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**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 this outcome led to an overall decrease in subjective well-being in the UK compared to a control group. The effect is driven by individuals who hold an overall positive attitude towards the EU and shows little signs of adaptation. Subjective well-being of those with a very negative attitude towards the EU increases in the short-run but turns negative, possibly due to unmet expectations. Using three different measures of socio-economic connection between the UK and other European countries, we generally do not find evidence supporting the presence of spillover effects of the Brexit referendum result on subjective well-being of individuals in other EU countries.

Key words: subjective well-being, happiness, Brexit, referendum, election

JEL Codes: D72; I30; I31; I38

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# 1 Introduction

Following a relatively heated campaign leading up to a referendum with a record turnout of 72.2%, the UK voted in favour of leaving the European Union (EU)—also known as ‘Brexit’—on 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 UK) on 24 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.<sup>1</sup> Although Brexit built some momentum leading up to the referendum<sup>2</sup>, the outcome was still rather unexpected as exit-polls on referendum day were estimating a victory for Remain.<sup>3</sup>

This paper studies the effects of this referendum’s result on subjective well-being—“experienced utility” (Kahneman et al., 1997)—in the UK. 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, and Breinlich et al. (2016a) and Breinlich (2016b) on household income and inflation, respectively. Similar views have 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>4</sup> More recently, a UK government long-term analysis suggested that any type of Brexit will adversely affect the economy.<sup>5</sup> These factors 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 and 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; there is, however, little doubt that the referendum’s result led to an immediate devaluation of the pound sterling’s, possibly due to the expected implications on trade and economic growth (Johnson and Mitchell, 2017).

Second, whereas general election cycles in modern representative democracies result in parties often alternating in office, leaving the EU is expected to be permanent. 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

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<sup>1</sup>See articles in the media, including the [NewStatesman](#), [The Independent](#), and [The Telegraph](#).

<sup>2</sup>E.g., 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>3</sup>In fact, one of Brexit’s most prominent campaigners had accepted defeat as the voting was coming to a close that day. See article in [The Independent](#).

<sup>4</sup>See media articles, including BBC (2017a, 2017b, 2017c) and [The Guardian](#).

<sup>5</sup>See 2018 [EU Exit report](#).

not find any significant effects of election results.<sup>6</sup> The effects of 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 literature also suggests that 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 (Loewenstein and Ubel, 2008; Oswald and Powdthavee, 2008; Wilson and Gilbert, 2008; Bradford and Dolan, 2010), or because the policies of mainstream political parties have gradually converged—possibly due to the Median Voter Theorem (Matsusaka, 2005).<sup>7</sup> Referenda capture only two, diametrically opposing, options with the decision-making process satisfied by the majority rule (May’s Theorem, 1952), so are often 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 relationship 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 (e.g., 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, 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 UK compared to those in 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). This study, 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. In a similar spirit to ours, Powdthavee

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<sup>6</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 contrary to Liberini et al. (2017a) who, using the same data, find that higher levels of life satisfaction are an important determinant of supporting the incumbent. Ward (2015) 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. (2017b) 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>7</sup>Also referred to as Hotelling’s principle of minimal differentiation.

et al. (2017) 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 subjective well-being in the UK against pooled pre- and post-referendum trends in other countries.

Second, we use a question on respondents' feelings of being an EU citizen to contemporaneously estimate heterogeneous effects of the referendum's result and also estimate how individuals' subjective well-being adapts depending on their attitudes towards the EU. Powdthavee et al. (2017) 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. To the extent that the unobserved (former wave) preferences for Brexit might have been different to those observed in the latter wave, this would have implications for the difference-in-differences estimates.

Third, our choice of a European-wide dataset is motivated by an additional research question. With Brexit arguably having implications for EU's integration and economy, it is plausible for it to have an effect on other EU countries. Following an approach similar to Dolan et al. (2016), we introduce separate treatment effects to test for any spillover effects of the referendum's result on subjective well-being in other EU countries. We use three measures to determine the economic 'exposure' of a country to the UK—advanced by Chen et al. (2018a), the IMF (Chen et al., 2018b) and Dhingra et al. (2017)—which we use to split countries in our sample into four groups of exposure. We subsequently include as separate treatment effects with the group least exposed acting as the reference group.

Our main finding is that the Brexit result led to an overall decrease in subjective well-being in the UK. The effect is mostly driven by individuals who hold an overall positive attitude towards the EU, with little signs of adaptation within the span of our sample (up to autumn 2017). For those with very negative attitudes towards the EU, we find short-term increases in subjective well-being, followed by significant decreases, perhaps because of some sort of distress or unmet expectations of a 'hard(er) Brexit'. Overall, we do not find much evidence supporting the presence of spillover effects in other countries; which only appear to be present for the group of countries mostly connected to the UK in only one (the narrowest) of the three measures of socio-economic exposure considered here. It is important to note at this point that as Brexit has not yet officially occurred, what we are essentially measuring are the effects of the referendum result and during the initial stages of the 'Brexit transition period'.

The rest of this study is structured as follows. Section 2 describes the data and empirical methods; Section 3 presents the results and robustness tests; and Section 4 discusses our findings and concludes.

## 2 Empirical Strategy

### 2.1 Data and Methods

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), HM Treasury (2011), OECD (2013), and National Research Council (2013).

We use data spanning over the period 2015-2017, 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) and 88.3 (Nov 2017) after the referendum. The following countries are included in every wave in our sample: Austria, Belgium, Bulgaria, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden, and the United Kingdom.

We estimate a difference-in-differences (DiD) model, where the treatment group consists of individuals interviewed in the UK and the control group consists of individuals interviewed in the 27 remaining EU countries in our data. Our baseline specification is given by equation 1:

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

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 UK and zero otherwise;  $PostBrexit$  is a dummy variables 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;  $\xi_t$  is the year of interview fixed effect<sup>8</sup>; and  $Macro$  is a vector of some key macroeconomic indicators to control for different macroeconomic conditions in these

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<sup>8</sup>We do not control for day of week fixed effects due to data unavailability in waves 83.3 and 85.2. Our results are, however, robust to the exclusion of these waves and the introduction of day-of-the-week fixed effects.

countries over time.

In particular, vector  $X$  includes the following individual observables: gender, marital status, age and age squared, type of community, education, occupation, social status<sup>9</sup>, number of children aged under 14, number of people present during the interview and the time of the interview. The vector  $Macro$  consists of the monthly unemployment rate, monthly inflation rate and quarterly GDP per capita obtained from Eurostat (Di Tella et al., 2001). Robust standard errors are clustered at the country-wave level and are reported throughout.

