The economic cost of a referendum. The case of Brexit*

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Abstract

This paper estimates how GDP would have behaved in the United Kingdom after the Brexit referendum in the absence of the mentioned poll using the Synthetic Control Method. We contribute to the research on the effects of Brexit by quantifying the macroeconomic cost of this referendum before the actual Brexit has taken place. We find a large and significant negative effect of the Brexit referendum on the GDP of UK. This loss is increasing in time representing, in 2017 $Q4$, 1.71% of the observed GDP of UK.

Keywords: Brexit; synthetic control method; populism; economic cost.
JEL codes: E65, F15, F40, F52.

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1 Introduction

After the financial crisis of 2008, political parties in favor of leaving the European Union and/or the Euro have increasingly taken the attention of many voters in almost every country. Party for Freedom in the Netherlands, National Front in France, Lega Nord and Five Star Movement in Italy and UK Independence Party in the United Kingdom are some examples of populist groups that have based their campaigns on messages against the European club, blamed of being the main obstacle to welfare enhancing policies by limiting national sovereignty. The European Union membership referendum, celebrated in UK on June 23 2016 and known as "Brexit referendum", has been the first success of one of these parties. Both the decision to celebrate the referendum and its result were largely unexpected.

This comparative case study quantifies the consequences that this decisive referendum has produced on the UK economy so far by using Synthetic Controls Method (SCM) (Abadie and Gardeazabal, 2003; Abadie et al., 2010). By using a data-driven process to build the counterfactual, the SCM reduces ambiguities in the choice of the comparison units and makes transparent the comparability of the characteristics of the treated unit with the control units. In particular, the SCM makes explicit the contribution of each non-treated unit to the construction of the counterfactual and does not allow extrapolating outside the support of the control units, as traditional regression methods do. In our study, we exclude from the baseline "donor pool" of countries (i.e., those which could potentially be used to build the baseline counterfactual for UK in case of no Brexit Referendum) all European countries and United States because, given their high level of economic integration with UK, they could be indirectly affected by the Referendum. Moreover, following the recommendations of Doudchenko and Imbens (2016) and Ferman and Pinto (2018), we control for unobserved heterogeneity fixed in time and we present the pre-treatment fit obtained after de-trending the series.[1]

In the economic literature, some papers have concentrated on estimating the benefits from being a member of the European Union, therefore implicitly providing evidence on the cost of leaving the union. According to Haskel et al. (2007), increases in Foreign Direct Investment (FDI) lead to growth in national productivity. Following Dhingra et al. (2016), "multinational firms bring in better technological and managerial know-how, which directly raises output in their operations. FDI also stimulates domestic firms to improve – for example, through stronger supply chains and tougher competition". In the same report, the authors claim that higher trade costs with the EU (after Brexit) are likely to reduce FDI inflows. The researchers also found that, for new entrants, the EU membership exerted a positive effect on FDI: they estimate an increase in FDI of about 28% on average. In this line, Campos et al. (2018) found that, in the context of EU membership, "there are significant and substantial net benefits from deep integration in terms of higher per capita GDP and labor productivity ". The study concludes that per capita GDP and labor productivity increased in the United Kingdom due to the EU membership.

Many studies provide forecasts of the cost of leaving the EU for the UK. The

[1] Also the contemporaneously written contribution of Born and Schularick (2018) estimates the short run effect of the Brexit referendum by using the SCM and finds similar results. However, in the baseline donor pool they include European countries and they do not address the issues raised by Doudchenko and Imbens (2016) and Ferman and Pinto (2018).

most common approaches are econometric models (VAR, NiGEM), New Quantitative Trade Models (NQTM) and Computable General Equilibrium (CGE) models. Using a NQTM Dhingra et al. (2017) quantify the 'static' effects of Brexit on average household income. "In an 'optimistic' scenario, the UK will experiment a 1.3% fall in average income (or £850 per household). In a 'pessimistic' scenario, characterized by higher increases in trade costs, Brexit lowers average income by 2.6% (£1,700 per household)". In terms of GDP, "the overall fall in the UK is £26 billion to £55 billion". Some authors use instead CGE models. In particular, Ciuriak et al. (2015), Latorre et al. (2018) and Valverde and Latorre (2018) found that the estimated changes in GDP after Brexit with respect to the no Brexit scenario are $[-2.54\%, -0.97\%]$, $[-2.53\%, -1.23\%]$ and $[-1.15\%, -0.5\%]$, respectively. Moreover, institutions like OECD and Her Majesty's Treasury also carried out their own analysis demonstrating even more dramatic results than the ones present before. On the one hand, Kierzenkowski et al. (2016) suggest that Brexit will produce losses for British economy equivalent to a non-growth of -3.3% of its GDP. They derived this result by using CGE and NiGEM models. On the other hand, the British government department (Treasury, 2016) estimate that the cost of Brexit will range between $[-7.5\%, -3.8\%]$. To make this prediction, they implemented VAR and NiGEM models.

Though the bulk of the literature predicts sizable losses for UK after Brexit, some researchers, mass media and politicians still defend that Brexit will not have relevant economic costs for the British economy. We find that these costs emerge even before the UK abandons the UE. The cost of this referendum is estimated in $\$2,376.76$ per citizen in the UK during the year 2017. We also find that this effect is increasing in the time and, therefore, it might be a lower bound for the future consequences of Brexit. In Section 2 and Section 3 we describe the empirical methodology and the data. In Section 4 and Section 5 we present the main results. Section 6 is devoted to robustness checks and Section 7 concludes.

2 Model

We estimate the impact of the Brexit referendum on the UK GDP using a comparative case study analysis. Following the seminal work of Abadie et al. (2010), we construct a counterfactual for the UK GDP, a so-called synthetic UK, as a weighted average of the GDP of other developed and developing economies. The aim is to choose the weights that are able to replicate the actual dynamics of the UK GDP before referendum and to use them to build the counterfactual GDP of the UK in the post-referendum period.

The effect of the treatment (referendum) on the treated (UK) in this model is represented by:

$$\tau_{it} = Y_{it}^T - Y_{it}^C$$

where:
Y_{it}^T = represents the outcome of unit "i" in case of treatment.
Y_{it}^C = represents the outcome of unit "i" in case of no treatment.

In the post-treatment period, for a treated individual, we can observe only Y_{it}^T, our aim is to estimate Y_{it}^C for the post-treatment periods $t \geq T_0$ (where T_0 is the period when the treatment takes place).

Abadie et al. (2010) express the potential outcomes through the following model:

$$Y_{it}^T = \delta_t + \alpha_{it} D_{it} + \mathcal{V}_{it}$$

$$Y_{it}^C = \delta_t + \mathcal{V}_{it}$$

$$\mathcal{V}_{it} = \theta_t Z_i + \lambda_t \omega_i + \varepsilon_{it}$$

where,
Z_t = vector of independent variables
λ_t = common factor (unknown)
θ_t = vector of parameters
ω_i = unobservable term (specific for each country)
ε_{it} = transitory shock (zero-mean)
D_{it} = takes value 1 when the country (i) is treated (UK in this case), and 0 otherwise.
$\alpha_{it} D_{it} = \tau_{it}$

In this setting, Y_{it} and Z_{it} are observables for $N+1$ countries ($i = 1$ represents the country that is treated, and $i = 2, ..., N+1$ represent the non-treated countries). Treatment occurs at time $t = T_0$ and $t \in [1, T]$.

