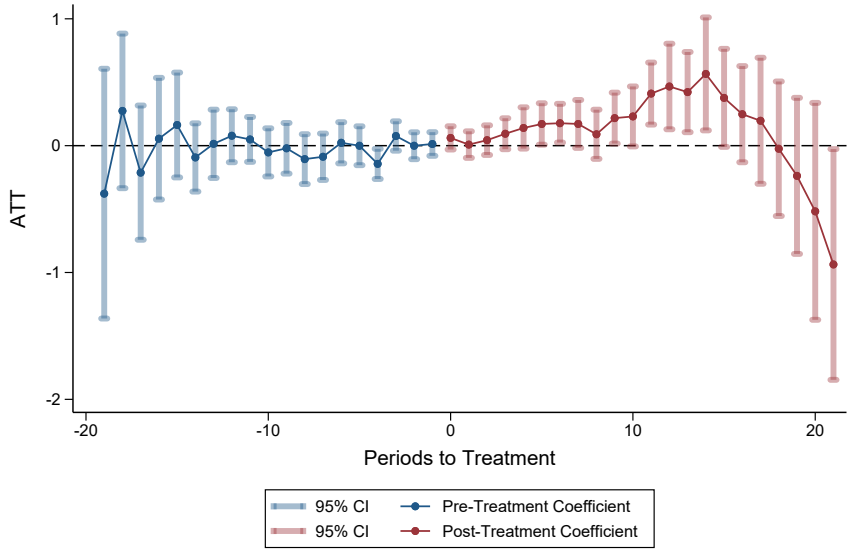
Figure D.6: Negative shocks and Probability to vote UKIP/Brexit Party. 300 jobs lost.



Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. Thick (thin) lines signify the 90% (95%) confidence interval.

D.7 Manufacturing Shocks Only

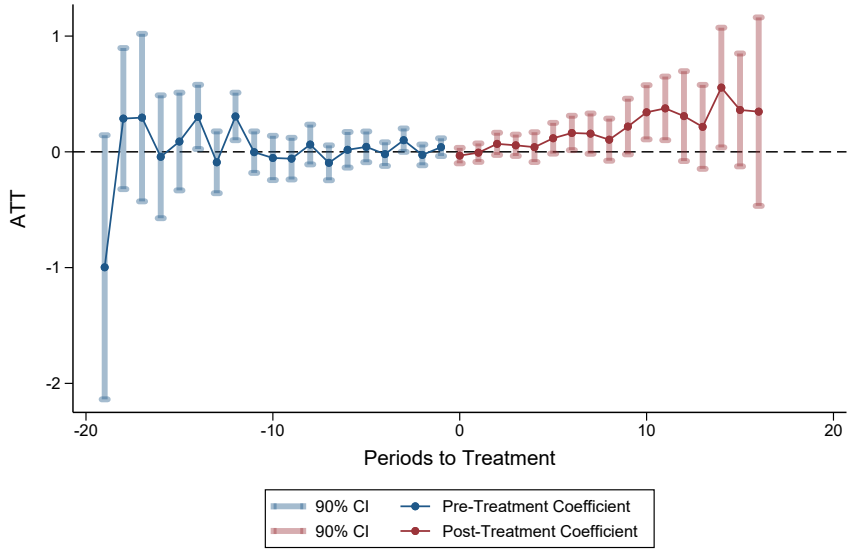
Figure D.7: Negative shocks and probability to vote UKIP/Brexit: Manufacturing only.



Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. Thick (thin) lines signify the 90% (95%) confidence interval. We include only LADs affected by ERM announcements involving the manufacturing sector.

