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Gauging the Gravity of the Situation

The Use and Abuse of Expertise in Estimating the Economic Costs of Brexit

Christoph Semken and Colin Hay

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Abstract

HM Treasury's estimation of the economic consequences of Brexit – using standard macroeconomic models – during the EU referendum campaign represents a remarkable intervention in a highly politicized public debate. It raises a series of questions about the use of economic expertise. Through a detailed theoretical and empirical critique of the Treasury's methodology – and a reassessment of the likely effects of Brexit in light of this – we cast doubt on the utility of their approach, highlighting methodological issues, unrealistic assumptions, and misrepresentations of established facts. In the process we seek to identify some of the wider implications for the use and potential abuse of economic expertise in highly charged political contexts, such as the EU referendum debate.

Keywords: Brexit, DSGE model, economic consequences, economic expertise, gravity model, HM Treasury, methodology

Résumé

L'évaluation des conséquences du Brexit effectuée par le Trésor britannique à l'aide de modèles macroéconomiques standards, lors de la campagne du référendum, constitue une intervention d'importance au sein d'un débat public hautement politisé. Elle soulève une série de questions quant à l'usage de l'expertise économique. Au moyen d'une critique théorique et empirique détaillée de la méthode utilisée par le Trésor britannique – et d'une réévaluation des effets probables du Brexit à la lumière de cette critique – nous remettons en cause la pertinence de l'intervention du Trésor, en soulignant ses problèmes méthodologiques, des hypothèses, pour certaines, irréalistes et une présentation erronée des faits. Nous cherchons ainsi à caractériser les implications de l'usage abusif de l'expertise économique dans un contexte fortement politisé, comme pouvait l'être le débat sur le Brexit.

Mots-clés: Brexit, conséquences économiques, méthodologie, modèle EGSD, modèle gravitationnel, expertise économique, Trésor britannique

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Gauging the Gravity of the Situation: The Use and Abuse of Expertise in Estimating the Economic Costs of Brexit

1 Introduction

Ever since the UK government committed publicly to a referendum on leaving the EU (commonly referred to as "Brexit") in 2015, economists have sought to forecast the economic impact of different Brexit scenarios (e.g., IMF 2016; Rusticelli et al. 2016). These estimates are important, because the economic costs and benefits of EU membership were among the key arguments used by both sides during the referendum campaign. In addition, they play an important role in the continued debate about the relative merits of a variety of different Brexit scenarios. But predicting such effects, certainly with any precision, is no easy task – and is made all the more complex by the highly charged political context into which any such projections must be thrown.

The stakes could scarcely have been higher, then, when HM Treasury was charged by then Prime Minister David Cameron and then Chancellor of the Exchequer George Osborne to estimate the potential economic damage of Brexit for the British economy and for British households. The estimation that it reached and published (HM Treasury 2016) – and the methodology it used to produce it – is the ostensible subject of this paper. Both are interesting in a variety of different respects: for the peculiar and privileged status of the discipline of economics in the public debate that they again indicate (Fourcade 2009); for the "expert paternalism" and the depoliticization and technicization of an essentially political choice that they represent (Benoit and Hay 2019; Hay 2020); for what they reveal about the perhaps inherently "imagined" and "socially constructed" character of the future (Andersson 2012; 2018; Beckert and Suckert 2021; Fourcade, Ollion, and Algan 2015; Suckert 2020); and for the inherent methodological challenges of such an imagining. HM Treasury's methodology did not so much assess the cost of a credible economic future for Britain after Brexit as estimate the (historical) benefit accruing from EU accession.

Acknowledgements. We would like to thank Ian Hurst from the National Institute for Economic and Social Research (NIESR) for his excellent support in modeling the effects of Brexit using NIESR's general equilibrium model. We are thankful to David Rosell and seminar participants at Universitat Pompeu Fabra for their useful comments. *Funding.* We gratefully acknowledge the financial support of Sciences Po and Princeton University (grant EUROGLOB 2016), as well as the Centre for European Studies and Comparative Politics (CEE). The project leading to these results has received funding from "la Caixa" Foundation (ID 100010434), under agreement LCF/BQ/DR19/11740006. *Reproducibility.* The code to reproduce our results is available in our Open Science Framework repository at https://osf.io/2dgfp/.

In what follows, our focus is essentially twofold. First, we are concerned with the question of the extent to which gravity and off-the-shelf general equilibrium models, as deployed by the Treasury, are an appropriate means for gauging the economic effects of potential changes in the terms of economic interdependence between economies. Through a detailed theoretical and empirical critique of the Treasury's methodology we cast doubt on their utility in providing such counterfactual and prospective estimations, especially when intended as interventions in a highly politicized public debate in which the use of expertise itself would come to be challenged (Hay 2020). Second, we attempt to draw out some of the wider implications for the use and potential for abuse of economic expertise in highly charged political contests, such as the EU referendum debate. We point to the spurious precision of the estimation in light of the (necessary) imprecision in the assumptions on which it was predicated.

Above all we show that, in responding to the challenge of providing, in effect, a point estimation of the potential costs of Brexit, the Treasury was forced to call on a series of potentially problematic (and in some cases, entirely arbitrary) premises. We reveal serious data limitations and methodological flaws – while also discussing the need for appropriately cautionary remarks to accompany any stylized modeling/estimation of this kind (reliant as it is on essentially arbitrary assumptions and inadequate data).

But our aim is not just to reveal the problematic (and perhaps necessarily problematic) character of the Treasury's estimation, exposing, in the process, its theoretical and methodological deficiencies. Our aim is also to correct, as best we can, these estimations using similar techniques with more robust assumptions and data. We use a counterfactual analysis with three main steps. First, estimates are obtained from panel data about the (average) association of EU membership with bilateral trade, bilateral foreign direct investment (FDI), and productivity. This is achieved additionally by considering three comparison groups: countries in the European Economic Area (EEA), countries with free trade agreements (FTAs), and all other countries – trading under World Trade Organization (WTO) rules. Second, it is assumed that the UK's trade, FDI, and productivity will gradually decrease by a fraction of the difference between the estimated "EU effect" and the baseline "effect" under each of the comparison groups. For example, HM Treasury assumes that trade flows will decrease by exactly 100 percent (50 percent for the lower bound estimate) over fifteen years – an ad hoc assumption which we question in the next section. Finally, these figures are used as inputs in a dynamic stochastic general equilibrium (DSGE) model to estimate the overall effect of leaving the EU on the UK's gross domestic product (GDP) within the next fifteen years.

Using this methodology, HM Treasury finds that GDP will be 3.8 percent less if the UK exits the EU and joins the EEA, 6.2 percent less if it negotiates an FTA, and 7.5 percent less if it trades (as now seems more likely) under WTO rules. The underlying estimates for the effects of EU membership, EEA membership, and having an FTA in place are

Table 1 HM treasury results

	EU Membership		EEA Membership		FTA	
	Trade	FDI	Trade	FDI	Trade	FDI
Joining, % gain	+155%	+35%	+76%	–	+24%	–
Coefficient	0.766	0.298	0.566	0.069	0.219	–
Standard error	0.048	0.119	0.029	–	0.090	–
Leaving, % loss (GDP)	–3.8%		–6.2%		–7.5%	

Notes: The results are estimated using the methodology described in Section 2. The trade columns refer to trade in goods only. No standard errors are provided by HM Treasury for insignificant results (e.g., coefficient EEA, coefficient for FDI). Moreover, they do not consider percentage gains for insignificant variables. No effect of having an FTA in place is estimated for FDI – see equation (2). The loss in GDP is calculated using different values from those in row one, as HM Treasury makes several additional assumptions (see text).

summarized in Table 1. As we explain below, the results are based on log-linearized regressions. Thus, the central estimate for the percentage gain is calculated from a coefficient β_i through the formula e^{β_i}–1.[1]

While many more aspects of the study could be scrutinized theoretically, evaluated methodologically, and assessed critically, we focus on the two panel data estimates – those for trade and FDI – on which the rest of the analysis is based. We argue that there are several reasons to believe that the estimates are misleading. First, while HM Treasury estimates an average EU effect, we show that the UK is *not* like an average EU member state. Second, it is important to compare the UK to a relevant group. We argue that using the whole world as a sample – as done by HM Treasury for the trade estimate – may bias the result. We re-estimate the trade and FDI models, while applying remedies to each of these problems.

In a short conclusion we seek to draw out the implications of our analysis not just for Brexit and the estimation of its likely effects, but also for the wider – and increasingly crucial – question of the role of and appropriate use of technical economic expertise in prospective public policy judgments of this kind.

1 The overall percentage gain for trade is actually lower in HM Treasury (2016, 165), compared with row one of Table 1 and even before step two of the counterfactual analysis, because they also do an analysis for services and average the two results. Throughout the rest of this paper, we focus on the raw coefficients, noting that they can easily be converted in percentage changes using this formula.

