Counterfactual Analysis in Macroeconometrics: An Empirical Investigation into the Effects of Quantitative Easing*

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Abstract

This paper is concerned with *ex ante* and *ex post* counterfactual analyses in the case of macroeconometric applications where a single unit is observed before and after a given policy intervention. It distinguishes between cases where the policy change affects the model's parameters and where it does not. It is argued that for *ex post* policy evaluation it is important that outcomes are conditioned on *ex post* realized variables that are invariant to the policy change but nevertheless influence the outcomes. The effects of the control variables that are determined endogenously with the policy outcomes can be solved out for the policy evaluation exercise. An *ex post* policy ineffectiveness test statistic is proposed. The analysis is applied to the evaluation of the effects of the quantitative easing (QE) in the UK after March 2009. It is estimated that a 100 basis points reduction in the spread due to QE has an impact effect on output growth of about one percentage point, but the policy impact is very quickly reversed with no statistically significant effects remaining within 9–12 months of the policy intervention.

Keywords: Counterfactuals, policy evaluation, macroeconomics, quantitative easing (QE), UK economic policy

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1 Introduction

The term "counterfactual" has a wide range of uses in philosophy, history, economics and statistics. In philosophy counterfactual scenarios are often used in the analysis of causality, e.g. Lewis (1973). Pearl (2009) provides an overview of the concepts and develops an analysis of causality based on structural models. In history counterfactuals are posed by "what if" questions, such as "what would the U.S. economy have been like in 1890 had there been no railroads?", Fogel (1964). In economics alternative counterfactuals (hypothetical states of the world) are considered in decision making under uncertainty. In statistics and econometrics counterfactuals are used in policy evaluations (e.g. Heckman, 2008 & 2010). The above uses whilst quite distinct are closely connected. However, in this paper we shall focus on the use of counterfactuals in *ex post* macroeconometric policy evaluation, using the case of quantitative easing after March 2009 in the UK as an example.

By a counterfactual we mean "what would have occurred if some observed characteristics or aspects of the processes under consideration were different from those prevailing at the time." For instance, what if the level of a policy variable, x_t, is set differently, or what if the parameters of the process that determines x_t are changed. In effect, we are interested in comparing an *ex post* realized outcome with a counterfactual outcome that could have obtained under certain assumptions regarding the policy variable. Such an *ex post* policy counterfactual policy evaluation exercise differs from *ex ante* counterfactual analysis that contributes to the decision making leading to adoption of a new policy, in a sense we make precise below.

In the analysis of policy evaluation it is important to distinguish between micro and macro cases. In the former case policy is applied across many different units decomposed into those affected by policy (the "treated" group) and those that are not (the "untreated" group) within a given time frame. This is the typical case in the microeconometric (micro) policy evaluation, surveyed, for example, by Imbens & Wooldridge (2009). The other case is where the policy is applied to a single or a few units but over two different time periods: a "policy off" and a "policy on" period. This is the typical case in macroeconometric (macro) policy evaluation. The micro policy evaluation problem has been the subject of a large literature, also known as the treatment effect literature. In contrast, there is less systematic methodological discussion of macro policy evaluation. To be specific, suppose that we have units $i = 1, 2, ..., N$ observed over time periods, $t = 1, 2, ..., T$. In the micro analysis N tends to be large and T small, whereas in the macro analysis N tends to be small and T large, including observations both before and after the policy intervention.

In this paper we consider both the case of many units with a single time period and a single unit with many time periods, but focus on the methodological issues arising from the latter case of an *ex post* macro policy evaluation exercise. We suppose that the counterfactuals being considered can be generated from an explicit econometric model, such as a simultaneous equations model or a rational expectations model, and emphasise the invariance assumptions required for the validity of such counterfactual exercises. We show that it is important to distinguish between *ad hoc* policy changes when policy instruments are shocked over one or more time periods, as compared to more fundamental policy interventions where one or more parameters of a policy rule are changed.

Examples of macro counterfactuals are: what was the effect of terrorism on the Basque country? Abadie & Gardeazabal (2003); what would have happened to the economies of the UK and the eurozone had the UK joined the euro in 1999? Pesaran, Smith & Smith (2007); what was the effect on growth in Hong Kong of political and economic integration with mainland China? Hsiao, Ching & Wan (2011); what was the effect of monetary shocks in the US? Angrist & Kuersteiner (2011); what would have been the effect of the Federal Reserve following a different policy rule? Orphanides & Williams (2011); what was the effect of Quantitative Easing in the UK? the case discussed below.

In both microeconometrics and macroeconometrics there have been disputes about the importance of structural modelling for policy evaluation. But what is meant by "structural" differs depending on the problem and context. The microeconometric issues are debated by Imbens (2010) and Heckman (2010). In macroeconomics, structural models have been identified with DSGE models, of a particular type, which have major limitations in addressing the policy questions that arose after the recent crisis.

We argue that for estimation of policy effects we need to consider conditional models with parameters that are invariant to policy change. A full structural specification is not always necessary and different types of structures are needed for different purposes. A structural model that helps identify a particular parameter of interest need not be appropriate for policy analysis where the policy change could initiate direct and indirect impacts on outcomes.

