# The Economic Consequences of the Brexit Vote[*]

Benjamin Born Gernot J. Müller Moritz Schularick

Petr Sedláček

December 22, 2017

**Abstract**

The unexpected outcome of the Brexit vote in June 2016 provides a rare macroeconomic experiment to study the aggregate consequences of a sudden change in expectations regarding future economic prospects. Using synthetic control methods, we show that forward looking households and businesses lowered spending in response to the vote, causing an output loss of more than 1 percent. Heightened economic policy and macroeconomic uncertainty explain close to half of the observed output loss. But a large part of the consequences of the vote also reflects a downgrade of expectations about long-run income, not only a wider dispersion of potential outcomes.

*Keywords:* Brexit, European Union, Synthetic control method, VAR,
Anticipation effects, Economic policy uncertainty, Macroeconomic uncertainty

*JEL Codes:* E65, F13, F42

[*]Born: University of Bonn and CEPR, born@uni-bonn.de, Müller: University of Tübingen and CEPR, gernot.mueller@uni-tuebingen.de, Schularick: University of Bonn and CEPR, schularick@uni-bonn.de, Sedláček: University of Oxford and CEPR, petr.sedlacek@economics.ox.ac.uk. We thank Chris Redl for sharing data for the UK macroeconomic uncertainty index and Rubén Domínguez Díaz and Emily Swift for excellent research and editorial assistance, respectively. The usual disclaimer applies.

# 1 Introduction

On June 23, 2016 the UK electorate unexpectedly voted to leave the European Union. Economists disagree about the economic consequences of this momentous political choice, not least because the extent of Britain's future disintegration with the European economy depends on policies that are yet to be specified. According to a recent survey by Sampson (2017), predictions for the long run decline of per capita incomes range from 1 to 10 percent. For macroeconomics as a science, however, the Brexit vote offers a unique opportunity. This is because the unexpected referendum result provides a rare natural *macroeconomic* experiment to study the aggregate consequences of a sudden change in expectations with respect to future economic prospects.[1]

In this paper, we estimate the extent to which a change in expectations about UK's economic future has already impacted the macroeconomy, *before* Brexit actually happens. Such effects are possible in view of the intrinsically forward-looking behavior of households and businesses. In addition to quantifying the overall effect of changing expectations, we also distinguish between the impact of a change in the average expectation ("anticipation effect") and a (temporary) increase in the dispersion of expectations ("uncertainty effect").

Accordingly, we make two major contributions. First, we estimate the overall impact of a change in expectations in response to the Brexit vote result. Crucially, our analysis relies on a systematic, entirely data-driven approach utilizing the synthetic control technique that was recently added to the toolbox of empirical macroeconomics by Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2015). The results show that the Brexit vote has already had a substantially negative impact on UK output. Our second core contribution is to quantify the degree to which the measured overall costs reflect a downgrade in the average expectation about Britain's economic future or a (temporary) increase in the dispersion of expectations. We unscramble the anticipation and the uncertainty effect with the help of a structural VAR model in the second part of the analysis. We document that a large part

[1]Fuchs-Schündeln and Hassan (2016, p. 925) define "natural experiments as historical episodes that provide observable, quasi-random variation in treatment subject to a plausible identifying assumption. The "natural" in natural experiments indicates a researcher did not consciously design the episode to be analyzed, but can nevertheless use it to learn about causal relationships."

of the overall costs of the Brexit vote can be attributed to gloomier average expectations of economic agents and is thus indicative of long-run effects.

Measuring the aggregate effects of important policy interventions represents a distinct challenge for the discipline of macroeconomics. The Brexit vote is a case in point. The outcome of the referendum represented an announcement of a future change in policy which, in addition, is befogged by high uncertainty. This immediately raises the question of the "counterfactual," which would measure how the UK economy would have developed had it not been for the referendum. In this paper, we follow Abadie and Gardeazabal (2003) and let an algorithm determine which combination of other economies matches the growth trend of the UK economy before the Brexit vote with the highest possible accuracy. Which economies get picked by the algorithm and what weight they are assigned is entirely data-driven. The better the algorithm constructs a doppelganger for the UK economy as a weighted combination of other economies *before* the "referendum shock", the more accurate our results will be. We use the largest available country dataset to obtain the best match possible.

The construction of the synthetic doppelganger makes it possible to assess the quantitative impact of the Brexit vote on the UK economy. Our identification assumption is that the doppelganger continues to evolve in the way the UK economy would have in absence of the referendum. Therefore, the difference in output between the UK economy and its doppelganger captures the causal effect of the referendum. Importantly, our approach does not hinge on having the right economic model for the British, the European, or the global economy, nor do we need to assume a particular Brexit deal emerging from future negotiations. Instead, we directly measure the overall economic effects of the change of the economic outlook due to the Brexit vote.[2]

We show that the economic costs of the Brexit vote are already visible and quite large. In the third quarter of 2017 the output loss due to the Brexit vote amounted to approximately 1.3 percent, and the cumulative loss in GDP was close to 20 billion

[2]Our analysis rests on the assumption that any development since the referendum is causally linked to the Brexit vote. This seems plausible in a situation when trade negotiations between the UK and the EU have not yet begun. This assumption is also supported by so called time-placebo studies, described below.

pounds. Under current forecasts, the cumulative costs are expected to grow to about 65 billion pounds by end-2018. At this point in time, UK output can be expected to be 2.2 percent below the level that would have been observed in the absence of the Brexit vote. Following Abadie et al. (2015), we also conduct a number of time- and country-placebo tests, reassuring us of the causal effect of the Brexit vote.

Having estimated the realized economic costs of the Brexit vote since June 2016, we seek to understand their deeper causes. In particular, Bloom (2009) has shown in an influential contribution that so-called "second-moment shocks" can impact investment decisions adversely. More recently, Born and Pfeifer (2014), Fernández-Villaverde et al. (2015), and Baker et al. (2016) provide evidence that uncertainty about economic policy is detrimental to economic activity. The unexpected outcome of the Brexit vote triggered a massive spike in uncertainty. For instance, the economic policy uncertainty (EPU) index compiled by Baker et al. (2016) reached an all time high in July 2016 and stayed high since.[3]

As higher uncertainty is likely to depress economic activity—independent of a change in the long-run economic outlook—some part of our estimated costs of the Brexit vote may reflect a transitory "uncertainty effect" that could dissipate again once uncertainty decreases. To assess the extent to which the estimated effects of the Brexit vote on output are driven by a downgrade of the average economic outlook for the UK, we purge the estimates from the output loss that is due to increased uncertainty. For this purpose, we build on Baker et al. (2016) and Jurado et al. (2015) and estimate a structural vector autoregression (VAR) model on quarterly time-series data for the UK over the period 1997Q1 to 2017Q2. We identify uncertainty shocks recursively where we allow uncertainty shocks to play a fairly large role: all variables may be contemporaneously affected by an uncertainty shock.

We then use the VAR model to simulate a counterfactual time series for output by switching off uncertainty shocks in the post-2016Q2 period. We find that the increase in uncertainty triggered by the referendum accounts for less than 1/2 of the economic costs

[3] The index is normalized to a mean of 100 prior to 2011. It reached a value of 800 in June 2016, up from 430 in May and a value of 1140 in July 2016; see www.policyuncertainty.com for details.

of the Brexit vote. Put differently, more than 1/2 of the gap in performance between the UK economy and its synthetic doppelganger since the referendum is attributable to gloomier average expectations of economic agents. British households and businesses already predict that the economic future of their country outside the European Union will be less bright.

Our paper relates to work on the impact of (trade policy) uncertainty on international trade (see e.g. Handley and Limão 2015, 2017; Limão and Maggi 2015; Novy and Taylor 2014). We also share a focus of analysis with studies of macroeconomic experiments at the aggregate level (Alesina and Fuchs-Schündeln 2007; Fuchs-Schündeln and Hassan 2016). Billmeier and Nannicini (2013), in particular, also use the synthetic control approach to study the impact of economic liberalizations. Finally, our paper complements a number of influential studies on the instantaneous macroeconomic impact of anticipated future (policy) changes or, more generally, "news" (see e.g. Barsky and Sims 2011, 2012; Beaudry and Portier 2006; Schmitt-Grohé and Uribe 2012).

In regards to our application, only very few studies have investigated the immediate implications of the Brexit vote. An exception is Ramiah et al. (2016) who show that the response of cumulative abnormal returns in different sectors after the referendum is mostly negative. Breinlich et al. (2017) argue that the inflation increase following the post-referendum pound depreciation amounts to about a 400 pound consumption loss for the average British household. Finally, Berg et al. (2017) use a matching strategy to show that bank lending dropped by 20 percent in the syndicated loan market after the Brexit vote.

The remainder of this paper is organized as follows. In the following section we describe how we apply the synthetic control method to measure the output effect of the Brexit vote. Section 3 zooms in on the transmission mechanism and quantifies the roles of economic uncertainty and shifts in expectations. A final section concludes.