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:

$$\begin{aligned}
SWB_{icmt} = & \beta_0 + \beta_1 UK_{ic} \times Autumn2016_{mt} + \beta_2 UK_{ic} \times Spring2017_{mt} + \\
& + \beta_3 UK_{ic} \times Autumn2017_{mt} + \beta_4 UK_{ic} + \beta_5 Autumn2016_{mt} + \\
& + \beta_6 Spring2017_{mt} + \beta_7 Autumn2017_{mt} + Macro_{cmt} + \\
& + X'_{icmt} \Gamma + \phi_c + \kappa_m + \xi_t + u_{icmt}
\end{aligned} \tag{2}$$

where  $Autumn2016$  is a dummy variable equal to one if the individual is interviewed after the referendum and during autumn 2016 (wave 86);  $Spring2017$  is a dummy variable equal to one if interviewed in May 2017 (wave 87), a year after the referendum; and  $Autumn2017$  is a dummy variable equal to one if interviewed in autumn 2017 (wave 87).

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’ attitudes towards the EU instead. This is not necessarily a 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 ‘Brexiteer’ or a ‘Remainer’. A question on wider attitudes towards the EU might, thus, be considered to be appropriate.

This variable is available in only some of these waves and captures respondents’ feelings of being an EU citizen given by the response to the following question “*You feel you are a*

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<sup>9</sup> Note that as of 2004 the Eurobarometer does not include a question on income. We proxy, to some extent, the relationship between subjective well-being and income by including a subjective measure of social status (“*Do you see yourself and your household belonging to: (a) working class, (b) lower middle class, (c) middle class, (d) upper middle class, or (e) higher class?*”). Such measures have been shown to reflect respondents’ relative socio-economic status as compared to their peers or to their own past situation, and arguably are a more important determinant of subjective well-being than absolute income (Ferrer-i-Carbonell, 2005; Luttmer, 2005; Clark et al., 2008; Cheung and Lucas, 2016).

*citizen of the EU: Yes, definitely; Yes, to some extent; No, not really; No, definitely not*".<sup>10</sup>

We assume it is unlikely for a respondent in the UK with broadly positive feelings of being an EU citizen to have a preference for Brexit, and for a respondent with broadly negative feelings to have a preference for Remain.

We use this measure to split the UK sample in heterogeneous sub-groups, each of which are then interacted with the post-treatment indicator. This specification is given in equation 3:

$$\begin{aligned}
SWB_{icmt} = & \beta_0 + \beta_1 UKVeryPos_{ic} \times PostBrexit_{mt} + \beta_2 UKPos_{ic} \times PostBrexit_{mt} + \\
& + \beta_3 UKNeg_{ic} \times PostBrexit_{mt} + \beta_4 UKVeryNeg_{ic} \times PostBrexit_{mt} + \\
& + \beta_5 UKVeryPos_{ic} + \beta_6 UKPos_{ic} + \beta_7 UKNeg_{ic} + \beta_8 UKVeryNeg_{ic} + \\
& + \beta_9 PostBrexit_{mt} + Macro_{cmt} + X'_{icmt} \Gamma + \phi_c + \kappa_m + \xi_t + u_{icmt}
\end{aligned} \tag{3}$$

We then test for adaptation following the Brexit vote for each group of respondents based on this measure.

## 2.2 Spillover Effects

Our final model considers the international implications of the UK's referendum for EU membership. There is no precedent of a country exiting the EU<sup>11</sup>; thus the Brexit vote is, arguably, capable of having socio-economic and political implications that might have a spillover effect on subjective well-being of other EU member countries.

Examples of such an approach include Kim and Kim (2018) who test the effects of the Charlie Hebdo shooting on mental health in France, and also consider the spillover effects of this adverse event on neighbouring countries (Belgium, Germany, and Switzerland); and Dolan et al. (2016) who, in an approach similar in spirit to ours, estimate the subjective well-being impact of hosting the 2012 Olympics in London (treatment group) compared to Paris and Berlin (pooled control group), and then subsequently include Paris as a separate treatment, in addition to London, due to its earlier interest and bid to host the 2012 Games and keep only Berlin as the control group.

Determining the countries that Brexit could have a spillover effect on is not as trivial an exercise and is an approach that is susceptible to speculative arguments. For example, in contrast to Kim and Kim (2018), we do not have any prior reason to believe that neighbouring countries are necessarily more closely related to the UK and, consequently, more likely to be affected in this setting; thus, a distance-based measure seems to be a rather inappropriate approach here. Moreover, even if countries plausibly affected by Brexit could

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<sup>10</sup>Waves including this question are: 83.3 (May 2015); 84.3 (Nov 2015); 85.2 (May 2016); 86.2 (Nov 2016); 87.3 (May 2017); and 88.3 (Nov 2017).

<sup>11</sup>Greenland, that left the then European Economic Community in 1985, is a constituent country of the Kingdom of Denmark.

be identified, the spillover effect flowing to these might differ in terms of intensity.

Here, our approach towards spillover effects is summarised as follows. With much of the discussion on Brexit being within the sphere of its economic consequences, we consider spillover effects in terms of measures of economic integration between the UK and other EU countries. Our review of the literature in this topic identified three main papers which we consider for completeness and robustness purposes. These include Chen et al. (2018a), who develop a measure capturing trade links between the UK and the EU; Dhingra et al. (2017), who develop a measure accounting for trade and fiscal transfers; and an IMF report (Chen et al., 2018b), which develops a more comprehensive measure accounting for trade, financial linkages, and migration.

Rather than applying subjective cut-off points to allocate countries in different groups of intensity to Brexit, we overcome this challenge by splitting each measure into four groups of countries depending on the quartiles of the respective distributions of those measures. Finally, we include the top three quartiles given by each index as separate treatment effects and estimate our DiD model with the first, least ‘exposed’, quartile being the reference group. Our model of spillover effects extends the baseline specification and is given in equation 4:

$$\begin{aligned}
SWB_{icmt} = & \beta_0 + \beta_1 UK_{ic} \times PostBrexit_{mt} + \beta_2 Q2_{ic} \times PostBrexit_{mt} + \beta_3 Q3_{ic} \times PostBrexit_{mt} + \\
& + \beta_4 Q4_{ic} \times PostBrexit_{mt} + \beta_5 UK_{ic} + \beta_6 Q2_{ic} + \beta_7 Q3_{ic} + \beta_8 Q4_{ic} + \beta_9 PostBrexit_{mt} + \\
& + Macro_{cmt} + X'_{icmt} \Gamma + \phi_c + \kappa_m + \xi_t + u_{icmt}
\end{aligned} \tag{4}$$

where  $Q_{j=[2,4]}$  are time-invariant dummy variables takes the value of one if the respondent was interviewed in the country whose economy is ‘highly linked’ ( $Q4$ ), ‘linked’ ( $Q3$ ), and ‘not linked’ ( $Q2$ ) to the UK; and zero otherwise. ‘Least linked’ ( $Q1$ ) being the reference group of countries.