We are going to create a counterfactual for the United Kingdom by constructing a weighted average of a donor pool of countries' outcomes. These weights, $W = (w_2, ..., w_{N+1})$, are allocated to each country such that this counterfactual is as similar as possible to the UK GDP before the treatment. They have the restrictions of being non-negative, $w_i \geq 0$ $(i = 2, ..., N+1)$, and to sum up to one, $\sum_{i=2}^{N+1} w_i = 1$. Under these restrictions, SCM safeguards against extrapolation outside the support of the control individuals, following the "philosophy" of matching estimators. Abadie et al. (2010) show that as long as we choose W^* such that the outcome and the covariates of the synthetic counterfactual coincide with those of the treated individual $(i = 1)$ in the pre-treatment period

$$\sum_{i=2}^{N+1} w_i^* Y_{it} = Y_{1t}$$

$$\sum_{i=2}^{N+1} w_i^* Z_i = Z_1,$$

then

$$\hat{\tau_{it}} = Y_{1t} - \sum_{i=2}^{N+1} w_i^* Y_{it} \quad for\ all \quad t \geq T_0$$

is an unbiased estimator of τ_{it}.

In practice, W^* is chosen solving the following minimization problem:

$$min(X_1 - X_C W)' V (X_1 - X_C W)$$
$$s.t. = \begin{cases} w_i \geq 0 \; (i = 2, ..., N+1) \\ \sum_{i=2}^{N+1} w_i = 1 \end{cases} \quad (1)$$

where:
$X_1 = (k \; x \; 1)$ is a vector of pre-EU referendum (pre-treatment period) economic characteristics of the treated country. This vector X_1 contains all the pre-treatment values of UK GDP and the pre-treatment elements of vector Z for the UK.
$X_C = (k \; x \; N)$ is the matrix containing the same above characteristics for the N possible control countries (donor pool). In other words, each column of matrix X_C contains the variables of X_1 but for each non-treated country.
$V = (k \; x \; k)$ is the diagonal matrix (symmetric and positive semi definite) reflecting the relative importance of the different characteristics or economic predictors included in X.

Therefore, the optimal W^* minimizes the pre-treatment distance between the vector of characteristics of the treated country and the vector of the synthetic control characteristics according to the metric V. Matrix V is chosen among all positive definite and diagonal matrices such that the mean squared prediction error of the outcome variable during the pre-treatment period is minimized (see Abadie and Gardeazabal 2003).[2]

The SCM rests on two basic identification assumptions. The set of pre-treatment variables chosen should not anticipate the effects of the treatment and donor pool countries (countries that can potentially be selected to create the synthetic control) should not be affected, directly or indirectly, by the intervention. The former requirement means that the effect of the treatment has to start at the date (T_0) chosen without anticipation effects. That is, in the case of Brexit, the effect of the EU leaving referendum should be only observable after UK´s referendum took place. For the latter requirement to hold, we do not include countries that are members of the EU in the donor pool, because they are likely to be affected by the intervention. However, there are also countries outside the EU that could be indirectly affected by the intervention and therefore should not be included in the donor pool. We will further discuss, in the next section, why and which are the countries that are not selected as potential units to create a synthetic counterfactual.

Comparative studies are characterized, as all evaluation studies, by the uncertainty related to the ignorance about the ability of the control group to reproduce the counterfactual of the treated unit in the absence of treatment. Moreover, large sample inferential techniques usually does not apply in this type of studies (because of the small number of units in the control group). We follow Abadie et al. (2010) by using exact inferential techniques, that do not require a large number of control units. As in permutation tests, we implement the SCM for each country in the donor pool and we study whether the estimated effect of the treatment on the UK is large enough relative to the (exact) distribution of these estimated placebo effects. This

[2] In the empirical part, we use the Stata module *synth* specifying the *nested* option.

in space placebo test is presented in Section 5 together with an *in time placebo test*. The latter consists in applying the SCM to the UK randomizing the treatment date (T_0).

In Section 6 we present several robustness checks. First, inspired by the methodologies introduced by Abadie et al. (2015) and Campos et al. (2018), we address the concern that our results could be driven by the selection of countries present in the donor pool due to spill-over effects or to idiosyncratic shocks which could hit them in the post-treatment period.

Ferman and Pinto (2018) shows that, in a realistic setting of imperfect pre-treatment fit, the SCM estimator might be inconsistent even in situations in which a fixed effects/DID estimator would not, because the former is not able to wash out time constant unobserved heterogeneity. Therefore, also following Doudchenko and Imbens (2016),[3] in the second robustness check we employ a model using demeaned data. For this purpose we rewrite the notation of the economic characteristics to $(\ddot{X}_1 = X_1 - \bar{X}_1)$ and $(\ddot{X}_C = X_C - \bar{X}_C)$, where $\bar{X}_j$ contains the pre-treatment average characteristics for each unit. So that, the new minimization problem that solves for the vector of optimal weights W^* is given by:

$$\begin{aligned} &min(\ddot{X}_1 - \ddot{X}_C W)'V(\ddot{X}_1 - \ddot{X}_C W) \\ &s.t. = \begin{cases} w_i \geq 0 \; (i = 2, ..., N+1) \\ \sum_{i=2}^{N+1} w_i = 1 \end{cases} \end{aligned} \tag{2}$$

If time constant unobserved heterogeneity is additively separable, by using the above demeaned variables, we will take it into account.

According to Ferman and Pinto (2018), in the presence of non-stationary trends, a close to perfect pre-treatment match would not guarantee the asymptotic unbiasedness of the SCM if there is correlation between treatment assignment and common factors beyond the non-stationary trends. In other words, when pre-treatment fit is imperfect, in models that present a combination of $I(1)$ and $I(0)$ common factors, the SCM estimator does not reconstruct well the factors related to $I(0)$ common factors. Under this framework, $I(0)$ common factors should be uncorrelated with the treatment assignment, so that the SCM estimator is asymptotically unbiased. In a final robustness check, as they suggest, we present the pre-treatment fit after eliminating non-stationary trends and we use it as a diagnosis test for the SCM.

3 Data

We use quarterly country-level panel data for the period 2013 $Q1 - 2017$ $Q4$. The announcement of the Brexit poll took place on 20 February 2016, whereas the referendum pooling day occurred on 23 June 2016 (our treatment date). Therefore, we consider 13 periods before the treatment (Brexit) and 6 periods after the treatment.

[3] Doudchenko and Imbens (2016) point out that an implicit assumption of SCM is ruling out the possibility that the outcome of the treated unit is systematically larger (or lower) than those of other units. They propose to include an intercept parameter in the objective function of the SCM minimization problem.

As we mentioned before, the synthetic UK is constructed as a weighted average of potential countries. The control units that belong to the donor pool are 12 non-European countries: Australia, Canada, Chile, Japan, South Korea, Mexico, New Zealand and Turkey (member-countries of the OECD), Brazil, India and South Africa (partner-countries of the OECD) and Russia.[4]

The outcome variable Y_{it}, is the real Gross Domestic Product (GDP) in 2010 U.S. million dollars. The pre-treatment characteristics used as predictors in X_1 and X_0 are all pre-treatment outcome lags and the pre-treatment average of some selected predictors based on the literature on economic growth (see Abadie and Gardeazabal 2003): secondary school enrollment (percentage gross), foreign direct investment outflows and inflows (as a percentage of GDP), Business Confidence Index (BCI) and population density.[5]

4 Results

By applying the synthetic control method technique we find that the best replication of UK characteristics before the Brexit referendum (2016 Q2) is a weighted average of the following four countries: Mexico, Brazil, Korea and India (ordered from the highest weight to the lowest one). Table 1 shows the set of optimal weights that we use to reproduce UK after the referendum in the absence of Brexit poll. This set of weights is going to be used to obtain the synthetic United Kingdom after the treatment date (2016 Q2).