D.8 UKIP Only

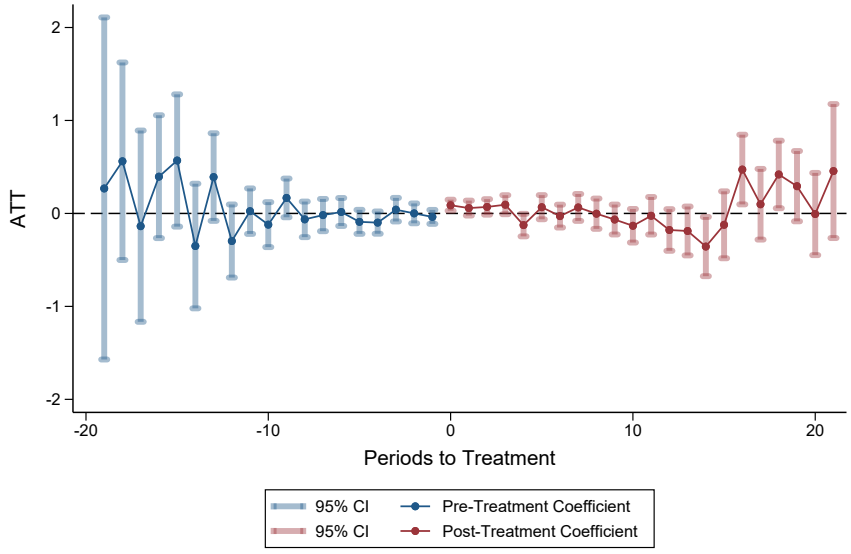
Figure D.8: Negative shocks and probability to vote UKIP (no Brexit Party).



Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. Thick (thin) lines signify the 90% (95%) confidence interval.

D.9 Positive Shocks

Figure D.9: Positive shocks and Probability to vote UKIP/Brexit Party.



Notes: Coefficients are estimated via Equation 2, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. Thick (thin) lines signify the 90% (95%) confidence interval.

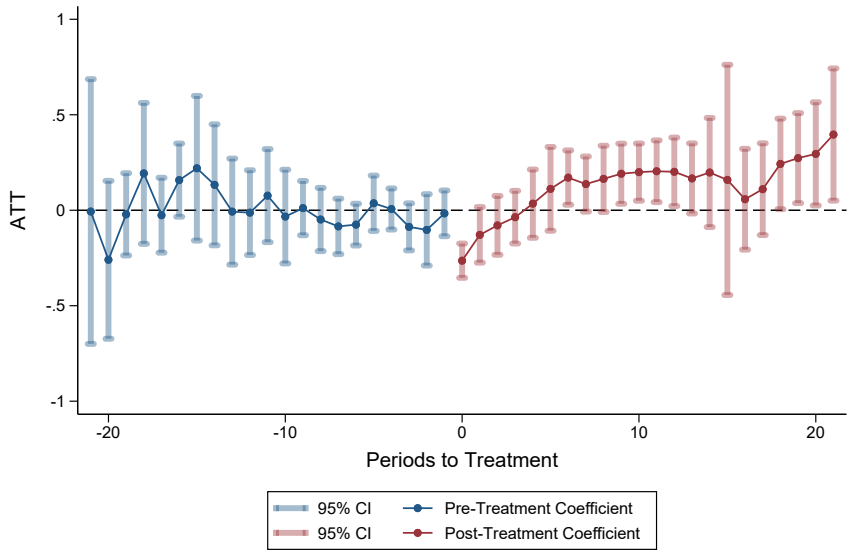
D.10 Personal Economic Shocks: Cross-Wave Unemployment

We estimate the impact of personal, rather than contextual, economic shocks by implementing again a staggered DID design Callaway and Sant'Anna (2021), and applying a doubly robust DID estimator based on stabilized inverse probability weighting and ordinary least squares (Sant'Anna and Zhao, 2020). We estimate the following equation, capturing the impact of becoming unemployed across two consecutive BES waves:

$$y_{i,d,w} = \delta_0 + \delta_1 \text{Job Loss}_{i,d,w-1} + \gamma_i + \eta_w + u_{i,d,w} \tag{3}$$