Further note that in considering a different potential set of countries for the control group resulting under  $Q1$  for each index, this specification implicitly serves as a robustness test to our baseline model.

### 2.3 Identifying Assumptions

The key identifying assumption of the DiD design is that—controlling for the variables we noted previously—the treatment group would have similar trends to the control group in the absence of the treatment; that is, in the absence of the referendum taking place.

The pre-treatment period in Figure 1 offers some *prima facie* evidence of similar trends between subjective well-being in the UK and the that of the remaining EU countries. We proceed by estimating placebo-time treatment regressions at different points in time pre-referendum as the means of providing more robust empirical evidence on the trend between

control and treatment pre-referendum. As most pre-treatment surveys are only weeks apart, we pool them together as follows: those conducted during spring 2015 (i.e., the surveys included in Eurobarometer 83 (83.1, 83.3, 83.4)); the survey carried out in autumn 2015 (i.e., 84.3); and those conducted during Spring 2016 (i.e., Eurobarometer 85 surveys (85.1, 85.2 and 85.3)). We then estimate five equation 1-type models between waves 85 vs. 84, 84 vs. 83, 85 vs. 83, 85 vs. 83 and 84 pooled, and finally 84 and 85 pooled vs. 83.

If the DiD identifying assumption holds, there should be no statistically significant difference in the estimated coefficient between the treatment and the control group in these placebo-time treatment regressions. This is indeed what we find in all cases we examine, suggesting that the identifying assumption holds; see Table A1 in the appendix.

## 3 Results

### 3.1 Graphical evidence

In Figure 1 we graph average subjective well-being for the UK and for the EU27 over the span of our sample. Treatment and control follow the same trend before the referendum. In the immediate period following the referendum (autumn 2016), we observe a pronounced drop in subjective well-being in the UK as compared to the EU, with a further decrease being observed about a year after the referendum (spring 2017). Subjective well-being in the UK appears to be improving by autumn 2017.

Figure 2 shows average subjective well-being right before (spring 2016) and right after (autumn 2016) the referendum, in the UK and the EU27. Although we are comparing two time periods spanning over different seasons of the year, we are interested in the immediate effect the Brexit vote had on different groups of people, based on their feelings of being an EU citizen.

Individuals who definitely do not feel they are an EU citizen report higher levels of subjective well-being on average in autumn 2016 as compared to spring 2016 (3.275 before and 3.435 after), while those who definitely do feel they are an EU citizen report lower levels (3.541 before and 3.457 after). The direction of these average changes for these groups on either end of the spectrum provides some first evidence of an alignment between preference utility and experience utility.

Those with less strong feelings show smaller changes on average. The subjective well-being of those who say they do not really feel they are an EU citizen remained about the same (3.386 before; 3.382 after) and of those who do feel an EU citizen to some extent demonstrated a decrease (from 3.478 to 3.450), which was, however, smaller than the decrease reported by those with a very positive attitude towards the EU.

In contrast, average subjective well-being exhibits small changes in between these two periods in the EU27. Here, there is an increase for people who definitely do not feel they are an EU citizen (2.475 before; 2.527), but this increase is much lower than in the UK,

both in absolute and percentage terms. Subjective well-being of those who answered that they definitely or to some extent feel they are an EU citizen remained practically the same (definitely: 3.232 before; 3.230 after. To some extent: 3.006 before; 3.001 after), as opposed to the decrease observed in both groups within the UK. Those not really feeling citizens of the EU reported an increase (2.747 before; 2.797 after) as opposed to a practically no change within the UK.

## 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 macroeconomic controls. Compared to the pre-Brexit period, subjective well-being in the UK decreases significantly post-Brexit by 0.062 points (column 1) and by 0.055 points once macroeconomic controls are considered (column 2).

Table 1, columns 3 and 4, present a similar set of results testing for adaptation to the Brexit result based on the estimation of equation 2. The effect of the Brexit result appears to become even more pronounced in the short term (from autumn 2016 to spring 2017) and there does not seem to be evidence of adaptation within the span of our sample although the effect is somewhat smaller by autumn 2017. More specifically, focusing on the more complete model that includes macroeconomic controls (column 4), in autumn 2016, a few months following the referendum, subjective well-being in the UK decreased by 0.032 points. This coefficient broadly doubled in the spring (-0.078) and autumn (-0.064) of the following year.

Next, we test for heterogeneous effects based on UK respondents' feelings of being an EU citizen, given by equation 3. The results, shown in Table 2 column 1, suggest that the overall decrease in subjective well-being in the UK estimated previously stems from those with positive feelings. In fact, the effect is larger (in absolute terms) the more positive one's feelings are; for example, it more than doubles from the 'positive' to the 'very positive' categories. On the contrary, we do not find significant differences in subjective well-being for those with a negative attitude towards the EU.

Table 2, column 2, presents heterogeneous effects on adaptation to the Brexit result. The effect on subjective well-being generally seems to consistently become more pronounced over time for those with overall positive (i.e., the 'very positive' and 'positive' groups) feelings of being an EU citizen, thus are far from showing any signs of adaptation. The interesting finding here arises for those 'definitely not feeling' a citizen of the EU (the 'very negative' group). For these individuals, we find a significant increase in subjective well-being in autumn 2016, arguably capturing the satisfaction of their preferences following the referendum's result. This increase (0.114) is about as large (in absolute terms) as the decrease in subjective well-being for the 'very positive' group (-0.125) over the same time period (i.e. autumn 2016). By spring 2017, however, the sign of this coefficient reverses to

a significant reduction, and this might be due to either to some sort of distress or unmet expectation resulting from the exit negotiations. The effect turns statistically insignificant by autumn 2017.

Given these heterogeneous effects of the Brexit result in the UK, the question that naturally arises is whether subjective well-being of UK respondents with a certain feeling towards being an EU citizen is significantly different from that of their EU counterparts with whom they share the same feelings. We answer this question by separately stratifying the sample by respondents’ feelings and re-estimating equation 1. Results are presented in Table 3 and suggest that UK respondents with overall positive feelings (columns 1-2) are significantly less satisfied with their life compared to their EU counterparts. The reverse does not appear to be the case for those holding overall negative feelings (columns 3 and 4), whose subjective well-being is not significantly different even when compared to that of EU respondents holding equally negative feelings.

### 3.3 Spillover Effects

This section presents the results for potential spillover effects of Brexit. We consider three measures based on Chen et al. (2018a), Dhingra et al. (2017), and the IMF (Chen et al., 2018b). The country-composition of each measure is slightly different. The IMF’s includes all 27 countries in our sample and, compared to the other two, is a more comprehensive measure which accounts for trade, financial linkages, and migration. Chen et al.’s (2018a) includes 26 countries in our sample, with the exception of Croatia (correlation coefficient with IMF’s measure is 0.865). Dhingra et al.’s (2017) includes 19 countries in our sample, with the exception of Bulgaria, Croatia, Cyprus, Estonia, Latvia, Lithuania, Luxembourg, and Malta. It also considers the effects of a “soft” and “hard” Brexit—a terminology used to determine the success of the exit negotiations between the UK and the EU (correlation coefficient with IMF’s measure is 0.987 and 0.99 under a “soft” and “hard” Brexit, respectively); we consider both scenarios for completeness and for robustness purposes.