Consider the effects of a change in a policy (intervention) variable, x_t, on a target or outcome variable, y_t. Suppose that y_t and x_t can also be affected by a set of control variables, z_t, which need not be invariant to changes in x_t. Finally, suppose that there exists a second set of variables, w_t, that could affect y_t or z_t, but are known to be invariant to changes in x_t. We argue that for evaluation of the effect of a policy change, we only need to consider a model of y_t conditional on x_t and w_t. There is no need for a structural model that involves all the four variable types.[1] We do not need to condition on z_t but benefit from conditioning on w_t. In considering evaluation,

[1] This argument is closely related to what Heckman (2010, p.359) calls Marschak's maxim: that all that is required to answer many policy questions are policy invariant combinations of the structural parameters rather than the structural parameters themselves.

we distinguish between policy changes that alter the parameters of the conditional model, as compared to *ad hoc* policy changes that do not affect the parameters of the reduced form policy equations.

A simple example of travel mode choice may clarify the issues. Suppose that for a unit of interest (such as an individual, firm or government) at time t we observe the setting of the policy variable $X = x_0$, (go by bus) and the outcome variable $Y = y_0$, (bus travel time) and then at time $t+1$, we observe $X = x_1$, (go by train) and $Y = y_1$ (train travel time). We also observe certain variables, $\mathbf{w}_t$, which are invariant to the decision about the indiviudal's mode of travel. These might include day of the week, weather on that day, or bus and train timetables. Then for sufficient observations in each state and a given set of invariances, we can make probabilistic statements about the values of Y in period t had the individual gone by train rather than by bus. In practice we consider such counterfactuals all the time. When we consider them *ex ante*, to make the decision whether to travel by bus or by train, we may not know the weather on the day or other variables which influence travel time on each mode. When we consider them *ex post* we have a lot more information about realizations, such as the actual travel times on each mode or the factors influencing travel times. *Ex post* we can ask how long the travel time would have been on the travel mode not chosen. In order to make this prediction we do not need to know all factors affecting journey time, those we do not know we treat as part of the random error. Clearly, if we look at the weather forecast before deciding which mode of travel to take, the weather is not invariant and should not be included in the control variates, $\mathbf{w}_t$. Prior information about the context is crucial to specifying the counterfactual and the appropriate variables that are relevant to the outcome but invariant to the policy choice.

We begin Section 2 with a consideration of the literature on treatment effects that primarily use the cross-sectional observations, to highlight the different issues that are involved in the counterfactual analysis of a single unit over time as compared to the counterfactual analysis of many cross section units over a given time interval. We then proceed to the time-series case of counterfactual analysis for a single unit over time in Section 3.

We use the example of quantitative easing (QE) in the UK to illustrate our procedure in Section 4. The estimates suggest that if QE in the UK after March 2009 caused a permanent 100 basis points reduction in the spread, in line with the estimate adopted by researchers at the Bank of England, this would have an impact effect on the growth rate of output of about one percentage point. But this effect is very quickly reversed with no permanent effect on growth. Thus while the change in the spread could have unintended permanent distributional effects (between savers and borrowers), the intended effect of the policy on real output is likely to be temporary. Some concluding remarks are provided in Section 5.

2 Counterfactual analysis across many units

Although it is not our primary focus, it is useful to consider the problem of counterfactual analysis in a purely cross-sectional set up to highlight how it differs from the time-series case which is our focus. The cross-sectional studies assume that there are sufficient number of units that are subject to the treatment and that the effects of the treatment (if any) are fully materialized over the given observation interval. It is also further assumed that there exists a sufficient number of units in a control group who have not been subject to the treatment, but share common characteristics with the treated. In contrast, in pure time series applications there are no control units and the effects of the treatment (policy) might be distributed over time and could be subject to reversal.

2.1 Identification of the treatment effect using cross-sectional data

Suppose that a continuous target (or outcome) variable, y_i, and a vector of exogenous covariates, $\mathbf{w}_i$, are observed for a sample of $i = 1, 2, ..., N$ units (individuals) in a given time period, and there is a discrete policy treatment denoted by a dummy variable x_i that takes the value of unity if individual i is treated, and zero for the untreated. Denote the outcomes for the treated individuals by $y^{\mathcal{T}}_{i_{\mathcal{T}}}$, $i_{\mathcal{T}} = 1, 2, ..., N_{\mathcal{T}}$, and for the untreated ones by $y^{\mathcal{U}}_{i_{\mathcal{U}}}$, $i_{\mathcal{U}} = 1, 2, ..., N_{\mathcal{U}}$, so that $N = N_{\mathcal{T}} + N_{\mathcal{U}}$. We distinguish the index, $i_{\mathcal{T}}$ or $i_{\mathcal{U}}$, to emphasise that we are considering the observed outcomes for different units not the actual and counterfactual outcomes for the same unit. To estimate the effect of treatment, we require observations on both treated and untreated. If the proportion treated is $p = N_{\mathcal{T}}/N$ we require that $0 < p < 1$, or more generally if $p(\mathbf{w}_i) = p(x_i = 1 \mid \mathbf{w}_i)$ is the probability of treatment conditional on covariates, we require $0 < p(\mathbf{w}_i) < 1$. This assumption ensures that for each value of $\mathbf{w}_i$ there are both treated and untreated units. Given data on x_i and $\mathbf{w}_i$, the propensity score $p(\mathbf{w}_i)$ can be estimated.

