

ORIGINAL ARTICLE

# The effect of the Brexit referendum result on subjective well-being\*

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## Abstract

We study the effect of the Brexit referendum result on subjective well-being in the United Kingdom. Using a quasi-experimental design, we find that the referendum's outcome led to an overall decrease in subjective well-being in the United Kingdom compared to a control group. The effect is driven by individuals who hold an overall positive image of the European Union and shows little signs of adaptation during the Brexit transition period. Economic expectations are potential mechanisms of this effect.

## KEYWORDS

Brexit, election, happiness, referendum, subjective well-being

## JEL CLASSIFICATION

D72; I30; I31; I38

## 1 | INTRODUCTION

Following a relatively heated campaign leading up to a referendum with a record turnout of 72.2%, the United Kingdom voted in favour of leaving the European Union (EU)—also known as ‘Brexit’—on

\*The authors are grateful to Chen et al. (2018b) and Dhingra et al. (2017) for sharing part of their data on the effects of Brexit on other countries. The authors thank Aris Angelis, Paul Dolan, Caterina Gennaioli, Carol Graham, Amanda Henwood, Laura Kudrna, Stella Ladi, Kate Laffan, Sara Machado, Pedro Martins, Laia Maynou-Pujolras, Robert Metcalfe and Olivier Wouters for useful comments and suggestions. Comments from three anonymous reviewers and the editor greatly improved the paper. Authors' names in alphabetical order. All authors contributed equally to the study. The research reported in this paper is not the result of a for-pay consulting project; the authors do not have a financial interest in the topic of this study and there is no conflict of interest.

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23 June 2016 with a majority of 51.9% of the votes. The referendum outcome was somewhat unexpected as opinion polls before June 2016 tended to favour 'Remain'. For example, YouGov polls (a popular opinion poll in the United Kingdom) on 25 July 2015, a year before the referendum, found Remain to be up 44% against Brexit 38%. This was also the case on 25 April 2016, just months before the referendum. This tendency for Remain was consistently reflected in betting odds as well (New Statesman, 2016; The Independent, 2016a; The Telegraph, 2016). Although Brexit built some momentum leading up to the referendum<sup>1</sup>, the outcome was still rather unexpected as exit-polls on referendum day were estimating a victory for Remain.<sup>2</sup>

This paper studies the effects of this referendum's result on subjective well-being—'experienced utility' (Kahneman et al., 1997)—in the United Kingdom. Our investigation is motivated by a number of unique characteristics that make the effects of the EU membership referendum on subjective well-being worth examining.

First, the Brexit referendum result is a major policy change with potentially significant socio-economic implications; see, for example, Sampson (2017) and an OECD report (Kierzenkowski et al., 2016) on the adverse economic consequences of Brexit; Dhingra et al. (2017) on the effects on trade, Driffield and Karoglou (2018) on inwards foreign direct investment and Breinlich et al. (2016a) and Breinlich et al. (2016b) on household income and inflation, respectively. Similar views had been echoed by the governor of the Bank of England, warning of lower living standards, higher inflation, job cuts and possibly a recession as a result of leaving the EU.<sup>3</sup> A UK government long-term analysis also suggested that any type of Brexit will adversely affect the economy (HM Government, 2018). Such economic effects have been shown to be directly related with reductions in subjective well-being (Boyce et al., 2013; De Neve et al., 2018; Di Tella et al., 2001; Kassenboehmer & Haisken-DeNew, 2009; Lucas et al., 2004; Luechinger et al., 2010). It is important to note though that not all of these, rather gloom, economic predictions have (yet) materialised, as EU law still applies in the United Kingdom until the end of 2020. There is, however, little doubt that the referendum's result led to an immediate devaluation of the Pound sterling, possibly due to the expected implications on trade and economic growth (Johnson & Mitchell, 2017), as well as unprecedented levels of economic uncertainty (Bloom et al., 2018).

Second, whereas general election cycles in modern representative democracies result in parties often alternating in office, leaving the EU is expected to be permanent. If voters are not happy with the government, they can choose to vote them out of office after a term, or at least they know that there is an option to remove the government from power in the next election, and laws can be changed by any future government. However, leaving the EU cannot be reversed; or at least re-joining the EU would not be straightforward. There is mixed evidence in the literature on the effect of holding office or of elections on subjective well-being. Di Tella and MacCulloch (2005) find increases in life satisfaction when the government is of the same ideological position as that of the respondent; Kinari et al. (2015) find supporters of the winning (losing) party to be significantly happier (unhappier); Pierce et al. (2016) find only 'partisan losers' to be negatively affected; Metcalfe et al. (2008) do not find any

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<sup>1</sup>For example, only days before the referendum both campaigns were at 45% each according to this poll (YouGov poll, 4 July 2016). YouGov surveys available via <https://whatukthinks.org/eu/questions/if-there-was-a-referendum-on-britains-membership-of-the-eu-how-would-you-vote-2/?pollster%5B%5D=yougov>.

<sup>2</sup>In fact, one of Brexit's most prominent campaigners had accepted defeat as the voting was coming to a close that day (The Independent, 2016b).

<sup>3</sup>See media articles, including BBC (2017a, b, c) and The Guardian (2017).

significant effects of election results.<sup>4</sup> Examining this referendum's result will thus offer us a better understanding of the effects of (claimed) irreversible voting outcomes on subjective well-being.

Third, and related to the above, this is a single-issue referendum, rather than an election on a broad range of proposed policies. The literature suggests that general election outcomes tend to have a rather short-term effect on subjective well-being (Kinari et al., 2015; Pierce et al., 2016). It is unclear whether—in this setting—this is still because of some innate tendency of individuals to adapt to various unfavourable outcomes (Bradford & Dolan, 2010; Loewenstein & Ubel, 2008; Oswald & Powdthavee, 2008; Wilson & Gilbert, 2008), or because the policies of mainstream political parties gradually converge—possibly due to the Median Voter Theorem (Matsusaka, 2005).<sup>5</sup> However, single-issue referenda capture only two, diametrically opposing, options with the decision-making process satisfied by the majority rule (May's Theorem, 1952), so are likely to leave a significant part of the voting population dissatisfied. Further note that this single-issue referendum led to rather high levels of polarisation, and a Member of Parliament was murdered just a week before the referendum.

Our study further relates to the literature on the link between *decision utility*—or *preference utility* (Adler, 2013)—and *experienced utility*. The results from this literature generally point towards a concordance between choices and subjective well-being which is, however, far from perfect and depends on the measure of subjective well-being used (Benjamin et al., 2012, 2014; Perez-Truglia, 2015). Voting is arguably the most fundamental expression of an individual's preferences in representative democracies, hence the study of voting outcomes and subjective well-being offers a suitable setting to study this relationship in larger populations.

In considering the effects of the Brexit result on subjective well-being, this study builds on the existing literature and makes the following contributions. First, using data from the Eurobarometer between 2015–2019, we consider the referendum as the 'event' of a quasi-experiment and apply a difference-in-differences approach, to identify its effects on subjective well-being of individuals in the United Kingdom compared to those in a control group of other European countries, which are not exposed to the treatment (referendum). A previous study on the Brexit referendum result uses a narrower outcome measure, physician antidepressant prescriptions (Vandoros et al., 2018); which, as the authors acknowledge, is not as informative in determining the well-being implications for society as whole and does not capture any changes in mood or mental health of people who do not take antidepressants. There have also been reports of individuals that have been experiencing mental health issues following the referendum (Katshu, 2019; The Independent, 2017). In a similar spirit to ours, Powdthavee et al. (2019) use subjective well-being measures as the outcome, but consider a UK-based control group in their difference-in-differences model. Despite the Brexit outcome being unexpected, the referendum itself was not an exogenous event to a UK-based control group. Introducing an external control group allows us to benchmark pre- and post-referendum trends in

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<sup>4</sup>The reverse relationship (i.e. subjective well-being affecting voting intentions/election outcomes) has also been the focus of several studies. Using British panel data Metcalfe et al. (2008) find that more satisfied Conservative supporters are less likely to vote compared to Labour supporters. This is in contrast to Liberini et al. (2017) who, using the same data, find that higher levels of life satisfaction are an important determinant of supporting the incumbent. Ward (2019) uses Eurobarometer data to study over 130 parliamentary elections in 15 European countries, finding a positive association between subjective well-being and the vote share received by the incumbent. Herrin et al. (2018) present similar findings for the 2016 US Presidential election. Liberini et al. (2019) study the effect of subjective well-being on stated preferences for Brexit, finding that dissatisfaction with one's financial situation, not life in general, to be an important determinant.