# 2 The output effect of the Brexit vote

The unexpected outcome of the Brexit vote constitutes a natural macroeconomic experiment enabling us to study the aggregate consequences of a sudden change in expectations regarding future economic prospects.[4] In order to evaluate its causal impact on UK's macroeconomy, however, one needs to define an appropriate comparison economy. Such a comparison economy needs to track the actual UK economy as closely as possible prior to the referendum, but must be left unaffected by the Brexit vote.

We follow Abadie and Gardeazabal (2003) and Abadie et al. (2010, 2015) and use synthetic control methods to construct precisely such a doppelganger to the UK economy. The identifying assumption allowing us to quantify the causal effect of the referendum is that the UK economy would have developed as the doppelganger, had it not been for the Brexit vote.[5]

We can then directly quantify the costs of the Brexit vote by the "doppelganger gap": the difference between UK's actual output performance relative to that of the doppelganger economy. Lastly, we run a number of tests showing that our estimated effects indeed reflect a causal impact of the referendum shock.

## 2.1 Constructing the doppelganger

We construct the doppelganger as a synthetic control unit. For this purpose we consider a "donor pool" of 30 OECD countries and quarterly observations for real GDP during the period from 1995Q1–2016Q2.[6] Our procedure thus assumes that a possible treatment effect materializes after 2016Q2. Moreover, we assume that the countries in the donor pool are not affected by the treatment. We relax both assumptions in our analysis below.

The doppelganger is a weighted average of the countries in the donor pool. The

[4]For the longest time prior to the referendum a Brexit vote seemed very unlikely. In the run up to the vote most polls suggested a victory for "Remain", except for a brief period in early June when "Leave" was ahead in the poll of polls, see `https://whatukthinks.org/eu/opinion-polls/poll-of-polls/`.

[5]In the context of natural experiments, our identifying assumption therefore amounts to the premise that, given economic fundamentals, the UK economy and its doppelganger were equally likely to obtain the "treatment" of the Brexit vote.

[6]Specifically, we normalize real GDP to unity in 1995 in each country. See Appendix A.1 for further details on the dataset.

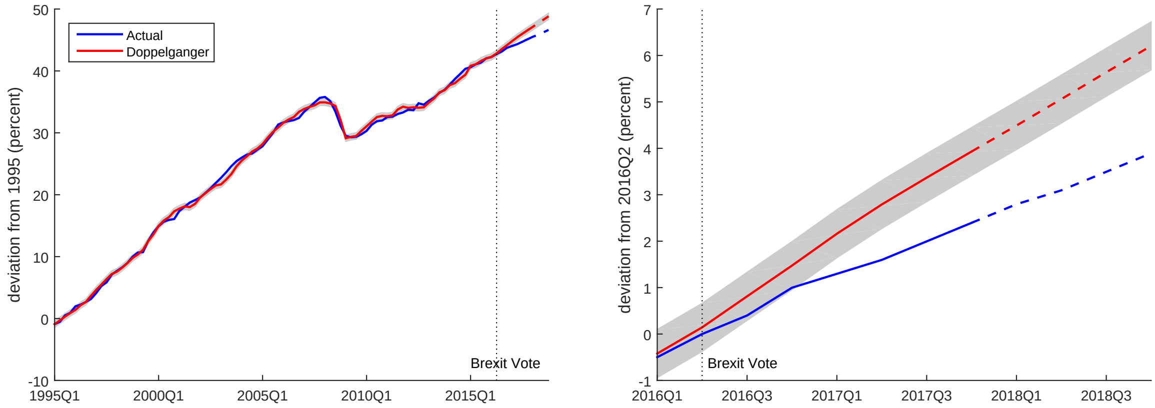


Figure 1: Real GDP of the UK. Actual data (blue line) vs doppelganger (red line). *Note:* shaded area is one standard deviation of difference prior to Brexit vote. Dashed lines are forecasts. Data sources for UK before 2016Q1: OECD Economic Outlook, 2016Q1 – 2017Q3: ONS, 2017Q4–2018Q4: forecasts by BoE, doppelganger: OECD Economic Outlook.

weights are determined by minimizing the distance between GDP of the UK and of the doppelganger prior to the treatment. Formally, we let $X_1$ denote the $(86 \times 1)$ vector of observations for real GDP in the UK and let $X_0$ denote a $(86 \times 30)$ matrix with observations for real GDP in the countries included in the donor pool. Finally, we let $W$ denote a $(30 \times 1)$ vector of weights $w_j$, $j = 2, \ldots, 31$. Then, the doppelganger is defined by $W^*$ which minimizes the following mean squared error:

$$(X_1 - X_0 W)' V (X_1 - X_0 W) \ , \tag{2.1}$$

subject to $w_j >= 0$ for $j = 2, \ldots, 31$ and $\sum_{j=2}^{31} w_j = 1$. In this expression, $V$ is a $(30 \times 30)$ symmetric and positive semidefinite matrix.[7]

Turning to the results, the left panel of Figure 1 displays the time series for real GDP in the UK (blue line) and in the doppelganger economy (red line). The dashed lines indicate periods for which only forecasts are available. The shaded area represents one standard deviation of the pre-treatment difference between the UK and its doppelganger. Note that the match is imperfect as our procedure determines 30 parameters (country weights)

[7] The choice of $V$ is not trivial since it affects the weighted mean squared error of the estimator and represents the different relevance assigned to the characteristics in $X_1$ and $X_0$. Abadie and Gardeazabal (2003) choose $V$ to be a nonnegative diagonal matrix with higher weights allocated to units with large predictive power on the outcome variable of interest. We follow Abadie et al. (2010) and choose the elements of $V$ using a data-driven cross-validation approach.

Table 1: Composition of the doppelganger: country weights

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Australia | <0.01 | Austria | <0.01 | Belgium | <0.01 | Canada | 0.15 |
| Chile | <0.01 | Czech Republic | <0.01 | Estonia | <0.01 | Finland | <0.01 |
| France | <0.01 | Germany | <0.01 | Greece | <0.01 | Hungary | 0.24 |
| Iceland | <0.01 | Ireland | 0.04 | Israel | <0.01 | Italy | 0.03 |
| Japan | 0.25 | Korea | <0.01 | Luxembourg | 0.04 | Mexico | <0.01 |
| Netherlands | <0.01 | New Zealand | 0.04 | Norway | 0.03 | Portugal | <0.01 |
| Slovak Republic | <0.01 | Slovenia | <0.01 | Spain | <0.01 | Sweden | <0.01 |
| Switzerland | <0.01 | United States | 0.19 | | | | |

in order to match more than 80 observations. This being said, prior to the referendum both series display a very high degree of co-movement—both at low and high frequencies. We are thus confident that the doppelganger provides a meaningful counterfactual which allows us to quantify the effect of the referendum shock on economic activity in the UK.

Table 1 displays the country weights (rounded to the second digit) which define the doppelganger economy. The United States and Canada, but also Japan and Hungary are assigned the largest weights. Together they account for more than 80 percent of the doppelganger dynamics. This is plausible, given the position of the UK in the world economy and the fact that it operates within the EU, but outside the Euro area (like Hungary). There are also smaller contributions from Ireland, Italy, Luxembourg, New Zealand, and Norway.

## 2.2 Measuring the immediate output effect of the Brexit vote

We are now in a position to quantify the output effect of the referendum shock. In order to do this we contrast the output performance in the UK and in the doppelganger economy in the quarters following the referendum shock. For this purpose the right panel of Figure 1 zooms in on the post-referendum period. As before, the shaded area corresponds to one standard deviation of the pre-treatment difference between output of the UK and the doppelganger. While traditional statistical inference in comparative case studies is difficult (for instance due to small samples and the absence of randomization), we interpret a post-treatment path of GDP that leaves the shaded area as evidence of a significant output effect of the referendum shock. To facilitate the quantitative assessment we express output deviations vis-à-vis the UK level in 2016Q2.

A number of observations stand out. While throughout the second half of 2016 there is hardly any effect of the referendum shock, a significant effect begins to materialize since 2017Q1. In fact, the UK seems to embark on a different growth trajectory relative to the doppelganger. By the end of the third quarter of 2017, output in the UK falls short of the doppelganger level by about 1.3 percent of GDP. The cumulative loss in terms of 2016 GDP equals approximately 20 billion pounds. Under current forecasts, the trend is going to persist and by the end of 2018 the doppelganger gap will amount to some −2 percent.

### 2.3 Placebo tests

Are these effects causal? To back the notion that the doppelganger gap is indeed caused by the referendum shock we conduct a number of placebo experiments (Abadie et al. 2010, 2015). The basic idea of the placebos is very intuitive. We can be confident that the synthetic control estimator captures the causal effect of an intervention as long as similar magnitudes are not estimated in cases where the intervention did not take place. Given that we are investigating the causal effect of an intervention at a particular point in time and in a particular country, there are two sets of placebo studies that naturally present themselves.