Table 4 lists the countries included in each measure, split into four quartile groups based on the distribution of each measure: from least affected (bottom quartile, Q1) to most affected (upper quartile, Q4). The quartile split of each measure results in similarities, but also important differences. For example, Greece, Romania and Slovenia are consistently in Q1 across all measures considered; as is the case for the Czech Republic and Sweden in Q2 and Belgium, Ireland and the Netherlands in Q4. Some notable differences include Cyprus (which falls under Q1 in Chen et al. (2018a), but under Q4 in IMF’s measure) and Poland (which falls under Q3 in Chen et al. (2018a) and Dhingra et al. (2017), but under Q1 in IMF’s measure). In the majority of cases however, countries switch by one quartile group between the different measures.

Table 5 reports the estimated results following the estimation of equation 4. Note that the estimate of our main treatment effect,  $UK \times Post$  Brexit, is robust and even somewhat

more pronounced to that of the baseline model (Table 1, column 2). The effect of the Brexit result in the UK is somewhat reduced when considering the Dhingra et al. index, which is however based on a smaller sample of countries. Given each measure’s different composition of countries within the first quartile group (Q1) acting as the reference group, these estimates offer robust and compelling evidence of the effect of the Brexit result on subjective well-being in the UK.

Overall and across measures, the spillover estimates do not suggest that the Brexit result has a significant effect in the subjective well-being of other countries irrespective of their degree of association to the UK. The only exception within this investigation is the fourth quartile under the Chen et al. (2018a) index; for this group of countries more closely related to the UK, Brexit led to a significant reduction in subjective well-being. This effect is not negligible and is about half the effect estimated for the UK.

### 3.4 Robustness

As discussed, the estimates of our main treatment effect are robust to the inclusion of macroeconomic controls and to different compositions of a reference group, as evidenced in the spillover regressions. In this section, we offer additional robustness tests.

First, we estimate heterogeneous effects of the Brexit result on subjective well-being, using a different proxy variable to capture respondents attitudes towards the EU. In doing so, we rely on a variable given by the response to the following question: “*In general, does the EU conjure up for you a very positive, fairly positive, neutral, fairly negative or very negative image?*”; a question included in every wave in our sample.<sup>12</sup> Second, we repeat the spillover regressions using five groups of countries, with cut-off points based on quintiles of the distribution of each measure, rather than quartiles. Third, we repeat our main specification by considering placebo outcomes; that is, outcomes which we do not expect the Brexit result to have an effect upon.

Overall, the results presented in this section reinforce our findings regarding the effect of the Brexit vote on subjective well-being.

#### 3.4.1 Heterogeneity: Image towards the EU

Table 6, column 1, shows the heterogeneous effects based on individuals’ EU image. The results are consistent between this and our original measure of feeling an EU citizen, and suggest that subjective well-being decreased for those being positive towards EU. Similar to the main estimate, this is not the case for the ones having a negative image towards the EU. Results further suggest a significant decrease in subjective well-being for those with a neutral image towards the EU; though smaller in absolute terms as compared to that of the ‘positive’ groups.

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<sup>12</sup> Note that the correlation coefficient between feeling a citizen of the EU’ and ‘image towards the EU’ is 0.44.

Table 6, Column 2, presents adaptation estimates based on this measure. The interpretation of these estimates is similar to those in the main analysis. Here, we do not generally find significant estimates for those with a ‘very negative’ EU image, but we do find a significant reduction in reported subjective well-being for those in the ‘negative’ group by autumn 2017.

Stratifying the sample depending on this measure produces similar results as those presented in Table 3: UK respondents with an overall positive and also neutral image towards the EU are significantly less satisfied with their life compared to their EU counterparts. The reverse does not appear to be the case for those holding a negative attitude, whose subjective well-being is not significantly different even when compared to that of EU respondents holding an equally negative image towards the EU.<sup>13</sup>

### 3.4.2 Spillover effects on quintiles of exposure

Results for the spillover effects based on quintiles are reported in Table 7; a list of countries included in each quintile, by measure, is provided in Table A2 in the appendix.

The results hold a similar interpretation as above, with the Brexit vote generally not having spillover effects other than for, now, the two top quintiles (Q4 and Q5) interaction effects only for the case of the Chen et al. (2018a) measure. Under this specification, we find increases in subjective well-being following the referendum for countries in Q2 for the IMF measure. This result might be viewed as expectations of potential benefits, such as businesses relocating their activities, accruing to this group of countries (Austria, Italy, Portugal, Slovakia and Spain) following Brexit. In all cases, the estimate for our main treatment effect is statistically significant and even more pronounced.

### 3.4.3 Placebo outcomes

For our placebo outcomes we consider two measures. The first is based on whether the individual is in favour of a tax on financial transactions<sup>14</sup> (available in Eurobarometer 83.1, 83.3, 84.3, 85.2, 86.2, 87.3 and 88.3), and the second whether he/she thinks that crime is considered as an important issue in their country (available in Eurobarometer 83.3, 84.3, 85.2, 86.2, 87.3 and 88.3). The latter is a binary variable hence we estimate equation 1 using a probit model.

Results, presented in Table 8, suggest that there are no significant differences for these outcome measures following the Brexit result.<sup>15</sup>

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<sup>13</sup>These results are available upon request.

<sup>14</sup>Given by the question: “*Thinking about reforming global financial markets, please tell me whether you are in favour or opposed to the introduction of a tax on financial transactions*”, with responses including ‘strongly in favour, fairly in favour, fairly opposed, and strongly opposed’.

<sup>15</sup>Given the smaller samples in Table 8 due to data availability, note that the corresponding subjective well-being regressions following the estimation of an equation 1 model for these samples still yield a statistically significant estimate for the main treatment effect, equal to -0.062 and -0.06 for the tax and crime models, respectively.

## 4 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 and Stutzer, 2000), the evidence of the effect of the resulting outcome on subjective well-being is still mixed. This paper studies the effect of the Brexit referendum result on subjective well-being. The rather unexpected result of this single-issue referendum is worth examining for at least two reasons. First, it has already had an adverse effect in the UK economy (Johnson and Mitchell, 2017), with future projections not being very positive<sup>16</sup>; second, it is a permanent decision, which has implications for the degree of adaptation to this outcome.