<sup>5</sup>Also referred to as Hotelling's principle of minimal differentiation.

subjective well-being in the United Kingdom against pooled pre- and post-referendum trends in other countries.

Second, we use a question on respondents' image of the EU to contemporaneously estimate heterogeneous effects of the referendum's result and also estimate how individuals' subjective well-being adapts depending on their image of the EU. Powdthavee et al. (2019) split their sample in control and treatment groups based on individuals' stated preference for Brexit in the second of their two-wave panel; thus, making the implicit assumption that respondents do not exhibit preference reversals between waves. Democracy, however, revolves around the transformation of peoples' voting preferences. Thus, to the extent that the unobserved (former wave) preferences for Brexit might have been different to those observed in the latter wave, this may have implications for their difference-in-differences estimates.

Third, following the related literature (e.g. Gerber & Huber, 2010) we consider economic expectations—both for the respondents' household (egotropic) and for the country as a whole (sociotropic)—for each group of respondents based on their image of the EU as potential mechanisms of the subjective well-being-Brexit relationship.

Our main finding is that the Brexit vote led to an overall decrease in subjective well-being in the United Kingdom, with little signs of adaptation within the span of our sample. The effect is mostly driven by individuals who hold an overall positive image of the EU. Economic expectations, especially the ones about the national economy, are potential drivers of this effect.

## 2 | EMPIRICAL STRATEGY

To investigate the effect of the 2016 EU referendum on subjective well-being we use data from the Eurobarometer Survey Series, the European Commission's public opinion survey. This is a cross-sectional face-to-face survey of individuals in EU member states conducted usually twice a year (spring and autumn) since the 1970s. About 1,000 respondents are interviewed in each country-wave on a range of topics including their satisfaction with life given by the question *on the whole, are you very satisfied, fairly satisfied, not very satisfied, or not at all satisfied with the life you lead?*. Such measures are increasingly being used in academic and policy circles to evaluate policy, intangibles and non-market goods; see for example, van Praag and Baarsma (2005), Dolan and Kahneman (2008), Luechinger and Raschky (2009), Metcalfe et al. (2011), Levinson (2012), and Dolan et al. (2019), as well as HM Treasury (2011), OECD (2013), and National Research Council (2013).

The empirical approach involves the estimation of a difference-in-differences (DiD) model, essentially comparing treatment and control groups before and after the intervention. This specification requires an appropriate control group, following the same trend as the treatment group (the United Kingdom) prior to the intervention (Brexit referendum) and not affected by the intervention itself. Challenged by a natural quasi-experimental design, we proceed by considering an objective measure developed by the IMF (Chen et al., 2018b) to determine each of the remaining 27 EU country's relationship to the United Kingdom.<sup>6</sup> This measure accounts for trade, financial linkages and migration. We subsequently split the EU 27 countries into quartile groups, from least linked (bottom quartile, Q1) to highly linked to the United Kingdom (upper quartile, Q4), and consider the bottom quartile, Q1, as the control group. This is justified under the assumption

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<sup>6</sup>This objective measure in determining the control group further reduces experimenter bias, such as determining some subjective criteria for countries to be included in the control group.

that countries within Q1 (including Austria, Bulgaria, Croatia, Greece, Poland, Romania and Slovenia) are the least likely to be affected by the outcome of the Brexit referendum; thus satisfying the stable unit treatment assumption, ruling out spillover effects to the group of untreated countries.<sup>7</sup>

This approach in determining the control group shares similarities with Ahlfeldt and Kavetsos (2014) who, in estimating the impact of regional interventions on property prices, consider the outer—more distant—properties from the intervention as the appropriate control group. In our study, note that ‘distance’ is a comprehensive measure of trade, financial linkages and migration, incorporated in the IMF’s measure. Nonetheless, these seven countries within Q1 are also geographically distant from the United Kingdom, which is an intuitively appealing property as well.

We use data spanning over the period 2015–2019, which includes waves 83.1 (Feb–Mar 2015), 83.3 (May 2015), 83.4 (May–June 2015), 84.3 (Nov 2015), 85.1 (Apr 2016), 85.2 (May 2016) and 85.3 (June 2016) before the referendum; and waves 86.1 (Sep–Oct 2016), 86.2 (Nov 2016), 86.3 (Nov–Dec 2016), 87.1 (Mar 2017), 87.3 (May 2017), 88.1 (Sep–Oct 2017), 88.2 (Oct 2017), 88.3 (Nov 2017), 89.1 (Mar 2018), 89.2 (Apr 2018), 89.3 (Jun 2018), 90.1 (Sep 2018), 90.2 (Oct 2018), 90.3 (Nov 2018), 90.4 (Dec 2018), 91.1 (Feb 2019) and 91.2 (Mar 2019) after the referendum. Our baseline specification is given by Equation (1):

$$SWB_{icmt} = \beta_0 + \beta_1 UK_{ic} \times PostBrexit_{mt} + \beta_2 UK_{ic} + \beta_3 PostBrexit_{mt} + X'_{icmt} \Gamma + \phi_c + \kappa_m + \xi_t + u_{icmt} \quad (1)$$

where  $SWB$  is the subjective well-being of respondent  $i$  at country  $c$ , interviewed in month  $m$  and year  $t$ ;  $UK$  is a time-invariant dummy variable equal to one if the respondent is interviewed in the United Kingdom and zero otherwise;  $PostBrexit$  is a dummy variable equal to one if the respondent is interviewed after the Brexit referendum result;  $X$  is a vector of individual characteristics/observables;  $\phi_c$  is the country fixed effect;  $\kappa_m$  is the month of interview fixed effect; and  $\xi_t$  is the year of interview fixed effect.<sup>8</sup>

In particular, vector  $X$  includes the following individual observables: gender, marital status, age and age squared, type of community, education, occupation and number of children aged under 14.<sup>9</sup> Note that we do not control for macroeconomic conditions, including GDP per capita, the unemployment rate and inflation rate. As these are likely to be affected by the outcome of interest (Johnson & Mitchell, 2017), they can be viewed as ‘bad controls’ (Angrist & Pischke, 2009), thus justifying their exclusion from the model. Robust standard errors clustered at the country-wave level are reported throughout.

Despite the dependent variable being ordinary, we estimate Equation (1) using OLS (see Ferreri-Carbonell & Frijters, 2004, for a discussion in this context); but also report estimates based on an ordered logit model.

We next test for heterogeneous effects of the referendum’s outcome on UK respondents. In the absence of a Brexit-specific question in a cross-country dataset such as the Eurobarometer, we use a proxy variable to capture respondents’ attitude towards the EU instead. This is not necessarily a

<sup>7</sup>Q2 includes France, Italy, Lithuania, Luxembourg, Portugal, Slovakia and Spain; Q3 includes the Czech Republic, Estonia, Finland, Germany, Hungary, Latvia and Sweden; and Q4, the group of countries mostly related to the United Kingdom according to the IMF’s measure, includes Belgium, Cyprus, Denmark, Ireland, Malta and the Netherlands.

<sup>8</sup>We do not control for day of week fixed effects due to data unavailability in some waves. Our results are, however, robust to the exclusion of these waves and the introduction of day-of-the-week fixed effects.

<sup>9</sup>Note that as of 2004 the Eurobarometer does not include a question on income. This is not necessarily a limitation of this study as income could be directly affected by the outcome of the vote and thus constitute a ‘bad control’ in the regression models (Angrist & Pischke, 2009).

limitation. Given the heated campaign leading up to this vote, as well as the ongoing equally heated debate on this matter, focusing on a Brexit-specific question could possibly make some respondents reluctant to reveal their true preference and/or sentiments towards the matter (Berinsky, 1999) and avoid being labelled as a ‘Brexit’ or a ‘Remainer’. A question on wider attitudes towards the EU might, thus, be considered to be appropriate.