2 Methods

Gravity

To estimate the effect of EU membership on bilateral trade and FDI flows (or "EU effect"), HM Treasury uses gravity models. These models are based on the observation that trade (and many other types of economic flow) can be approximated extraordinary well through the following relationship:

$$T_{ijt} = \frac{Y_{it}^{\eta_1} \cdot Y_{jt}^{\eta_2}}{D_{ij}^{\eta_3}} \tag{1}$$

where T_{ijt} are trade (or other) flows from i to j, Y_{it} is the GDP of country i at time t, and D_{ij} is the physical distance between i and j. For trade, the estimated coefficients are around $\hat{\eta}_1 \approx \hat{\eta}_2 \approx \hat{\eta}_3 \approx 1$ and the model is called a gravity model due its resemblance to Newtonian physics (see Anderson 2011).

In the modern gravity literature, more complex versions of the model are used. These modern versions have been extensively micro-founded for both trade and FDI (for a recent review, see Head and Mayer 2014). For brevity's sake, however, we take a data-focused approach in deriving them. The key concern is that many variables can explain bilateral trade and FDI flows, including language barriers, colonial ties, economic integration, as well as institutional and cultural similarities (see, e.g., Bénassy-Quéré, Coupet, and Mayer 2007; Stein and Daude 2007; Medve-Bálint 2014). In order to control for many of these observed and unobserved factors, the most commonly used version of the gravity equation today includes country-pair and time fixed effects, plus the explanatory variables of interest and relevant controls. Adding those to (1) and taking logs, gives us the regression equations used by HM Treasury:

$$\begin{aligned} ln(T_{ijt}) = {} & \alpha_{ij} + \gamma_t + \alpha_1 ln(Y_{it} \cdot Y_{jt}) + \alpha_2 ln(P_{it} \cdot P_{jt}) \\ & + \beta_1 EU2_{ijt} + \beta_2 EU1_{ijt} + \beta_3 EEA_{ijt} + \beta_4 FTA_{ijt} + \varepsilon_{ijt} \end{aligned} \tag{2}$$

$$\begin{aligned} ln(F_{ijt}) = {} & \alpha_{ij}' + \alpha_1' ln(Y_{it}) + \alpha_2' ln(Y_{jt}) + \alpha_3' ln(P_{it}) + \alpha_4' ln(P_{jt}) + \alpha_5' EMU2_{ijt} \\ & + \beta_1' EU2_{ijt} + \beta_2' EU1_{ijt} + \beta_3' FTA_{ijt} + \varepsilon_{ijt}' \end{aligned} \tag{3}$$

where T_{ijt} and F_{ijt} are trade and FDI flows; α_{ij} and γ_t are country-pair and time fixed effects;[2] P_{it} is the population of country i; and $EU2_{ijt}$, $EU1_{ijt}$, EEA_{ijt}, FTA_{ijt}, $EMU2_{ijt}$ are dummy variables indicating whether both countries are in the EU, one country is in the EU, both countries are in the EEA, there exists an FTA between the two countries, and whether they are both in the euro area at time t, respectively.[3]

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2 Note that the country-pair fixed effect α_{ij} makes D_{ij} redundant because it is time-invariant.

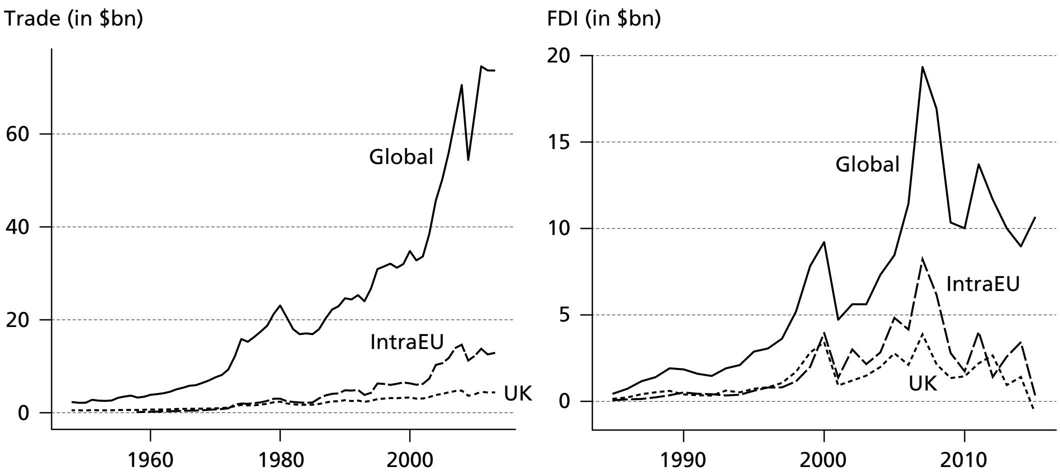
3 HM Treasury also includes a dummy to indicate whether one country is in the euro area in (3). However, the estimated coefficient is close to zero and insignificant in all their regressions, which is why we have left it out.

While both practices are used in the literature, HM Treasury does not explain why they use the product of GDP and population in (2) and the individual variables in (3). We conjecture that HM Treasury used the two different approaches because they are used in the methodology of papers whose data they use in the case of trade (Glick and Rose 2002) and which they cite in the case of FDI (Yeyati, Stein, and Daude 2003). However, neither study gives an explanation for using one approach over the other. In the absence of any theoretical justification, HM Treasury could have increased comparability between the two types of flows by using the same control variables. Similarly, they do not explain why (2) includes time fixed effects (as is customary), while (3) does not, an issue that we discuss in the next section.

Problems with HM Treasury estimates

There are a number of issues with the regression equations used by HM Treasury. First, there are the discrepancies between the two equations. We do not investigate the choice of products versus individual regressors. However, we think that leaving out time fixed effects from the FDI regression is a major problem, as the latter fluctuate substantially over the global business cycle, as shown in Figure 1 (right) – and even more so than trade flows, shown in Figure 1 (left). This means that the EU coefficient may be confounded by business cycle fluctuations if, for example, countries predominantly join during upturns. The time fixed effects may have been left out to aid statistical significance, because the coefficient for FDI found by HM Treasury is only slightly different from zero at the 5 percent significance level (see Table 1), and using fixed effects is equivalent to adding eleven variables which – in this case – can easily change the significance level.

Figure 1 Global, intra-EU, and UK flows

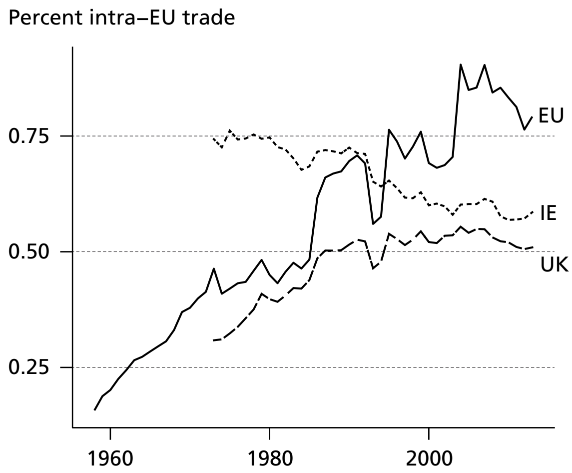


Notes: All flows are those observed in the respective datasets. In particular, because the FDI sample is restricted to country-pairs involving an OECD member state (see Appendix B), global FDI flows are underestimated.

Second, taking logs is not as innocent a strategy as it might appear. Because the dependent variable can take on zero (for trade and FDI) and negative values (for FDI), the log-linearized regressions (2) and (3) can be applied only to a subset of the data. In particular, all results will be conditional upon T_{ijt} or F_{ijt} being strictly positive. Furthermore, the Ordinary Least Squares (OLS) estimator might be biased in the presence of heterogeneous errors (Silva and Tenreyro 2006). We come back to this issue in the Conclusion.

Third, the main coefficient of interest in both regressions – $EU2_{ijt}$ – will measure the *average* effect of EU membership on bilateral flows. However, the UK is not like an average EU member state. To see this, Figure 2 and Figure 3 show the evolution of intra-EU trade flows as a fraction of total trade flows for the average member state, the UK, and Ireland. Both countries joined the EU in 1973, but they have had vastly different experiences and, importantly, neither is in line with the average. This may be due to each country's unique historical experience – persistent ties to former colonies in the case of the UK, for example. It suggests that the estimated average EU effect is not a good approximation for the gains made by the UK (and hence the potential losses arising from Brexit).

Figure 2 Intra-EU trade flows



Fourth, the sample used by HM Treasury (at least for trade) includes low-, middle-, and high-income countries. Yet, in the presence of some unobserved confounding factors, the coefficients of interest may be biased by not differentiating between these groups. Moreover, some of the variables (such as EU1 or FTA) may genuinely be different for different sets of countries. More worrying still, the data for many non-OECD countries is likely to suffer from measurement bias and includes many zeros (Buehler and White 2015). Similar to the argument above, the UK will not become like an average economy – it will (probably) still be an advanced economy – post-Brexit. Thus, the relevant comparison group comprises other advanced economies, not the entire world.

Proposed methodology

To address (some of) the above-described issues we proceed in three steps. The first step consists of running HM Treasury's regressions using OLS in a frequentist framework with additional variables. To address the issue surrounding UK exceptionalism (the fact that EU membership may not have the same effect on bilateral flows involving the UK as for an average EU member state), we introduce an interaction term EU2:non-UK$_{ijt}$ that equals one if both countries are in the EU and neither of them is the UK.[4] Next, to tackle the representativeness of the sample, we interact the main regressors of the trade regression with a non-OECD$_{ijt}$ dummy, assuming that OECD countries are a better comparison group for the UK.[5] Lastly, we introduce time fixed effects into the FDI regression to address possible confounding effects of business cycles.