First, we run twelve time-placebo tests for which we shift the treatment date artificially backward in time: we consider treatment dates in all quarters from 2013Q2 to 2016Q1. In each instance, we construct a new doppelganger using exactly the same approach as in the benchmark specification. These doppelgangers are bound to differ from the baseline doppelganger, because the pre-treatment sample is shorter. Yet if there is indeed a causal effect of the actual treatment, then the placebo doppelgangers should diverge from the actual developments in the UK no earlier than the actual treatment took place.

The left panel of Figure 2 shows the results together with the series for actual GDP (blue line) and our benchmark doppelganger (red line). Each grey line represents the path of a doppelganger obtained for one placebo treatment. Reassuringly, each time placebo estimate tracks the pattern of the baseline doppelganger closely. Importantly, despite that

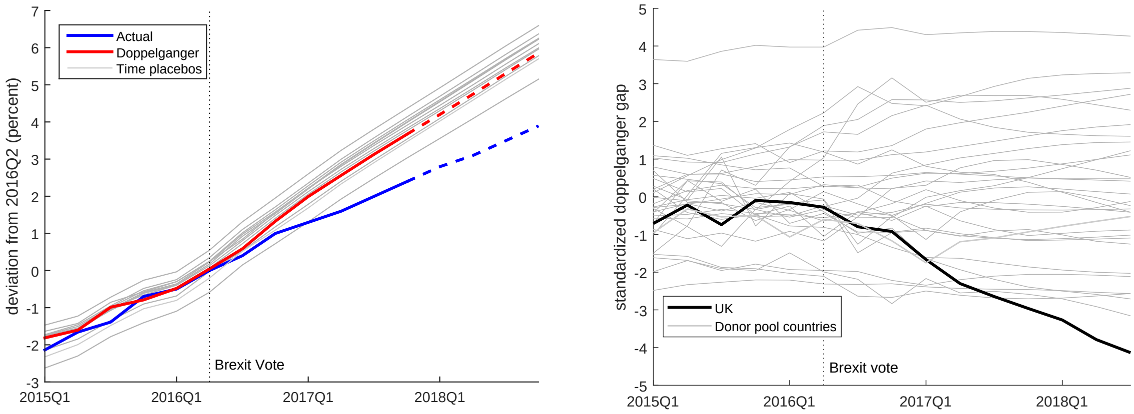


Figure 2: Placebo tests. *Note:* left panel shows real GDP of UK (blue line) and baseline doppelganger (red line), with gray lines representing time placebo doppelganger estimates with fictitious Brexit vote dates ranging from 2013Q2 to 2016Q1. Right panel shows the UK doppelganger *gap* (thick black line), with gray lines representing country placebo doppelganger gaps estimated by considering fictitious Brexit votes in all donor pool economies. For comparability, all doppelganger gaps are normalized by their respective pre-Brexit standard deviations.

the time-placebo studies work with earlier “fictitious” Brexit-vote dates, the resulting synthetic controls diverge from the UK data only after the actual treatment at the end of 2016Q2.

In a second set of tests, we estimate synthetic controls for each of the countries in the donor pool while exposing them to a placebo treatment at the end of 2016Q2. Once again, if our benchmark estimate for the UK is picking up the causal effect of the referendum shock, its effect should dominate any possible impact of the fictitious Brexit votes in the donor pool countries. The right panel of Figure 2 shows the doppelganger gaps for each of the country-placebo experiments, each represented by a thin grey line. For comparability, all doppelganger gaps are normalized by their respective pre-Brexit standard deviation. Relative to the country placebo estimates, the UK doppelganger gap stands out, both in terms of size and the systematic nature of the post-Brexit vote deviation. This again suggests that it is caused by the referendum shock and not random.[8]

[8]In principle, other countries may have been affected by the Brexit vote outcome through spill-over effects. Such interlinkages between countries would violate the assumptions about the donor pool countries not being affected by the treatment. Appendix A.3 shows additional country placebo results providing evidence against these concerns.

# 3 What drives the doppelganger gap?

The Brexit vote triggered a change in expectations regarding future policies. The doppelganger gap which has emerged since the vote—but before Brexit has actually happened—is likely to reflect forward-looking decisions of households and firms. Specifically, household and firm responses my be driven by a downgrade in the average economic outlook but also an increase in uncertainty about the future.

In what follows we first provide evidence that consumption and investment (gross fixed capital formation) indeed play a crucial role in explaining the doppelganger gap. Second, we document that increased uncertainty can explain at most half of the doppelganger gap. These results therefore attribute the majority of the output costs of the Brexit vote to gloomier average expectations of households and firms about the economic future of the UK.

## 3.1 The role of consumption and investment

In this part we quantitatively evaluate the contribution of forward-looking households and firms to the doppelganger gap. Specifically, we construct a counterfactual GDP path where we replace, in turn, consumption and investment in the UK with their respective doppelgangers (using expenditure shares of 2016Q2). On the basis of these counterfactuals, we show that consumption and investment (gross fixed capital formation) disproportionately contributed to the doppelganger gap. A negative contribution implies that the variable declined in the UK relative to the doppelganger.

The left panel of Figure 3 shows the results: both variables contribute to the doppelganger gap—the Brexit vote has caused a decline of consumption and investment (relative to the doppelganger). Except for the third quarter of 2016, the contribution of consumption is somewhat larger than that of investment. The right panel of Figure 3 shows a counterfactual GDP path (dashed line) assuming that both investment and consumption had been unaffected by the Brexit vote. It turns out that in this scenario the doppelganger gap is approximately closed. Taken together the other expenditure

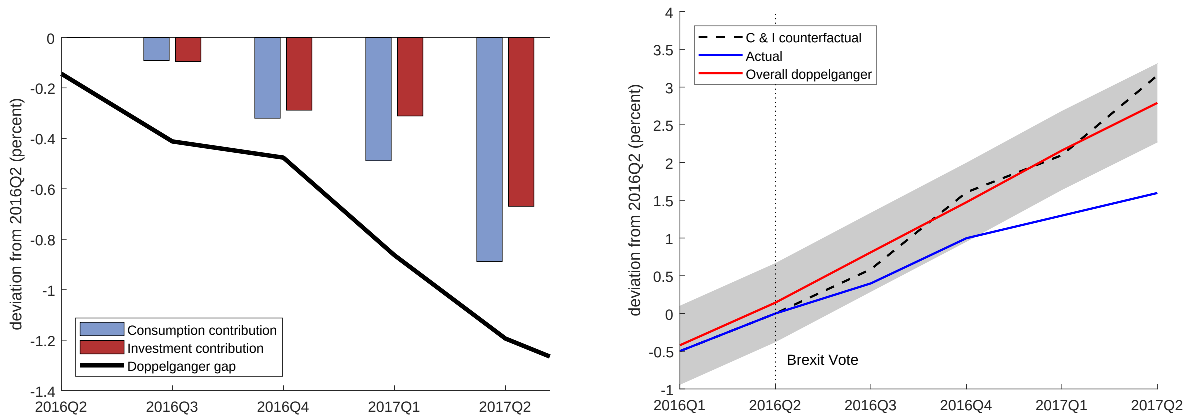


Figure 3: Accounting for the doppelganger gap. *Note:* left panel shows contribution of consumption (blue) and investment (red); right panel shows counterfactual GDP path (dashed line) assuming that both investment and consumption had been unaffected by the Brexit vote.

components therefore made only a moderate contribution to the doppelganger gap. Their individual contributions have also been limited during our sample period.[9]

Overall, our findings are consistent with the notion—central to modern macroeconomics—that the private sector responds in a forward-looking manner to an anticipated policy change.[10] In our case, it is clear the Brexit will amount to a bundle of policy measures which will result in economic disintegration of the UK and the European Union. Whether this is because of higher tariffs, non-tariff barriers or both, it is likely to bring about a reduction of living standards which, in turn, may rationalize reduced investment and consumption expenditures: not only in the future, but—because of anticipation effects—already today.

## 3.2 The role of uncertainty

In addition to a possible downgrade in the average economic outlook, however, the Brexit vote also increased economic uncertainty considerably—not least because the details of Brexit are still unclear. The heightened uncertainty due the Brexit vote is also likely to take its toll on investment and consumption expenditures, quite independently of any

[9] An exception is the third quarter of 2016, when there was a large built-up of inventories, offset by a decline of net exports (both in value and volume terms), despite the strong depreciation of the pound (Office for National Statistics 2016).

[10] Our focus is on consumption and investment spending. For evidence on how the Brexit vote impacts firms' financing decisions, see Berg et al. (2017).

anticipation effects. In fact, even if the economic outlook were unchanged on average, an increase of uncertainty will hamper economic activity (see Bloom 2009, for a seminal contribution). Hence, in order to assess the extent to which the doppelganger gap identified above is due to anticipation effects, we seek to purge it of the contribution of increased economic uncertainty.