There is considerable work in the related literature suggesting that attitudes towards the EU are multidimensional (see, e.g. Boomgaarden et al., 2011; Goldberg & de Vreese, 2018; Hobolt & Brouard, 2011). In the absence of such specific measures we consider a general one based on the image the respondent holds for the EU: *In general, does the EU conjure up for you a very positive, fairly positive, neutral, fairly negative or very negative image?*, included in every wave in our sample.<sup>10</sup> We assume it is unlikely for a respondent in the United Kingdom with a broadly positive EU image to have a preference for Brexit, and for a respondent with a broadly negative EU image to have a preference for Remain. We use the response categories to split the UK sample in sub-groups, each of which are then interacted with the post-treatment indicator to test for heterogeneous effects. This specification is given in Equation (2):

$$\begin{aligned}
 SWB_{icmt} = & \beta_0 + \beta_1 UKVeryPos_{ic} \times PostBrexit_{mt} + \beta_2 UKPos_{ic} \times PostBrexit_{mt} + \\
 & + \beta_3 UKNeutral_{ic} \times PostBrexit_{mt} + \beta_4 UKNeg_{ic} \times PostBrexit_{mt} + \\
 & + \beta_5 UKVeryNeg_{ic} \times PostBrexit_{mt} + \beta_6 UKVeryPos_{ic} + \beta_7 UKPos_{ic} + \\
 & + \beta_8 UKNeutral_{ic} + \beta_9 UKNeg_{ic} + \beta_{10} UKVeryNeg_{ic} + \beta_{11} PostBrexit_{mt} + \\
 & + X'_{icmt} \Gamma + \phi_c + \kappa_m + \xi_t + u_{icmt}
 \end{aligned} \tag{2}$$

Using the calendar dimension of the survey waves following the Brexit result, we next test for adaptation. As discussed, this is an important motivation of this study, due to this vote being based on a single-issue referendum with non-reversible and non-trivial implications. Due to the calendar proximity between, and sometimes overlap of, survey waves we pool waves together into seasons of the year—spring and autumn—and estimate the following model:

$$\begin{aligned}
 SWB_{icmt} = & \beta_0 + \beta_1 UK_{ic} \times Autumn2016_{mt} + \beta_2 UK_{ic} \times Spring2017_{mt} + \\
 & + \beta_3 UK_{ic} \times Autumn2017_{mt} + \beta_4 UK_{ic} \times Spring2018_{mt} + \\
 & + \beta_5 UK_{ic} \times Autumn2018_{mt} + \beta_6 UK_{ic} \times Spring2019_{mt} + \beta_7 UK_{ic} + \\
 & + \beta_8 Autumn2016_{mt} + \beta_9 Spring2017_{mt} + \beta_{10} Autumn2017_{mt} + \\
 & + \beta_{11} Spring2018_{mt} + \beta_{12} Autumn2018_{mt} + \beta_{13} Spring2019_{mt} + \\
 & + X'_{icmt} \Gamma + \phi_c + \kappa_m + \xi_t + u_{icmt}
 \end{aligned} \tag{3}$$

where *Autumn2016* is a dummy variable equal to one if the individual is interviewed after the referendum and during autumn 2016 (wave 86); and similarly for *Spring2017* (wave 87), *Autumn2017* (wave 88), *Spring2018* (wave 89), *Autumn2018* (wave 90) and *Spring2019* (wave 91).

<sup>10</sup>Note that when referring to any evidence based on ‘attitudes’ in this paper, we are referring to this specific measure studied here. Another candidate measure is one of EU identity *you feel you are a citizen of the EU: yes, definitely; yes, to some extent; no, not really; no, definitely not*. As mentioned, this captures only a particular dimension of attitudes towards the EU and results based on this measure cannot be generalised across attitudes (correlation coefficient with EU image measure is 0.49). Furthermore, this question is not available in every survey leading, decreasing our sample size by more than 55%. Results based on EU identity are along the same lines as those for EU image, and are available from the authors upon request.



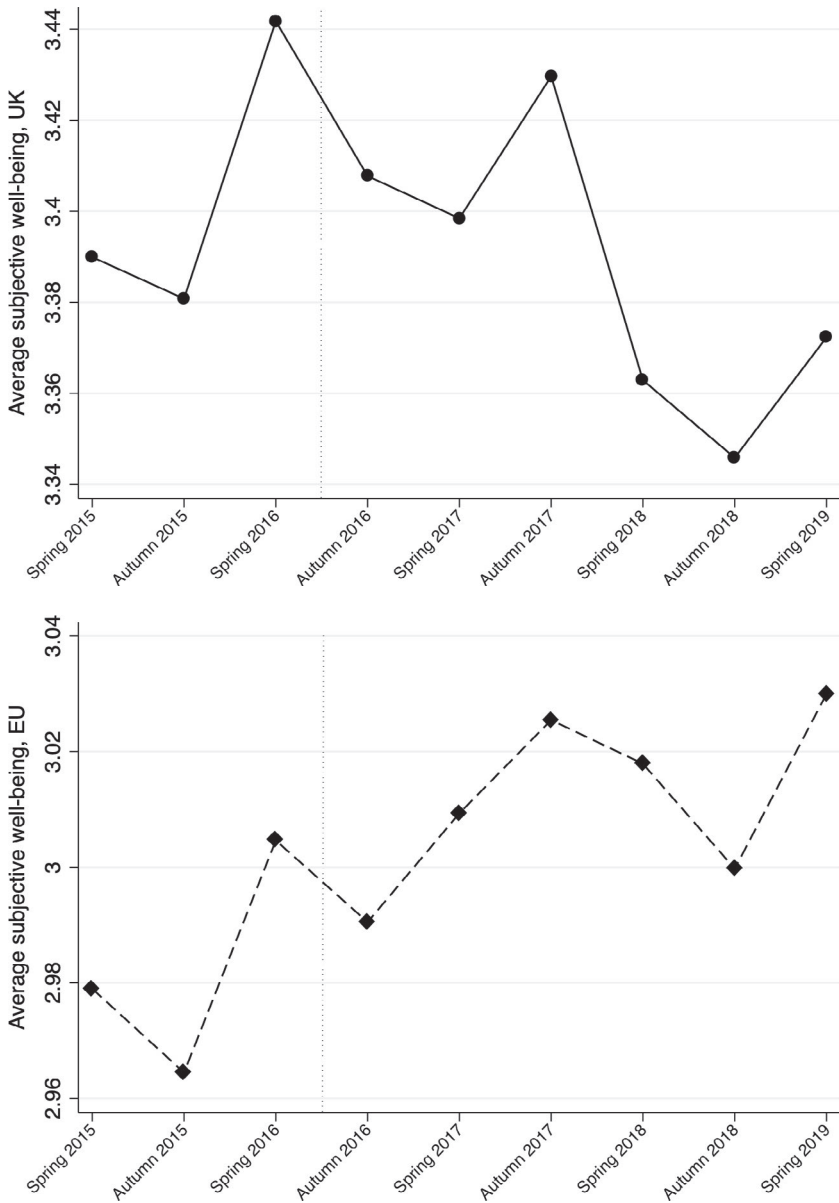


FIGURE 1 Average subjective well-being, UK (top) and EU (bottom)

### 3 | RESULTS

#### 3.1 | Graphical evidence

In Figure 1 we graph average subjective well-being for the United Kingdom and for the control group over the span of our sample. The vertical line separates the plot in the pre- and post-referendum periods. Treatment and control appear to follow the same trend before the referendum, satisfying the pre-trend assumptions for a DiD model; we discuss this more in Section 3.3 below where we offer formal

TABLE 1 Brexit result and subjective well-being

	OLS		Ologit	
	(1)	(2)	(3)	(4)
UK × Post Brexit	−0.06*** (0.02)	−0.061*** (0.019)	−0.183*** (0.058)	−0.203*** (0.056)
UK	0.582*** (0.019)	0.601*** (0.019)	1.798*** (0.06)	1.935*** (0.06)
Post Brexit	0.002 (0.023)	0.003 (0.023)	0.003 (0.061)	0.008 (0.064)
Individual Characteristics	No	Yes	No	Yes
Country FE	Yes	Yes	Yes	Yes
Month/Year FE	Yes	Yes	Yes	Yes
(pseudo) $R^2$	0.22	0.273	0.108	0.137
$N$	191,236	186,788	191,236	186,788

Notes: Regressions in columns 1–2 are OLS based on Equation (1) without and with individual characteristics; columns 3–4 are the equivalent ordered logit regressions. Robust standard errors clustered at the country-wave level reported in parentheses.

\*\*\* $p < 0.01$

tests for this assumption. The United Kingdom and control group trends clearly move in opposite directions following the referendum, with that of the United Kingdom clearly decreasing.

### 3.2 | Regression results

Table 1, columns 1 and 2, present the baseline regression estimates for Equation (1), where we initially estimate our model without the inclusion of individual characteristics.<sup>11</sup> Compared to the pre-Brexit period, subjective well-being in the United Kingdom decreases significantly post-Brexit by 0.06 points (column 1), with a marginal difference (0.061 points, column 2) once individual characteristics are considered. The estimated coefficients of an ordered logit model offer consistent results (columns 3–4).