In the first two columns of Table 2 we compare the pre-treatment values of the explanatory variables of the United Kingdom with those of its synthetic counterpart.[6] The values of the diagonal elements of V linked to each predictor, which are reported in column 3, indicate that, once the pre-treatment values of the GDP are controlled for, the other predictors have very low predicting power (this is common in many applications of SCM). This explains why the weights that we have obtained produce a close to perfect fit for the lagged values of GDP while for the other variables, whose $V-weights$ are close to zero, some differences persist (Abadie et al., 2010).

With the information provided by Table 1 we can estimate the evolution of GDP in the United Kingdom in the absence of Brexit referendum (synthetic UK),

[4]We studied whether the potential donor countries suffer from a structural change (break) in their economic growth during the studied years. We found that, according to Perron Test (Perron, 1997), Indonesia presents a structural break in the 2016Q3. This intuition was confirmed by Chow Test (Chow, 1960), and this break is due to a sharp fall of Indonesia's GDP. Thus, Indonesia is discarded, despite the fact that, when it was included in the donor pool it got an optimal weight of 0 ($W^* = 0$). That means that Indonesia was not relevant when forming the synthetic counterfactual of UK. We also started our study with China and Israel in the donor pool, but we decided to let them outside the sample because they presented problems of missing values in their datasets. We initially excluded all European member states and Switzerland because they maintain a high level of economic relationships with UK and therefore they could be affected by the treatment. For the same reason, we have not included the United States (US). For the rest of non-European countries, we have not found data for this purpose.

[5]In the Appendix A, we list more precisely all the variables that are included in the study and their sources. We also tried to use a set of additional predictors: the share of GDP accounted for by agriculture, the share of GDP accounted for by manufacturing, investment and final consumption. Nevertheless, the results remained robust and the fit in the pre-treament period did not improve.

[6]The predictors that are not GDP are averaged over the entire pre-referendum period.

Table 1: Synthetic weights for the United Kingdom

Country	Weight (w*)
Australia	0
Canada	0
Chile	0
Japan	0
Korea	0.179
Mexico	0.554
New Zealand	0
Turkey	0
Brazil	0.201
India	0.066
Russia	0
South Africa	0

Table 2: Balancing of predictors and V-weights

	UK	Synthetic UK	Predictor V-weight
GDP (2013 Q1)	2343303	2343567	0.08
GDP (2013 Q2)	2355970	2353594	0.00
GDP (2013 Q3)	2376004	2377849	0.00
GDP (2013 Q4)	2388334	2394805	0.18
GDP (2014 Q1)	2408849	2412180	0.14
GDP (2014 Q2)	2429396	2431156	0.00
GDP (2014 Q3)	2447887	2445851	0.16
GDP (2014 Q4)	2466521	2468233	0.00
GDP (2015 Q1)	2474971	2477142	0.00
GDP (2015 Q2)	2489198	2488505	0.20
GDP (2015 Q3)	2499567	2505913	0.00
GDP (2015 Q4)	2517709	2510589	0.11
GDP (2016 Q1)	2523036	2531478	0.13
DNPOP	267.4253	162.3938	3.948e-07
SCSEC	125.7761	94.14681	1.330e-07
FDII	52.3811	30.2239	6.918e-07
FDIO	58.22206	12.1585	5.269e-07
BCI	100.9391	99.79369	1.158e-07

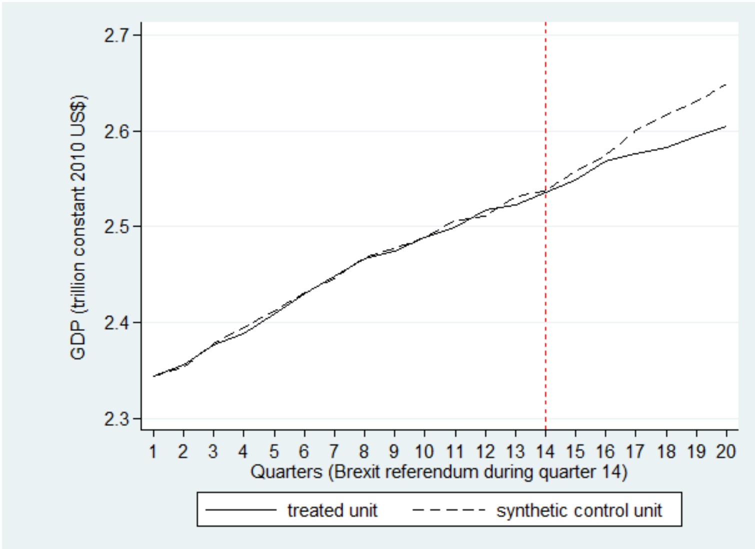
Note: Columns 1-2 of this table present the characteristics of UK and its synthetic control before the Brexit referendum. The last five predictors (population density, secondary school enrollment, foreign direct investment inflows/outflows, business confidence index) are averaged across the entire pre-treatment period. Column 3 presents the optimal V-weight that each predictor receives.

after the second quarter of 2016 and compare this series with the actual evolution of GDP for UK. The resulting graph is shown in Figure 1.[7] This figure, jointly

[7] The "x" axis of Figure 1 and of the other figures represents the time periods in quarters.

with Table 2, suggests that there is a combination of countries that reproduce the economic characteristics of UK before the EU referendum. During the first thirteen quarters (2013Q1-2016Q1), UK and synthetic UK grew almost together. It is only after the referendum, when these two series start diverging. SCM interprets the difference between the GDP of UK and its synthetic counterpart, after the polling date, as the effect of the EU referendum.

Figure 1: GDP of UK and its synthetic counterpart



Note: This figure presents the GDP of the UK and the estimated GDP for the synthetic UK before and after the Brexit referendum.

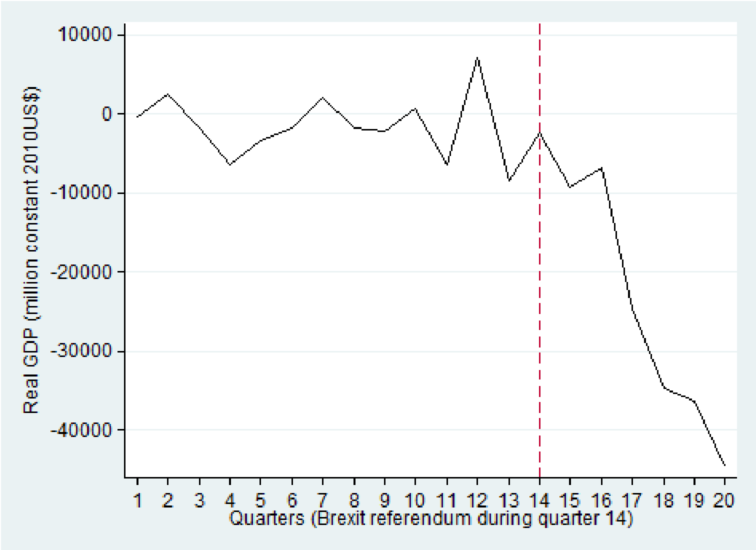
Therefore, it makes sense to plot the difference between the two series, so that we can more easily evaluate how the GDP of the synthetic unit evolves compared to the one of UK. Figure 2 shows the evolution of the GDP gap between these two units over the time. This gap is the estimated difference in GDP as a consequence of the EU referendum for the UK ($\hat{\tau_{it}}$). Despite the fact that the United Kingdom has experienced an increase in real GDP after the EU referendum, Figure 2 suggests that this increase would have been much higher if EU referendum would not have taken place (Governor of the Bank of England, Mark Carney, declared on November 2017 that "British economy should really be booming, and it is just growing").[8] It is worth mentioning that GDP gap is increasing in time, which means that the effect of Brexit referendum might be only a lower bound of the future Brexit effect.