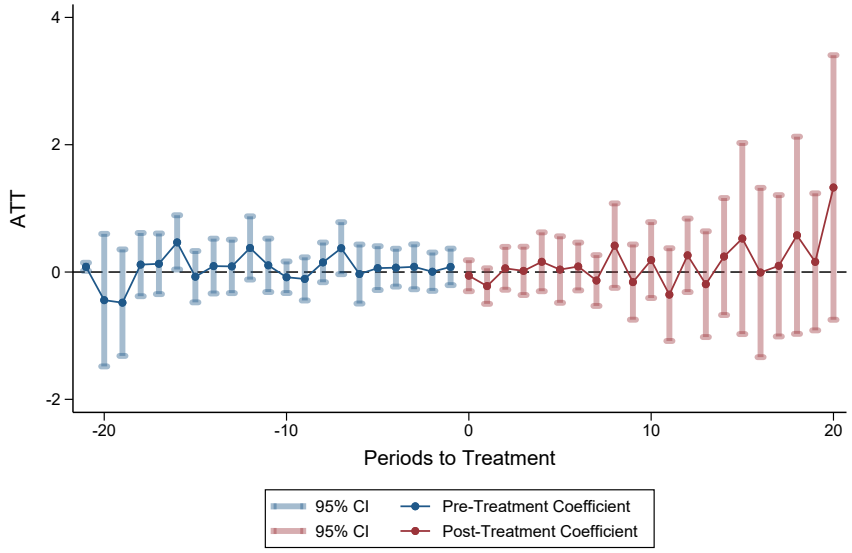
where $y_{i,d,w}$ is the outcome for respondent i from LAD d, in wave w. *Job Loss*$_{i,d,w-1}$ is an indicator equal to 1 if *i* reported being employed in wave *w-1* and is unemployed at the time of w (else 0). We restrict the sample to BES respondents interviewed in (at least) two consecutive waves who have answered the question about their employment status in both instances. γ_i and η_w are respondent and wave fixed effects. We employ the default robust and asymptotic standard errors, obtained using Influence Functions, as proposed by Callaway and Sant'Anna (2021).

Figure D.10: Employment shocks and egotropic economic response.



Notes: Coefficients are estimated via Equation 3, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. Thick (thin) lines signify the 90% (95%) confidence interval.

Figure D.11: Employment shocks and Probability to vote UKIP/Brexit Party.



Notes: Coefficients are estimated via Equation 3, using the staggered DID design proposed by Callaway and Sant'Anna (2021) and its relative Stata command (*csdid*). All specifications include respondent and wave fixed effects. Robust and asymptotic standard errors, obtained using Influence Functions. Thick (thin) lines signify the 90% (95%) confidence interval.

E Benchmarking the BES-ERM Dataset

In this section, we aim to replicate well-established correlations in the literature between individual attitudes and exposure to contextual economic shocks. Extant research has exploited China's accession to the World Trade Organization (WTO) as an instrument to estimate the causal, long-term impact of economic shocks on attitudes and voting, to find that globalization-related economic shocks increase support for PRR parties in Western Europe (Colantone and Stanig, 2018b), and for Brexit — whose key political proponent was UKIP — in the UK (Colantone and Stanig, 2018a). We ask whether our data allows us to replicate such a dynamic, which contrasts with our short-term results while being congruent with the longer-term ones.

Exploiting the panel structure of BES, we can capture within-respondent, cross-wave variation in attitudes, and study whether they are associated with the *intensity* of exposure to, *respectively*, negative and positive "shocks" at the LAD level in *any* BES wave preceding the interview. This specification, albeit non-causal in nature, is merely aimed at benchmarking our findings against existing scholarship, and at gaining insights into the political and electoral ramifications of economic shocks over the years. We estimate:

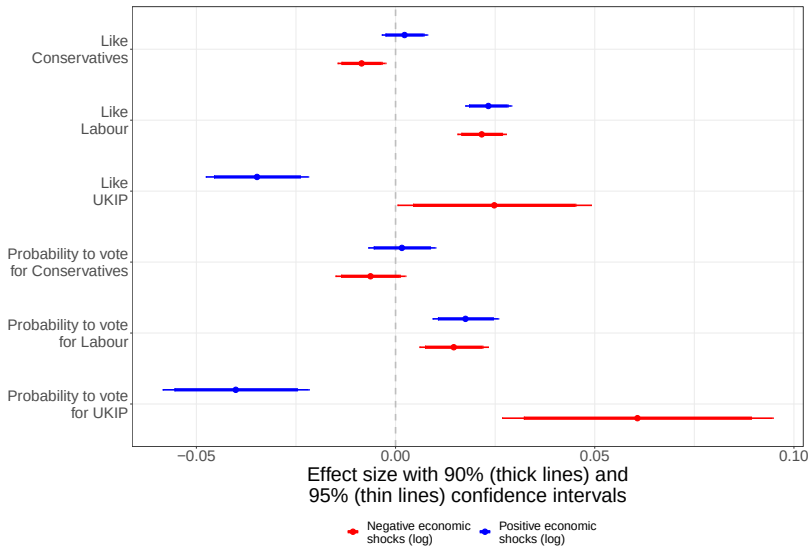
$$y_{i,d,w} = \alpha + \beta_1 \text{Shocks}_{d,w-1} + \delta_i + \theta_d + \lambda_w + \epsilon_{i,d,w} \tag{4}$$

Where $y_{i,d,w}$ is the outcome variable from respondent i, from LAD d, interviewed in wave w, $Shocks_{d,w-1}$ a count variable registering the log number of negative (positive) restructuring events occurring in the respondent's LAD until the wave preceding the interview $(w-1)$. δ_i, θ_d and λ_w capture, respectively, respondent, LAD and BES wave fixed effects.

In Figure E.1, we show evidence supporting the scholarly finding that the intensity of exposure to contextual negative (positive) economic shocks decreases (increases) support for the incumbent, *i.e.* the Conservative party, but not its electoral "viability." Interestingly, being exposed to a higher number of contextual economic shocks, regardless of their nature, increases support for Labour and,

possibly due to the longer distance between the shock and the recording of the responses, or to a cumulative effect, in the reported probability to vote for the party. Economic shocks, irrespective of their type, seem to benefit the center-left, mainstream opposition party. As for UKIP, we observe a situation in line with conventional wisdom. Specifically, being exposed to a larger number of positive economic announcements decreases the positive assessment and probability of voting for UKIP, whereas negative shocks have the opposite effect.

Figure E.1: Economic shocks intensity, political attitudes and voting intentions.



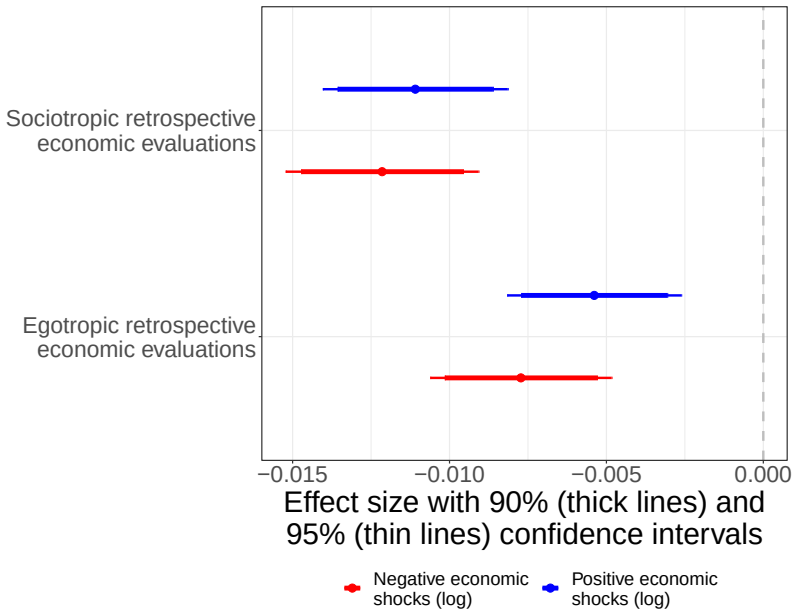
Notes: The dependent variable under scrutiny is reported in each row. All specifications include respondent, LAD and wave fixed effects. Thick (thin) lines signify the 90% (95%) confidence interval. Full regression tables in Appendix E.1.

These results clearly illustrate the theoretical problem illustrated by Margalit (2019a): it is *ex ante* difficult to make an educated guess about which party will benefit, at the expense of the incumbent, from a negative economic event. Radical right populist parties can gain consensus (and electoral support) in dire economic times. At the same time, the mainstream, center-left opposition could also benefit from negative economic shocks.