Table 2 presents heterogeneous estimates based on UK respondents' image of the EU given by Equation (2). These suggest that the overall decrease in subjective well-being in the United Kingdom stems from those with generally positive attitudes towards the EU (*positive* and *very positive* categories), as well as those with *neutral* attitudes. In fact, the effect is larger (in absolute terms) the more positive these one's EU image is. In contrast, we do not find significant differences in subjective well-being post-Brexit vote for those with a generally negative EU image. Results are consistent in the OLS and the ordered logit models.

Table 3 presents the results for adaptation to the Brexit vote. For the UK sample as a whole the estimates suggest there are no significant effects in overall life satisfaction in autumn 2016 (the wave following the referendum) with significant reductions in all waves thereafter. The estimates are more pronounced the further away we move from the referendum, with the interaction effects associated

<sup>11</sup>As noted, our regression models exclude macroeconomic variables. Including these does not change the magnitude and statistical significance of the main coefficient of interest.



TABLE 2 Subjective well-being heterogeneous effects

	OLS	Ologit
	(1)	(2)
UK Very Positive × Post Brexit	−0.101 <sup>***</sup> (0.038)	−0.377 <sup>***</sup> (0.137)
UK Positive × Post Brexit	−0.082 <sup>***</sup> (0.026)	−0.276 <sup>***</sup> (0.079)
UK Neutral × Post Brexit	−0.076 (0.027)	−0.256 <sup>**</sup> (0.084)
UK Negative × Post Brexit	−0.032 (0.027)	−0.084 (0.088)
UK Very Negative × Post Brexit	−0.027 (0.044)	−0.132 (0.142)
UK Very Positive	0.238 <sup>***</sup> (0.048)	0.942 <sup>***</sup> (0.171)
UK Positive	0.119 <sup>***</sup> (0.039)	0.443 <sup>***</sup> (0.131)
UK Neutral	0.093 <sup>**</sup> (0.041)	0.334 <sup>**</sup> (0.14)
UK Negative	0.071 (0.043)	0.246 (0.149)
UK Very Negative	0.11 <sup>**</sup> (0.05)	0.051 <sup>**</sup> (0.168)
Post Brexit	0.004 (0.023)	0.013 (0.064)
Individual Characteristics	Yes	Yes
Country FE	Yes	Yes
Month/Year FE	Yes	Yes
(pseudo) $R^2$	0.272	0.137
$N$	184,354	184,354

Notes: Regressions based on Equation (2) (column 1 is OLS; column 2 is the equivalent ordered logit regression). Robust standard errors clustered at the country-wave level reported in parentheses.

\*\*\* $p < 0.01$ , \*\* $p < 0.05$

with spring and autumn 2018 being about twice as large as compared to those of spring and autumn 2017. These results, thus, do not offer evidence of adaptation in subjective well-being within the span of the sample.

We re-estimate this model by considering heterogeneous effects based on ‘EU image’ and plot the estimated coefficients (along with the 95% and 99% confidence intervals) in Figure 2; estimates are reported in Table A1 in the Appendix. The effect of the Brexit vote is generally more pronounced for those with overall positive (*very positive* and *positive* categories) and *neutral* image of the EU. For those with a relatively negative EU image, we generally do not find statistically significant effects.

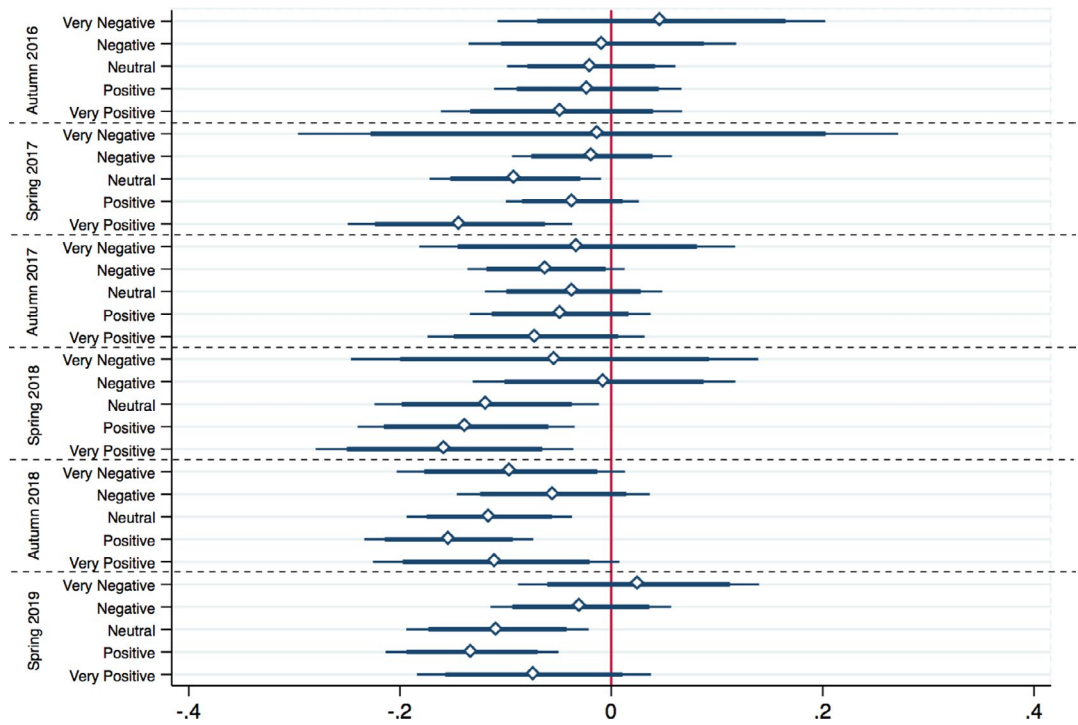
TABLE 3 Adaptation

	OLS	Ologit
	(1)	(2)
UK × Autumn 2016	−0.008 (0.032)	−0.032 (0.101)
UK × Spring 2017	−0.048** (0.02)	−0.159*** (0.054)
UK × Autumn 2017	−0.044** (0.021)	−0.135** (0.059)
UK × Spring 2018	−0.101*** (0.029)	−0.323*** (0.093)
UK × Autumn 2018	−0.104*** (0.023)	−0.355*** (0.069)
UK × Spring 2019	−0.063** (0.028)	−0.221*** (0.076)
UK	0.601*** (0.019)	1.936*** (0.061)
Autumn 2016	−0.004 (0.033)	−0.006 (0.091)
Spring 2017	0.075*** (0.018)	0.205*** (0.049)
Autumn 2017	0.08*** (0.029)	0.235*** (0.08)
Spring 2018	0.041** (0.018)	0.11** (0.051)
Autumn 2018	0.04 (0.025)	0.12 (0.071)
Spring 2019	0.048 (0.028)	0.145 (0.075)
Individual Characteristics	Yes	Yes
Country FE	Yes	Yes
Month/Year FE	Yes	Yes
(pseudo) $R^2$	0.273	0.137
$N$	186,788	186,788

Notes: Column 1 is OLS based on Equation (3). Column 2 is the equivalent ordered logit model. Robust standard errors clustered at the country-wave level reported in parentheses.

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ .

Overall, the lack of positively significant estimates for ‘partisan winners’—that is, those with relative negative attitudes in this case—is also documented in Pierce et al. (2016), who find only ‘partisan losers’ to be negatively affected, arguably due to the asymmetric impact losses have on subjective well-being as compared to gains (Boyce et al., 2013; De Neve et al., 2018).



**FIGURE 2** Heterogeneous effects following the referendum *Notes:* Diamonds represent OLS estimated coefficients. Thick lines either side of them represent the 99% confidence interval; thin lines either side of them represent the 95% confidence interval [Colour figure can be viewed at [wileyonlinelibrary.com](http://wileyonlinelibrary.com)]

### 3.3 | Threats to identification

#### 3.3.1 | Pre-referendum trends

The pre-intervention common trend between treatment and control is a key identifying assumption for the DiD model. For the pre-referendum period, Figure 1 offers some prima facie evidence of similar trends between subjective well-being in the United Kingdom and that of the group of control countries. We complement the graphical evidence by estimating a time-varying model similar to that of Autor (2003) and Ahlfeldt and Kavetsos (2014), with lags and leads associated with the treated group (i.e. the United Kingdom). Note that the leads of this model are the equivalent of our adaptation model given by Equation (3); and thus, to put it in different words, the lags are a model similar to Equation (3) where the pre-referendum periods are equally broken down into spring/autumn anticipatory waves. For the pre-referendum parallel trends assumption to hold, all lagged coefficients should be statistically insignificant. This is indeed the case for the coefficients of the lagged/anticipatory interaction terms for both the OLS and ordered logit model presented in Table 4. Note that the purpose of this exercise is not to focus on the lead estimates—which is the purpose of Equation (3), Table 3—yet note that the lead estimates are similar to those of our adaptation estimates, with any differences in statistical significance attributed to the change in the reference group associated with this model.