We evaluate the size of Brexit referendum effect compared to the size of British GDP in Table 3. In the third column we report the estimated GDP gap as a percentage of the actual GDP. This ratio is increasing for almost every period. In the last quarter of 2017, GDP in synthetic UK is estimated to be \$44.5 billion higher than in the actual United Kingdom. Hence, Brexit represents a fall (economic cost) of 1.71% over the actual GDP for period 2017 $Q4$. During the whole period (2016Q3-2017Q4) that we analyze, the United Kingdom should have grown \$156.03 billion

Being the quarter 1 = 2013$Q1$, the quarter 14 = 2016$Q2$ (treatment period) and the quarter 20 = 2017$Q4$.

[8] Carney declared the above statement on 5 November 2017 to Robert Peston, a British journalist, on his TV program "Peston on Sunday".

Figure 2: Estimated difference between the actual and the synthetic GDP of UK



Note This figure presents the gap between the UK series and its synthetic counterpart. It shows that before Brexit referendum the gap is constant and it is around zero. However, after the poll the gap drops sharply.

more than it actually grew. This cumulative non-growth represents 1.01% over the total cumulative GDP for the same period of time (six quarters for which we have available data), a cost of $2,376.76 per UK citizen.

Table 3: Size of GDP gap

	Observed GDP (2010 US$ millions)	GDP gap (US$ millions 2010)	Gap over original GDP (%)
2016-Q3	254875.75	-9281.91	-0.36%
2016-Q4	2567829.25	-6873.86	0.27%
2017-Q1	2575888.5	-24594.16	-0.95%
2017-Q2	2582240.75	-34593.79	-1.34%
2017-Q3	2594141.75	-36196.55	-1.40%
2017-Q4	2604230.5	-44489.34	-1.71%

Note: Columns 1 and 2 present the data of the actual value GDP and the estimated GDP gap of the UK, respectively. Column 3 reports the gap as a percentage of the actual GDP.

5 In space and in time Placebo effects

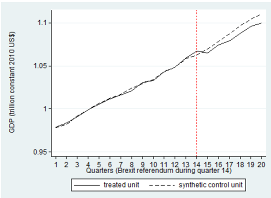
In this section we apply exact inference techniques to "in-space" placebo experiments and we perform an "in-time" placebo study.

The first placebo study is conducted "in space", by assigning the original treatment to each country included in the donor pool.[9] Therefore, we assign the treatment

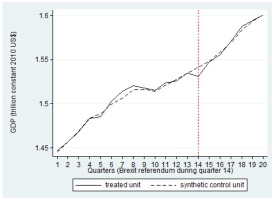
[9]Figure 3 does not present the estimated treatment effect for India and New Zealand because

to countries that did not experienced it. We observe the effect of the treatment for these countries in Figure 3. The effect obtained in Figure 2 is considerably larger when compared with the placebo effects estimated for the donor countries in Figure 3 (only for Russia we estimate a negative, though imprecise, effect). Therefore, we consider that the effect of Brexit referendum for the United Kingdom is big enough to be considered significant.

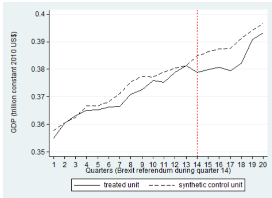
Figure 3: In-space Placebo Test.



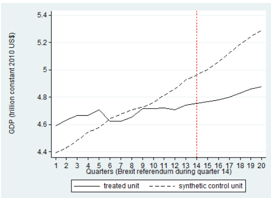
(a) Synthetic Australia



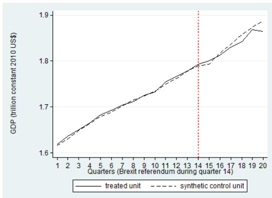
(b) Synthetic Canada



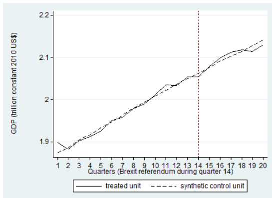
(c) Synthetic Chile



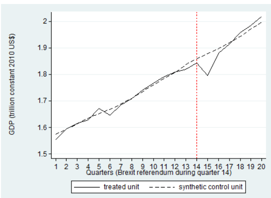
(d) Synthetic Japan



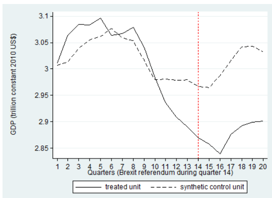
(e) Synthetic Korea



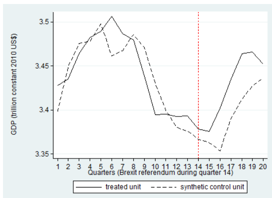
(f) Synthetic Mexico



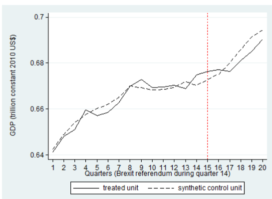
(g) Synthetic Turkey



(h) Synthetic Brazil



(i) Synthetic Russia



(j) Synthetic South Africa

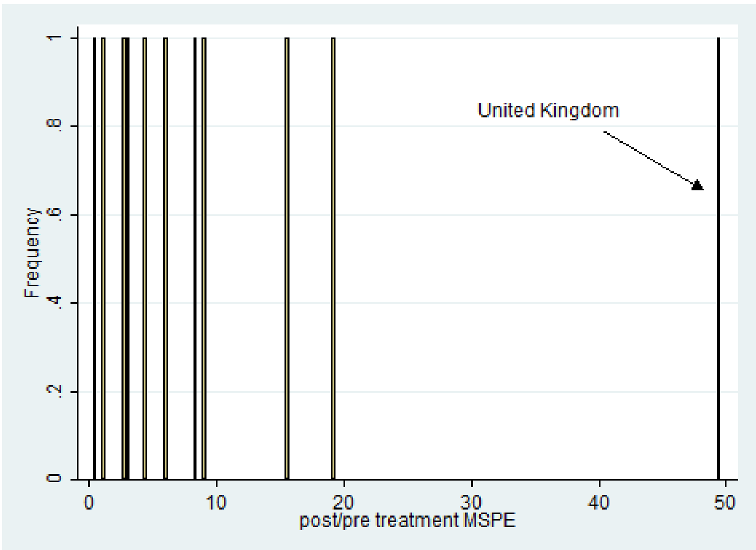
Note: We assign the treatment to countries (used in the donor pool) that have not experienced it and we estimate its (placebo) effect by SCM. The effect for UK reported in Figure 2 is considerably larger when compared with the effects for the donor countries presented in this figure.