We measure uncertainty using two widely used proxies, namely the Baker et al. (2016) Economic Policy Uncertainty (EPU) index and the macroeconomic uncertainty (MU) measure of Jurado et al. (2015), which has recently been computed for the UK by Redl (2017). For our application, it is especially important that the uncertainty measures capture *mean-preserving* increases in uncertainty. In the case of EPU, Baker et al. (2016, Table IV) show that controlling for future expectations changes little of their results. Similarly, the MU index is based on the conditional variance of the *unforecastable* component common to a large number of macroeconomic and financial variables, and therefore corrects for expected changes in the mean.

To quantify the role of uncertainty shocks for economic activity in the UK after the Brexit vote we estimate a structural vector autoregression (VAR) on quarterly time series data. Formally, we employ the following model:

$$Y_t = c + A(L)Y_{t-1} + \nu_t \ , \tag{3.1}$$

where $c$ is a constant term, $A(L)$ is a lag polynomial of order 4, and $\nu_t \sim (0, \Omega)$ is a vector of white noise errors. The vector of endogenous variables $Y_t$ contains the following six variables: the log of the MU proxy, the log of the EPU index, the log of stock returns, the bank rate of the Bank of England, the log of real gross capital formation, and the log of real GDP. Due to limited availability of the uncertainty proxies, we estimate the VAR model on observations for the period 1997Q1 until 2017Q2.[11]

We identify uncertainty shocks on the basis of a Choleski decomposition where we order variables as listed above. As we order the uncertainty proxies first and second,

[11] In specifying our VAR model we largely follow Baker et al. (2016), except for the addition of the MU proxy and the data frequency. We estimate the model on quarterly data since our interest is focused on how GDP responds to uncertainty shocks. Details on the data can be found in the Appendix A.1.

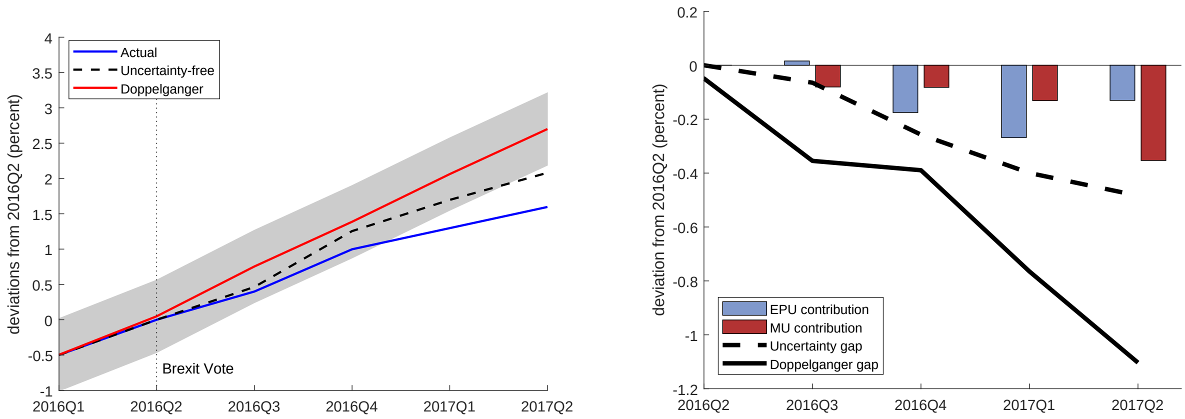


Figure 4: The role of uncertainty. *Note:* left panel: counterfactual GDP path (dashed line) when uncertainty shocks are shut off; right panel: contribution of economic policy uncertainty (blue) and macroeconomic uncertainty (red) to the doppelganger gap.

we allow uncertainty shocks to play the largest possible role: all variables may respond contemporaneously to an uncertainty shock. Uncertainty, instead, responds only sluggishly to the state of the economy as captured by the other variables included in the VAR model. In the appendix, we perform a number of robustness exercises which include the relaxation of our identification assumptions. There, we also display impulse response functions to an uncertainty shock which we find to be in line with what Baker et al. (2016) and Jurado et al. (2015) find for US data.

We use the estimated VAR model to quantify the contribution of uncertainty to the doppelganger gap. The left panel of Figure 4 shows the time path of actual UK GDP and its doppelganger, together with that of "uncertainty-free" GDP (dashed line). The latter is constructed by setting all uncertainty shocks to zero following the Brexit vote. As, by construction, all six shocks taken together generate time-paths of the endogenous variables which exactly track the data, switching off the two uncertainty shocks provides us with the counterfactual GDP path which would have been observed in the absence of uncertainty shocks. Note that this experiment is designed to give a potentially large role to uncertainty when it comes to accounting for the doppelganger gap, because we do not consider the possibility that there are uncertainty shocks after 2016Q2 which are unrelated to the Brexit vote.

That said, the figure documents that the contribution of uncertainty to the doppelganger gap is somewhat limited: uncertainty shocks accounts for about 1/3 of the gap. This can also be seen in the right panel of Figure 4 which presents the distinct contributions of macroeconomic uncertainty and economic policy uncertainty (their sum is shown as the "uncertainty gap") to the doppelganger gap. Both types of uncertainty had a detrimental, but limited effect on the UK economy following the Brexit vote.

In the appendix, we report results of various robustness checks. Overall, we find that the contribution of heightened uncertainty to the doppelganger gap is sizeable, but does not exceed 50 percent. This is noteworthy, because in our framework we give uncertainty shocks the largest possibility to impact economic activity. Not only do we include two measures of uncertainty in our VAR model. We also order them before the other variables such that they may impact them within the quarter and switch off *all* uncertainty shocks when we simulate a counterfactual GDP path. Despite this—once we strip the doppelganger gap of the uncertainty effect—more than half of the original gap remains. The remaining gap reflects anticipation effects: households and firms in the UK have reduced their expenditures because their economic outlook for the long-run has deteriorated as a result of the Brexit vote.

# 4 Conclusion

Natural experiments in macroeconomics are rare, but when they occur they offer unique insights into causal mechanisms and the validity of major assumptions underlying macroeconomic models. The unexpected outcome of the Brexit referendum in June 2016 offers such a window on causal relationships. Britain's future disintegration from the world's largest economy and trading block allows us to study the aggregate consequences of a sudden and fundamental change in expectations regarding future economic prospects.

In this paper, we exploit the natural experiment of the Brexit vote in order to assess whether and to what extent anticipated policy changes impact economic activity before they are actually implemented. We find that this is indeed the case. By the third quarter of 2017 the Brexit vote has caused a reduction of GDP by approximately 1.3 percent. On current forecasts the effect is likely to grow over the coming years. Zooming in on the behavior of the private sector, we find that households and businesses have adjusted their behavior in anticipation of Brexit, as macroeconomic theory predicts.

However, while the Brexit referendum shapes Britain's economic present in addition to its future it also raised economic uncertainty. The binary choice question "Should the United Kingdom remain a member of the European Union or leave the European Union?" left important issues open. And while the direction of future economic policies seems clear, the exact extent of Britain's economic disintegration from Europe remains unclear.

Against this background, we show that a wider dispersion of future economic outcomes, that is, heightened economic policy and macroeconomic uncertainty, accounts for close to half of the observed effects. Stripping the overall output loss due to the Brexit vote of the effect of heightened uncertainty—which is arguably temporary—leaves us with anticipation effects of households and firms, which have downgraded their expectations about future incomes. These effects are not only large, but to the extent that they reflect long-run outcomes, they are also there to stay.

## References

Abadie, Alberto and Javier Gardeazabal (2003). "The economic costs of conflict: a case study of the Basque country". *American Economic Review 93* (1), 113–132.

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller (2010). "Synthetic control methods for comparative case studies: estimating the effect of California's tobacco control program". *Journal of the American Statistical Association 105* (490), 493–505.

——— (2015). "Comparative politics and the synthetic control method". *American Journal of Political Science 59* (2), 495–510.

Alesina, Alberto and Nicola Fuchs-Schündeln (2007). "Goodbye Lenin (or not?): the effect of communism on people". *American Economic Review 97* (4), 1507–1528.

Baker, Scott R., Nicholas Bloom, and Steven J. Davis (2016). "Measuring economic policy uncertainty". *Quarterly Journal of Economics 131* (4), 1593–1636.

Barsky, Robert B. and Eric R. Sims (2011). "News shocks and business cycles". *Journal of Monetary Economics 58* (3), 273 –289.

——— (2012). "Information, animal spirits, and the meaning of innovations in consumer confidence". *American Economic Review 102* (4), 1343–77.

Beaudry, Paul and Franck Portier (2006). "Stock prices, news, and economic fluctuations". *American Economic Review 96* (4), 1293–1307.

Berg, Tobias, Anthony Saunders, Larissa Schäfer, and Sascha Steffen (2017). ""Brexit" and the contraction of syndicated lending". mimeo.