We extend the evidence above by performing additional tests for the common trend assumption between control and treatment groups for the pre-referendum data. To this end, we estimate three Equation (1)-type models between the following waves: ‘Spring 2015 and Autumn 2015 pooled vs.

TABLE 4 Time-varying regression for common trends

	OLS	Ologit
	(1)	(2)
<i>Lags:</i>		
UK × Autumn 2015	0.028 (0.032)	0.053 (0.088)
UK × Spring 2016	0.014 (0.032)	0.057 (0.089)
<i>Leads:</i>		
UK × Autumn 2016	0.002 (0.036)	0.001 (0.11)
UK × Spring 2017	-0.038 (0.026)	-0.127 (0.069)
UK × Autumn 2017	-0.034 (0.027)	-0.103 (0.073)
UK × Spring 2018	-0.091 <sup>***</sup> (0.034)	-0.291 <sup>***</sup> (0.102)
UK × Autumn 2018	-0.094 <sup>***</sup> (0.029)	-0.323 <sup>***</sup> (0.081)
UK × Spring 2019	-0.053 (0.033)	-0.189 <sup>**</sup> (0.087)
UK	0.591 <sup>***</sup> (0.026)	1.904 <sup>***</sup> (0.075)
Autumn 2015	0.033 (0.04)	0.113 (0.111)
Spring 2016	0.03 (0.024)	0.085 (0.063)
Autumn 2016	0.063 <sup>**</sup> (0.031)	0.204 <sup>**</sup> (0.085)
Spring 2017	0.073 <sup>***</sup> (0.018)	0.20 <sup>***</sup> (0.05)
Autumn 2017	0.116 <sup>***</sup> (0.035)	0.352 <sup>***</sup> (0.097)
Spring 2018	0.04 <sup>**</sup> (0.019)	0.105 <sup>**</sup> (0.052)
Autumn 2018	0.076 <sup>**</sup> (0.032)	0.237 <sup>***</sup> (0.089)
Spring 2019	0.046 (0.028)	0.14 (0.076)

(Continues)

TABLE 4 (Continued)

	OLS	Ologit
	(1)	(2)
Individual Characteristics	Yes	Yes
Country FE	Yes	Yes
Month/Year FE	Yes	Yes
(pseudo) $R^2$	0.273	0.137
$N$	186,788	186,788

Notes: Regression is OLS (column 1) and ordered logit (column 2). Robust standard errors clustered at the country-wave level reported in parentheses.

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ .

Spring 2016' (83 and 84 vs. 85); 'Spring 2015 vs. Autumn 2015 and Spring 2016 pooled' (83 vs. 84 and 85); and finally 'Spring 2015 vs. Spring 2016' (83 vs. 85). Compared to the results of Table 4, this exercise essentially exhausts all remaining combinations for the comparison of pre-referendum survey waves. Estimates for all interaction terms reported in Table 5 are statistically insignificant for both the OLS and ordered logit model, further strengthening the common trend identifying assumption.

There are, of course, inevitable limitations of data of this sort. The pre-treatment parallel trend assumption—a key identifying assumption of the DiD approach—is evidently not going to hold for an infinite number of time periods prior to the intervention. In this study, our pre-treatment period does not extend prior to 2015. Doing so would violate the parallel trend assumption between the treatment and control groups. In other words, adding more waves before 2015 would prevent us from having a valid counterfactual. As with any quasi-experimental study satisfying the parallel trend assumption, this would only be considered as a limitation if diverging trends in life satisfaction prior to 2015 reoccur in the post-treatment period due to some unobservable factor that is unrelated to the treatment. If that were the case, it would have implications for our identifying strategy and would, thus, render our control group unsuitable. This possibility has implications for our 'adaptation' estimates presented in Table 3, making one more inclined to rely on inferences for periods relatively closer to the intervention for which the counterfactual is more likely to be valid.

### 3.3.2 | Control group

As discussed in Section 2, our control group consists of countries in the lowest quartile of the distribution of a measure of socio-economic linkage to the United Kingdom calculated by the IMF (Chen et al., 2018b). Although the set of countries within this lowest quartile satisfying the pre-intervention common trend requirement for a DiD estimation, the question that remains is: how robust is this categorisation of countries within the lowest quartile as compared to other possible measures?

Our review of the literature in this area identified two additional measures that could be used instead. The first is described in Chen et al. (2018a), who develop a measure only based on trade links between the United Kingdom and other EU countries; the second is adopted by Dhingra et al. (2017) who, in addition to trade links, also account for fiscal transfers. The latter additionally consider the effects of a 'soft' and 'hard' Brexit, a terminology used to determine the success of the exit negotiations between the United Kingdom and the EU.

Our preference for the IMF measure in determining the control group for our analysis results from two considerations: (a) it is a measure accounting for trade, financial linkages and migration; and

TABLE 5 Pre-referendum common trends

	OLS			Ologit		
	(1)	(2)	(3)	(4)	(5)	(6)
<i>Waves:</i>	(83 & 84) vs. 85	83 vs. (84 & 85)	83 vs. 85	(83 & 84) vs. 85	83 vs. (84 & 85)	83 vs. 85
UK × Post	0.007 (0.028)	0.017 (0.029)	0.014 (0.031)	0.046 (0.072)	0.054 (0.074)	0.056 (0.081)
UK	0.542*** (0.022)	0.536*** (0.026)	0.541*** (0.026)	1.705*** (0.063)	1.693*** (0.073)	1.718*** (0.072)
Post	0.031 (0.024)	-0.008 (0.029)	0.03 (0.024)	0.085 (0.06)	-0.01 (0.077)	0.083 (0.06)
Individual Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Month/Year FE	Yes	Yes	Yes	Yes	Yes	Yes
(pseudo) $R^2$	0.276	0.276	0.274	0.137	0.137	0.136
$N$	57,081	57,081	49,039	57,081	57,081	49,039

Notes: Columns 1–3 are OLS; Columns 4–6 are ordered logit. Robust standard errors clustered at the country-wave level reported in parentheses.

\*\*\* $p < 0.01$

(b) offers data for all EU countries included in the Eurobarometer sample. Thus, it overall offers a more comprehensive measure both in terms of cross-country ‘linkage’ and data inclusiveness. Indeed, despite the very strong correlation between the measures (correlation coefficient between IMF and Chen et al. (2018a) is 0.865; and with Dhingra et al. (2017) under soft/hard Brexit is 0.987/0.99), Chen et al.’s (2018a) does not include Croatia, whereas Dhingra et al.’s (2017) does not include Bulgaria, Croatia, Cyprus, Estonia, Latvia, Lithuania, Luxembourg and Malta.

Table 6 lists the countries included in each measure, split into quartile groups based on the distribution of each measure: from least linked (bottom quartile, Q1) to most linked (upper quartile, Q4). Although the quartile split of countries by measure may varies, the objective of this exercise is to compare those within the least linked group, Q1. Of the IMF’s seven countries in Q1 listed in the final column, three (Greece, Romania and Slovenia) are in the Q1 of the other measures. Bulgaria is not included in the Dhingra et al. sample, making it another common measure between Chen et al. and the IMF measure. Croatia, included in the IMF’s analysis, is not part of the analysis of any of the other two measures. This leaves us with Austria and Poland for the IMF’s Q1: of which, Austria enters Q2 and Poland enters Q3 for both the other measures. Placing the focus on Chen et al.’s and Dhingra et al.’s list of Q1 countries, the differences with the IMF arise in the inclusion of Italy, Spain, Portugal and Cyprus in some of the former measures. Italy, Spain and Portugal enter Q2 for the IMF measure. Cyprus is the most notable quartile switch—from Q1 based on a trade measure (Chen et al. (2018a) to Q4 in the IMF measure—which might be not as surprising given the cultural links between the United Kingdom and Cyprus arguably incorporated through the migration aspect of the IMF measure.