As a sanity check, we confirm that BES respondents living in LADs more impacted by negative restructuring events are more likely to report a deterioration in their retrospective egotropic and sociotropic economic evaluations (Figure E.2). Yet, as shown in Figure E.3, our analysis fails to

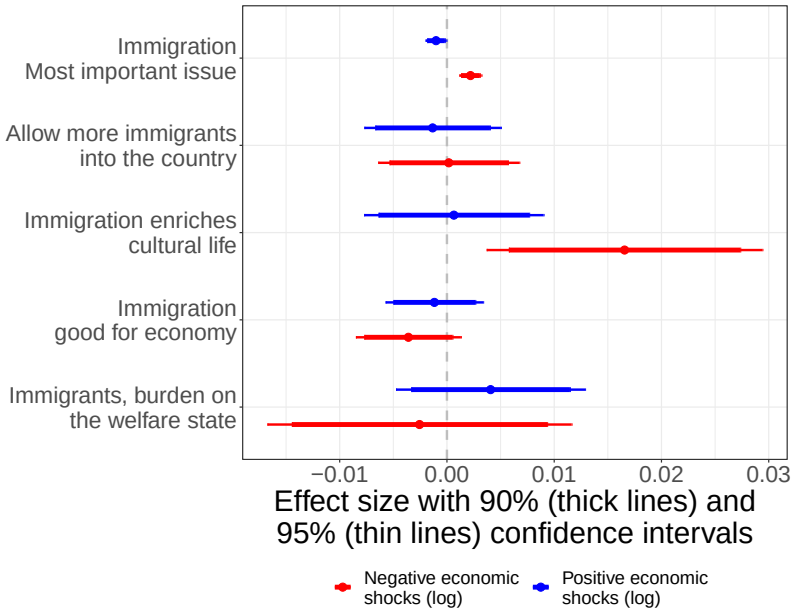
retrieve a significant link between the intensity of exposure to economic shocks and attitudes towards immigration (although the salience of this matter increases in more depressed areas).

Figure E.2: Economic shocks intensity, sociotropic and egotropic economic response.



Notes: The dependent variable under scrutiny is reported in each row. All specifications include respondent, LAD and wave fixed effects. Standard errors are clustered at the LAD level. Thick (thin) lines signify the 90% (95%) confidence interval. Full regression tables in Appendix E.2.

Figure E.3: Economic shocks intensity and attitudes towards immigration.



Notes: The dependent variable under scrutiny is reported in each row. All specifications include respondent, LAD and wave fixed effects. Standard errors are clustered at the LAD level. Thick (thin) lines signify the 90% (95%) confidence interval. Full regression tables in Appendix E.3.

E.1 Regression tables: Political attitudes and voting

Table E.1: Long-term impact of shocks: Probability of voting for Conservatives

	Probability of voting for Conservatives				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	−0.006				
	(0.004)				
Positive economic shocks (log)		0.002			
		(0.004)			
Bankruptcy/closure economic shocks (log)			−0.011**		
			(0.005)		
Internal restructuring economic shocks (log)				−0.003	
				(0.004)	
Offshoring/delocalization/outsourcing economic shocks (log)					0.004
					(0.006)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	439330	439330	439330	439330	439330
R-squared	0.845	0.845	0.845	0.845	0.845

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Probability of voting for Conservatives (0-10). The estimations include individual, survey wave, and LAD fixed effects.

Table E.2: Long-term impact of shocks: Probability of voting for Labour

	Probability of voting for Labour				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	0.015***				
	(0.004)				
Positive economic shocks (log)		0.018***			
		(0.004)			
Bankruptcy/closure economic shocks (log)			0.009*		
			(0.005)		
Internal restructuring economic shocks (log)				0.021***	
				(0.004)	
Offshoring/delocalization/outsourcing economic shocks (log)					0.011*
					(0.006)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	438850	438850	438850	438850	438850
R-squared	0.831	0.831	0.831	0.831	0.831

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Probability of voting for Labour (0-10). The estimations include individual, survey wave, and LAD fixed effects.