Billmeier, Andreas and Tommaso Nannicini (2013). "Assessing economic liberalization episodes: a synthetic control approach". *The Review of Economics and Statistics 95* (3), 983–1001.

Bloom, Nicholas (2009). "The impact of uncertainty shocks". *Econometrica 77* (3), 623–685.

Born, Benjamin and Johannes Pfeifer (2014). "Policy risk and the business cycle". *Journal of Monetary Economics 68*, 68–85.

Breinlich, Holger, Elsa Leromain, Dennis Novy, and Thomas Sampson (2017). "The consequences of the Brexit vote for UK inflation and living standards: first evidence". Mimeo, London School of Economics.

Fernández-Villaverde, Jesús, Pablo A. Guerrón-Quintana, Keith Kuester, and Juan F. Rubio-Ramírez (2015). "Fiscal volatility shocks and economic activity". *American Economic Review 105* (11), 3352–3384.

Fuchs-Schündeln, Nicola and Tarek Hassan (2016). "Natural experiments in macroeconomics". *The Handbook of Macroeconomics.* Vol. 2. Elsevier, 923–1012.

Handley, Kyle and Nuno Limão (2015). "Trade and investment under policy uncertainty: theory and firm evidence". *American Economic Journal: Economic Policy 7* (4), 189–222.

Handley, Kyle and Nuno Limão (2017). "Policy uncertainty, trade, and welfare: theory and evidence for China and the United States". *American Economic Review 107* (9), 2731–2783.

Jurado, Kyle, Sydney C. Ludvigson, and Serena Ng (2015). "Measuring uncertainty". *American Economic Review 105* (3), 1177–1216.

Limão, Nuno and Giovanni Maggi (2015). "Uncertainty and trade agreements". *American Economic Journal: Microeconomics 7* (4), 1–42.

Ludvigson, Sydney C., Sai Ma, and Serena Ng (2015). "Uncertainty and business cycles: exogenous impulse or endogenous response?" NBER Working Paper 21803.

Novy, Dennis and Alan M. Taylor (2014). "Trade and uncertainty". NBER Working Paper 19941.

Office for National Statistics (2016). *Quarterly national accounts: quarter 3 2016*.

Ramiah, Vikash, Huy Pham, and Imad Moosa (2016). "The sectoral effects of Brexit on the British economy: early evidence from the reaction of the stock market". *Applied Economics*, 1–7.

Redl, Chris (2017). "The impact of uncertainty shocks in the United Kingdom". Working Paper No. 695. Bank of England.

Sampson, Thomas (2017). "Brexit: the economics of international disintegration". *Journal of Economic Perspectives 31* (4), 163–184.

Schmitt-Grohé, Stephanie and Martín Uribe (2012). "What's news in business cycles". *Econometrica 80* (6), 2733–2764.

# A Appendix

## A.1 Data

The donor group contains all OECD countries for which we were able to obtain contiguous real GDP data starting in 1995Q1 (see Table 1 for a list). For the pre-Brexit-vote period, we use real GDP from the OECD Economic Outlook database, both for the UK and for the doppelganger. For the doppelganger, we also use this dataset for the post-Brexit-vote period, where data from 2017Q2 onwards are forecasts. For the UK from 2016Q1 to 2017Q3, we splice the OECD data in 2015Q4 with realized growth rates from the Office of National Statistics (ONS). From 2017Q4 till 2018Q4, we use real GDP growth rate forecasts from the Bank of England. The data for the decomposition exercise is build similarly. Consumption and investment for the control group have been obtained from the OECD Quarterly National Accounts and from the Office of National Statistics (ONS) for the UK. Real private consumption is the sum of real final consumption expenditure of both households and non-profit institutions serving households, real investment is total gross fixed capital formation.

For the uncertainty analysis, we use the Baker et al. (2016) Economic Policy Uncertainty index available at `www.policyuncertainty.com`. The index is based on a (standardized) count of newspaper articles containing the terms uncertain or uncertainty, economic or economy, and one or more policy-relevant terms. The macroeconomic uncertainty index, based on Jurado et al. (2015), has been computed for the UK in Redl (2017) and has been made available to us by Chris Redl. Stock market returns are based on the Datastream total market total returns index for the UK (TOTMKUK). The policy rate is the Bank of England bank rate.

## A.2 Further details on country placebo studies

The main text showed that the UK doppelganger gap stands out compared to estimates of a fictitious Brexit vote event in all the donor pool countries. This appendix shows alternative measures quantifying the country placebo results presented in the main text and provides a discussion.

Let us first define two statistics enabling a comparison between the UK doppelganger gap and the respective country placebo estimates. Both statistics are relative measures of pre- and post-treatment fit in the UK and the donor countries. These statistics are the relative root mean squared prediction error (RMSPE) and the maximum absolute prediction error (MAPE). These relative measures are defined as $\rho_1 = RMSPE_{post}/RMSPE_{pre}$ and $\rho_2 = MAPE_{post}/MAPE_{pre}$. The pre-intervention fit is given by

$$RMSPE_{pre} = \sqrt{\frac{1}{T_0 - 1} \sum_{t=1}^{T_0 - 1} (Y_t^* - Y_t)^2} \tag{A.1}$$

$$MAPE_{pre} = \max |Y_t^* - Y_t|, \quad t \in [1, T_0 - 1] \tag{A.2}$$

The post-treatment measures of fit are defined similarly with the treatment date prediction error normalized to zero

$$RMSPE_{post} = \sqrt{\frac{1}{T - T_0 - 1} \sum_{t=T0}^{T} \left(Y_t^* - Y_t - Y_{T_0}^* + Y_{T_0}\right)^2} \tag{A.3}$$

$$MAPE_{post} = \max \left|Y_t^* - Y_t - Y_{T_0}^* + Y_{T_0}\right|, \quad t \in [T_0, T] \tag{A.4}$$

Figure 5 depicts these two relative measures highlighting that the UK stands out with a particularly large post-treatment doppelganger gap. This provides further support for our findings in the main text that the estimates provide significant evidence in favor of a negative effect of the Brexit vote on UK GDP.

Let us now discuss the above definitions in more detail. First, the reason for considering relative measures of fit is that different countries are characterized by different degrees of accuracy with which the synthetic control tracks the data. In our sample, this heterogeneity in the degree of accuracy is enormous. While the root mean squared prediction error between 1995 and 2016 is 0.005 in the UK, it is e.g. 0.13 in Greece or 0.08 in Ireland. The average pre-treatment RMSPE for the donor countries is almost five times as large as that for the UK. Therefore, when comparing the post-treatment deviations across countries, one must take into account the much poorer fit of the synthetic controls in the donor pool countries. Second, the reason for normalizing the post-treatment prediction error

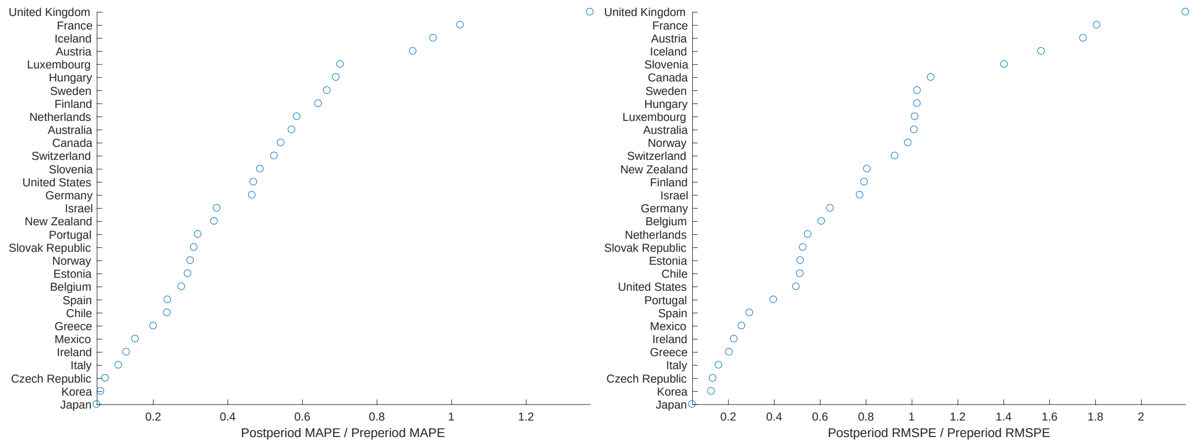


Figure 5: Relative measures of the pre- and post-treatment doppelganger gaps. *Note:* left panel shows the relative maximum absolute prediction error $\rho_2$, the right panel shows the relative root means squared prediction error $\rho_1$.

to zero at the treatment date accounts for the fact that the post-treatment time-path of the prediction error may be a continuation of previous trends rather than the result of the treatment. Examples of this are given in Figure 6 which plots the log-difference between the synthetic control and the data for the UK and three other countries which exhibit large post-treatment deviations. However, as is apparent from the figure, these post-treatment deviations are the result of a poor prior fit, rather than of the Brexit vote. Therefore, normalizing the treatment prediction error to zero accounts for the fact that certain countries "inherit" a large deviation around 2016 simply due to the poor fit of the synthetic control.