In addition to the above arguments, we proceed by estimating our baseline specification (Equation 1) with respect to the lowest quartile group of countries for each of the other measures. The Table 1, ‘column 2-equivalent’ estimates for the main coefficient of interest are:  $-0.072$  ( $p < 0.01$ ,  $st.error = 0.02$ ) for Chen et al. (2018a), and  $-0.061$  ( $p < 0.01$ ,  $st.error = 0.021$ ) for both scenarios in Dhingra et al. (2017). The latter are precisely the same estimates as those in Table 1, column 2, confirming

TABLE 6 Exposure measures—quartiles

	Chen et al. (2018a)	Dhingra et al. (2017)		IMF
		Soft Brexit	Hard Brexit	(Chen et al., 2018b)
<i>Quartile</i>				
Q1	<b>Bulgaria</b>	<b>Greece</b>	<b>Greece</b>	Austria
	Cyprus	Italy	Italy	<b>Bulgaria</b>
	<b>Greece</b>	<b>Romania</b>	<b>Romania</b>	Croatia
	Italy	<b>Slovenia</b>	<b>Slovenia</b>	<b>Greece</b>
	Portugal	Spain	Spain	Poland
	<b>Romania</b>			<b>Romania</b>
	<b>Slovenia</b>			<b>Slovenia</b>
Q2	Austria	Austria	Austria	France
	Estonia	Finland	Finland	Italy
	Finland	France	France	<b>Lithuania</b>
	Latvia	Portugal	Portugal	Luxembourg
	<b>Lithuania</b>	Slovakia	Slovakia	Portugal
	Spain			Slovakia
Q3	<b>Czech Rep.</b>	<b>Czech Rep.</b>	<b>Czech Rep.</b>	<b>Czech Rep.</b>
	Denmark	Germany	Denmark	Estonia
	Hungary	Hungary	Germany	Finland
	Luxembourg	Poland	Poland	Germany
	Poland	<b>Sweden</b>	<b>Sweden</b>	Hungary
	Slovakia			Latvia
	<b>Sweden</b>			<b>Sweden</b>
Q4	<b>Belgium</b>	<b>Belgium</b>	<b>Belgium</b>	<b>Belgium</b>
	France	Denmark	Hungary	Cyprus
	Germany	<b>Ireland</b>	<b>Ireland</b>	Denmark
	<b>Ireland</b>	<b>Netherlands</b>	<b>Netherlands</b>	<b>Ireland</b>
	<b>Malta</b>			<b>Malta</b>
	<b>Netherlands</b>		<b>Netherlands</b>	

Notes: Chen et al.'s (2018a) measure includes 26 countries within our sample (except Croatia); Dhingra et al. (2017) includes 19 countries (except: Bulgaria, Croatia, Cyprus, Estonia, Latvia, Lithuania, Luxembourg, Malta); IMF's (Chen et al., 2018b) includes all 27 countries in our sample. Countries in bold fonts appear consistently in the same distributional quartile across measures.

the closer correlation between the IMF and Dhingra et al. (2017), both being more comprehensive measures as compared to Chen et al.

### 3.3.3 | Placebo outcomes

Finally, we repeat the estimation of our main specification by also considering placebo outcomes; that is, outcomes for which we do not have any prior to believe the Brexit result would have an effect upon.



The quest for such an outcome within our data is far from trivial, especially when considering the impact Brexit will have in UK policy-making; recall that the Eurobarometer is the EU Commission's public opinion poll. Also note that, irrespective of outcome, this latter still has to satisfy DiD's pre-intervention common trend assumption.

We initially considered a handful of outcomes, including whether individuals consider crime; terrorism; the environment, climate and energy issues; education; and pensions, respectively, to be an important issue in their country. Of these, only education satisfied the DiD common trend assumption. Given the binary outcome of this variable, we estimate Equation (1) using a logit model, yielding a statistically insignificant estimate for the main coefficient of interest ( $UKxPost=-0.088$ ,  $st.error=0.075$ ), offering further reassurance that the effects of the Brexit result on subjective well-being are not spurious. Given the availability of this outcome in those specific waves, we estimate a subjective well-being regression over that same sample for comparison purposes. Also, for direct comparison to the binary placebo regression, we create a binary life satisfaction variable by merging the relatively positive and negative life satisfaction response categories, respectively, and estimate a logit model. The estimate of the treatment effect is  $-0.17$  ( $p<0.01$ ,  $st.error=0.068$ ). Estimates are equally statistically significant if we use life satisfaction's four-scale response category.

Finally, we estimate Equation (1) with the EU image measure as the dependent variable. Our aim here is not to treat this measure as a placebo outcome per se, rather to test whether this attitude changes because of the referendum's outcome, thus having implications for our DiD estimates. The results of this regression—for which the identifying assumption of common pre-referendum trend holds—are reported in Table A2 in the appendix. The coefficient of the interaction variable is statistically insignificant in all models implying that EU image is not significantly affected by the referendum's outcome.

## 4 | MECHANISMS

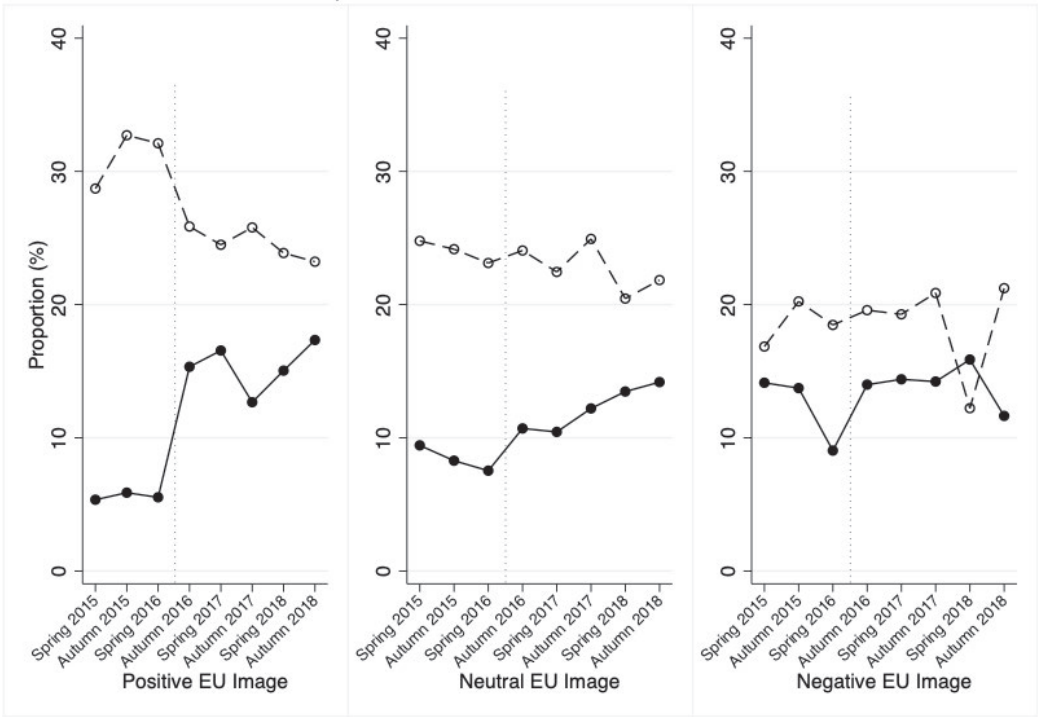
The purpose of this section is to consider plausible mechanisms explaining the estimated reduction in subjective well-being in the United Kingdom following the referendum. To this aim, we consider UK respondents' expectations for the next 12 months for (a) the financial situation of their household (egotropic belief) and (b) the economic situation of the country (sociotropic belief).<sup>12</sup> The related literature suggests that election outcomes determine such beliefs (e.g. Anderson et al., 2004; Glasgow & Weber, 2005; Gerber & Huber, 2010). Response categories to both questions are: *better*; *worse*; *the same*.

As the negative effect of the Brexit vote on subjective well-being seems to be driven by those with an overall positive or neutral image of the EU (see Table 2), we calculate the percentages for the *better* and *worse* response categories over time by respondents' attitude towards the EU and plot these in Figure 3. For a more informative picture, we merge the *very positive* and *positive* EU image categories, and similarly for the *very negative* and *negative* categories. For the case of expectations, we omit the same response category for two reasons: First, to de-clutter the graphs, as the sum of percentages amongst the three response categories adds up to 100 (the excluded category can, thus, be calculated); and second, given the pooled cross-sectional data the interpretation of the same is not trivially interpreted. Finally, our data stops in Autumn 2018 as expectations were not included in Spring 2019 surveys.