We provide a formulation that relates easily to the time-series case and for simplicity assume a single covariate, but allow the parameters to be randomly heterogeneous and distributed independently of the covariate.[2] Specifically, we assume that

$$y^{\mathcal{T}}_{i_{\mathcal{T}}} = \alpha^{\mathcal{T}}_{i_{\mathcal{T}}} + \beta_{i_{\mathcal{T}}} + \gamma^{\mathcal{T}}_{i_{\mathcal{T}}} w^{\mathcal{T}}_{i_{\mathcal{T}}} + \varepsilon^{\mathcal{T}}_{i_{\mathcal{T}}},\ \varepsilon^{\mathcal{T}}_{i_{\mathcal{T}}} \sim IID(0, \sigma^2_{\mathcal{T}});\ i_{\mathcal{T}} = 1, 2, ..., N_{\mathcal{T}},$$

$$y^{\mathcal{U}}_{i_{\mathcal{U}}} = \alpha^{\mathcal{U}}_{i_{\mathcal{U}}} + \gamma^{\mathcal{U}}_{i_{\mathcal{U}}} w^{\mathcal{U}}_{i_{\mathcal{U}}} + \varepsilon^{\mathcal{U}}_{i_{\mathcal{U}}},\ \varepsilon^{\mathcal{U}}_{i_{\mathcal{U}}} \sim IID(0, \sigma^2_{\mathcal{U}}),\ i_{\mathcal{U}} = 1, 2, ..., N_{\mathcal{U}};$$

$$\beta_{i_{\mathcal{T}}} = \beta + v_{i_{\mathcal{T}}},\ v_{i_{\mathcal{T}}} \sim IID(0, \sigma^2_v);$$

$$\alpha^{\mathcal{T}}_{i_{\mathcal{T}}} = \alpha^{\mathcal{T}} + \eta^{\mathcal{T}}_{i_{\mathcal{T}}},\ \eta^{\mathcal{T}}_{i_{\mathcal{T}}} \sim IID(0, \sigma^2_{\mathcal{T},\eta});\quad \alpha^{\mathcal{U}}_{i_{\mathcal{U}}} = \alpha^{\mathcal{U}} + \eta^{\mathcal{U}}_{i_{\mathcal{U}}},\ \eta_{i_{\mathcal{U}}} \sim IID(0, \sigma^2_{\mathcal{U},\eta});$$

$$\gamma^{\mathcal{T}}_{i_{\mathcal{T}}} = \gamma^{\mathcal{T}} + \xi^{\mathcal{T}}_{i_{\mathcal{T}}};\ \xi^{\mathcal{T}}_{i_{\mathcal{T}}} \sim IID(0, \sigma^2_{\mathcal{T},\xi});\quad \gamma^{\mathcal{U}}_{i_{\mathcal{U}}} = \gamma^{\mathcal{U}} + \xi^{\mathcal{U}}_{i_{\mathcal{U}}};\ \xi^{\mathcal{U}}_{i_{\mathcal{U}}} \sim IID(0, \sigma^2_{\mathcal{U},\xi}).$$

[2] Hsiao, Li, Liang & Xie (2011) consider the case of correlated random coefficient models.

Using $x_i = 1$ for the treated units and $x_i = 0$ for the untreated, the model can be written compactly as

$$y_i = \alpha^{\mathcal{U}} + (\alpha^{\mathcal{T}} - \alpha^{\mathcal{U}} + \beta)x_i + \gamma^{\mathcal{T}}(w_i^{\mathcal{T}} x_i) + \gamma^{\mathcal{U}}\left[w_i^{\mathcal{U}}(1 - x_i)\right] + u_i, \tag{1}$$

where

$$u_i = x_i(v_i + \eta_i^{\mathcal{T}} + \xi_i^{\mathcal{T}} w_i^{\mathcal{T}} + \varepsilon_i^{\mathcal{T}}) + (1 - x_i)(\eta_i^{\mathcal{U}} + \xi_i^{\mathcal{U}} w_i^{\mathcal{U}} + \varepsilon_i^{\mathcal{U}}).$$

It is clear that the treatment effect, β, can only be identified if $\alpha^{\mathcal{T}} = \alpha^{\mathcal{U}} = \alpha$, namely that if there are no systematic differences between the two groups apart from the treatment. Under this condition, we have

$$y_i = \alpha + \beta x_i + \gamma_i w_i + u_i, \tag{2}$$

where $\gamma_i = \gamma^{\mathcal{T}}$ and $w_i = w_i^{\mathcal{T}}$ if $x_i = 1$, and $\gamma_i = \gamma^{\mathcal{U}}$ and $w_i = w_i^{\mathcal{U}}$, if $x_i = 0$.

Then, noting that $x_i(1-x_i) = 0$, necessary conditions for identification of β in the cross-section model (1) are: $0 < p < 1$, $\alpha^{\mathcal{T}} = \alpha^{\mathcal{U}}$ and

$$E\left[x_i\left(v_i + \eta_i^{\mathcal{T}} + \xi_i^{\mathcal{T}} w_i^{\mathcal{T}} + \varepsilon_i^{\mathcal{T}}\right)\right] = 0,$$
$$E\left[x_i w_i^{\mathcal{T}}\left(v_i + \eta_i^{\mathcal{T}} + \xi_i^{\mathcal{T}} w_i^{\mathcal{T}} + \varepsilon_i^{\mathcal{T}}\right)\right] = 0,$$
$$E\left[(1 - x_i) w_i^{\mathcal{U}}\left(\eta_i^{\mathcal{U}} + \xi_i^{\mathcal{U}} w_i^{\mathcal{U}} + \varepsilon_i^{\mathcal{U}}\right)\right] = 0.$$

The above assumptions require that treatment, $x_i = 1$, should not be correlated with characteristics of the treated or the covariates, and that for the treated the covariates are not correlated with the characteristics of the treated. The assumptions could fail if the assignment or selection into the treatment or non-treatment groups was on the basis of the individual component of the treatment effect, v_i, or their intercept, $\eta_i^{\mathcal{T}}$. The correlation of x_i with v_i or $\eta_i^{\mathcal{T}}$ has been a major focus of the microeconometric literature.