A more refined way of testing the significance of the estimated effect of Brexit is considering the ratio of the of post-2016Q2 MSPE (Mean Squared Predictor Error) over the pre-2016Q2 MSPE for the United Kingdom and the other control countries. The mean squared predictor error measures the average gap for each country with

the model is not able to reproduce the GDP pre-treatment path of these two countries. i.e. the optimization routine is not able to find the minimum of equation 1. One of the virtues of SCM is exactly that it does not allow extrapolating outside the support of the control units, as traditional regression methods do.

respect to its synthetic counterpart. A large post-treatment MSPE does not indicate a large effect of the treatment if the synthetic control does not match the real data accurately before the treatment (i.e. if pre-treatment MSPE is also large). For this reason, Abadie et al. (2010) suggested to compare the ratio between post-intervention MSPE and pre-intervention MSPE of the treated unit to those of the units contained in the donor pool. Figure 4 shows that for the United Kingdom the post-treatment MSPE is about 50 times larger than the pre-intervention MSPE. The MSPE ratio for UK is well above the MSPE ratios for the control countries. As shown by Abadie et al. (2010), we can use these MSPE ratios to test the null hypothesis that the treatment has no effect and it is assigned randomly. The chance to select a country at random, with a MSPE ratio as high as the one for the UK, is $1/11 \simeq 0.09$, that is below the conventional 10% level of significance used in statistics. Then, we reject the null hypothesis and conclude that Brexit referendum had a significant effect on UK GDP.

Figure 4: Ratio of post Brexit referendum MSPE over pre Brexit referendum MSPE for the UK and the donor countries

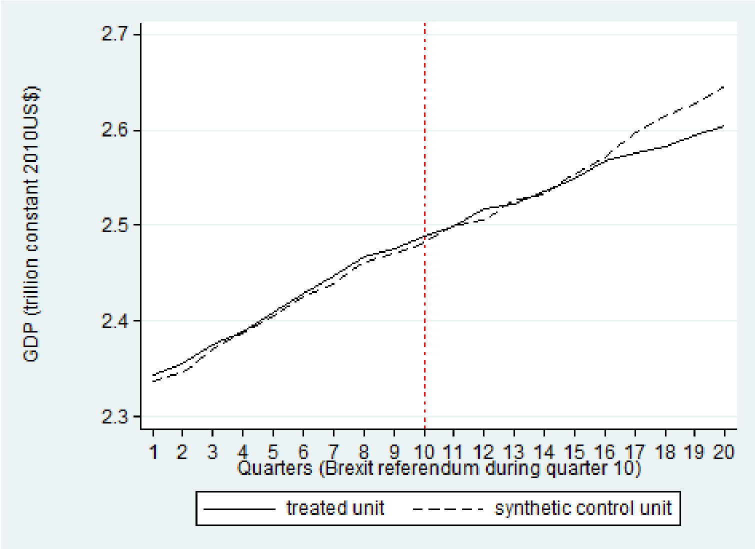


Note: This figure presents the ratios of post/pre MSPE (Mean Squared Predictor Error) for the UK and the countries in the donor pool.

The second placebo study is conducted "in time", by reallocating the date of the Brexit referendum to a period before the current referendum in order to assess anticipation effects. In particular, we set a new artificial date for Brexit referendum, $2015Q2$ (just in the middle of the time period horizon).[10] Figure 5 reports the results obtained when applying the SCM using the same donor pool but setting $T_0 = 2015Q2$. The synthetic United Kingdom almost perfectly replicates the trend of GDP in the UK along the period $2013Q1 - 2015Q1$ (before treatment). But most importantly, GDP trends, for both the UK and the synthetic UK, do not diverge until the period $2016Q4$, as in the baseline estimates. Until this moment, both units grew together. There is no jump around the new artificial treatment date.

[10]Note that period 10 corresponds to the date 2015Q2.

Figure 5: In-time Placebo Test: GDP of the United kingdom and GDP of the Synthetic United Kingdom



Note: This graph shows the GDP of the UK and its synthetic counterpart obtained by imposing a different period of treatment ($T_0 = 2015Q2$).

6 Robustness

The first two robustness checks investigate the sensitivity of our results to the chosen weights, i.e. how dependent are our results on the specific control countries. Indeed, our results could be biased by unobserved shocks and treatment spillovers affecting some donor countries in the post-treatment period. The last two robustness checks deal with the concerns raised by Ferman and Pinto (2018).[11] They suggest removing time-constant unobserved heterogeneity from the series and studying the performance of the SCM after detrending the series.

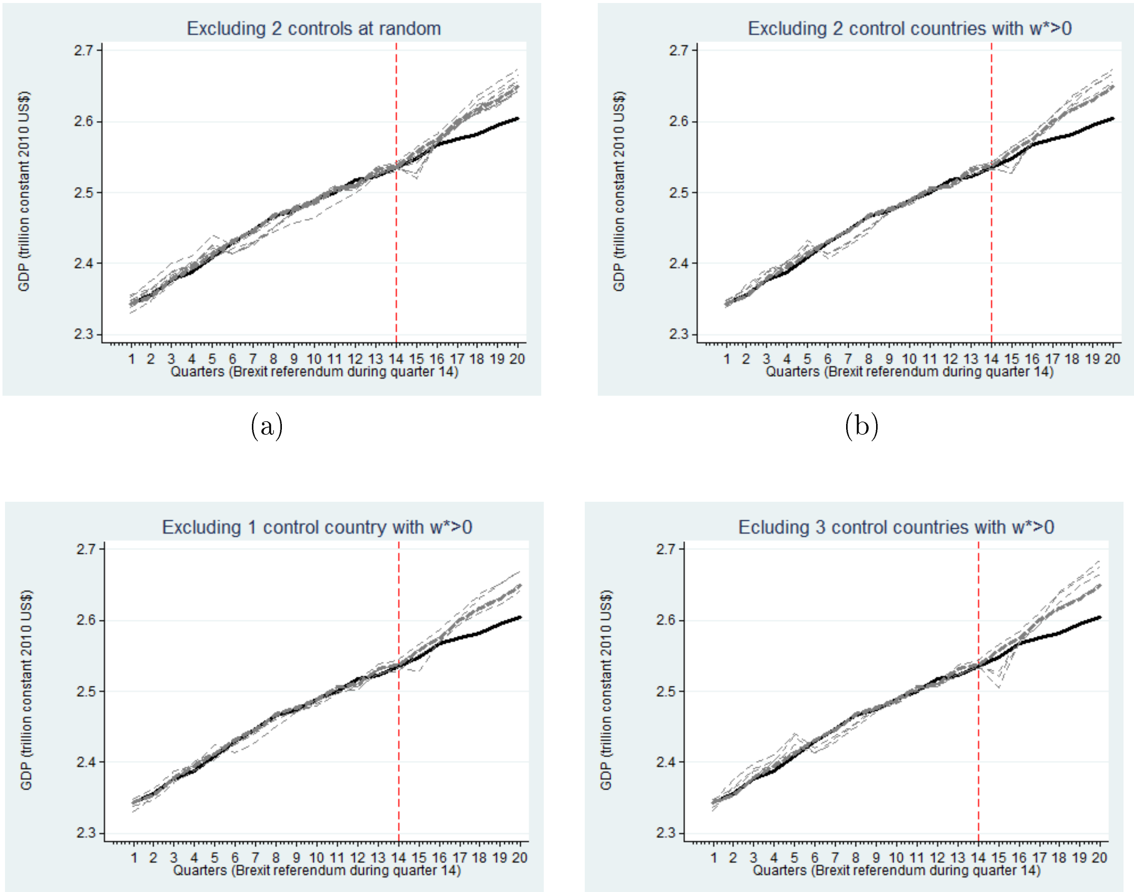
The first robustness check can be presented as a combination of four different tests inspired by the work of Abadie et al. (2015). In all of them we exclude some countries from the donor pool and we study how the estimated effect of Brexit changes when we try to reproduce the synthetic UK using these restricted samples. Figure 6 summarize the results of these analyses. The black dashed line identifies the actual GDP of UK, the gray thick dashed line corresponds the baseline synthetic UK estimated in Section 3 and the thin gray dashed lines indicate the synthetic UKs obtained by restricting the baseline donor pool. In graph (a), we exclude a pair of countries from the donor pool at random and we repeat this step 10 times. In graph (b), we present the results obtained by discarding a pair of countries among those that received positive weights in the baseline estimates. In this way we obtain other six alternative donor pools.[12] In graph (c), we apply the SCM by using the four possible donor pools obtained by removing one of the countries that received a positive weight in the baseline estimates. Finally, in graph (d) we report the estimates stemming from the four possible donor pools which can be obtained by excluding at the same time a different triplet of countries among those that received

[11] See Section 2 and the additional explanations given below in this section.