Using data from the Eurobarometer between 2015-2017 and applying a quasi-experimental design, we find that life satisfaction post-referendum significantly decreased in the UK as a whole as compared to a pool of other EU countries. This effect is robust to the inclusion of macroeconomic controls. This estimate is not negligible; to put it in context, it is 1.3 times larger than the effect of widowhood on subjective well-being and about 1/6 of that of unemployment (which is one of the largest significant adverse effects on subjective well-being to which there is no adaptation—Lucas et al., 2004; Knabe and Rätzl, 2011). We do not find evidence of adaptation to the referendum's outcome, at least not within the span of our sample. The overall reduction in subjective well-being does, however, appear to be mitigated in the final wave of our data.

Estimating heterogeneous effects of the Brexit result by individuals' attitudes towards the EU—measured by one's 'feeling a citizen of the EU'—suggests that those with an overall positive attitude report significantly lower levels of subjective well-being; with the effect being more pronounced the more positive one's EU attitude is. Under the assumption that individuals with positive attitudes towards the EU do not have a preference for Brexit, then these estimates are perhaps not as surprising. More importantly, and contrary to other studies in the literature that find a rather fast adaptation process to electoral results (Kinari et al., 2015; Pierce et al., 2016; Powdthavee et al., 2017), we do not generally find their levels of subjective well-being to adapt within our time span. In addition to preferences not being met, this might reflect the uncertainty that lies ahead as the country goes through the transition period to exit the EU (Wilson and Gilbert, 2008).

Our results do not support the reverse for the case of those with negative attitudes towards the EU; their 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.

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<sup>16</sup>See, for example, the [Bank of England's report](#) commissioned by the House of Commons Treasury Committee, as well as a recent [analysis by the UK Government](#). (November 2018)

Looking in more detail on the specific time periods following the referendum for these individuals with overall negative attitudes towards the EU is, however, more informative. Subjective well-being of those ‘definitely not feeling a citizen of the EU’ (‘very negative’ attitude) significantly increased in autumn 2016; that is, in the coming months following the Brexit result. In relative terms, this increase in subjective well-being is about as large as the decrease experienced by those with a very positive ‘feeling of being an EU citizen’ over the same time period (i.e., autumn 2016; see Table 2, column 2). This is in line with Powdthavee et al. (2017) who find those with a stated preference for Brexit to be more satisfied, and those with a stated preference for Remain to be less satisfied, post-referendum. Our estimates suggest though that this positive effect is not sustained over time. It turns significantly negative by spring 2017—perhaps reflecting some sort of a distress or unmet expectations following the evolving exit negotiations and, even possibly a dissatisfaction from deviating away from a ‘hard(er) Brexit’—and statistically insignificant by autumn 2017—which could indeed be due to adaptation to this outcome as their underlying preference for Brexit is being satisfied.

In our final model, we account for spillover effects of the Brexit result. We use three measures of socio-economic integration to split our sample into quartiles based on the level of a country’s link (or ‘exposure’) to the UK: a measure accounting only for trade links (Chen et al., 2018a); one accounting for trade and fiscal transfers (Dhingra et al., 2017); and one accounting for trade, financial linkages and migration, available from the IMF (Chen et al., 2018b). We generally do not find strong evidence in favour of the presence of spillover effects during the Brexit transition period, which appear to be present for the top quartile of countries mostly related to the UK for only the case of the first (i.e., least inclusive) measure considered here.

As the bottom quartile group—i.e., the control group in the spillover regressions—differs across our three measures, these regressions serve implicitly as a robustness tests of our main treatment effect. In all cases, the estimate for the effect of the Brexit result on subjective well-being in the UK is negative and statistically significant. These results are robust to dividing countries in quintiles, instead of quartiles, of the corresponding distribution of countries’ linkage to the UK.

This study, like any other, is not free of limitations. The Eurobarometer data we use is 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. 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 general attitudes individuals in our sample hold towards the EU; and although being rather unlikely, say, for a UK individual ‘feeling

being a citizen of the EU' having a preference for Brexit, we cannot strictly exclude this possibility for the reverse case of those individuals with a rather negative attitude towards the EU. To illustrate, consider the case of a UK individual 'definitely not feeling being a citizen of the EU'. This might arise because of his/her dissatisfaction with certain EU policies that are correlated with such feelings but not necessarily with a preference for Brexit. This argument might indeed, partly, justify why our regression results estimate a significant decrease in subjective well-being for those with a positive attitude towards the EU (i.e., offering evidence of preference and experience utility coinciding), and no statistical evidence—but in one occasion as discussed above—of a positive effect for those with a negative attitude towards the EU despite the UK now 'taking back control'. One should also weigh this limitation of observing preferences towards Brexit against the possibility of individuals feeling reluctant to state their sentiments in a Brexit-specific question, to perhaps avoid being labelled as a 'Brexiteer' or a 'Remainer'.

Notwithstanding these issues, this study offers robust evidence of an overall decrease in subjective well-being for individuals in the UK during the Brexit transition period. With the UK set to formally exit the EU in March 2019, the long-term effects of Brexit on subjective well-being will undoubtedly be the focus of future research.

# Tables

Table 1: Brexit result and subjective well-being

	(1)	(2)	(3)	(4)
UK×Post Brexit	-0.062*** (0.013)	-0.055*** (0.014)		
UK×Autumn 2016			-0.034** (0.015)	-0.032** (0.015)
UK×Spring 2017			-0.082*** (0.012)	-0.078*** (0.012)
UK×Autumn 2017			-0.075*** (0.014)	-0.064*** (0.016)
UK	0.527*** (0.014)	0.474*** (0.027)	0.527*** (0.014)	0.476*** (0.027)
Autumn 2016			-0.018 (0.016)	-0.016 (0.027)
Spring 2017			0.049*** (0.01)	0.032** (0.013)
Autumn 2017			0.015 (0.014)	0.015 (0.016)
Post Brexit	-0.008 (0.012)	-0.005 (0.012)		
Controls	Yes	Yes	Yes	Yes
Macroeconomic Controls	No	Yes	No	Yes
$R^2$	0.28	0.28	0.28	0.28
$N$	366,928	366,928	366,928	366,928

*Notes:* Regressions are OLS based on Eq. 1 (columns 1 and 2) and Eq. 2 (columns 3 and 4); without and with macroeconomic controls. Controls include: individual characteristics, country effects, month of the year effects, and year effects. Robust standard errors clustered at the country-wave level reported in parentheses.