## A.3 Robustness regarding donor pool

The country placebo studies in the main text reveal that the UK stands out in terms of its post-treatment deviation, relative to the average pre-treatment fit. This suggests that indeed our baseline results are picking up a causal effect of the Brexit vote on UK GDP, since other countries do not display large deviations following their own (fictitious) Brexit referendum. Nevertheless, several countries (Austria, France, Iceland and Slovenia) do display relative post/pre-treatment deviations larger than 1 (see Figure 5), suggesting that perhaps they were subject to spillover effects from the UK. Such spillovers, however, would violate the assumption of no treatment in the donor pool countries.

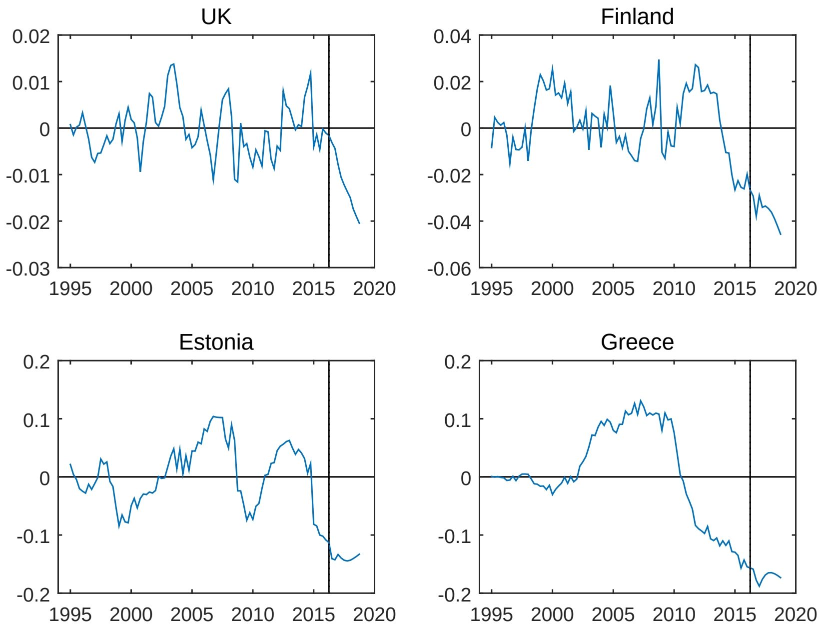


Figure 6: Deviations of synthetic controls from data (in log points).

In this subsection, we investigate whether our benchmark results are robust to the exclusion of certain countries from the donor pool. We consider two exercises. First, we re-estimate our baseline model, but exclude Austria, France, Iceland and Slovenia from the donor pool of countries which display large post-treatment doppelganger gaps. This exercise checks the robustness of our baseline results to the presence of cross-country spill over effects. Table 2 displays the country weights in the baseline and those obtained when re-estimating the model while excluding Austria, France, Iceland and Slovenia from the donor pool. Figure 7 then shows the evolution of UK GDP, the baseline synthetic control and the doppelganger based on the restricted donor pool. As can be seen, the estimated weights using the restricted sample are very similar to those in the baseline estimation. Similarly, the resulting synthetic control is almost indistinguishable from it's baseline counterpart.

Second, we iteratively re-estimate our baseline model while omitting in each iteration one of the countries which received a positive weight in the baseline estimation. This robustness check investigates whether one particular donor pool country is the main driver of our results. Figure 8 shows the baseline doppelganger gap together with that estimated with the restricted pool of countries, each omitting one of the countries which

Table 2: Composition of doppelganger in restricted donor pool excluding large post-treatment doppelganger gaps

| | | | | | |
|---|---|---|---|---|---|
| | | *baseline* | | | |
| Australia | 0.00 | Austria | 0.00 | Belgium | 0.00 |
| Canada | 0.15 | Chile | 0.00 | Czech Republic | 0.00 |
| Estonia | 0.00 | Finland | 0.00 | France | 0.00 |
| Germany | 0.00 | Greece | 0.00 | Hungary | 0.24 |
| Iceland | 0.00 | Ireland | 0.04 | Israel | 0.00 |
| Italy | 0.03 | Japan | 0.25 | Korea | 0.00 |
| Luxembourg | 0.04 | Mexico | 0.00 | Netherlands | 0.00 |
| New Zealand | 0.04 | Norway | 0.03 | Portugal | 0.00 |
| Slovak Republic | 0.00 | Slovenia | 0.00 | Spain | 0.00 |
| Sweden | 0.00 | Switzerland | 0.00 | United States | 0.19 |
| | | *restricted donor pool* | | | |
| Australia | 0.00 | Austria | NA | Belgium | 0.00 |
| Canada | 0.15 | Chile | 0.00 | Czech Republic | 0.00 |
| Estonia | 0.00 | Finland | 0.00 | France | NA |
| Germany | 0.00 | Greece | 0.00 | Hungary | 0.23 |
| Iceland | NA | Ireland | 0.04 | Israel | 0.00 |
| Italy | 0.04 | Japan | 0.24 | Korea | 0.00 |
| Luxembourg | 0.04 | Mexico | 0.00 | Netherlands | 0.00 |
| New Zealand | 0.05 | Norway | 0.00 | Portugal | 0.00 |
| Slovak Republic | 0.00 | Slovenia | NA | Spain | 0.00 |
| Sweden | 0.00 | Switzerland | 0.00 | United States | 0.21 |

*Note:* Doppelganger weights on donor pool countries in the baseline specification and that in which Austria, France, Iceland and Slovenia are left out.

received a positive weight in our baseline specification. While there is some variation in the estimates, the overall conclusion remains unchanged indicating that the Brexit vote resulted in substantial output losses. The lowest estimated effect obtains in the case when Hungary is excluded from the donor pool. Even then, however, the output loss amounts to almost 2 percent of GDP at the end of 2018. Moreover, by excluding countries from the donor pool, we sacrifice goodness of fit. The case when Hungary is excluded, the mean squared prediction error worsens by 25 percent. In fact, the one-standard-deviation bands around the doppelganger gap in the baseline and the case when Hungary is excluded overlap suggesting that one cannot easily distinguish the two estimates quantitatively. Finally, Table 3 shows the new weights estimated using the restricted donor pools.

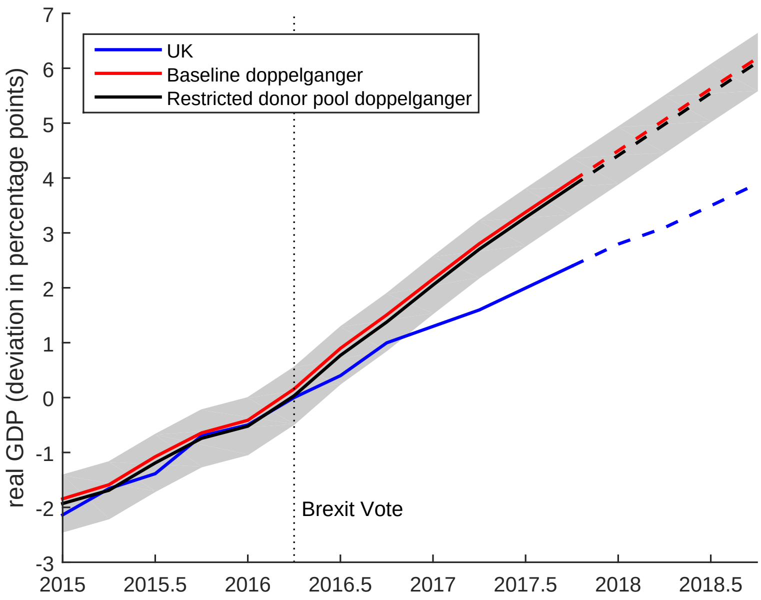


Figure 7: UK (blue line) vs. baseline doppelganger (red line) vs. restricted donor pool doppelganger (black line) in which we exclude large post-treatment doppelganger gaps. *Note:* dashed lines are forecasts; shaded area denotes one standard deviation of the pre-treatment difference between UK and Baseline doppelganger. UK before 2016Q1 based on OECD EO data, 2016Q1 – 2017Q3 based on ONS realizations, 2017Q4 – 2018Q4 based on BoE forecasts. Synthetic country based on OECD EO data (forecasts for 2017Q2 – 2018Q4).