While we have given several credible theoretical reasons as to why the HM Treasury estimates may be biased or misleading, some may disagree with our arguments. It would therefore be desirable to have a data-driven way to pick the best model. To do this, the second step of our analysis uses Bayesian model selection. Unlike its frequentist counterpart, the Bayesian statistical framework allows us to easily select one out of several models.[6] This is achieved by calculating the "posterior" probability of each model – its probability given the data. Of all the proposed models, the one with the highest posterior probability is most likely to have generated the data.

An additional advantage of the Bayesian framework is that one can obtain coefficient point estimates – or indeed an entire probability distribution for each coefficient – without having to pick a model. This is done by means of "Bayesian model averaging," a process in which the coefficient of interest is obtained from every model, multiplied by the model's posterior probability and added together to get a weighted average. We use this method to obtain a robust new estimate of the EU coefficients.[7] One difference with the more commonly used frequentist framework is that Bayesian statistics require us

4 Here we are not differentiating between the UK as the sender or the receiver of trade and FDI. Depending on how the estimate is used afterwards, it may be desirable to consider both cases separately. The "non" version is used to facilitate an easy comparison between the EU coefficient that is relevant for the Brexit analysis.

5 By main regressors, we mean all regressors except the country pair α_{ij} and time fixed effects γ_t. The former are invariant with respect to (current) OECD membership; while the latter are not interacted for computational reasons. In particular, we would not be able to demean the data to eliminate the fixed effect – an efficient and (under certain assumptions) consistent estimation strategy (Wooldridge 1997; 2001). The FDI regression is already restricted to the OECD sample, see Appendix A.

6 Common frequentist methods – such as the likelihood ratio test – can only compare a maximum of two non-nested models.

7 To make the Bayesian approach computationally feasible, we demean the data first. That is, we obtain probabilities conditional on all fixed effects α_{it} and γ_t and being the same as the MLE estimates.

to choose "prior" probabilities. These quantify the probability we assign to each model (and coefficient values) before using the data. We use uninformative prior probabilities throughout. As a result, we assign each model the same prior probability.[8]

3 Results

In this section we describe the results of our various analyses. The data sources and comments on potential dissimilarities with the data used by HM Treasury are described in Appendix A. Some descriptive statistics are provided in Appendix B.

New models (frequentist)

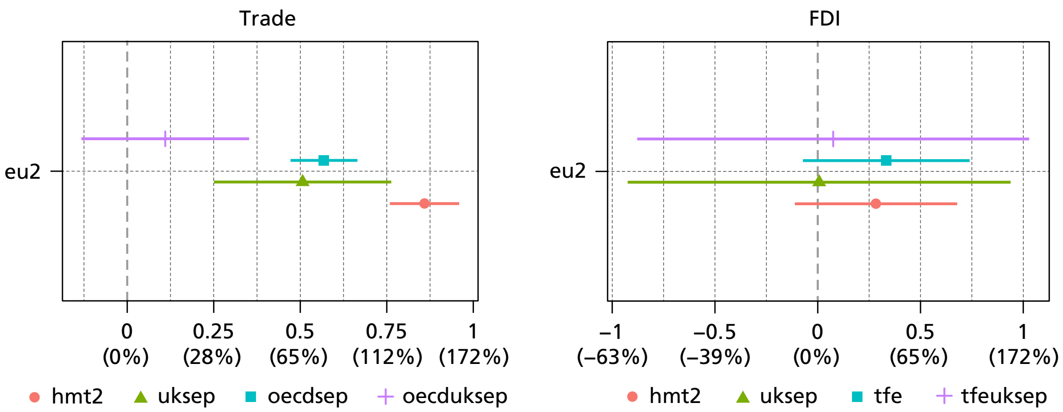
First, we analyze the results under the proposed changes in the frequentist framework. Figure 3 visualizes the EU coefficient for different models, while Appendix C contains the remaining regression coefficients. The left panel shows the results for the trade regression, based on (2). The "hmt2" estimate (column 2 in Table S.3) replicates the findings in HM Treasury.[9] Even though we use the same primary data source, the results are somewhat different. This may be driven by one of two problems. Either some of the secondary data sources (e.g., for GDP and population) could be noncongruent, or HM Treasury removed observations (because they report only 390,521 observations). Fortunately, our result is both within the 95 percent confidence interval of the original estimate and higher, so that we can consider it conservative (when trying to show that the original estimate had an upward bias).

Next, model "uksep" (column 3 in Table S.3) reports the results of a regression in which we added the EU2-non-UK interaction term, to capture differential effects for the UK as opposed to other EU member states. Both the original EU2 and the new dummy are highly significant ($p < 0.1$). The EU effect – when controlling for non-UK flows and hence only considering flows that involve the UK – more than halves, from 136 percent to 66 percent. This confirms our conjecture that the EU effect for trade is different for

8 The prior distribution for the regression coefficients is $p(\alpha, \beta) = N(0, 10^6 \cdot \sigma \cdot I)$, for sigma it is $p(\sigma) = IG(10^{-3}, 10^{-3})$, and for all models it is $p(M_i) = \frac{1}{n}$ where n is the number of models considered.

9 In Figure S.3, column 1 shows the baseline estimate with regular standard errors, whereas column 2 shows the same results with robust standard errors, clustered at the pair level. There is a clear case for clustering the errors because there is time dependence within country pairs due to unobserved factors. While HM Treasury does not discuss the estimation technique used, our robust standard error for the EU2 coefficient (0.051) is very close to theirs (0.048). We thus consider the findings replicated.

Figure 3 EU2 coefficient (frequentist models)



Notes: Shown are point estimates and 95% confidence intervals for the EU2 coefficient under different models. The models are explained in Table S.3 for trade and Table S.4 for FDI, respectively. The model names in the legend correspond to the second row in each table.

the UK than for the average member state. In the HM Treasury analysis, however, no account is taken, and no mention made, of the potential methodological implications arising from assuming the UK to be an average EU case in the estimation of the potential effects of Brexit.

To test problems of measurement bias and the representativeness of the sample, the model "oecdsep" considers differential effects for OECD and non-OECD countries (column 4 in Table S.3). Comparing "hmt2" and "oecdsep," we see that allowing the control variables to have different effects for non-OECD countries also decreases the EU effect – from 136 percent to 77 percent – again confirming our intuition. Because we do not vary the time fixed effects (as explained above), column 5 of Table S.3 shows the baseline regression only for OECD countries. All significant estimates are very similar, suggesting that the differential effect is well captured by the interactions introduced.

Finally, "oecduksep" (column 6 of Table S.3) shows the results for our preferred model, which combines the two approaches. Here, the EU effect becomes insignificant. This is indeed highly problematic for HM Treasury, because it shows that not just the quantitative but even the qualitative result obtained by the UK government is sensitive to the introduction of UK and OECD interaction terms to address issues of representativeness.

Turning now to investment flows, the right panel of Figure 3 shows the corresponding results for FDI flows. Again "hmt2" (columns 2 in Table S.4) replicates HM Treasury's model – equation (3). Again, we find our EU2 coefficient estimate (33 percent) to be well within the confidence interval provided by HM Treasury (7–70 percent; see Table 1). Yet, our estimated standard error is substantially larger, so that we fail to reject the hypothesis that EU membership has no effect on bilateral FDI flows.

Conversely, while the EU2-non-UK interaction term we introduce in model "uksep" (column 3 in Table S.4) has the expected sign, it only decreases the estimated standard error. Similarly, introducing time fixed effects – with and without the interaction term, in models "tfe" and "tfeuksep" (columns 4 and 5 in Table S.4), respectively – still does not permit us to conclude that EU membership has an effect. The fit – as measured by the (un-adjusted) within-R^2 – also does not appear to improve.

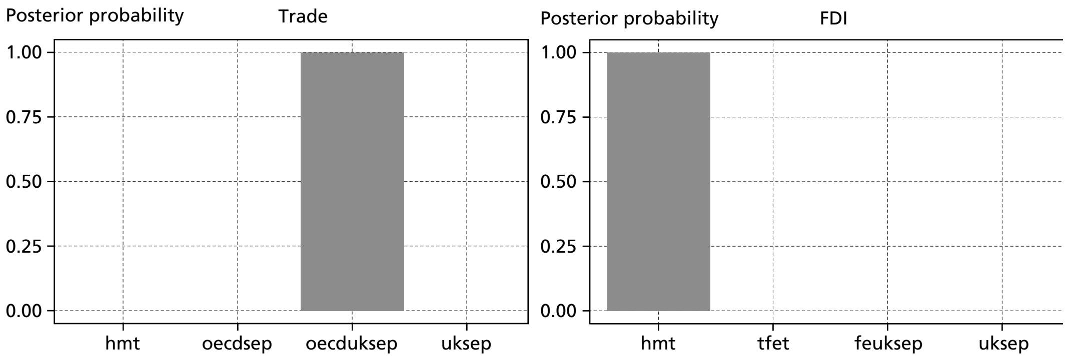
In sum, the estimated EU effect on trade decreases, both when considering the UK separately and when letting the controls have differential effects for non-OECD countries; whereas we fail to find a significant effect for FDI in any model. These findings are highly relevant. Considering the UK as being different from other EU member states and comparing the UK to an average OECD country – rather than an average country in the world economy – each roughly halve the estimated percentage gains in trade from joining the EU. HM Treasury did not take into consideration either of these important caveats. Moreover, the effect of joining the EU on UK trade cannot be reliably estimated when taking both points into account. Finally, as we anticipated from the marginally significant effect in the original analysis, the statistical significance of the gains in FDI from joining the EU are sensitive to the use of a slightly different dataset and the deletion of outliers done by HM Treasury. As a result, we cannot replicate the reported findings for FDI flows. Considering the UK economy as being different from the average EU economy and/or controlling for the effect of time further reduce the precision of the estimate.