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

Table 2: Subjective well-being heterogeneous effects based on ‘feel EU citizen’

	(1)	(2)
UKVeryPositive×Post Brexit	-0.142*** (0.03)	
UKPositive×Post Brexit	-0.066*** (0.019)	
UKNegative×Post Brexit	-0.004 (0.044)	
UKVeryNegative×Post Brexit	-0.018 (0.079)	
UKVeryPositive×Autumn 2016		-0.125*** (0.028)
UKVeryPositive×Spring 2017		-0.171*** (0.028)
UKVeryPositive×Autumn 2017		-0.132*** (0.03)
UKPositive×Autumn 2016		-0.032*** (0.013)
UKPositive×Spring 2017	-	-0.075*** (0.014)
UKPositive×Autumn 2017		-0.094*** (0.017)
UKNegative×Autumn 2016		0.003 (0.042)
UKNegative×Spring 2017		0.018 (0.042)
UKNegative×Autumn 2017		-0.031 (0.043)
UKVeryNegative×Autumn 2016		0.114*** (0.037)
UKVeryNegative×Spring 2017		-0.174*** (0.037)
UKVeryNegative×Autumn 2017		-0.006 (0.038)
Controls	Yes	Yes
Macroeconomic Controls	Yes	Yes
$R^2$	0.282	0.282
$N$	129,821	129,821

*Notes:* Regressions are OLS. Column 1 based on Eq. 3. Column 2 applies a variant of Eq. 2. Regressions control for the roots of interaction terms. Controls include: individual characteristics, country effects, month of the year effects, and year effects. Robust standard errors clustered at the country-wave level reported in parentheses.

\*\*\*  $p < 0.01$

Table 3: Effects of Brexit result on subjective well-being, stratified by ‘feel EU citizen’

	(1)	(2)	(3)	(4)
	Very Positive	Positive	Negative	Very Negative
UK×Post Brexit	-0.124*** (0.03)	-0.048*** (0.016)	0.007 (0.054)	-0.012 (0.066)
UK	0.338*** (0.051)	0.457*** (0.053)	0.44*** (0.067)	0.566*** (0.108)
Post Brexit	0.001 (0.017)	0.002 (0.017)	0.041 (0.022)	0.051 (0.041)
Controls	Yes	Yes	Yes	Yes
Macroeconomic Controls	Yes	Yes	Yes	Yes
$R^2$	0.206	0.246	0.296	0.325
$N$	36,155	54,370	27,083	12,213

*Notes:* Regressions are OLS based on Eq. 1 with stratified samples depending on responses to the ‘feel EU citizen’ measure. Controls include: individual characteristics, country effects, month of the year effects, and year effects. Robust standard errors clustered at the country-wave level reported in parentheses.

\*\*\*  $p < 0.01$

Table 4: Exposure measures — quartiles

<i>Quartile</i>	Chen et al. (2018a)	Dhingra et al. (2017)		IMF
		Soft Brexit	Hard Brexit	(Chen et al., 2018b)
Q1	Bulgaria	<b>Greece</b>	<b>Greece</b>	Austria
	Cyprus	Italy	Italy	Bulgaria
	<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	Lithuania
	Latvia	Portugal	Portugal	Luxembourg
	Lithuania	Slovakia	Slovakia	Portugal
	Spain			Slovakia
				Spain
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>
	Malta			Malta
	<b>Netherlands</b>			<b>Netherlands</b>

*Notes:* Chen et al.'s (2018a) measure includes 26 countries within our sample (except Croatia); Dhingra et al.'s (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.

Table 5: Spillover effects based on quartiles

	Chen et al. (2018a)	Dhingra et al. (2017)		IMF
		Soft Brexit	Hard Brexit	(Chen et al., 2018b)
UK×Post Brexit	-0.068*** (0.019)	-0.047** (0.021)	-0.046** (0.021)	-0.06*** (0.019)
Q2×Post Brexit	-0.016 (0.016)	0.027 (0.019)	0.027 (0.02)	0.009 (0.017)
Q3×Post Brexit	-0.015 (0.016)	0.001 (0.018)	0.001 (0.018)	-0.01 (0.015)
Q4×Post Brexit	-0.032** (0.015)	-0.009 (0.018)	-0.008 (0.019)	-0.022 (0.015)
UK	0.912*** (0.116)	0.931*** (0.02)	0.938*** (0.125)	0.575*** (0.098)
Q2	0.214*** (0.039)	0.497*** (0.143)	0.507*** (0.138)	0.088 (0.099)
Q3	0.312*** (0.051)	0.337*** (0.033)	0.338*** (0.032)	-0.033 (0.027)
Q4	0.477*** (0.125)	0.902*** (0.249)	0.101*** (0.031)	0.342*** (0.05)
Post Brexit	0.007 (0.017)	-0.005 (0.02)	-0.006 (0.02)	-0.001 (0.017)
Controls	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes
$R^2$	0.286	0.289	0.289	0.028
$N$	353,040	280,841	280,841	366,928

*Notes:* Regressions are OLS based on Eq. 4. Chen et al.'s (2018a) measure based on 26 countries; Dhingra et al.'s (2017) measure based on 19 countries; IMF's (Chen et al., 2018b) measure based on all 27 countries in our sample. Controls include: individual characteristics, country effects, month of the year effects, and year effects. Robust standard errors clustered at the country-wave level reported in parentheses.

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

Table 6: Heterogeneous effects based on ‘EU image’

	(1)	(2)
UKVeryPositive×Post Brexit	-0.107*** (0.036)	
UKPositive×Post Brexit	-0.075*** (0.017)	
UKNeutral×Post Brexit	-0.058*** (0.021)	
UKNegative×Post Brexit	-0.051 (0.028)	
UKVeryNegative×Post Brexit	-0.039 (0.058)	
UKVeryPositive×Autumn 2016		-0.062 (0.045)
UKVeryPositive×Spring 2017		-0.185*** (0.034)
UKVeryPositive×Autumn 2017		-0.09*** (0.033)
UKPositive×Autumn 2016		-0.054*** (0.016)
UKPositive×Spring 2017		-0.082*** (0.015)
UKPositive×Autumn 2017		-0.091*** (0.024)
UKNeutral×Autumn 2016		-0.043*** (0.018)
UKNeutral×Spring 2017		-0.105*** (0.018)
UKNeutral×Autumn 2017		-0.042 (0.026)
UKNegative×Autumn 2016		-0.031 (0.034)
UKNegative×Spring 2017		-0.054 (0.028)
UKNegative×Autumn 2017		-0.07** (0.03)
UKVeryNegative×Autumn 2016		0.007 (0.056)
UKVeryNegative×Spring 2017		-0.056 (0.012)
UKVeryNegative×Autumn 2017		-0.069 (0.072)
Controls	Yes	Yes
Macroeconomic Controls	Yes	Yes
$R^2$	0.28	0.28
$N$	362,083	362,083

*Notes:* Regressions are OLS. Column 1 based on Eq. 3. Column 2 applies a variant of Eq. 2. Regressions control for the roots of interaction terms. Controls include: individual characteristics, country effects, month of the year effects, and year effects. Robust standard errors clustered at the country-wave level reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$