[12] Remember that in the baseline estimates only four countries obtained positive weights.

a positive weight in the baseline estimates. In all the four graphs, we observe that the synthetic units generated fit accurately the series of GDP for the United Kingdom before 2016Q2. Moreover, after this period, these synthetic units diverge, similarly to what happens to the baseline synthetic UK estimated in Section 3. Consequently, this figure suggests that the baseline results presented in the previous section are not driven by the inclusion in the donor pool of some specific country, in this way alleviating possible concerns about unobserved post-Brexit shocks.

Figure 6: Robustness check using the original donor pool sample



(a) (b) (c) (d)

Note: In this robustness check, we restrict the original donor pool of countries in 4 different ways, in order to study how robust are the results to changes in the composition of the pool. In Figure a) we extract, by pairs, countries from the baseline donor pool (at random). In Figure b) we extract, by pairs, countries from the baseline donor pool that have received $w^* > 0$ in the baseline estimation. In Figure c) we extract, individually, countries from the baseline donor pool that have received $w^* > 0$ in the baseline estimation. In Figure d) we extract from the donor pool groups of three countries that have received $w^* > 0$ in the baseline estimation.

For the second robustness check we augment the number of potential control countries by allowing other European countries to enter in the donor pool.[13] We take

[13]The countries included are Austria, Belgium, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Italy, Latvia, Luxembourg, Netherlands, Norway, Poland, Portugal, Slovak Republic, Slovenia, Spain, Sweden, Switzerland. We do not consider Iceland and Ireland because they do not have data for the Business Confidence Index (BCI) variable.

this decision after verifying that we do not have enough evidence showing that the GDP of these European countries is affected by this referendum (i.e. that that there are spillover effects of the referendum).[14] Among the countries belonging to this augmented set, made up by 34 countries (the baseline 12 countries and 22 European countries), we first select randomly 1000 donor pools made up by 12 countries, which is the size of the baseline donor pool. Then we estimate with the SCM the cost of the Brexit referendum for UK using these different and randomly chosen donor pools and we compare these estimates with our baseline estimate. Figure 7 shows the results of this robustness check. The black straight line represents the actual GDP of the United Kingdom, the dashed black line represents the baseline synthetic UK (built using the baseline donor pool made up by 12 non-European countries) and the gray dashed lines represent the different synthetic UKs estimated using these randomly chosen donor pools. Following the logic of Abadie et al. (2010), in Figure 7 we present only the synthetic UKs which attain a pre-treatment fit similar to the one obtained by the baseline synthetic UK and are, therefore, comparable with our baseline estimates. As a measure of pre-treatment fit we adopt the pre-treatment normalized mean squared error index introduced by Ferman et al. (2017):[15]

$$\widetilde{R}^2 = 1 - \frac{\frac{1}{T_0-1}\sum_{t=1}^{T_0-1}(Y_{1t}-\hat{Y}_{1t})^2}{\frac{1}{T_0-1}\sum_{t=1}^{T_0-1}(Y_{1t}-\bar{Y}_1)^2}. \tag{3}$$

where Y is the observed GDP of the real UK, $\hat{Y}$ is the estimated GDP of the synthetic UK and $\bar{Y}_1 = (\sum_{t=1}^{T_0-1} Y_{1t})/(T_0-1)$.

Given that the $\widetilde{R}^2$ of the baseline synthetic UK is about 0.99, we retain in Figure 7 those synthetic UKs with a $\widetilde{R}^2 \geq 0.9$ (i.e. about 70% of them; the results are not sensitive to changing this threshold). We find that the bulk of these synthetic UKs, built by randomly choosing different donor pools made up by 12 countries, imply similar or greater estimated losses due to the Brexit referendum. Therefore, also according to this robustness check, our results are not sensitive to changes in the countries included in the donor pool.[16]

The third robustness check is presented in Figure 8. Following equation 2, before applying the standard SCM we purge its average pre-Brexit GDP from every country's GDP.[17] As explained in Section 2, in the realistic setting of imperfect pre-treatment fit, and differently from the standard SCM, this demeaned SCM is robust

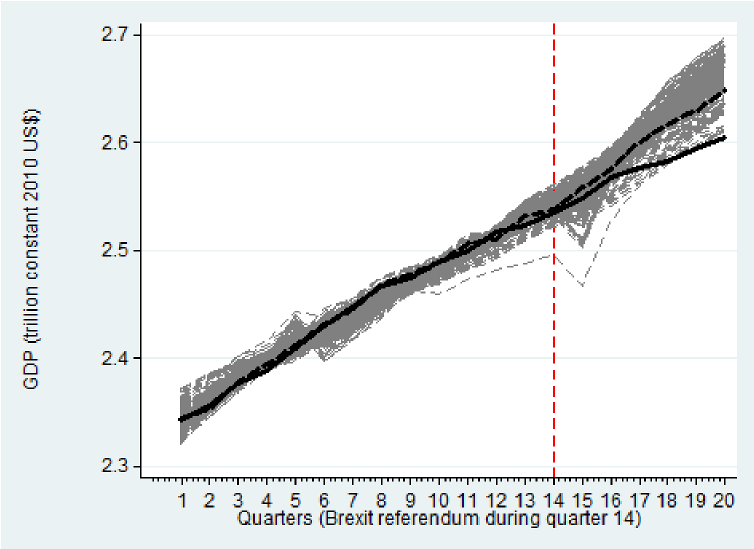
[14] We have estimated the synthetic unit for each of these EU countries by using the baseline donor pool of countries (non-European states). We do not find any significant effect of the Brexit referendum on the GDP of each of these economies (see Appendix B).

[15] This measure will be equal to 1 when the pre-treatment fit is perfect. However, differently from the standard R^2, it can be negative.

[16] This robustness check is probably less accurate than the previous one, because we cannot be completely sure that there are no spillover effects on the aggregate of the rest of European countries. Indeed, it might be the case that the poll affects these countries as a group, even though this effect is not significant for each European country.

[17] For this and the last robustness check, we use the baseline set of donor countries (i.e. we do not consider European countries). Moreover, following Ferman and Pinto (2018), we exclude from the explanatory variables all the variables different from GDP. Results including the same set of explanatory variables of the baseline specification are basically identical.