Model selection (Bayesian)

To assess which model is most supported by the data, we repeat the previous exercise in a Bayesian framework, to check inductively which model best fits the data. Conveniently, in Bayesian statistics the comparison between models can be done easily using their posterior probability – the probability that the data arose from any one model – as discussed in Section 2. We start by discussing the estimated coefficients.

Figure S.1 in Appendix D shows the estimated distribution for the EU2 coefficient, while the point estimates of the remaining coefficients are shown in Figure S.2. Consistent with the Bernstein-von Mises theorem, the mean coefficients and standard errors are very close to those obtained under the frequentist framework. Moreover, the distribution around the mean is approximately normal. In other words, the two methods of inference – frequentist and Bayesian – give us similar estimated effects and confidence regions. The estimated effect of joining the EU on trade and FDI flows under our various specifications is thus robust to the estimation method used. Because the posterior distributions are calculated using a Markov Chain Monte Carlo (MCMC) simulation, we provide traceplots showing the simulations in Appendix E. All simulation chains stabilized around the estimated mean coefficient. Hence, we are confident that the number of simulations we perform is sufficient.

Figure 4 Model probabilities (Bayesian Model Selection)



Next, we obtain the posterior probability of each model, shown in Figure 4. For both trade and FDI, one model has an estimated posterior probability near one. That is, the data overwhelmingly supports one model each. In the case of trade (left panel), the model with the highest posterior probability – 99.86 percent – includes both the UK and OECD interaction terms. In other words, the data strongly suggests differential effects of joining the EU for the UK and other member states. Likewise, it strongly supports separately estimating the effects of other variables for OECD and non-OECD countries, which reduces the estimated EU effect. On the other hand, the right panel of Figure 4 shows that the FDI data overwhelmingly favors the original regression model, which has a posterior probability of 99.95 percent. This is in line with the near-constant R^2 observed in Table 3, already precluding additional explanatory power.

Finally, we use Bayesian model averaging – weighing each estimated coefficient by the posterior probability of the model – to find estimates for the EU effect that do not require us to choose one model. Doing this, we find an effect of joining the EU on UK trade flows of 12 percent ($\hat{\beta}_1 = 0.111$) and 40 percent ($\hat{\beta}_1' = 0.292$) on FDI flows. These estimates are very close to those of models "oecduksep" for trade and "hmt2" for FDI. This is not surprising, given both models' high posterior probability. However, these results remain imprecisely estimated, as discussed in the previous subsection.

4 Simulating Brexit

Until now, we have discussed HM Treasury's way of estimating the economic benefits from joining the EU. To gauge the consequences of Brexit, HM Treasury makes assumptions about how much of these benefits are lost over fifteen years following the UK's exit from the EU. The resulting estimates of losses in trade and FDI are then used as inputs in a general equilibrium model, to generate a figure for the final change in GDP. Below we first explain this methodology in more detail, discuss potential issues, and present a new range of estimates. The issues are summarized, together with the anticipated effect, in Table 2.

Table 2 Summary of modeling issues

Issue	Effect: biased estimate	Effect: uncertainty under-estimated	Addressed in sensitivity analysis
General approach			
Missing markets and incorrect transmission mechanisms (e.g., no FDI, no migration in model)	x	x	
Omission of baseline model uncertainty (due to simplifying assumptions and estimation of parameters)		x	
Trade			
Estimate average EU – instead of UK-specific – effect	x		x
All countries – not advanced economies – as reference group	x		x
Coefficients on trade diversion and EEA effect set to zero (because zero is included in the confidence interval)		x	
Brexit arbitrarily assumed to completely reverse effect of joining linearly over exactly 15 years	x	x	x
Difference between EU and EEA membership: lower and upper estimates replaced by central estimate		x	x
FDI and WTO scenario: 50% of lower estimate, 100% of upper estimate		x	x
67% confidence interval used instead of conventional 95%		x	
FDI			
Arbitrary removal of 'outliers'	x	x	x
Brexit arbitrarily assumed to completely reverse effect of joining linearly over exactly 15 years	x	x	x
Use of scaled trade value for EEA and FTA scenarios (because confidence interval includes zero)	x		x
FDI from other countries assumed to be affected in the same way as FDI from EU (because of platform FDI)	x		
Only used in model through effect on productivity	x		
Productivity			
Central estimate for impact of FDI on productivity used for lower and upper estimates		x	
Productivity loss over 15 years applied to all years	x		
Half elasticity used for FDI, full elasticity used for trade	x		x
Arbitrary 1% "long-run impact" added to final estimate	x	x	x

Methodology and problems

HM Treasury uses a model developed by the National Institute of Economic and Social Research (NIESR), called the National Institute Global Econometric Model (NiGEM). NiGEM is a new-Keynesian general equilibrium model. That is, the model assumes that agents have rational expectations (use all the available information to make the best possible prediction about the future state of the economy and their own economic situation). Temporary deviations from the "efficient" market equilibrium and slow adjustments arise from the presence of sticky wages and prices.

Many of the most important economic interactions – such as goods markets, labor markets, capital markets, international trade and some government policy – are included in the model. As a result, the model can capture some general equilibrium effects, which are potentially very important in the case of Brexit. For example, if renewed trade barriers decrease exports, companies are likely to hire less labor or reduce wages, which in turn decreases aggregate demand and leads to additional job or wage losses, and so on. The overall effect on GDP may therefore be higher than the initial decrease in exports. At the same time, because Brexit is likely to increase demand for locally produced goods, which become relatively cheaper, there is also a countervailing increase in employment. Thus, even the qualitative overall impact on employment, wages, and GDP is not always clear *a priori*, making it more important still to use a general equilibrium model.

Like all macroeconomic models, NiGEM is based on many simplifying assumptions about agents' behavior in, and the structure of, the economy. To calibrate the model, several variables are taken from the data (e.g., prices, population, unemployment, government expenditure), while a host of parameters are estimated separately for each country to fit the data. For example, consumption C is modeled as being determined by real personal disposable income *RDPI*, real net financial wealth *RNFW*, and real tangible wealth *RTW* through the equation $\ln C = \alpha + \beta \ln RDPI + (1 - \beta) \ln(RNFW + RTW)$, where the latter three are taken from the data, whereas and are estimated for each country. The remaining structural equations and data sources can be found in the NiGEM manual (NIESR 2019).

For its analysis HM Treasury uses NiGEM v4.15 with the following modifications. UK government expenditure (*UKGC* and *UKGI*) is exogenously kept constant, thereby turning off a fiscal policy constraint that, in the standard model, limits budget deficits. The input variables used are *UKS* for trade and *UKTECHL* for productivity. Output is *UKY* relative to the baseline prediction (standard values for *UKS* and *UKTECHL*).

The first problem with the way in which HM Treasury used NiGEM is its failure to consider several credible and potentially important channels through which Brexit might affect the British economy. For example, no attempt is made to model changes in migration. Given the large number of EU nationals working in the UK who could leave after Brexit, this is not an innocuous oversight. Even FDI is not included directly in the model. HM Treasury opts only to simulate the effect of a decrease in FDI indirectly, through its impact on productivity. Yet, the level of investment has an important direct impact on the economy. Overall, HM Treasury changes only two input variables to model Brexit: trade and productivity. In consequence, the result may be seriously biased – in this case, in the direction of underestimating the likely cost of Brexit.

The second general problem is that HM Treasury does not account for the uncertainty inherent in the baseline model. It estimates the model with their estimated values for trade and productivity – including lower and upper bounds – and compares the resulting future GDP with the predictions of the standard model. However, due to both the many simplifying structural assumptions (for example, household consumption is determined solely by income and wealth, at a fixed ratio) and uncertainty around the baseline parameters, the standard model's central GDP forecast exhibits considerable uncertainty, especially for a fifteen-year prediction. Importantly, the uncertainty around estimated parameters of the model might interact with the impact of trade and productivity on income. Thus, any confidence bands estimated using the uncertainty around the estimated EU effect for trade and FDI alone – as in HM Treasury's account – are likely to underrepresent uncertainty in the final GDP estimate.