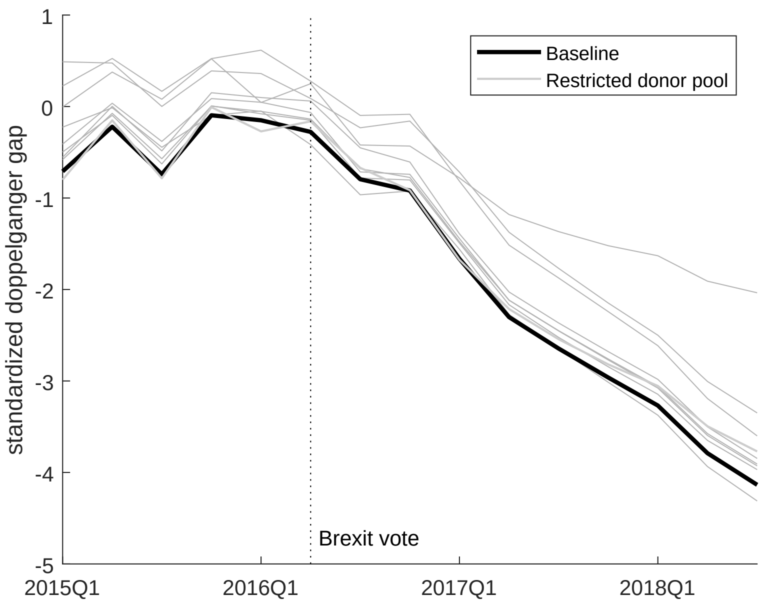


Figure 8: baseline doppelganger gap (thick black line) and restricted donor pool doppelganger gaps (thin gray lines). *Note:* restricted donor pool doppelganger gaps are obtained by re-estimating our baseline model while omitting iteratively each country which received a positive weight in our baseline specification.

Table 3: Composition of doppelganger in restricted donor pool excluding countries with positive baseline weights

| | baseline | I | II | III | IV | V | VI | VII | VIII | IX |
|---|---|---|---|---|---|---|---|---|---|---|
| Australia | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Austria | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Belgium | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Canada | 0.15 | NA | 0.28 | < 0.01 | 0.13 | < 0.01 | 0.16 | 0.16 | 0.13 | 0.25 |
| Chile | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Czech Rep. | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Estonia | < 0.01 | < 0.01 | 0.05 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | 0.01 | < 0.01 |
| Finland | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| France | < 0.01 | < 0.01 | < 0.01 | < 0.01 | 0.04 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | 0.02 |
| Germany | < 0.01 | 0.05 | < 0.01 | < 0.01 | < 0.01 | 0.18 | 0.03 | < 0.01 | < 0.01 | < 0.01 |
| Greece | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Hungary | 0.24 | 0.25 | NA | 0.27 | 0.25 | 0.26 | 0.24 | 0.26 | 0.22 | 0.24 |
| Iceland | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Ireland | 0.04 | 0.03 | 0.07 | NA | 0.04 | 0.03 | 0.05 | 0.04 | 0.04 | 0.06 |
| Israel | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Italy | 0.03 | 0.04 | 0.17 | 0.02 | NA | 0.10 | 0.04 | 0.01 | 0.04 | 0.03 |
| Japan | 0.25 | 0.20 | 0.25 | 0.21 | 0.26 | NA | 0.22 | 0.26 | 0.25 | 0.26 |
| Korea | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Luxembourg | 0.04 | 0.04 | < 0.01 | 0.08 | 0.04 | < 0.01 | NA | 0.05 | 0.04 | 0.03 |
| Mexico | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Netherlands | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| New Zealand | 0.04 | 0.07 | 0.17 | 0.03 | 0.02 | 0.12 | 0.06 | NA | 0.05 | 0.03 |
| Norway | 0.03 | < 0.01 | < 0.01 | 0.02 | 0.03 | 0.07 | 0.01 | 0.05 | NA | 0.09 |
| Portugal | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Slovak Rep. | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Slovenia | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Spain | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Sweden | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| Switzerland | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 | < 0.01 |
| U.S. | 0.19 | 0.32 | < 0.01 | 0.36 | 0.19 | 0.23 | 0.20 | 0.17 | 0.22 | NA |

*Note:* doppelganger weights on donor pool countries in the baseline specification and nine restricted donor pools. In each of the nine cases (I to IX) we omit one of the donor countries that received a positive weight in our baseline specification.

# B VAR results

This part of the appendix provides additional results for our VAR analysis in the main text. First, we show that the aggregate dynamics following the identified uncertainty shocks are consistent with findings in the literature. Second, we perform a series of robustness checks on our baseline specification.

## B.1 Impulse response functions

Figures 9 and 10 show the impulse response functions of the VAR variables to a one standard deviation increase in economic policy and macroeconomic uncertainty, respectively. As has been extensively discussed in the literature, both increases have contractionary effects.

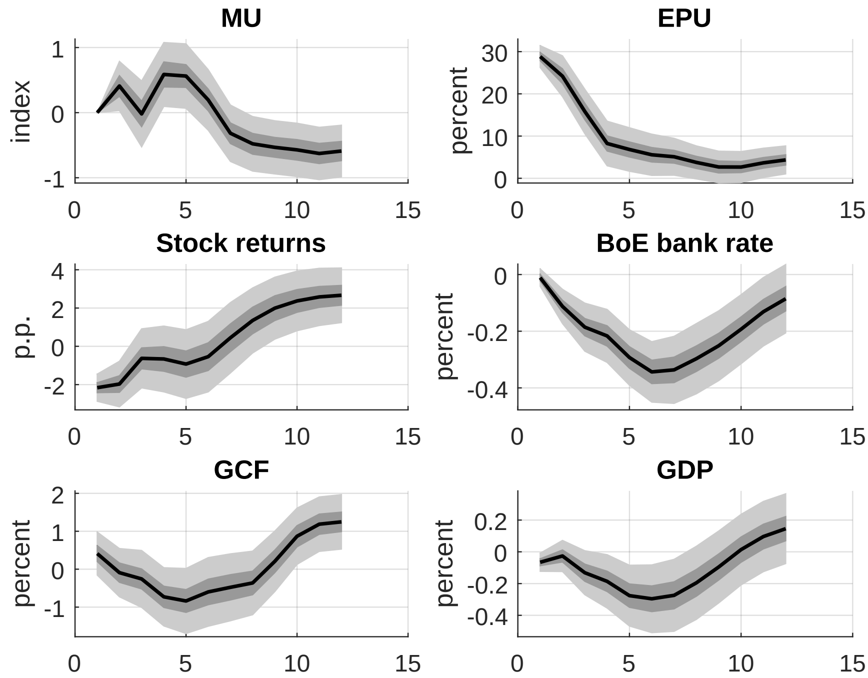


Figure 9: Impulse response functions: economic policy uncertainty. *Note:* impulse response functions to a one standard deviation increase in uncertainty. Light and dark shaded areas indicate one standard deviation and 90 percent bootstrapped confidence bands, respectively.

## B.2 Robustness

The baseline VAR specification includes four quarterly lags. Figure 11 shows that the baseline results are robust to alternative specifications. In particular, we first estimate the VAR using 3 lags as in Baker et al. (2016). Second, we estimate the VAR using a greater number of lags and finally we also consider a deterministic linear trend.

In addition to the lag and trend specifications, one may be concerned with the ordering of the uncertainty measure in the VAR. Figure 12 shows that the results are also robust to alternative orderings of the uncertainty measures in the VAR. The exception is when

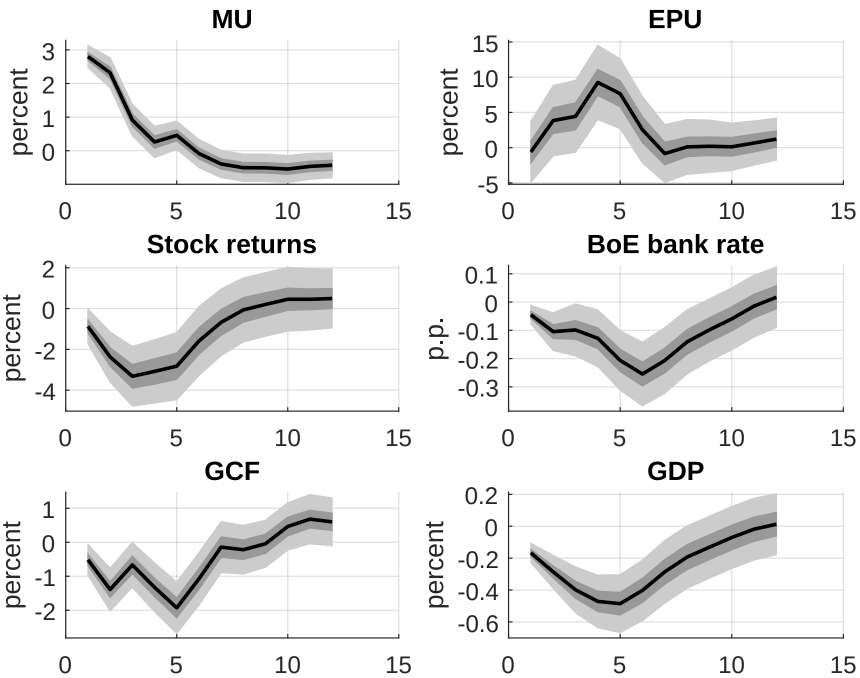


Figure 10: Impulse response functions: macroeconomic uncertainty. *Note:* impulse response functions to a one standard deviation increase in uncertainty. Light and dark shaded areas indicate one standard deviation and 90 percent bootstrapped confidence bands, respectively.

economic policy uncertainty is ordered last. However, this specification also produces an expansion following an increase in uncertainty (see Figure 13) suggesting that at a quarterly frequency this ordering is not suitable.