If $\alpha^{\mathcal{T}} = \alpha^{\mathcal{U}} = \alpha$, and $\gamma^{\mathcal{T}}$ and $\gamma^{\mathcal{U}}$ are homogenous, we can write (1)

$$y_i = \alpha + \beta x_i + \gamma^{\mathcal{T}}(w_i^{\mathcal{T}} x_i) + \gamma^{\mathcal{U}}\left[w_i^{\mathcal{U}}(1 - x_i)\right] + u_i \tag{3}$$

Define $\bar{w}_{\mathcal{T}} = N_{\mathcal{T}}^{-1}\sum_{i_{\mathcal{T}}=1}^{N_{\mathcal{T}}} w_{i_{\mathcal{T}}}^{\mathcal{T}}$, $\bar{w}_{\mathcal{U}} = N_{\mathcal{U}}^{-1}\sum_{i_{\mathcal{U}}=1}^{N_{\mathcal{U}}} w_{i_{\mathcal{U}}}^{\mathcal{U}}$, $s_{\mathcal{T}}^2 = N_{\mathcal{T}}^{-1}\sum_{i_{\mathcal{T}}=1}^{N_{\mathcal{T}}}(w_{i_{\mathcal{T}}}^{\mathcal{T}} - \bar{w}_{\mathcal{T}})^2$, and $s_{\mathcal{U}}^2 = N_{\mathcal{U}}^{-1}\sum_{i_{\mathcal{U}}=1}^{N_{\mathcal{U}}}(w_{i_{\mathcal{U}}}^{\mathcal{U}} - \bar{w}_{\mathcal{U}})^2$.

Assuming that $0 < p < 1$ then $s_{\mathcal{U}}^2 = N_{\mathcal{U}}^{-1}\sum_{i_{\mathcal{U}}=1}^{N_{\mathcal{U}}}(w_{i_{\mathcal{U}}}^{\mathcal{U}} - \bar{w}_{\mathcal{U}})^2 > 0$, and $s_{\mathcal{T}}^2 = N_{\mathcal{T}}^{-1}\sum_{i_{\mathcal{T}}=1}^{N_{\mathcal{T}}}(w_{i_{\mathcal{T}}}^{\mathcal{T}} - \bar{w}_{\mathcal{T}})^2 > 0$, and the least squares estimate of β in (3) is given by

$$\hat{\beta} = \left(\bar{y}^{\mathcal{T}} - \hat{\gamma}^{\mathcal{T}}\bar{w}^{\mathcal{T}}\right) - \left(\bar{y}^{\mathcal{U}} - \hat{\gamma}^{\mathcal{U}}\bar{w}^{\mathcal{U}}\right) = \hat{a}^{\mathcal{T}} - \hat{a}^{\mathcal{U}};$$

$\hat{\gamma}^{\mathcal{T}}$ is the estimated regression coefficient on the treated sample and $\hat{\gamma}^{\mathcal{U}}$ is the regression coefficient on the untreated sample. The estimate of $\hat{\beta}$ is the difference between the estimated intercepts from the two separate OLS regressions for the treated and the untreated. This brings out the role of the identifying assumption, $\alpha^{\mathcal{T}} = \alpha^{\mathcal{U}}$, which enables us to test if the effect of the treatment is statistically significant.

3 Counterfactual analysis for a single unit over time

3.1 A rational expectations framework

We now consider a policy evaluation problem where the aim is to estimate the "average" effect of a policy intervention, given time-series data for a single unit for both "policy off" and "policy on" periods. Given that we are considering a single unit and the objective is to measure the effect of the intervention on that unit, the selection problem discussed above will not arise.

We begin by abstracting from model and parameter estimation uncertainty, these are important in practice but are not specific to the issues of counterfactuals. We do, however, allow for the possibility that the policy intervention might change some of the model parameters in the context of a rational expectations model. We suppose that the single target or outcome variable y_t is affected directly by a single policy variable x_t and one or more control variates, $\mathbf{z}_t$. We also assume that there exists a set of variables, $\mathbf{w}_t$, that affect y_t or $\mathbf{z}_t$ but are invariant to changes in x_t and $\mathbf{z}_t$. Obvious examples of $\mathbf{w}_t$ are international oil prices or world output for policy interventions in the case of a small open economy such as the UK.[3] As our example of travel mode choice in the introduction illustrated the choice of the elements of $\mathbf{w}_t$ will depend on the context. In that example the choice by the individual to go by bus or train in unlikely to change the travel time of the bus or train, since the individual is viewed as being one amongst many that make the same travel choices. It is also implicitly assumed that such individual decisions are cross-sectionally independent.