For the case of egotropic expectations (Figure 3, top row), the percentage of the sample for those with a positive EU image expecting their household finances to become worse was rather stable pre-referendum at around 5.5%; this tends to triple post-referendum. The opposite is true for those with

<sup>12</sup>Included in waves 81.2, 81.4, 82.3, 83.3, 84.3, 85.2, 86.2, 87.3, 88.3, 89.1 and 90.3.

Expectations: Household Financial Situation



Expectations: National Economy

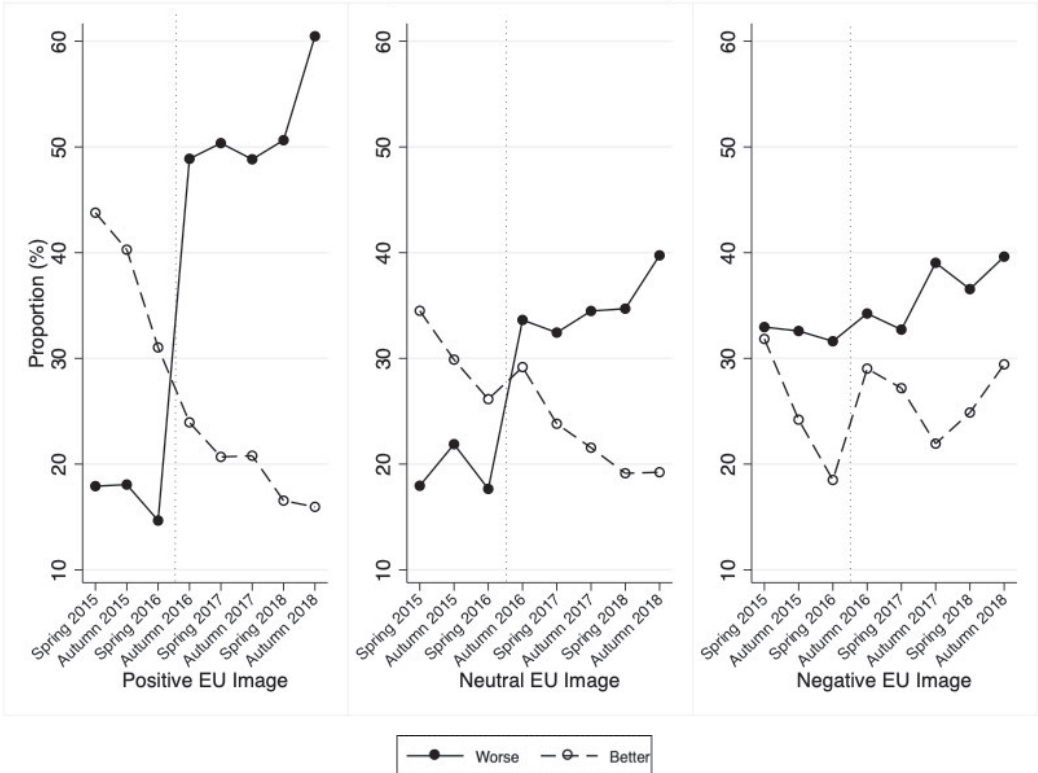


FIGURE 3 Egotropic (top row) and sociotropic (bottom row) economic expectations

positive expectations. A similar pattern is observed for those with a neutral EU image, although the changes in proportions here are not as large. For those with a negative EU image, there are some specific waves where the proportions might change (such as a large drop for those with positive expectations in Spring 2018, perhaps due to some uncertainty with the course of Brexit), but the overall trend in both lines is rather flat.

For the case of sociotropic expectations (Figure 3, bottom row), we observe more prominent changes. Considering the case of those with a positive EU image, the proportion of those with negative economic beliefs jumps up from around 18% pre-referendum to 50% post-referendum; reaching 60% in the final wave. We observe a similar change in proportions for those with a neutral EU image, which is again of a slightly smaller scale (from around 18% pre-referendum to around 35% post-referendum). For the case of those with a negative EU image we observe that the proportion of those expecting the economy to do better in the next 12 months decreases pre-referendum, but increases post-referendum: an expected result given their attitude for the EU. There is again a drop in autumn 2017, which might be due to the uncertainty surrounded with Brexit and the negotiations around the withdrawal agreement. At the same time, however, the proportion of those with a negative expectation also increases post-referendum. So these beliefs broadly appear to be cancelling each-other out.

The combined evidence in Figure 3 thus supports our earlier estimates finding significant reductions in subjective well-being for those with a positive and neutral EU attitude, but not for those with negative EU attitude. Our results are in line with Sorace & Hobolt, (forthcoming) who show increased pessimism about the economy shared amongst Remain voters.

## 5 | DISCUSSION

Over recent decades there has been considerable interest in reports of subjective well-being as a measure of individuals' experienced utility. Whereas more direct democracy leads to higher levels of subjective well-being (Frey & Stutzer, 2000), the evidence of the effect of such voting outcomes on subjective well-being is scarce. This paper studies the effect of the Brexit referendum result on subjective well-being. The rather unexpected result of this referendum is worth examining for at least three reasons. First, it has already had an adverse effect in the UK economy (Johnson & Mitchell, 2017), with future projections not being very positive<sup>13</sup>; second, it is a permanent decision, which has implications for the degree of adaptation to this outcome; and third, it was a single-issue referendum.

Using data from the Eurobarometer between 2015–2019 and applying a quasi-experimental design, we find that life satisfaction post-referendum significantly decreased in the United Kingdom as a whole as compared to a control group consisting of other EU countries. This estimate is not negligible; to put it in the context of subjective well-being magnitudes we look at relative effects with respect to other determinants. Based on this exercise, our estimate of the referendum's result on subjective well-being ( $-0.061$  from Table 1, column 2) is just as large as the coefficient of being widowed ( $-0.063$ ) and about 1/3 of that of being unemployed ( $-0.199$ ). The latter tends to have one of the largest adverse effects on subjective well-being, to which individuals do not seem to adapt to (see e.g. Knabe & Rätzl, 2011; Lucas et al., 2004) and thus offers a more reliable comparison for the purposes of estimating relative effects. We do not find evidence of adaptation to the referendum's outcome, at least not within the span of our sample focusing on the Brexit transition period.

<sup>13</sup>See, for example, the Bank of England's (2018) report commissioned by the House of Commons Treasury Committee and a recent analysis by the UK's HM Government (2018), both issued in November 2018.

Estimating heterogeneous effects of the Brexit result based on individuals' image of the EU suggests that those with an overall positive image report significantly lower levels of subjective well-being; with the effect being more pronounced the more positive one's image of the EU is. Under the assumption that individuals with an overall positive image towards the EU do not have a preference for Brexit, then these estimates are perhaps not as surprising. More importantly, and in contrast to other studies in the literature that find a rather fast adaptation process to voting outcomes (Kinari et al., 2015; Pierce et al., 2016; Powdthavee et al., 2019), we do not generally find the levels of subjective well-being to adapt within our sample's time span.

In addition to preferences not being met, this finding might to some extent reflect the affective reaction and the uncertainty during the transition period to exit the EU. First, individuals tend to have a greater affective reaction to unexpected events and to novel, as compared to recurrent, events (Wilson & Gilbert, 2008); the Brexit result is an example of both of these points. Second, uncertainty has a detrimental effect on economic activity (Born & Pfeifer, 2014; Fernández-Villaverde et al., 2015)—especially during times of political uncertainty (Julio & Yook, 2012)—and tends to rise during a recession and/or after a significant shock in the economy (Bloom, 2014). Uncertainty affects peoples' mood by preventing hedonic adaptation (e.g. people adapt faster to the certainty of an adverse health state than facing the uncertainty of being or not being in such a state: Frederick & Loewenstein, 1999), and has been shown to even lead to suicide (Vandoros et al., 2019). 'Policy uncertainty indices', based on media reports, suggest that uncertainty negatively affects investment and consumption (Baker et al., 2016). Such an index rose to unprecedented levels in the United Kingdom following the Brexit vote, above the levels reached during the recent financial crisis (Bloom et al., 2018). The multifaceted implications on Brexit (e.g. related to regulation, access to the EU market, etc), alongside the complex and long exit negotiation period between the United Kingdom and the EU, arguably amplifies uncertainty.<sup>14</sup> Using data from the Decision Maker Panel—a representative survey of businesses in the United Kingdom—Bloom et al. (2018) find that about 40% of UK businesses face Brexit-related uncertainty.