Table 7: Spillover Effects based on quintiles

	Chen et al. (2018a)	Dhingra et al. (2017)		IMF
		Soft Brexit	Hard Brexit	(Chen et al., 2018b)
UK×Post Brexit	-0.07*** (0.018)	-0.057** (0.022)	-0.056** (0.022)	-0.053*** (0.02)
Q2×Post Brexit	-0.003 (0.019)	0.011 (0.023)	0.014 (0.023)	0.048*** (0.018)
Q3×Post Brexit	-0.025 (0.016)	0.001 (0.02)	-0.001 (0.02)	-0.019 (0.018)
Q4×Post Brexit	-0.029** (0.015)	-0.016 (0.019)	-0.016 (0.019)	-0.001 (0.017)
Q5×Post Brexit	-0.032** (0.015)	-0.027 (0.019)	-0.028 (0.019)	-0.019 (0.017)
UK	0.931*** (0.116)	0.357*** (0.069)	0.353*** (0.069)	0.573*** (0.098)
Q2	0.267*** (0.038)	-0.555*** (0.067)	-0.077 (0.08)	-0.043 (0.031)
Q3	0.323*** (0.051)	-0.068 (0.008)	-0.232 (0.024)	0.102 (0.098)
Q4	0.493*** (0.127)	-0.449*** (0.049)	-0.448*** (0.049)	-0.034 (0.036)
Q5	0.664*** (0.061)	0.304 (0.186)	0.297 (0.186)	0.343*** (0.05)
Post Brexit	0.009 (0.016)	0.005 (0.021)	0.005 (0.021)	-0.008 (0.018)
Controls	Yes	Yes	Yes	Yes
Macroeconomic controls	Yes	Yes	Yes	Yes
$R^2$	0.286	0.289	0.289	0.028
$N$	353,040	280,841	280,841	366,928

*Notes:* Regressions are OLS based on Eq. 4. Chen et al.'s (2018a) measure based on 26 countries; Dhingra et al.'s (2017) measure based on 19 countries; IMF's (Chen et al., 2018b) measure based on all 27 countries in our sample. Controls include: individual characteristics, country effects, month of the year effects, and year effects. Robust standard errors clustered at the country-wave level reported in parentheses.

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

Table 8: Placebo outcomes

	Tax Financial Transactions	Crime
UK×Post Brexit	0.03 (0.03)	0.061 (0.056)
UK	-0.276*** (0.069)	-0.174 (0.096)
Post Brexit	0.231*** (0.03)	-0.137** (0.056)
Controls	Yes	Yes
Macroeconomic Controls	Yes	Yes
$R^2$	0.098	
Pseudo $R^2$		0.046
$N$	110,390	157,877

*Notes:* Column 1 regression is OLS based on Eq. 1. Column 2 regression is a probit of Eq. 1 given the binary outcome. Controls include: individual characteristics, country effects, month of the year effects, and year effects. Robust standard errors clustered at the country-wave level reported in parentheses.

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

# Figures

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

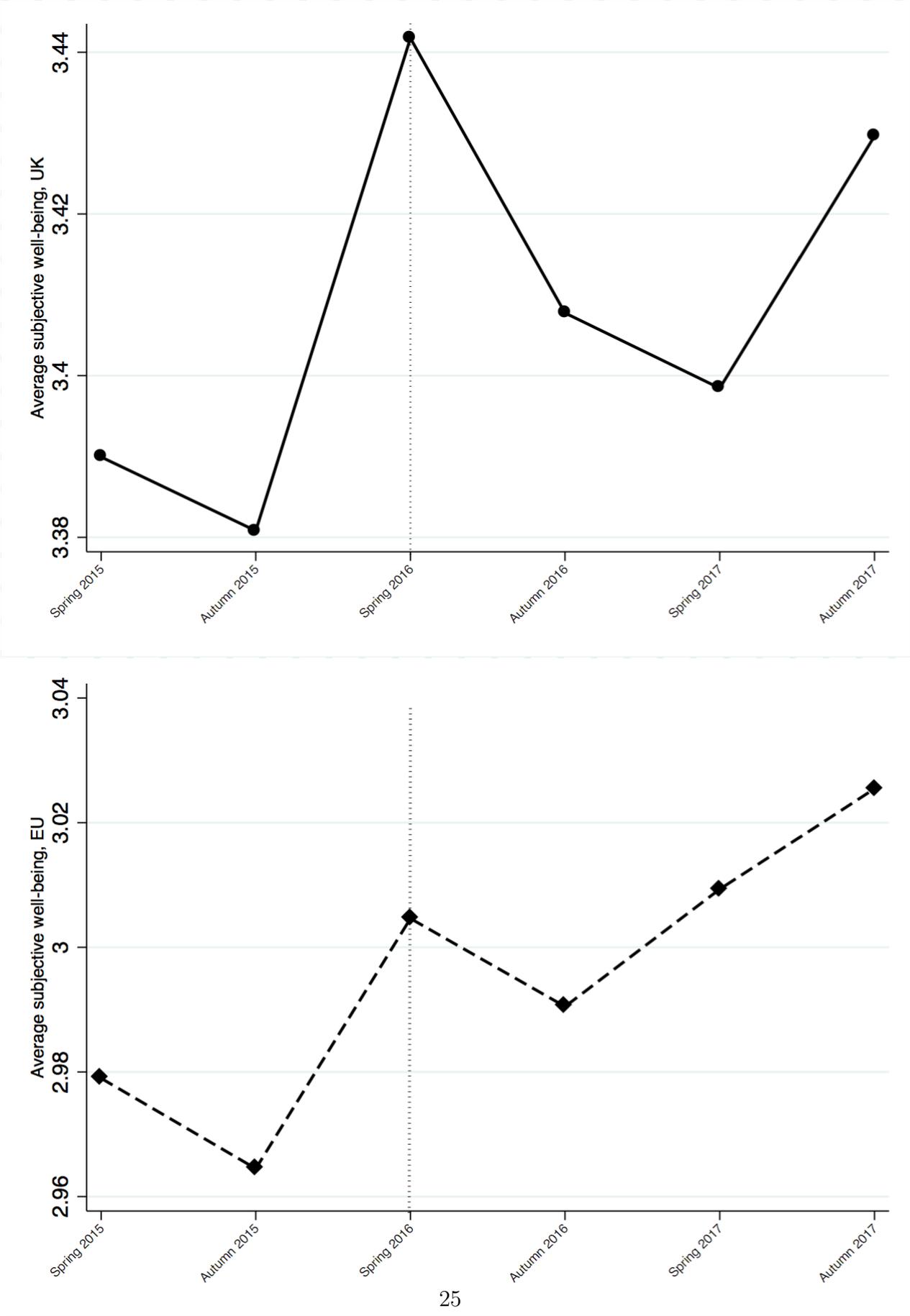
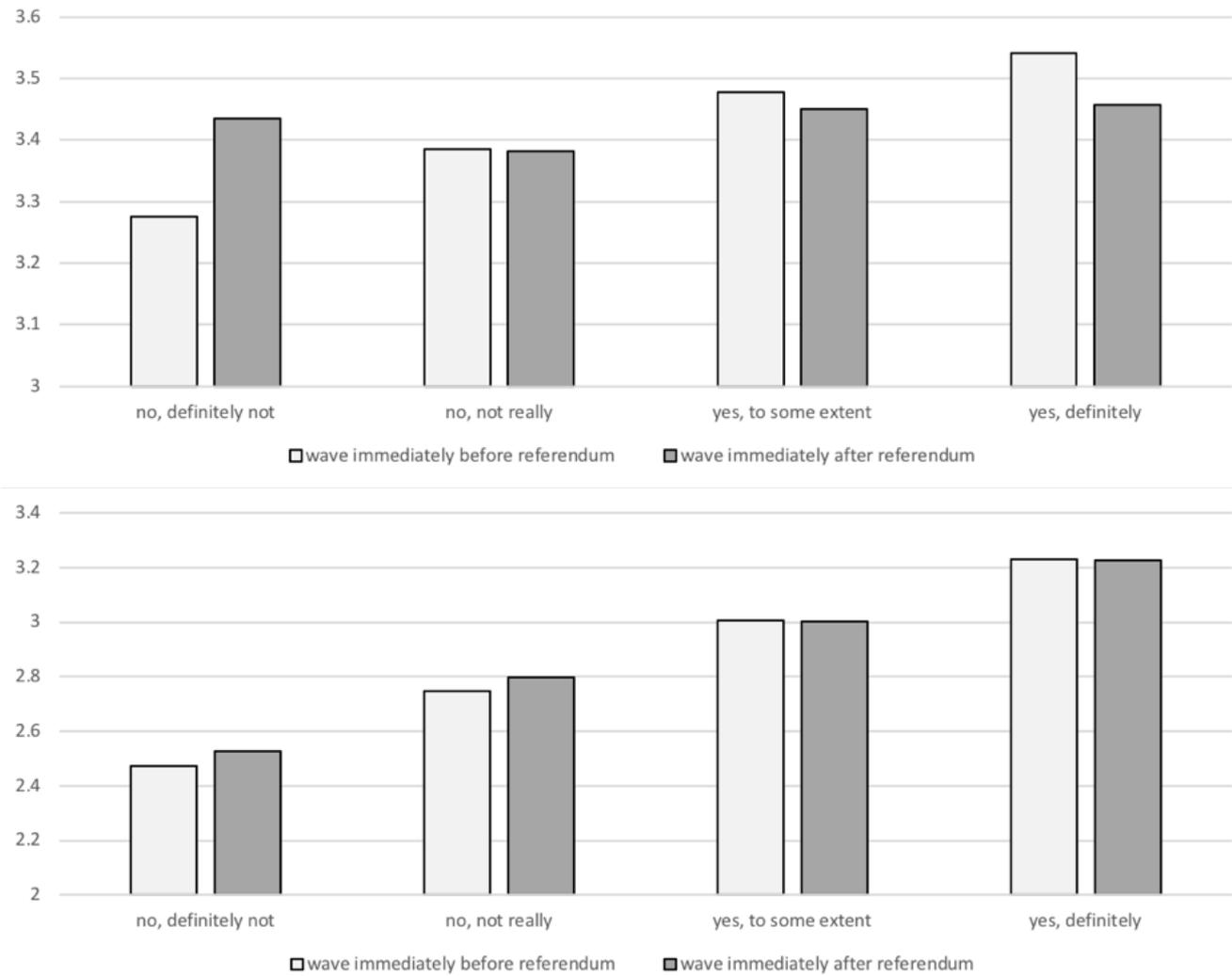