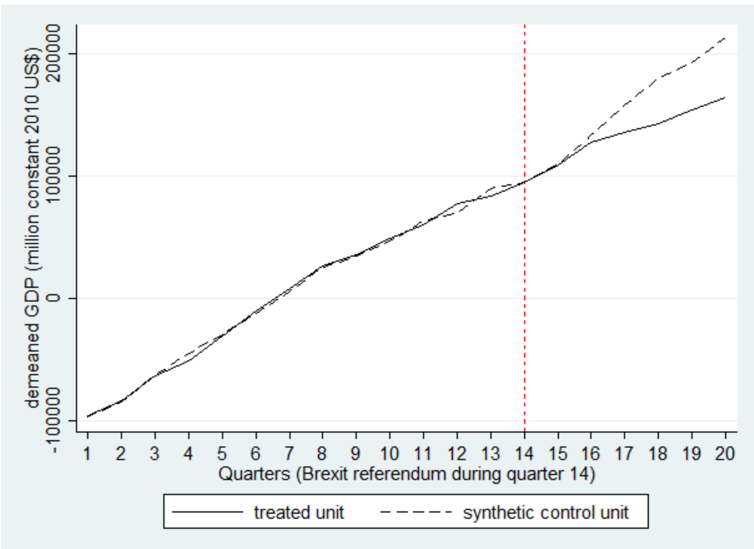
Figure 7: Random donor pools



Note: The black line of this figure represents the GDP of the UK and the back dashed line its baseline synthetic GDP. Each gray dashed line represents the synthetic GDP of UK estimated with randomly chosen donor pools potentially containing both European and Non-European countries. Each random donor pool contains 12 countries as the baseline donor pool and attains an $\widetilde{R}^2 \geq 0.9$.

to time constant unobserved heterogeneity correlated with selection into treatment. As shown in in Figure 8, the pre-treatment fit obtained using this demeaned SCM is close to perfect and the estimated treatment effect is very similar to the baseline result.

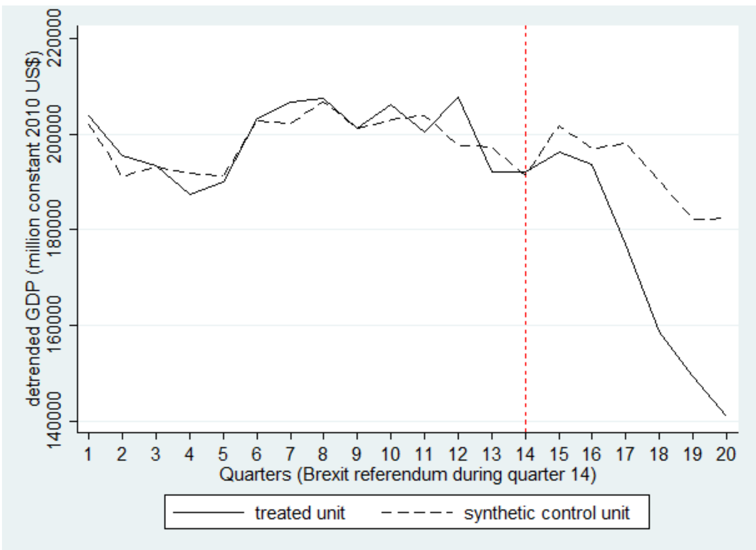
Figure 8: Demeaned SCM



Note: This figure present the results obtained by using the demeaned GDP following equation 2.

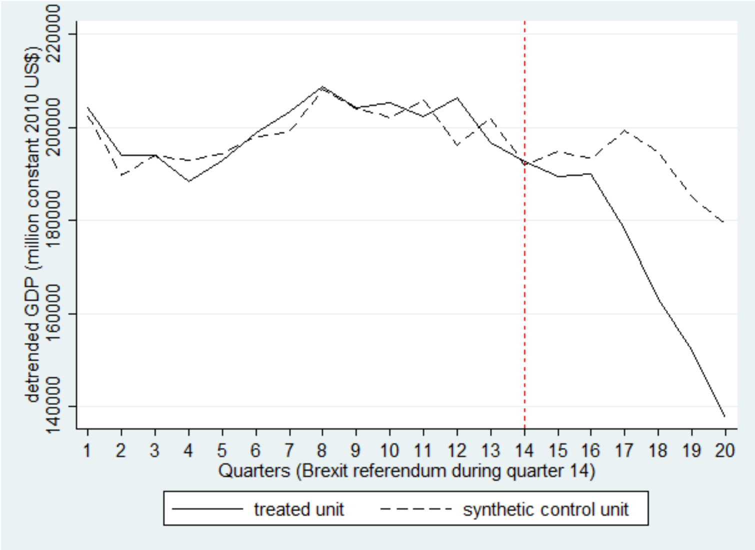
Finally, following the suggestions of Ferman and Pinto (2018), we check whether the close-to-perfect pre-treatment fit obtained for the GDP non-stationary series hides discrepancies in common factors beyond these non-stationary trends. Indeed, when such residual (and stationary) common trends are correlated to treatment assignment they may lead to asymptotic bias. By following the approach of Ferman and Pinto (2018), firstly we try to wash out the non-stationary common factor by subtracting, to controls and treated countries, the average of the controls' GDP in each period of time. If a common factor that has the same effect for every unit is the reason behind the non-stationarity, the resulting series should not feature non-stationary trends. Secondly, as an alternative procedure to de-trend the data, we subtract the trend obtained by fitting a fourth order polynomial to the GDP series. As shown in Figure 9 and Figure 10, both ways to de-trend the data seem successful: the series for the UK and the synthetic UK do not present a clear non-stationary trend. The pre-treatment fit obtained after eliminating the non-stationarity ($\widetilde{R}^2 = 0.65$ and $\widetilde{R}^2 = 0.55$ for the first and second de-trending procedures, respectively) is worst than the one obtained using the baseline not de-trended estimates (i.e. $\widetilde{R}^2 = 0.99$). This reduction in pre-treatment fit obtained after eliminating non-stationary trends is in line with the examples reported in Ferman and Pinto (2018), which apply this diagnosis tool to the influential studies of Abadie and Gardeazabal (2003) and Abadie et al. (2015). Moreover, it is also reassuring to notice that the pre-treatment gap between the real and the synthetic UK is far less relevant than the one observed during the post-treatment period.

Figure 9: De-trended output by extracting the pre-Brexit average GDP of the controls to the treated and control units



This figure presents the de-trended series of GDP for both the UK and the synthetic UK. We have de-trended the series by extracting, to treated and control countries, the average of the controls' output in every period of time.

Figure 10: De-trended output by fitting a fourth order polynomial



This figure presents the de-trended series of GDP for both the UK and the synthetic UK. We have de-trended the series by fitting a fourth order polynomial.

7 Conclusion

This paper estimates the effect that the European Union membership referendum has had on the British economy so far, focusing on its consequences on GDP. Using the SCM (Abadie et al., 2010), we construct the counterfactual GDP of the United Kingdom, after June 2016, as if the British referendum had not taken place. This paper quantifies the cost of the European Union membership referendum for the British economy in $156.03 billion, since the period when the referendum took place until December 2017. This figure is the equivalent to an accumulated non-increase in GDP of 1.01 percent. Our study finds that the referendum caused a lower GDP growth than the one that the UK would have experienced in the absence of referendum during almost all the periods analyzed after the Brexit poll. Furthermore, we found that this non-growth effect is increasing in time.

The evidence found in this paper complements the analyses predicting the future consequences of Brexit for UK by demonstrating that the outcome of the referendum itself has already produced a slowdown of the UK economy, before the actual Brexit has taken place. This finding is an early warning for populist European parties against irresponsible and unrealistic plans of abandoning smoothly the EU common market.