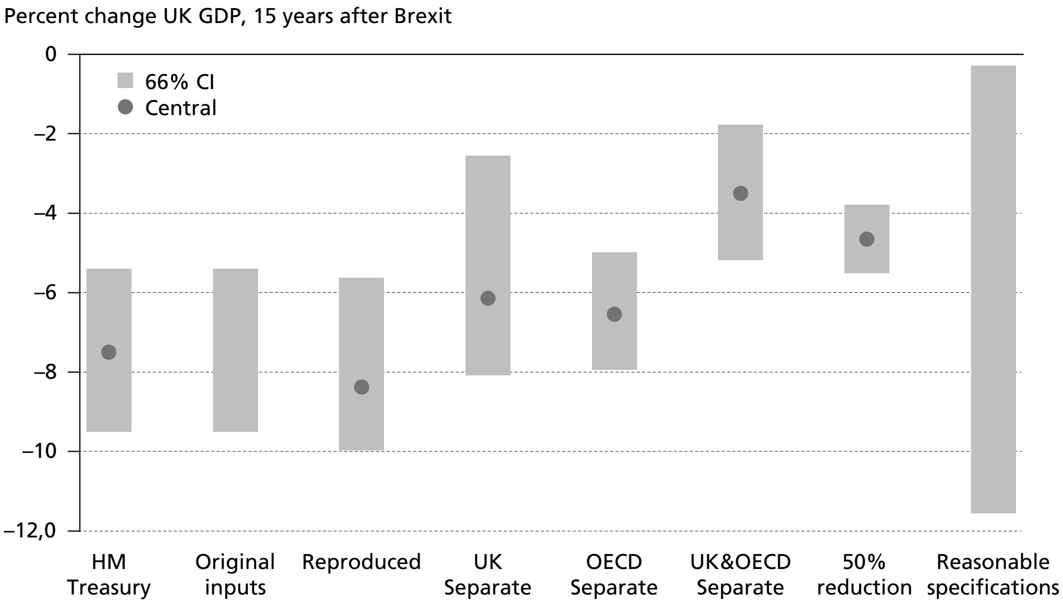
There are also several problems with converting the gravity estimates into model inputs. For example, HM Treasury assume that the EU effect will be completely reversed after exactly fifteen years and linearly so during the fifteen years. This is an ad hoc assumption that is not justified by any empirical evidence. As we show below, it has important implications for the estimated effect of Brexit. A number of other problematic methodological choices are explained in Appendix F.

Sensitivity analysis

Figure 5 shows the estimated difference in GDP fifteen years after Brexit – compared with the baseline/no Brexit forecast – under the WTO scenario for various NiGEM model inputs. In the first three models, no major changes have been made to the HM Treasury methodology. Indeed, the first model shows the estimated effect as reported by HM government. According to HM Treasury, leaving the EU and trading under WTO rules would reduce the UK's GDP by 7.5 percent some fifteen years after Brexit. The gray bars in the Figure represent the 67 percent confidence interval reported by HM Treasury (–9.5 percent to –5.4 percent). For the second model, we use the exact input values where they are reported (lower and upper bound) and calculate the prediction of NiGEM using the above modifications. The resulting estimates are identically equal (after rounding) to those of HM Treasury. We are therefore confident that we have used the model in the same way as HM Treasury.

The "Reproduced" model is our attempt to replicate HM Treasury's findings using the data on trade in goods and FDI from Section 3. The assumptions described above for converting the effect of joining the EU into the effect of leaving are maintained and the empirical models are the same. Because we do not have the data for trade in services, we use the estimates reported in HM Treasury here and throughout this section. We are also not provided with the data for trade in goods and services by partner country (see Appendix F for the methodology used by HM Treasury). Instead, we calculate the

Figure 5 Sensitivity analysis results



overall shares for goods and services, as well as the share of trade that involves other EU countries using HM Treasury's intermediary results.[10] Given that we do not use the exact country-by-country figures, our results may suffer from an aggregation bias. But the estimated effect of a "hard" Brexit – 8.4 percent lower GDP fifteen years later – is similar to the one found by HM Treasury and falls within its confidence interval. The confidence bounds are even closer to the original estimate (only 0.2 and 0.5 percentage point difference, respectively). We therefore consider the findings replicated.

The next three models – UK Separate, OECD Separate and UK & OECD Separate – use our new trade estimates, taken from Section 3. All other inputs are as in the original HM Treasury analysis. As shown in Figure 5, all three changes considerably decrease the estimated effect of Brexit. First, estimating a UK-specific EU effect reduces the estimated effect of Brexit on GDP to –6.1 percent. Second, comparing the UK to an average OECD country – instead of an average country in the world economy – by interacting all control variables with an OECD indicator, similarly reduces the effect to –6.4 percent. Combining the two changes in the model with highest posterior probability

10 We estimate the overall shares of trade in goods and trade in services from Table A.4 in HM Treasury, which presents an effect of joining the EU/EEA/FTA on overall trade. Minimizing the sum of squared differences gives us shares of 58 percent for goods and 42 percent for services. Taking these values as given and assuming that they apply equally to both EU and non-EU countries, we estimate the share of trade with the EU from Table A.6, which presents the total impact of leaving (before the manual adjustments). By again minimizing the sum of squared differences we estimate that trade with other EU countries makes up 50 percent of overall UK trade. Both values are broadly in line with recent government figures (Ward 2019).

further reduces the effect to –3.5 percent. Thus the effect is more than halved compared with the original estimate (–7.5 percent), by doing only one adjustment that is strongly supported by the data.

There are many other shortcomings discussed above that can be addressed. One particularly important assumption is that the effect of joining will be linearly reversed over fifteen years. Here it is interesting to note that were we to assume that only half the effect is reversed over fifteen years – either because the reversal takes even longer or because some of the gains of joining the EU persist even after exiting – the Brexit effect would decrease by 55 percent (3.7 percentage points).

Considering all possible combinations of the changes proposed above, we find a range of estimates that varies from 0.2 percent to 11.6 percent lower GDP, fifteen years after Brexit. The choices that give these results are listed in Appendix G, together with the results for some additional models. Note that our sensitivity checks still do not address many of the issues discussed above. One can easily construct reasonable confidence intervals that include positive or even more negative estimates. For example, applying the conventional 95 percent confidence interval to the final estimate gives this result.

5 Conclusion

Our principal conclusion is that there are, and were always likely to be, major and legitimate methodological and theoretical impediments and limitations to any attempt to furnish UK citizens – or even political elites privately – with a reliable point estimate as to the prospective effects of a hypothetical Brexit scenario.

This made the challenge for HM Treasury, from the outset, something of a *poisoned chalice*. Recognizing this has significant implications for how we think about the appropriateness of the use of technical economic expertise in public debates, above all those that are already highly charged politically and which take place in a context of low levels of trust in the expert adjudication of emotive political choices (see also Benoit and Hay 2019; Hay 2020). Our methodological critique of HM Treasury's modeling is, then, as much as anything a critique of the misuse of economic expertise – which was, in this case, used to give a spurious precision to a prospective evaluation whose results were always bound to be highly sensitive to the (often arbitrary) analytic assumptions chosen.

Yet, crucially, this is not to argue that those responsible for HM Treasury's estimation were wrong to think that Brexit would bring significant economic costs to UK citizens. It is, however, to argue that such economic costs could not be estimated, certainly using gravity and unadapted off-the-shelf DSGE models, with anything like the precision demanded, and that it was deeply problematic to assume (and to proceed on the basis) that they could. Much more appropriate, we contend, would have been to offer an account of the mechanisms in and through which costs and benefits to the UK economy post-Brexit might accrue and to point to the difficulties of estimating the size of these likely effects.

Ours is by no means, and should not be read as, an argument by default for Brexit. We find it difficult to envisage anything other than economic hardship associated with Brexit. But we challenge the public value of offering a spuriously precise estimation of such effects on the basis of limited and inadequate data, undefended, if necessary assumptions and poorly specified models of the type here deployed. Expertise, above all technical economic expertise that is likely to prove highly inaccessible to citizens, needs to be deployed with much more care and political sensitivity – and, above all, with many more appropriate caveats.

In the detailed analysis, we find that HM Treasury has in all likelihood significantly over-estimated the benefits the UK gets from EU membership in terms of bilateral trade and FDI flows. In the case of trade, we find that this "EU effect" is significantly smaller both for trade involving the UK and when other factors are allowed to have differential effects on OECD and non-OECD countries. In fact, under this model – which is supported by theoretical arguments and has an over 99 percent posterior probability in our Bayesian analysis – the coefficient that is relevant for the UK is not statistically significant. That is, we cannot rule out the possibility that the EU effect is nil, on average, for trade involving the UK.

By contrast, in the case of FDI we find support for HM Treasury's preferred model. Yet, when re-estimating it, we do not find a statistically significant EU investment effect. More precisely, even under the baseline model we cannot rule out that the EU effect is nil for FDI. This result may be driven by the low number of observations, due to a small sample of countries (OECD only) and short time frame (twelve years). We speculate that the statistically significant finding of HM Treasury here may well be due to a more aggressive deletion of outliers (see Appendix A).

These results come with a number of caveats. First, besides differing across country, Figure 2 and recent analyses (e.g., Mayer, Vicard, and Zignago 2019) suggest that the EU effect may also vary over time. Future research must surely take this into account, for example by estimating a smoothly time-varying coefficient. Estimates that put more weight on recent years are likely to be more relevant for a counterfactual analysis.