As noted earlier, it is important that we distinguish the cases where there is an exogenous, *ad hoc*, change in x_t from the case where there is a change in the process determining x_t. Let $\mathbf{q}_t = (y_t, \mathbf{z}_t')'$, $\mathbf{s}_t = (x_t, \mathbf{w}_t')'$, and suppose that the endogenous variables, $\mathbf{q}_t$, are determined by the following rational expectations model

$$\mathbf{A}_0 \mathbf{q}_t = \mathbf{A}_1 E_t(\mathbf{q}_{t+1}) + \mathbf{A}_2 \mathbf{s}_t + \mathbf{u}_t, \tag{4}$$

where $E_t(\mathbf{q}_{t+1}) = E(\mathbf{q}_{t+1} \mid \Im_t)$, and $\Im_t$ is the non-decreasing information set, $\Im_t = (\mathbf{q}_t, \mathbf{s}_t; \mathbf{q}_{t-1}, \mathbf{s}_{t-1}, ...)$. The processes generating the elements of $\mathbf{s}_t$ are given by

$$x_t = \rho_x x_{t-1} + v_{xt}, \text{ and } \mathbf{w}_t = \mathbf{R}_w \mathbf{w}_{t-1} + \mathbf{v}_{wt}$$

[3] We assume that there are a small number of variables in the vector $\mathbf{w}_t$. It there are a large number, the dimension could be restricted by Bayesian shrinkage or extracting principal components.

so that w_t is invariant to changes in x_t and $\mathbf{q}_t$. The errors, $\mathbf{u}_t$ and $\mathbf{v}_t$ have means zero, constant variances, and $E(\mathbf{u}_t\mathbf{v}_t') = 0$. The RE model could result from some well defined decision problem, and can be extended to allow for dynamics. But it is sufficiently general for our purposes.

We assume that apart from possible changes in the process determining x_t, that is changes in ρ_x, the model is stable in that the structural parameters ($\mathbf{A}_0$, $\mathbf{A}_1$, $\mathbf{A}_2$) are policy and time invariant. We will also require that certain parameters, such as $\rho = (\rho_x, vec(\mathbf{R}_w)')'$, are identified. There is a unique stationary solution if all the eigenvalues of $\mathbf{Q} = \mathbf{A}_0^{-1}\mathbf{A}_1$ lie within the unit circle. The unique solution is given by

$$\mathbf{A}_0\mathbf{q}_t = \mathbf{G}(\rho, \mathbf{a})\mathbf{s}_t + \mathbf{u}_t, \tag{5}$$

where $\mathbf{a} = vec(\mathbf{A}_0, \mathbf{A}_1, \mathbf{A}_2)$,

$$Vec(\mathbf{G}) = \left[(\mathbf{I}\otimes\mathbf{I}) - \left(\mathbf{R}'\otimes\mathbf{A}_1\mathbf{A}_0^{-1}\right)\right]^{-1} Vec\left(\mathbf{A}_2\right),$$

and $\mathbf{R}$ is defined by $\mathbf{s}_t = \mathbf{R}\mathbf{s}_{t-1} + \mathbf{v}_{st}$.

Equation (5) is the structural form of a standard simultaneous equations model. Assume that there are sufficient identifying restrictions to consistently estimate the unknown elements of the structural parameters, $\mathbf{A}_0$, $\mathbf{A}_1$, and $\mathbf{A}_2$. If the process determining policy changes, through a change in ρ_x, the parameters in $\mathbf{G}$ will not be invariant to the policy change. But if the change in policy is in the form of changes in the values of x_t achieved by shocking v_{xt}, the policy change does not cause a change in the parameters and the effect of the policy intervention can be computed using the reduced form equation

$$\mathbf{q}_t = \mathbf{A}_0^{-1}\mathbf{G}(\rho, \mathbf{a})\mathbf{s}_t + \mathbf{A}_0^{-1}\mathbf{u}_t = \mathbf{\Pi}\mathbf{s}_t + \mathbf{v}_t. \tag{6}$$

Therefore, for the analysis of *ad hoc* changes in the policy variable knowledge of structural parameters is not necessary, and the analysis can be based on the policy reduced form equation which for y_t is given by

$$y_t = \pi_1 x_t + \pi_2'\mathbf{w}_t + v_{yt}. \tag{7}$$

Suppose that a policy intervention is announced at the end of period T for the periods $T+1, T+2, ..., T+H$. The intervention is such that the "policy on" realized values of the policy variable are different from the "policy off" counterfactual which would have happened in the absence of the intervention. Define, the information set available at time t as $\mathbf{\Omega}_T = \{y_t, x_t, z_t, w_t$ for $t = T, T-1, T-2, ...\}$. The realized policy values are: $\mathbf{\Psi}_{T+h}(x) = \{x_{T+1}, x_{T+2}, ..., x_{T+h}\}$. The counterfactual policy values are: $\mathbf{\Psi}_{T+h}(x^0) = \{x^0_{T+1}, x^0_{T+2}, ..., x^0_{T+h}\}$.

Ex ante policy evaluation is relatively straightforward and can be carried out by comparing the effects of two alternative sets of policy values, say $\Psi_{T+h}(x^0)$ and $\Psi_{T+h}(x^1)$, or Ψ^0_{T+h} and

Ψ^1_{T+h}, for short. Notice that the expected sequence with "policy on" Ψ^1_{T+h} will differ from the realized sequence $\mathbf{\Psi}_{T+h}$, by implementation errors. The expected effects of "policy on" $\Psi_{T+h}(x^1)$ relative to "policy off" $\Psi_{T+h}(x^0)$ is given by

$$d_{T+h} = E(y_{T+h} \mid \mathbf{\Omega}_T, \Psi^1_{T+h}) - E(y_{T+h} \mid \mathbf{\Omega}_T, \Psi^0_{T+h}),\ h = 1, 2, ..., H$$

The evaluation of these expectations critically depends on the type of invariances assumed. These invariances would include whether the announced policy is credible, and whether the parameters would change.