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A Appendix - Data and sources

Table 4 describes the data and the sources which we used to perform the study. The database is composed by quarterly data and yearly data predictors. To create a quarterly database we turned the yearly data into quarterly ones. To do this, we copied the yearly data value and introduced it for the four quarters that form a year. With this treatment of the data we lose some variability in the trend of the predictors, but we do not change the real value of the data. The predictors that were originally obtained in yearly frequency are: DNPOP (Density of population), SCSEC (School enrollment, secondary) and the data measuring Foreign Direct Investment (FDII and FDIO). However, our main predictor, real GDP as well as BCI (Business Confidence Index) were originally obtained in quarterly frequency.

Table 4: Data and sources

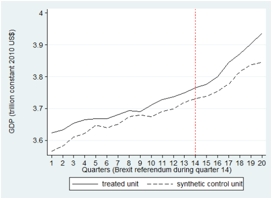
Variable	Description	Source
GDP	Real GDP (US$ millions 2010),by expenditure approach.	OECD
DNPOP	Population density (people per sq. km of land area).	World Bank
SCSEC	School enrollment, secondary (% gross).	"World Development Indicators" from the World Bank
FDII	Foreign Direct Investment inflows (% of GDP)	OECD
FDIO	Foreign Direct Investment outflows (% of GDP)	OECD
BCI	Business Confidence Index	OECD

B Appendix - European Spillovers

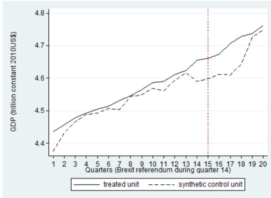
We first try to estimate the synthetic counterfactual for each European country using the baseline donor pool of countries (i.e. without European countries in the donor pool). The results of this exercise are shown in Figure 11. For some of these countries the SCM is not able to reconstruct the GDP pre-treatment path (i.e. the optimization routine is not able to find the minimum of equation 1). Among the rest of European countries to which we have successfully applied the SCM, the effect of the treatment could be considered significant only for those countries that present good pre-treatment match, and diverging series after the treatment period. Therefore, there are only four countries that could potentially suffer from spillover effects (indirect effects from Brexit). These countries are Netherlands, Norway, Poland and Switzerland. To evaluate the significance of these effects we do exact inference using the MSPE ratio, as in the main text.

Figure 12 shows that for none of these countries the effect of Brexit can be considered significant. The probability of choosing a country at random with a MSPE ratio as high as the one of Netherlands (Norway, Poland or Switzerland) is $2/11 \simeq 0.18$, that is above the conventional 10% level of significance used in statistics. Then, we cannot reject the null hypothesis and cannot conclude that Brexit referendum have had a significant effect on these European economies.

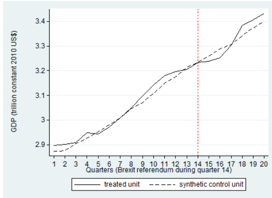
Figure 11: European Spillover Effects.



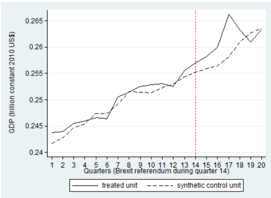
(a) Austria



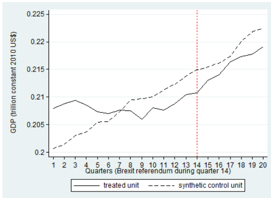
(b) Belgium



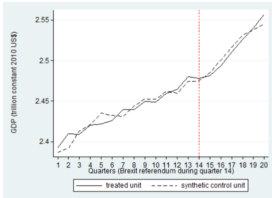
(c) Czech Republic



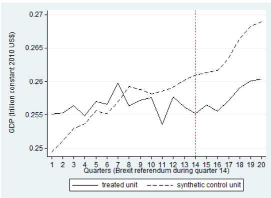
(d) Denmark



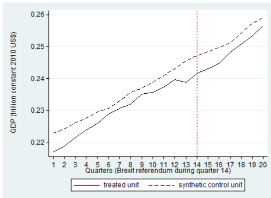
(e) Finland



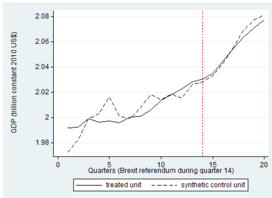
(f) France



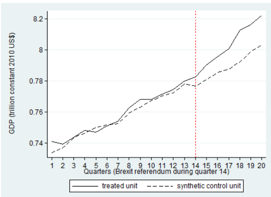
(g) Greece



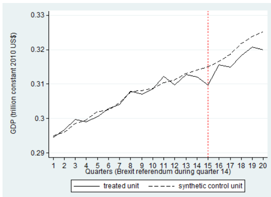
(h) Hungary



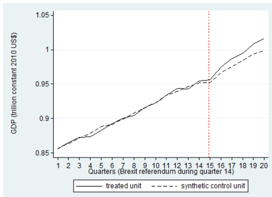
(i) Italy



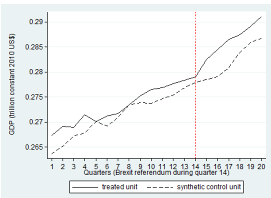
(j) Netherlands



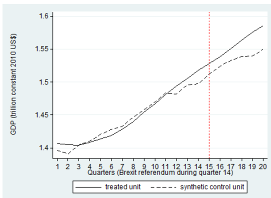
(k) Norway



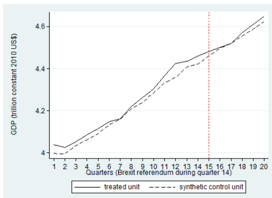
(l) Poland



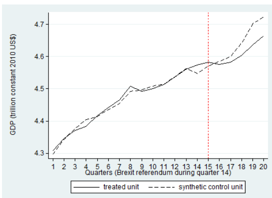
(m) Portugal



(n) Spain



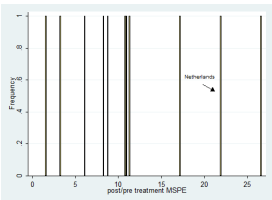
(o) Sweden



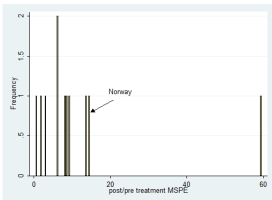
(p) Switzerland

Note: In this figure we assign the treatment to the European countries (that are not the UK) and we estimate its counterfactual using the baseline donor pool (with only non-European countries). This exercise shows that there are no clear individual spillover effects on the European countries analyzed. However there are four countries that present good pre-treatment matching and a jump on their GDP series and therefore they could potentially suffer from indirect effects of Brexit referendum. These countries are Netherlands, Norway, Poland and Switzerland and we will analyze them in depth in Figure 12.

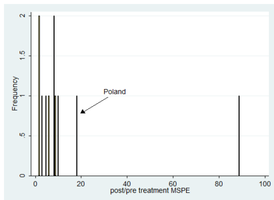
Figure 12: MSPE in European countries.



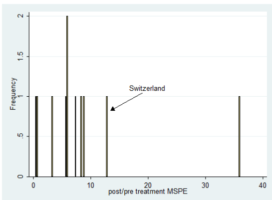
(a) Netherlands



(b) Norway



(c) Poland



(d) Switzerland

Note: This figure shows that the results of in space placebo studies in terms of MSPE ratios for selected *indirectly treated* European countries.