Table E.3: Long-term impact of shocks: Probability of voting for UKIP

	Probability of voting for UKIP				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	0.061***				
	(0.017)				
Positive economic shocks (log)		−0.040***			
		(0.009)			
Bankruptcy/closure economic shocks (log)			0.053***		
			(0.015)		
Internal restructuring economic shocks (log)				0.087***	
				(0.015)	
Offshoring/delocalization/outsourcing economic shocks (log)					0.133***
					(0.021)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	332883	332883	332883	332883	332883
R-squared	0.820	0.820	0.820	0.820	0.820

Note: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Probability of voting for UKIP (0-10). The estimations include individual, survey wave, and LAD fixed effects.

Table E.4: Long-term impact of shocks: Like/dislike - Conservatives

	Like/dislike - Conservatives				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	−0.009***				
	(0.003)				
Positive economic shocks (log)		0.002			
		(0.003)			
Bankruptcy/closure economic shocks (log)			−0.009***		
			(0.003)		
Internal restructuring economic shocks (log)				−0.010***	
				(0.003)	
Offshoring/delocalization/outsourcing economic shocks (log)					0.001
					(0.004)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	618546	618546	618546	618546	618546
R-squared	0.842	0.842	0.842	0.842	0.842

Note: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Like/dislike Conservatives (0-10). The estimations include individual, survey wave, and LAD fixed effects.

Table E.5: Long-term impact of shocks: Like/dislike - Labour

	Like/dislike: Labour				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	0.022***				
	(0.003)				
Positive economic shocks (log)		0.023***			
		(0.003)			
Bankruptcy/closure economic shocks (log)			0.023***		
			(0.003)		
Internal restructuring economic shocks (log)				0.028***	
				(0.003)	
Offshoring/delocalization/outsourcing economic shocks (log)					0.033***
					(0.004)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	617836	617836	617836	617836	617836
R-squared	0.814	0.814	0.814	0.814	0.814

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Like/dislike Labour (0-10). The estimations include individual, survey wave, and LAD fixed effects.

Table E.6: Long-term impact of shocks: Like/dislike - UKIP

	Like/dislike: United Kingdom Independence Party				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	0.025**				
	(0.012)				
Positive economic shocks (log)		−0.035***			
		(0.007)			
Bankruptcy/closure economic shocks (log)			0.023**		
			(0.010)		
Internal restructuring economic shocks (log)				0.063***	
				(0.011)	
Offshoring/delocalization/outsourcing economic shocks (log)					0.034**
					(0.016)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	454081	454081	454081	454081	454081
R-squared	0.832	0.832	0.832	0.832	0.832

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Like/dislike UKIP (0-10). The estimations include individual, survey wave, and LAD fixed effects.

E.2 Regression tables: Economic evaluations

Table E.7: Long-term impact of shocks: General economic retrospective evaluation - country

	General economic retrospective evaluation: country				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	−0.012***				
	(0.002)				
Positive economic shocks (log)		−0.011***			
		(0.002)			
Bankruptcy/closure economic shocks (log)			−0.011***		
			(0.002)		
Internal restructuring economic shocks (log)				−0.013***	
				(0.002)	
Offshoring/delocalization/outsourcing economic shocks (log)					−0.011***
					(0.002)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	514505	514505	514505	514505	514505
R-squared	0.653	0.653	0.653	0.653	0.653

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Sociotropic retrospective economic evaluations (1-5). The estimations include individual, survey wave, and LAD fixed effects.

Table E.8: Long-term impact of shocks: Personal economic retrospective evaluation - household

	Personal economic retrospective evaluation: household				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	−0.008***				
	(0.001)				
Positive economic shocks (log)		−0.005***			
		(0.001)			
Bankruptcy/closure economic shocks (log)			−0.004**		
			(0.002)		
Internal restructuring economic shocks (log)				−0.009***	
				(0.001)	
Offshoring/delocalization/outsourcing economic shocks (log)					−0.009***
					(0.002)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	528189	528189	528189	528189	528189
R-squared	0.560	0.560	0.560	0.560	0.560

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Egotropic retrospective economic evaluations (1-5). The estimations include individual, survey wave, and LAD fixed effects.