In the context of the above stylized model, the effects of *ad hoc* changes in the policy variable are given by

$$E(y_{T+h} \mid \mathbf{\Omega}_T, \Psi^0_{T+h}) = E\left(\pi_1 \left|\Psi^0_{T+h}\right.\right) x^0_{T+h} + \pi_2' E\left(\mathbf{w}_{T+h} \left|\Psi^0_{T+h}\right.\right), \text{ for } h = 1, 2, ..., H.$$

The policy reduced form equation, (7), is clearly mis-specified if the objective is to estimate the structural parameters. But for the counterfactual analysis, it is the total effect of the policy change which is needed, and this parameter is consistently estimated by the regression of y on x and w.

Under the assumption that w_t, the policy reduced form parameters (π_1 and π_2), and the errors, v_{yt}, are invariant to policy interventions we have the simple result that for $h = 1, 2, ...$ the effect of policy is:

$$d_{T+h} = \pi_1 \left(x^1_{T+h} - x^0_{T+h}\right). \tag{8}$$

It is clear that this result does not require the invariance of the structural parameters, but only that the policy reduced form parameters are invariant to policy intervention.

In cases where $\mathbf{w}_t$ and v_{yt} are invariant to the policy change but the parameters are not (possibly due to expectational effects as in (6)) we have

$$\begin{aligned} d_{T+h} &= E\left(\pi_1 \left|\Psi^1_{T+h}\right.\right) x^1_{T+h} - E\left(\pi_1 \left|\Psi^0_{T+h}\right.\right) x^0_{T+h} \\ &+ \left[E\left(\pi_2 \left|\Psi^1_{T+h}\right.\right) - E\left(\pi_2 \left|\Psi^0_{T+h}\right.\right)\right] w_{T+h} \end{aligned}$$

The parameters are treated as random variables since they may be changed by policy. In practice, the potential effects of policy change on the parameters must also be modelled. In the case of the rational expectations models the dependence of π_1 and π_2 on the policy parameters can be used to compute the expressions $E\left(\pi_1 \left|\Psi^1_{T+h}\right.\right)$, $E\left(\pi_1 \left|\Psi^0_{T+h}\right.\right)$, and $\left[E\left(\pi_2 \left|\Psi^1_{T+h}\right.\right) - E\left(\pi_2 \left|\Psi^0_{T+h}\right.\right)\right]$.

There are a number of advantages in basing the policy analysis directly on the policy reduced form equation (7). Using a full structural model for policy evaluation requires that all parameters are invariant to the policy intervention, but there may be circumstances, where the total effect is more likely to be invariant to the intervention than the marginal effects captured by the structural parameters. A policy reduced form equation of the type discussed above is likely to be more robust to the invariance assumption than a fully structural model. There may also be cases where it is more efficient to estimate the total effect directly, rather than indirectly from the full structural model. Estimating the full system of equations may be more sensitive to specification errors, as compared to the policy reduced form equation.

3.2 Allowing for dynamics

While equation (7) is a static model, the above analysis can be extended to dynamic models, with the difference that the counterfactual has to be computed recursively from the policy date, T, onward. When dynamic effects are included in the RE model, (4), the solution can be written as a general autoregressive distributed lag (ARDL) model in y_t,x_t and $\mathbf{w}_t$, (after solving out the effects of $\mathbf{z}_t$)

$$y_t = \lambda(L)y_{t-1} + \pi_1(L)x_t + \pi_2'(L)\mathbf{w}_t + v_{yt}, \text{ for } t = 1, 2, ..., T, T+1, ..., T+H$$

where

$$\lambda(L) = \lambda_1 L + \lambda_2 L^2 + ... + \lambda_p L^p, \text{ and } \pi_j(L) = \mathbf{a}_{j0} + \mathbf{a}_{j1}L + ... + \mathbf{a}_{jq_j}L^{q_j}, \ j = 1, 2.$$

As before $\Omega_T = \{y_t, x_t, z_t, w_t \text{ for } t = T, T-1, T-2, ...\}$, and two policy counterfactuals are $\Psi^0_{T+h} = \{x^0_{T+1}, x^0_{T+2}, ..., x^0_{T+h}\}$, and $\Psi^1_{T+h} = \{x^1_{T+1}, x^1_{T+2}, ..., x^1_{T+h}\}$. To illustrate how d_{T+h} can be derived in this case we consider the following simple specification

$$y_t = \lambda y_{t-1} + \pi_{10}x_t + \pi_{11}x_{t-1} + \pi_2'\mathbf{w}_t + v_{yt}, \text{ for } t = 1, 2, ..., T, T+1, ..., T+H \tag{9}$$

and note that

$$y_{T+h} = \pi_{10}\sum_{j=0}^{\infty}\lambda^j x_{T+h-j} + \pi_{11}\sum_{j=0}^{\infty}\lambda^j x_{T+h-1-j} + \pi_2'\sum_{j=0}^{\infty}\lambda^j \mathbf{w}_{T+h-j} + \sum_{j=0}^{\infty}\lambda^j v_{y,T+h-j},$$