Our results for those with an overall negative EU image do not support the reverse: corresponding estimates of subjective well-being are not statistically different compared to the control group. To this extent, our findings are similar to Pierce et al. (2016) who for the 2012 US Presidential election find the subjective well-being of 'partisan losers' (Republicans) to be negatively affected following the election, but that of 'partisan winners' (Democrats) remaining unaffected. Some coefficients are seldomly negative and significant, perhaps reflecting some sort of a distress or unmet expectations following the evolving exit negotiations and perhaps even a kind of dissatisfaction from deviating away from a 'hard(er) Brexit'.

This study took a leap into considering plausible mechanisms of the subjective well-being-Brexit effects. To this end, we consider both egotropic and sociotropic expectations of finances and the economy, respectively. This analysis indicates that those with a positive and neutral image of the EU are consumed by negative economic expectations, and especially so for the sociotropic domain.

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<sup>14</sup>Note that Theresa May, the UK Prime Minister between July 2016–July 2019, had to deal with several ministerial resignations in her cabinet and faced herself a no-confidence vote by members of the Conservative Parliamentary party in December 2018, followed by a motion of no-confidence in the House of Commons in January 2019. She survived both votes but was eventually forced to resign in June 2019 as her EU withdrawal agreement had failed to receive a majority of votes in three successive occasions. The United Kingdom went through snap elections in June 2017 under Theresa May's Premiership (2 years after the previously held general elections of May 2015) and again in December 2019 following Parliamentary deadlock over Brexit under Boris Johnson's Premiership.

This study, like any other, is not free of limitations. The Eurobarometer data we use are being broadly conducted in the spring and autumn of every year. With the EU membership referendum taking place in July 2016, we do not estimate the immediate aftermath of the Brexit result on subjective well-being; including its effect on heterogeneous sub-groups. Further note that we do not have information on what UK respondents in our sample voted for in the referendum, let alone whether they voted at all. To the extent that those who do vote are considered to be rational utility maximisers, information on their actual vote could allow us to more formally test the connection between decision/preference and experienced utility in this setting.

These inferences are instead drawn based on a general attitude individuals in our sample hold towards the EU; and although being rather unlikely, say, for a UK individual with a positive image of the EU having a preference for Brexit (Macdougall et al., 2020), we cannot strictly exclude the reverse case for individuals with a generally negative image. To illustrate, consider the case of a UK individual with a strongly negative EU image: this may stem from a dissatisfaction with certain EU policies, but not necessarily with a preference for Brexit per se. In other words, a negative EU image might be capturing a view for a ‘different Europe’, not ‘less Europe’ (Hobolt & Brouard, 2011). This argument might indeed, partly, justify why our regression results estimate a significant decrease in subjective well-being for those with a positive EU image (i.e. offering evidence of preference and experience utility coinciding), and an overall lack of increased subjective well-being for those with a negative EU image despite the United Kingdom now ‘taking back control’. One should, however, weigh this limitation of observing specific preferences towards Brexit against the possibility of individuals feeling reluctant to state their sentiments in a Brexit-specific question; not least, to avoid being labelled as a ‘Brexit’ or a ‘Remainer’ given the polarised sentiment around the time of the referendum.

Finally, expectations of own finances/the economy are certainly not fully or adequately exploring the mechanisms between the Brexit vote and subjective well-being. The descriptive evidence presented here is only suggestive of such; an investigation of a more comprehensive list of beliefs as plausible channels could be the focus of future research.

Notwithstanding these issues, this study offers robust evidence of an overall decrease in subjective well-being for individuals in the United Kingdom during the Brexit transition period. With the arguably devastating implications of the Coronavirus 2019 pandemic (COVID-19) on peoples’ lives and the economy—affecting countries within the European continent in the early months of 2020, thus overlapping with the UK’s official departure from the EU in January 2020—a study focusing on Brexit beyond the transition period (i.e. 2016–2019) will be nothing less than empirically challenging.

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**How to cite this article:** Kavetsos G, Kawachi I, Kyriopoulos I, Vandoros S. The effect of the Brexit referendum result on subjective well-being. *J R Stat Soc Series A*. 2021;184:707–731. <https://doi.org/10.1111/rssa.12676>

## APPENDIX

TABLE A1 Adaptation—Heterogeneous effects

	OLS	Ologit
	(1)	(2)
UK very positive × Autumn 2016	-0.047 (0.044)	-0.186 (0.169)
UK very positive × Spring 2017	-0.143*** (0.041)	-0.565*** (0.151)
UK very positive × Autumn 2017	-0.071 (0.04)	-0.283** (0.142)
UK very positive × Spring 2018	-0.158*** (0.047)	-0.518*** (0.169)
UK very positive × Autumn 2018	-0.109** (0.045)	-0.394** (0.154)
UK very positive × Spring 2019	-0.073 (0.043)	-0.326** (0.142)
UK positive × Autumn 2016	-0.022 (0.034)	-0.091 (0.12)
UK positive × Spring 2017	-0.037 (0.024)	-0.116 (0.069)
UK positive × Autumn 2017	-0.048 (0.033)	-0.157 (0.09)
UK positive × Spring 2018	-0.137*** (0.04)	-0.431*** (0.121)
UK positive × Autumn 2018	-0.154*** (0.031)	-0.53*** (0.102)
UK positive × Spring 2019	-0.132*** (0.032)	-0.457*** (0.085)
UK neutral × Autumn 2016	-0.019 (0.036)	-0.06 (0.096)
UK neutral × Spring 2017	-0.091*** (0.031)	-0.288*** (0.09)
UK neutral × Autumn 2017	-0.036 (0.032)	-0.116 (0.103)
UK neutral × Spring 2018	-0.118*** (0.041)	-0.402*** (0.137)
UK neutral × Autumn 2018	-0.115*** (0.03)	-0.401*** (0.094)
UK neutral × Spring 2019	-0.108*** (0.033)	-0.339*** (0.094)
UK negative × Autumn 2016	-0.008 (0.049)	-0.023 (0.152)
UK negative × Spring 2017	-0.018 (0.029)	-0.032 (0.102)
UK negative × Autumn 2017	-0.062** (0.029)	-0.156 (0.095)
UK negative × Spring 2018	-0.007 (0.048)	0.013 (0.159)
UK negative × Autumn 2018	-0.055 (0.035)	-0.171 (0.112)
UK negative × Spring 2019	-0.029 (0.033)	-0.117 (0.098)
UK very negative × Autumn 2016	0.047 (0.06)	0.086 (0.192)
UK very negative × Spring 2017	-0.013 (0.109)	-0.115 (0.353)
UK very negative × Autumn 2017	-0.032 (0.057)	-0.154 (0.181)
UK very negative × Spring 2018	-0.054 (0.074)	-0.202 (0.252)
UK very negative × Autumn 2018	-0.095** (0.042)	-0.321** (0.132)
UK very negative × Spring 2019	0.026 (0.044)	0.038 (0.134)
Individual characteristics	Yes	Yes
Country FE	Yes	Yes
Month/year FE	Yes	Yes
(pseudo) $R^2$	0.273	0.138
$N$	184,354	184,354

Notes: Regression is OLS (by combining Equations (2) and (3)). Column 2 is the equivalent ordered logit model. Robust standard errors clustered at the country-wave level reported in parentheses. Roots of interaction terms are included in the regressions, but not reported for brevity.

\*\*\* $p < 0.01$ , \*\* $p < 0.05$ .

TABLE A2 Brexit vote and 'EU image'

	OLS		Ologit	
	(1)	(2)	(3)	(4)
UK × Post Brexit	0.007 (0.029)	0.006 (0.029)	0.018 (0.059)	0.017 (0.059)
UK	-0.313*** (0.027)	-0.279*** (0.028)	-0.566*** (0.055)	-0.522*** (0.057)
Post Brexit	0.106*** (0.032)	0.105*** (0.032)	0.202*** (0.06)	0.205*** (0.061)
Individual characteristics	No	Yes	No	Yes
Country FE	Yes	Yes	Yes	Yes
Month/Year FE	Yes	Yes	Yes	Yes
(pseudo) $R^2$	0.076	0.113	0.027	0.042
$N$	194,607	190,233	194,607	190,233

Notes: Regressions in columns 1–2 are OLS based on Equation (1) with the dependent variable being 'EU image', without and with individual characteristics; columns 3–4 are the equivalent ordered logit regressions. Robust standard errors clustered at the country-wave level reported in parentheses.

\*\*\* $p < 0.01$