Finally, we consider estimating the baseline structural VAR with different variables, see Figure 14. The results are robust to including the share price index (OECD) instead of the total returns index, to considering aggregate employment instead of investment and to including a measure of financial uncertainty (computed by Redl 2017) as an additional source of uncertainty. In the latter case, financial uncertainty is ordered last as suggested by Ludvigson et al. (2015).

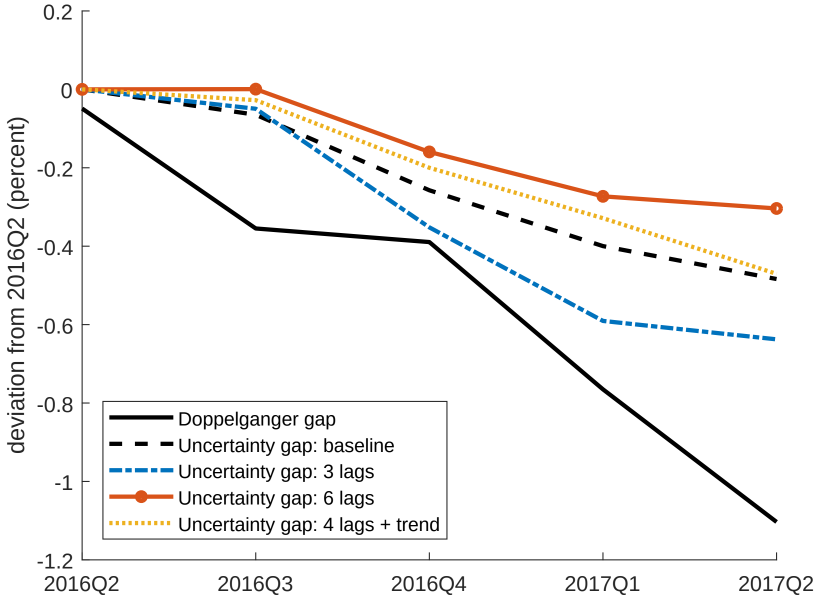


Figure 11: VAR results: robustness to lags and trends. *Note:* doppelganger gap (difference between doppelganger and data) and "uncertainty gaps" (difference between VAR-predicted time path without uncertainty shocks post 2016Q2 and data) for alternative specifications.

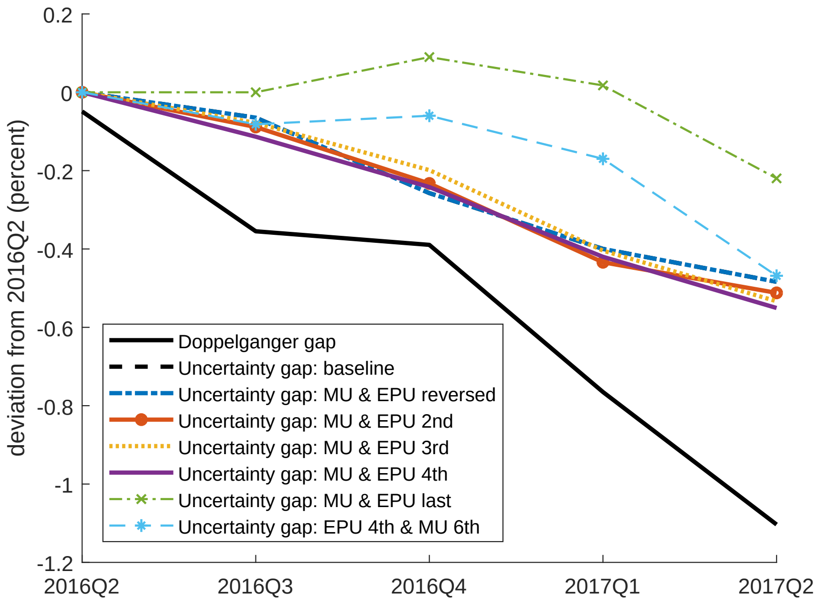


Figure 12: VAR results: robustness to uncertainty ordering. *Note:* doppelganger gap (difference between doppelganger and data) and "uncertainty gaps" (difference between VAR-predicted time path without uncertainty shocks post 2016Q2 and data) for alternative ordering of uncertainty in the VAR.

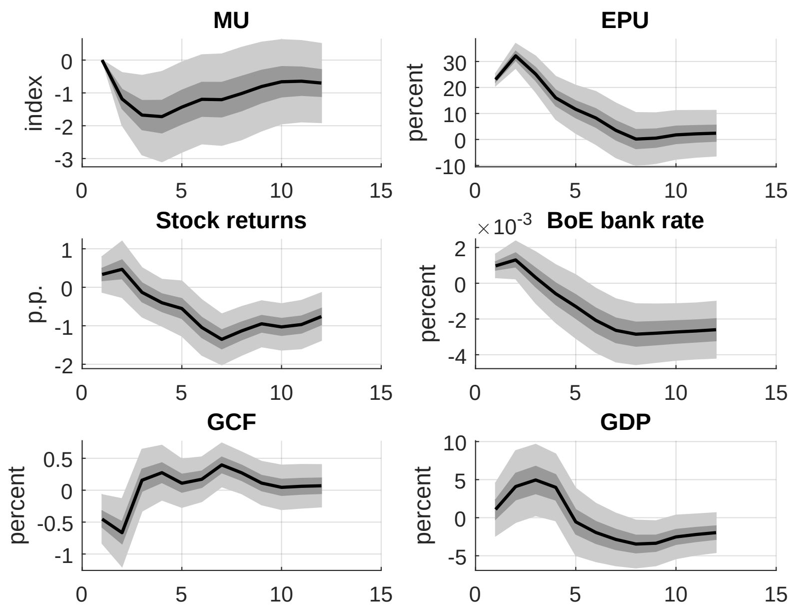


Figure 13: Impulse response functions: economic policy uncertainty ordered last. *Note:* impulse response functions to a one standard deviation increase in economic policy uncertainty. Light and dark shaded areas indicate one standard deviation and 90 percent bootstrapped confidence bands, respectively.

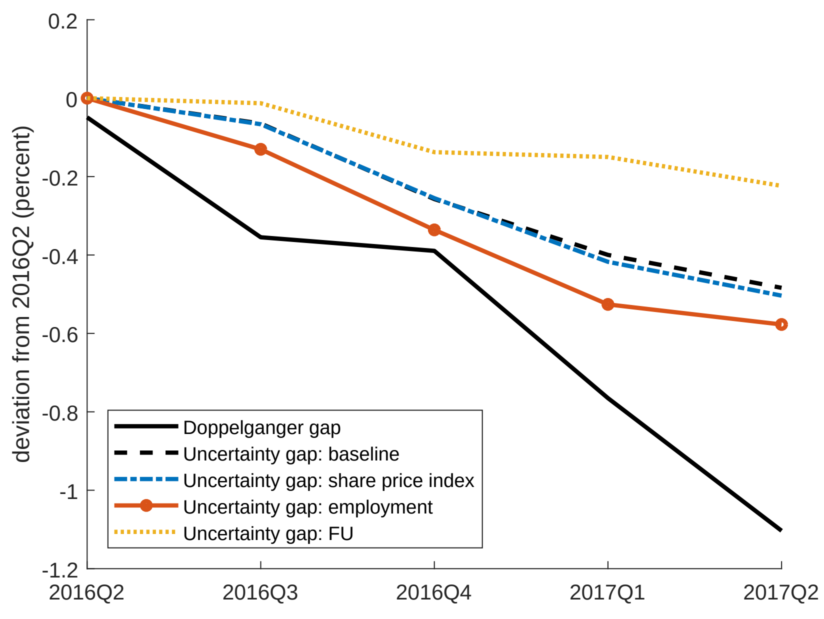


Figure 14: VAR results: robustness to other variables. *Note:* doppelganger gap (difference between doppelganger and data) and "uncertainty gaps" (difference between VAR-predicted time path of GDP without uncertainty shocks post 2016Q2 and data) for VAR specifications with other variables. "Share price index" replaces the total returns index with the share price index (OECD), "employment" replaces investment with aggregate employment (ONS) and "FU" includes a measure of financial uncertainty (see Redl 2017) ordered last in the VAR.