Figure 2: Average subjective well-being and heterogeneous effects before/after referendum, UK (top) and EU (bottom)



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# Appendix

Table A1: Placebo-time treatments pre-referendum

	(1)	(2)	(3)	(4)	(5)
<i>Eurobarometer Waves:</i>	85 vs. (83 & 84)	(84 & 85) vs. 83	85 vs. 84	84 vs. 83	85 vs. 83
UK×Post	0.03 (0.02)	0.016 (0.025)	0.034 (0.02)	-0.002 (0.018)	0.025 (0.026)
UK	0.49*** (0.036)	0.495*** (0.041)	0.503*** (0.047)	0.445*** (0.051)	0.498*** (0.042)
Post	-0.001 (0.018)	-0.006 (0.02)	0.011 (0.016)	-0.022 (0.019)	-0.003 (0.018)
Controls	Yes	Yes	Yes	Yes	Yes
Macroeconomic Controls	Yes	Yes	Yes	Yes	Yes
$R^2$	0.283	0.283	0.287	0.281	0.283
$N$	157,228	157,228	104,610	78,696	131,150

*Notes:* Controls include: individual characteristics, country effects, month of the year effects, and year effects. Robust standard errors clustered at the country-wave level reported in parentheses.

\*\*\*  $p < 0.01$

Table A2: Exposure measures — quintiles

<i>Quintile</i>	Chen et al. (2018a)	Dhingra et al. (2017)		IMF
		Soft Brexit	Hard Brexit	(Chen et al., 2018b)
Q1	Bulgaria Cyprus Italy Portugal Romania <b>Slovenia</b>	Greece Italy <b>Slovenia</b> Spain	Greece Italy <b>Slovenia</b> Spain	Bulgaria Croatia Greece Poland Romania <b>Slovenia</b>
Q2	<b>Austria</b> Estonia Finland Greece Spain	<b>Austria</b> Finland Portugal Romania	<b>Austria</b> France Portugal Romania	<b>Austria</b> Italy Portugal Slovakia Spain
Q3	Latvia Lithuania Luxembourg Poland Slovakia	France Germany Poland Slovakia	Finland Germany Poland Slovakia	Czech Rep. France Hungary Latvia Lithuania Luxembourg
Q4	Czech Rep. <b>Denmark</b> France Hungary <b>Sweden</b>	Czech Rep. <b>Denmark</b> Hungary <b>Sweden</b>	Czech Rep. <b>Denmark</b> Hungary <b>Sweden</b>	<b>Denmark</b> Estonia Finland Germany <b>Sweden</b>
Q5	<b>Belgium</b> Germany <b>Ireland</b> Malta <b>Netherlands</b>	<b>Belgium</b> <b>Ireland</b> <b>Netherlands</b>	<b>Belgium</b> <b>Ireland</b> <b>Netherlands</b>	<b>Belgium</b> Cyprus <b>Ireland</b> Malta <b>Netherlands</b>

*Notes:* Chen et al.'s (2018a) measure includes 26 countries within our sample (except Croatia); Dhingra et al.'s (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 quintile across measures.

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