Evaluating the effect of policy conditional on Ω_T gives

$$\begin{aligned} d_{T+h} = {} & \pi_{10}\sum_{j=0}^{h-1}\lambda^j(x^1_{T+h-j} - x^0_{T+h-j}) + \pi_{11}\sum_{j=1}^{h}\lambda^{j-1}(x^1_{T+h-j} - x^0_{T+h-j}) + \\ & \sum_{j=0}^{h-1}\pi_2'\lambda^j\left[E\left(\mathbf{w}_{T+h-j}\left|\Omega_T, \Psi^1_h\right.\right) - E\left(\mathbf{w}_{T+h-j}\left|\Omega_T, \Psi^0_h\right.\right)\right] + \\ & \sum_{j=0}^{h-1}\lambda^j\left[E\left(v_{y,T+h-j}\left|\Omega_T, \Psi^1_h\right.\right) - E\left(v_{y,T+h-j}\left|\Omega_T, \Psi^0_h\right.\right)\right]. \end{aligned}$$

Since, $\mathbf{w}_t$ is invariant to the policy change we have

$$d_{T+h} = \pi_{10} \sum_{j=0}^{h-1} \lambda^j (x^1_{T+h-j} - x^0_{T+h-j}) + \pi_{11} \sum_{j=1}^{h} \lambda^{j-1} (x^1_{T+h-j} - x^0_{T+h-j}), \tag{10}$$

which is a direct generalization of the static formulation.

The unknown parameters in the policy effects, d_{T+h}, can be computed using the policy reduced form equation. Assuming T is large, the parameters can be estimated either from the sample before the intervention, $t = 1, 2, ...T$ or from the whole sample available, $t = 1, 2, ...T + H$. We also need to consider the possible endogeneity of x_t. Suppose that the policy variable x_t is generated by

$$x_t = b_1(L)x_{t-1} + b_2(L)y_{t-1} + v_{xt}$$
$$b_j(L) = b_{j0} + b_{j1}L + ... + b_{js_j}L^{s_j}.$$

with v_{yt} and v_{xt} being correlated. To correct for the endogeneity, following Pesaran & Shin (1999), we model the contemporaneous correlation between v_{yt} and v_{xt}, by $v_{yt} = \delta v_{xt} + \eta_t$, where by construction v_{xt} and η_t are uncorrelated The parametric correction for the endogenous x_t is equivalent to augmenting the ARDL specification with an adequate number of lagged changes in x_t before estimation of the policy reduced form equation is carried out.

3.3 A test for policy effectiveness

In many cases we will want to make statements about the probability of policy being effective or test the hypothesis that the policy had no effect, we now consider this issue. Returning to the simple static specification, we noted above that the *ex ante* estimate of the effect of policy would be

$$d^{(\text{ex ante})}_{T+h} = \pi_1 \left(x^1_{T+h} - x^0_{T+h} \right). \tag{11}$$

However, *ex post* the realizations of policy variable might not coincide with the planned or intended values of x, and we would have

$$E\left(y_{T+h} \left| \Omega_T, \Psi_{T+h}, w_{T+1}, w_{T+2}, ..., w_{T+h} \right.\right) - E\left(y_{T+h} \left| \Omega_T, \Psi^0_{T+h}, w_{T+1}, w_{T+2}, ..., w_{T+h} \right.\right)$$
$$= \pi_1 \left(x_{T+h} - x^0_{T+h} \right) \tag{12}$$

where Ψ^1_{T+h} and x^1_{T+h}, the expected values of the policy variable given information at time t, may differ from the realisations Ψ_{T+h} and x_{T+h}, because of implementation errors.

We can also calculate the difference between the realized values of the outcome variable in the "policy on" period with the counterfactual for the outcome variable with "policy off":

$$d^{(\text{ex post})}_{T+h} = y_{T+h} - E\left(y_{T+h} \left| \Omega_T, \Psi^0_{T+h}, w_{T+1}, w_{T+2}, ..., w_{T+h} \right.\right)$$
$$= \pi_1 (x_{T+h} - x^0_{T+h}) + v_{y,T+h}. \tag{13}$$

Unlike the *ex ante* measure of the policy effects, the *ex post* measure given above depends on the value of the realized shock, $v_{y,T+h}$, and the statistical analysis of the effectiveness of the policy require relatively large post policy samples so that the influence of the random component can be minimized.

The *ex post* mean effect of the policy is given by

$$\bar{d}_H = \frac{\pi_1}{H}\sum_{h=1}^{H}(x_{T+h} - x^0_{T+h}) + \frac{1}{H}\sum_{h=1}^{H} v_{y,T+h}.$$

One could develop a test of $\bar{d}_H = 0$, using an estimator of $\pi_1, \widehat{\pi}_1$ for T and H sufficiently large. In the case where $H/T \to 0$ as $T \to \infty$, a test of the policy effectiveness hypothesis can be based on

$$\widehat{\bar{d}}_H = \hat{\pi}_1 \left[\frac{1}{H}\sum_{h=1}^{H}(x_{T+h} - x^0_{T+h})\right]$$

where $H^{-1}\sum_{h=1}^{H}(x_{T+h} - x^0_{T+h})$ is a measure of the average size of the policy change. The policy-effectiveness test statistic can now be written as

$$\mathcal{P}_H = \frac{\widehat{\bar{d}}_H}{\hat{\sigma}_{v_y}} \overset{a}{\sim} N(0,1), \tag{14}$$

where $\hat{\sigma}_{v_y}$ is the standard error of the policy reduced form regression.