E.3 Regression tables: Immigration evaluations

Table E.9: Long-term impact of shocks: Most Important Issue - Immigration

	Most Important Issue - Immigration				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	0.002***				
	(0.001)				
Positive economic shocks (log)		−0.001**			
		(0.000)			
Bankruptcy/closure economic shocks (log)			0.001**		
			(0.001)		
Internal restructuring economic shocks (log)				0.003***	
				(0.001)	
Offshoring/delocalization/outsourcing economic shocks (log)					0.003***
					(0.001)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	667646	667646	667646	667646	667646
R-squared	0.531	0.531	0.531	0.531	0.531

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: most important issue - immigration (binary). The estimations include individual, survey wave, and LAD fixed effects.

Table E.10: Long-term impact of shocks: Immigrants - Allow More or Less

	Immigrants: Allow More or Less				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	0.000				
	(0.003)				
Positive economic shocks (log)		−0.001			
		(0.003)			
Bankruptcy/closure economic shocks (log)			−0.000		
			(0.004)		
Internal restructuring economic shocks (log)				−0.001	
				(0.003)	
Offshoring/delocalization/outsourcing economic shocks (log)					−0.010**
					(0.005)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	379316	379316	379316	379316	379316
R-squared	0.834	0.834	0.834	0.834	0.834

Note: *** $p < 0.01$; ** $p < 0.05$; * $p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Allow fewer/more immigrants into Britain (0-10). The estimations include individual, survey wave, and LAD fixed effects.

Table E.11: Long-term impact of shocks: Immigration - Enriches or undermines cultural life

	Immigration: Enriches or undermines cultural life				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	0.017**				
	(0.007)				
Positive economic shocks (log)		0.001			
		(0.004)			
Bankruptcy/closure economic shocks (log)			0.008		
			(0.006)		
Internal restructuring economic shocks (log)				0.010*	
				(0.006)	
Offshoring/delocalization/outsourcing economic shocks (log)					−0.018**
					(0.009)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	419027	419027	419027	419027	419027
R-squared	0.830	0.830	0.830	0.830	0.830

Note: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Immigration enriches or undermines cultural life (1-7). The estimations include individual, survey wave, and LAD fixed effects.

Table E.12: Long-term impact of shocks: Immigration - Bad or good for economy

	Immigration bad or good for economy				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	−0.004				
	(0.002)				
Positive economic shocks (log)		−0.001			
		(0.002)			
Bankruptcy/closure economic shocks (log)			−0.004		
			(0.003)		
Internal restructuring economic shocks (log)				−0.005*	
				(0.002)	
Offshoring/delocalization/outsourcing economic shocks (log)					−0.013***
					(0.003)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	470164	470164	470164	470164	470164
R-squared	0.809	0.809	0.809	0.809	0.809

Note: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Immigration bad or good for economy (1-7). The estimations include individual, survey wave, and LAD fixed effects.

Table E.13: Long-term impact of shocks: Immigrants - Burden on the Welfare State

	Immigrants: Burden on the Welfare State				
	(1)	(2)	(3)	(4)	(5)
Negative economic shocks (log)	−0.003				
	(0.007)				
Positive economic shocks (log)		0.004			
		(0.004)			
Bankruptcy/closure economic shocks (log)			−0.004		
			(0.006)		
Internal restructuring economic shocks (log)				0.003	
				(0.007)	
Offshoring/delocalization/outsourcing economic shocks (log)					−0.006
					(0.009)
Respondent (ID) FE	✓	✓	✓	✓	✓
LAD FE	✓	✓	✓	✓	✓
Survey wave FE	✓	✓	✓	✓	✓
Observations	264134	264134	264134	264134	264134
R-squared	0.822	0.822	0.822	0.822	0.822

Note: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$. Fixed effects estimations with standard errors in parentheses. Dependent variables for all models: Immigrants are a burden on the welfare state (1-5). The estimations include individual, survey wave, and LAD fixed effects.