Second, the estimation technique we (and HM Treasury) use may not be fully adequate. In particular, taking logs, we cannot account for zeros or negative values in the dependent variable. One common solution is to use a nonlinear multiplicative estimator, called the Poisson fixed effects model, which has been shown to be consistent when the data is not Poisson distributed (Wooldridge 1999); a version that allows for weak exogeneity is also available (Wooldridge 1997). This option could be added to the current framework in future work, to eliminate a further source of bias. Using Bayesian model selection may also be a valuable contribution to the literature on the differences between the Poisson fixed effects and log-linearized OLS estimator (see, e.g., Larch et al. 2017). However, this commonly used approach would still require us to condition on FDI flows being non-negative. A further extension would be to use a hurdle model (or other methods) to take into account negative FDI inflows.

Third, we use the OECD countries as a group to find differential effects in the trade regressions. While this can reduce biases, it may not be the most efficient solution. Instead, we could estimate the relevant comparison group(s) from the data using, for example, mixture models.

Despite such caveats, these findings are important. But more important still, we argue, are the implications of our analysis for the supply of expertise, the use of expertise, and the likelihood of its politicization (a politicization to which the campaign for Brexit has contributed further). If political elites are to retain the capacity to deploy appropriate expertise in the public debate, that expertise needs to be used very differently to how it came to be deployed during the EU referendum campaign (see also Grundmann 2018). Above all it needs to be far more sensitive to the increasingly skeptical public audience it is likely to receive. It needs to share and to attempt to explain publicly the legitimate doubts and limitations which accompany it – with the expert taking responsibility not just for the communication of the finding itself (the headline drop in GDP or household income) but also what can and what cannot legitimately be inferred from it.

Though Brexit is, in our view, likely to prove a costly mistake for Britain in economic terms, HM Treasury's estimation of that cost is something of a case study not just in how *not* to conduct that estimation, but above all in how *not* to communicate expertise to the public. The misuse of expertise may not have made Brexit more likely; but it may well have made it more difficult to make a sound and realistic case for its economic cost.

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Appendix A: Data sources

Trade

Like HM Treasury we use bilateral trade data by Glick and Rose (2016), which is freely available on Andrew Rose's website.

FDI

HM Treasury uses bilateral FDI data from the OECD and UNCTAD, without providing details on how the two sources are combined. Because the latter are not freely available for all years, we obtained the FDI flow data from the OECD International direct investment database. Following the OECD (2017), we combined data from the BMD3 and BMD4 databases to get 31 years' worth of observations (1985–2015), before dropping those not considered by HM Treasury.

For each year, OECD member states report both inflows and outflows for selected partner countries. Therefore, flows from A to B are often reported twice: first as outflows by A and again as inflows by B. Our dependent variable "FDI flows" is the unweighted average of inflows and reversed outflows from the origin (home) to the destination (host) country where both are available, or whichever of the two is available.

After applying this treatment, flows are not observed for each country pair and year. Indeed, the 156,412 observations only cover 11 percent of the possible origin country, destination country, and year combinations. To mitigate a possible sample selection bias, we run separate regression of flows between OECD member states, which have a much higher coverage ratio: the resulting 27,374 data points cover 74 percent of the possible OECD country-pairs over the 31-year period. A further benefit of working with the OECD countries–only dataset is that it reduces the number of negative and non-positive observations. Doing so gives approximately the same number of observations as used by HM Treasury

Furthermore, we drop country-pairs involving Luxembourg, reducing the number of countries to 34. This is because both Luxembourg's inflows and outflows are consistently an order of magnitude higher than those of other countries. In addition, Luxembourg has the largest discrepancies among the OECD countries in many financial international investment statistics (Zucman 2013). Thus, including Luxembourg would likely bias the results. HM Treasury also reports the dropping of outliers but provide no details on the criteria used.

Control variables

Regarding the independent variables, we use GDP and population data from Head, Mayer, and Ries (2010), which were updated by the CEPII (2017). GDP figures come from the World Bank's World Development Indicators and Angus Maddison's historical dataset; FTA data from the World Trade Organization. We construct OECD, EEA, and euro area dummies ourselves.

Appendix B: Descriptive statistics

Table S.1 Trade descriptive statistics

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
pair	397,484	9,256.724	7,108.499	1	3,101	15,304	24,905
year	397,484	1,991.856	15.430	1,948	1,981	2,005	2,013
ltrade	397,484	1.247	3.504	–27.967	–0.976	3.663	12.506
lgdp	397,484	49.172	2.742	37.057	47.359	50.985	60.376
lpop	397,484	4.264	2.521	–7.072	2.689	5.942	14.346
eu2	397,484	0.011	0.104	0	0	0	1
eu1	397,484	0.215	0.411	0	0	0	1
fta	397,484	0.058	0.233	0	0	0	1
eea	397,484	0.002	0.045	0	0	0	1

Table S.2 FDI descriptive stat

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
pair	6,878	5,827.242	3,594.864	364	2,712	9,034	12,266
year	6,878	2,006.243	3.614	2,000	2,003	2,009	2,012
lfdi	6,878	4.356	3.092	–6.908	2.314	6.636	12.056
lgdp_o	6,878	26.866	1.512	22.461	26.004	27.902	30.414
lgdp_d	6,878	26.961	1.552	22.821	26.105	28.068	30.414
lpop_o	6,878	2.903	1.342	–1.269	1.986	4.080	5.749
lpop_d	6,878	2.883	1.462	–1.269	2.052	4.084	5.749
eu2	6,878	0.392	0.488	0	0	1	1
fta	6,878	0.317	0.465	0	0	1	1
euro	6,878	0.155	0.362	0	0	0	1

Appendix C: Full results of frequentist analysis

Table S.3 Trade frequentist estimation results

	hmt (1)	hmt2 (2)	uksep (3)	oecdsep (4)	oecd (5)	oecduksep (6)
eu2	0.859***	0.859***	0.507***	0.569***	0.578vz***	0.110
	(0.031)	(0.051)	(0.131)	(0.049)	(0.069)	(0.124)
eu1	–0.024**	–0.024	–0.024	0.015	0.028	0.010
	(0.010)	(0.022)	(0.022)	(0.037)	(0.040)	(0.037)
fta	0.364***	0.364***	0.364***	0.338***	0.323***	0.336***
	(0.013)	(0.029)	(0.029)	(0.037)	(0.039)	(0.037)
eea	0.642***	0.642***	0.641***	0.217**	0.137	0.214**
	(0.059)	(0.091)	(0.091)	(0.099)	(0.102)	(0.099)
lgdp	1.127***	1.127***	1.127***	1.546***	1.539***	1.553***
	(0.007)	(0.021)	(0.021)	(0.041)	(0.067)	(0.041)
lpop	–0.369***	–0.369***	–0.370***	–0.640***	–0.655***	–0.659***
	(0.012)	(0.038)	(0.038)	(0.133)	(0.139)	(0.133)
eu2_nonuk			0.392***			0.503***
			(0.132)			(0.122)
eu1_nonoecd				–0.046		–0.041
				(0.042)		(0.042)
fta_nonoecd				–0.054		–0.052
				(0.049)		(0.049)
lgdp_nonoecd				–0.440***		–0.447***
				(0.039)		(0.039)
lpop_nonoecd				0.405***		0.424***
				(0.135)		(0.135)
Robust SE	No	Yes	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Countries	All	All	All	All	OECD	All
Observations	397,484	397,484	397,484	397,484	25,872	397,484
R^2	0.069	0.069	0.069	0.073	0.329	0.073

Notes: The dependent variable is log trade flows. Columns (1) and (2) report estimates for equation (1). The remaining columns introduce control variables, as described in Section 2. Estimates are obtained using a panel data estimation, by first demeaning the data. Robust standard errors are calculated using White's formula and clustered at the country-pair level. R^2 is the un-adjusted within R^2. Data primarily come from Glick and Rose (2016) and CEPII (2017), see Appendix A. *p<0.1; **p<0.05; ***p<0.01.

Table S.4 FDI frequentist estimation results

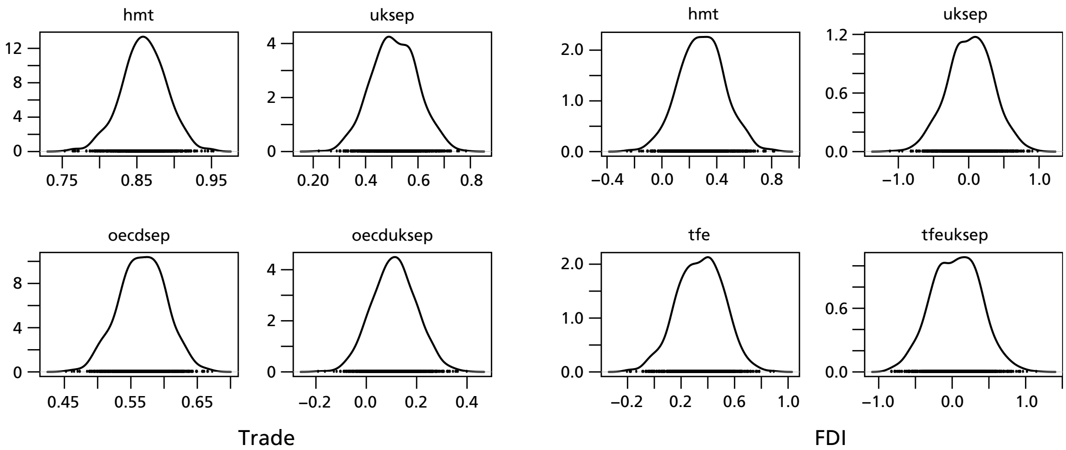
	hmt (1)	hmt2 (2)	uksep (3)	tfe (4)	tfeuksep (5)
eu2	0.284	0.284	0.007	0.333	0.075
	(0.175)	(0.202)	(0.475)	(0.207)	(0.487)
fta	0.067	0.067	0.066	0.099	0.098
	(0.135)	(0.145)	(0.145)	(0.145)	(0.145)
lgdp_o	0.524***	0.524***	0.522***	0.759***	0.753***
	(0.116)	(0.145)	(0.145)	(0.194)	(0.194)
lgdp_d	1.482***	1.482***	1.480***	1.743***	1.737***
	(0.127)	(0.164)	(0.164)	(0.202)	(0.202)
lpop_o	1.696**	1.696*	1.717*	2.828***	2.842***
	(0.780)	(1.014)	(1.016)	(1.084)	(1.086)
lpop_d	–5.241***	–5.241***	–5.211***	–3.944***	–3.921***
	(0.952)	(1.283)	(1.282)	(1.383)	(1.382)
euro	0.285*	0.285	0.279	0.361	0.356
	(0.161)	(0.219)	(0.218)	(0.221)	(0.221)
eu2_nonuk			0.300		0.280
			(0.463)		(0.471)
Robust SE	No	Yes	Yes	Yes	Yes
Pair FE	Yes	Yes	Yes	Yes	Yes
Time SE	No	No	No	Yes	Yes
Observations	6,878	6,878	6,878	6,878	6,878
R^2	0.148	0.148	0.148	0.030	0.030

Notes: The dependent variable is log FDI flows. Columns (1) and (2) report estimates for equation (2). The remaining columns introduce control variables, as described in Section 2. Estimates are obtained using a panel data estimation, by first demeaning the data. Robust standard errors are calculated using White's formula and clustered at the country-pair level. R^2 is the un-adjusted within R^2. Data primarily come from OECD (2017) and CEPII (2017), see Appendix A. *p<0.1; **p<0.05; ***p<0.01.

Appendix D: Results from Bayesian analysis

The following figure shows the results (posterior distribution) for the EU coefficient from the Bayesian estimation.

Figure S.1 Posterior distribution of the EU2 coefficient



Notes: Shown are posterior distributions for the EU coefficients, estimated using 1,000 MCMC iterations. Data was demeaned by country-pair and time first. Variables included are the same as in the corresponding regressions in Table S.3 and Table S.4. Priors are $p(\alpha,\beta)=N(0,10^6\cdot\sigma\cdot I)$ and $p(\alpha)=IG(10^{-3},10^{-3})$. Results for the remaining coefficients can be found below.

Figure S.2 Trade Bayesian results (all coefficients)

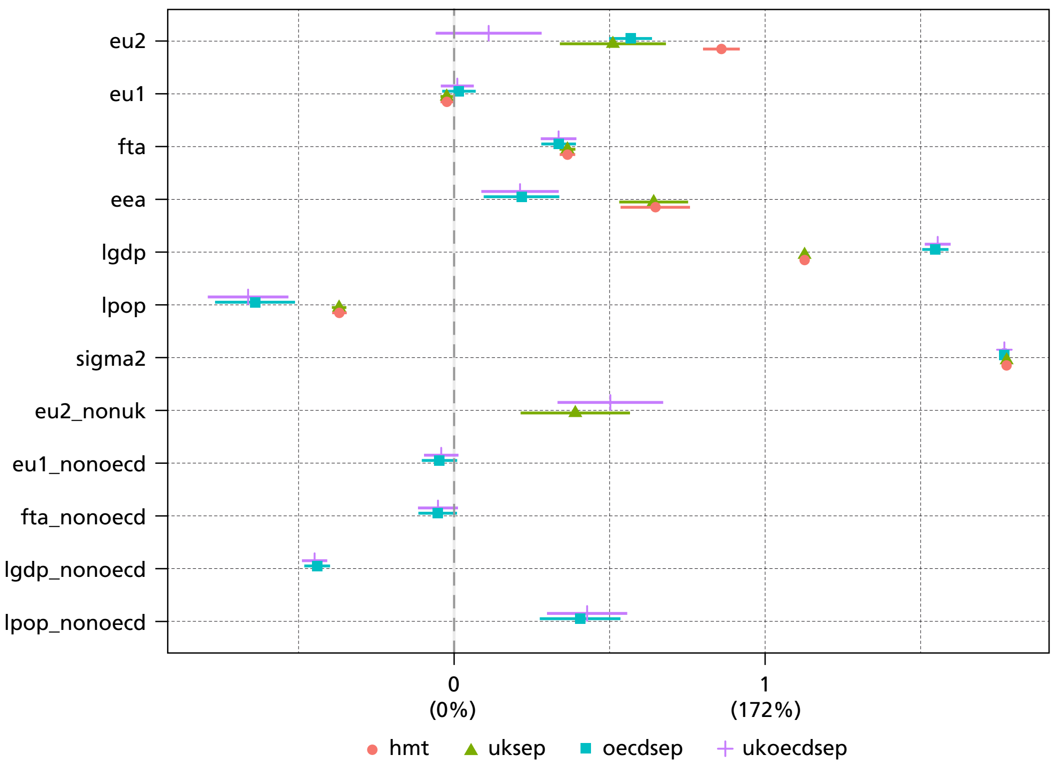
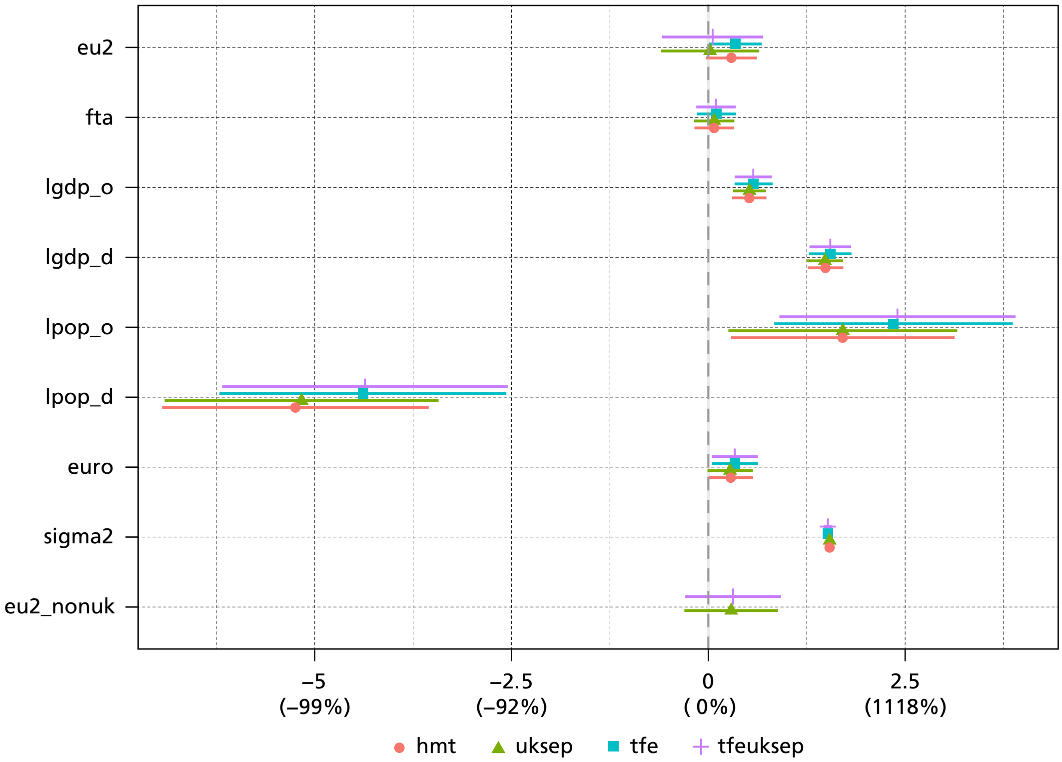
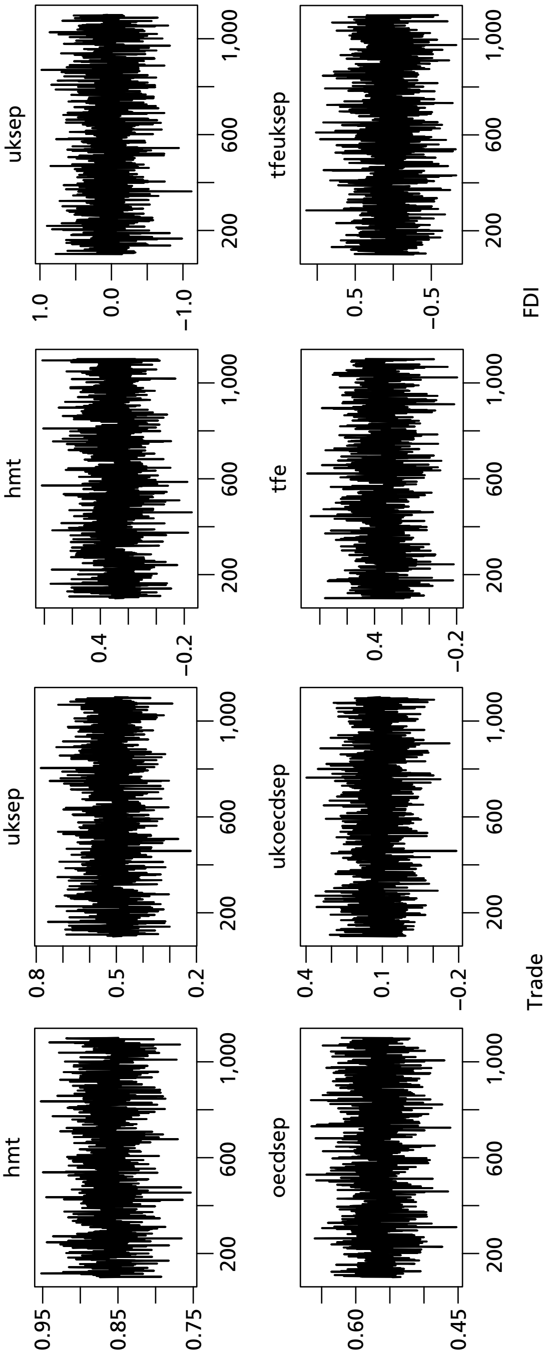


Figure S.3 FDI Bayesian results (all coefficients)



Appendix E: Traceplots for Bayesian analysis

Figure S.4 Traceplots for Bayesian analysis



Appendix F: Further modeling issues

The general issues with the model used to convert gravity estimates into an overall economic forecast are described in Section 4. Here we explain problems with specific steps taken by HM Treasury to convert their empirical estimates into model inputs.

Trade

To get an estimate for the effect of Brexit on trade flows, HM Treasury starts with the estimated effect of joining the EU on trade flows. We discussed the problems with their methodology at length in Section 2. Among other issues, the Treasury estimates the effect of joining for an average EU country (not for the UK specifically) and uses all countries (not just advanced economies). The effects for joining the EU, joining the EEA, and putting in place a free trade agreement are estimated separately for goods and services.

There are already problems with treating the potential effect of leaving the EU as a mirror image of the effect of joining. But HM Treasury compounds this difficulty in the way in which it converts one into the other. The first problem here is that it sets coefficients whose 95 percent confidence intervals include zero to zero (central estimate = lower bound = upper bound = 0). This is done with both the effect of joining the EU on trade in goods with non-EU countries (trade diversion; central estimate = –3.6 percent) and the effect of joining the EEA on trade in services (central estimate = –10.0 percent). No justification is given for this. Because the central estimate is in fact neither equal to zero nor precisely estimated, this biases the result and gives a spurious and misleading certainty to the predicted result.

Next, the remaining (non-zero) estimates are converted into reversed gains (for example, a 25 percent gain corresponds to a 20 percent loss: 100*1.25 = 125, 125*0.8 = 100). Moreover, because the NiGEM version used by HM Treasury includes only aggregate trade variables rather than bilateral trade relationships, the estimates are weighted by the share of UK trade in goods and services with EU, EEA, and other countries to obtain an estimate of the effect of leaving on overall trade. This step is necessary to bring the estimates to the model, although it again raises the issue of misrepresenting uncertainty arising from the fact that important channels are missing from the model (see above).

To obtain the final model input for the effect of leaving the EU on trade, HM Treasury makes a number of ad hoc modifications and assumptions. First, it assumes that the EU effect will be completely reversed after exactly fifteen years and linearly so during the fifteen years. Second, they replace the lower and upper bound for the EEA scenario (reversed EU effect – reversed EEA effect) with the central estimate, arbitrarily reducing uncertainty. Third, for the FDI and WTO scenarios they assume that the reduction will be the effect of Brexit under the EEA scenario plus 50 percent of the difference with the EEA scenario for the lower bound (the less extreme case), but 100 percent of the difference for the central and upper bound. Finally, they use a 67 percent confidence interval (one standard deviation) for the lower and upper bound, instead of the conventional (and previously used) 95 percent confidence interval.

FDI

The estimation of the effect of leaving the EU as if it were a simple inversion of the process of joining the EU is no less problematic for FDI than it is for trade. As in the case of trade, the estimation strategy for the effect of joining has several potential problems, which we discussed in Section 2. In particular, the data is not sufficient to use more robust country-pair fixed effects and the estimated confidence interval from the less stringent estimation used includes zero (in our analysis, without the undocumented deletion of outliers done by HM Treasury). The strategy for converting the effect broadly follows the same steps as in the case for trade. In particular, the assumptions about how much of the effect is reduced (exactly 100 percent in the central estimate and upper bound, linearly spread over fifteen years following Brexit) and the replacement of the bounds in the EEA scenario are the same. However, there are at least three problematic steps specific to FDI.

First, HM Treasury fails to find a significant effect of EEA membership and FTAs on FDI flows. Instead of using the central estimate and confidence intervals from the estimation (as customary) or erroneously assuming the effects are identically equal to zero (as done in the case of trade diversion and the effect of EEA membership on trade in services), HM Treasury uses the loss under the WTO scenario (reversed gain of EU membership), scaled by the difference in the *trade* effect between scenarios (EEA scenario loss = 38 percent of WTO scenario loss, FTA scenario loss = 78 percent of WTO scenario loss). This goes against the effects that HM Treasury estimated in their empirical analysis.

Second, because the FDI data used in the empirical analysis does not cover many non-EU countries, the effect of EU membership on FDI with other countries cannot be reliably estimated. Instead, HM Treasury "uses the best available estimate for the impact of EU membership on non-EU FDI flows, which means that EU membership affects EU FDI flows, and non-EU FDI flows, to the same extent" (HM Treasury 2016, 175). The argument for doing so is that the EU is very important for platform FDI (using one EU country to invest in other EU countries). Leaving aside the fact that platform FDI probably does not affect economic activity in the same way as greenfield investment (in, say, production plants), HM Treasury again understates the uncertainty inherent in this important ad hoc assumption by not providing any confidence bounds.

Third, as mentioned above, FDI is never used as an input into the model but only through its effect on productivity.

Productivity

To estimate the effect of Brexit on productivity (the second and final input for the model), HM Treasury combines its estimates of Brexit on trade and FDI with the elasticity of productivity with respect to trade and FDI (the percentage change in productivity induced by a 1 percent change in trade or FDI flows). For trade, values between 0.2 and 0.3 are taken from the literature. For FDI, several values between 0.03 and 0.08 are estimated using UK data, but in the end a value of 0.04 is chosen, ignoring the uncertainty arising from both the estimation strategy and the confidence interval around the central estimate.

The elasticity with regard to trade and FDI is then multiplied by the overall change in trade and FDI after fifteen years and applied to all years as a "level shift." This strategy is not explained and biases the estimate. That is because if changes to trade and FDI are believed to come through only gradually, then applying the change in productivity for the final year to all years is not consistent. The average productivity effect should be half the estimated effect in the final year, given the linear decrease in trade and FDI. In addition, using the effect after fifteen years for all years decreases the final GDP estimate further through general equilibrium and persistency effects. While these may be second order, using the estimated effect for the fifteenth year after Brexit – instead of half this effect – has potentially large effects. Whereas this value was used for trade, we found that we could only replicate HM Treasury's final inputs by using an FDI elasticity of 0.02, which gives an FDI-related decrease in productivity that – at least on average – is consistent with the linear decrease in FDI.

Appendix G: List of models and results

Table S.5 Models used in sensitivity analysis

Trade model	FDI model	Productivity elasticities	Reduction over 15 years	"Long-run" impact	Bound	Estimated change in GDP
Original	Original	Original	Original	1%	Upper Central Lower	–9.5% –8.4% –5.4%
Reproduced	**Reproduced**	Original	Original	1%	Upper Central Lower	–10.0% –8.4% –5.6%
UK Separate	Original	Original	Original	1%	Upper Central Lower	–8.1% –6.1% –2.6%
OECD Separate	Original	Original	Original	1%	Upper Central Lower	–7.9% –6.5% –5.0%
UK & OECD Separate	Original	Original	Original	1%	Upper Central Lower	–5.2% –3.5% –1.8%
Reproduced	**Reproduced**	Original	**50%**	1%	Upper Central Lower	–5.5% –4.7% –3.8%
UK & OECD Separate	**Reproduced**	**Half of specified**	Original	**0%**	Lower	–0.3%
Reproduced	**Reproduced**	**As specified**	Original	**2%**	Upper	–11.6%

Notes: Original model inputs are taken from HM Treasury. Changed values are highlighted in bold. Reproduced inputs are obtained using the same model as HM Treasury, see empirical section. Original productivity elasticities are 30% (upper bound), 25% (central estimate) and 20% (lower bound) for trade (as specified) and 2% for FDI (half of specified value, giving the correct average over 15 years). Original reduction over 15 years is 100% for central estimate and upper bound and 50% for lower bound estimates.

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