4 Unconventional monetary policy: an empirical application

We will illustrate the proposed approach to single-unit counterfactual analysis with a consideration of the effect of unconventional monetary policies, UMP, such as quantitative easing, QE. In practice, UMP have tended to be adopted when central banks have hit the zero lower bound for the policy interest rate, but in principle they could be adopted even interest rates are not at the lower bound. The term quantitative easing was used by the Bank of Japan to describe its policies from 2001, Bowman et al. (2011). During the financial crisis, starting in 2007, and particularly after the failure of Lehman Brothers in 2008 many central banks adopted UMP. The central banks differed in the specific measures used and had different theoretical perceptions of what the policy interventions were designed to achieve and the transmission mechanisms involved.[4] Borio and Disyatat (2010) classify such policies as balance sheet policies, as distinct from interest rate policies, and describe the variety of different types of measures adopted by seven central banks during the financial crisis.

In the UK QE involved exchanging one liability of the state - government bonds (gilts) - for another - claims on the central bank. That change in the quantities of the two assets would cause

[4] For instance Giannone et al. (2011), who discuss the euro area, distinguish the Eurosystems actions from the QE adopted by other Central Banks.

a rise in the price of guilts, decline in their yields, but also cause a rise in the prices of substitute assets such as corporate bonds and equities. The Bank of England believed[5] that QE boosts demand by increasing wealth and by reducing companies' cost of finance. It also increases banks liquidity and may prompt more lending. Event studies documented in Joyce et al. (2011) suggest that QE reduced the spread of long over short term government interest rates (the "spread") by 100 basis points from its introduction in March 2009. Thus the counterfactual we consider is the effect of a 100 basis points reduction in the spread, x_t, on output growth, y_t. Notice that this is what we called above an *ad hoc* intervention changing the level of the policy variable, as distinct from an intervention changing the parameters of a policy rule.

The data is taken from the Global VAR dataset, recently extended to 2011Q2.[6] Growth, y_t, is measured by the quarterly change in the logarithm of real GDP. In calculating the spread, the short and long interest rates are expressed as $0.25 \log(1 + R/100)$, where R is the annual percent rate. Figure 1 plots UK output growth and the spread over the full sample period 1979Q2-2011Q2.

The estimate that QE reduced the spread by 100 basis points is not uncontroversial, Meaning and Zhu (2011) estimate a smaller impact of about 25 basis points, but our estimates could be easily scaled downwards to match this alternative estimate. Kapetanios et al (2012), who examine the effects of QE on UK output growth and inflation, also use a reduction in spread of 100 basis points. In their analysis they are particularly concerned about structural change and use three time-varying VAR models that allow for the parameter change in different ways, but do not consider the possible effects of QE on other $\mathbf{z}_t$ type variables. Baumeister and Benati (2010) also use time varying VARs to assess the macroeconomic effects of QE in the US and UK, assuming the effect of QE in the UK was to reduce the spread by 50 basis points. But as our theoretical analysis highlights, the effects of structural breaks due to factors other than the policy change must be distinguished from the structural breaks that could result from the policy intervention. These studies are concerned with past parameter variations and implicitly assume that the policy intervention has no independent effects on parameter values.

Here we re-examine the effects of QE on UK output growth, and for reasons explained in the theoretical part of the paper we shall be using the policy reduced form approach rather than a full structural model. Like Kapetanios et al. (2012) we assume that QE caused a 100 basis points reduction in the spread.[7] We do not rule out that QE might have had an impact on other variables, such as $\mathbf{z}_t$, with indirect effects on output growth. But we recall that such $\mathbf{z}_t$-effects are solved out and are indirectly accommodated in our approach.

[5] For instance see the Financial Times article 4 May 2012 by Charlie Bean, the Bank's Deputy Governor.

[6] Described in Dees et al. (2007), with updates available at www-cfap.jbs.cam.ac.uk/research/gvartoolbox/index.html.G

[7] It is assumed that the reduction in spreads is permanent. But other time profiles for the policy effects of QE on spreads could also be considered.

As for the choice of the conditioning variables, $\mathbf{w}_t$, we use foreign output variables as they are unlikely to have been significantly affected by UK QE, but their inclusion allows for the possible indirect effects of unconventional monetary policies implemented in US and euro area on UK output growth. Figure 2 plots UK and US output growths, Figure 3, UK and euro output growths. Like Kapetanios et al. (2012) we assume that the reduction in the spread is permanent. But other time profiles for the policy effects of QE on spreads could also be considered. Again such modifications can be readily accommodate within our framework.

We first use a bivariate ARDL in output growth (y_t) and the spread between long and short interest rates (x_t). Pesaran and Shin (1999) show that ARDL estimates are robust to endogeneity and robust to the fact that y_t (stationary) and x_t (near unit root) have different degrees of persistence. The bivariate ARDL may be more robust to structural change, than models with a large number of variables and may reduce forecast uncertainty due to estimation error. The ARDL is also preferable to VAR models for counterfactual analysis since it allows efficiency gains by conditioning on contemporaneous policy variables.

A bivariate ARDL model with lag orders automatically selected by AIC (or SBC), gives model M1:

$$y_t = \alpha + \lambda y_{t-1} + \pi_{10} x_t + \pi_{11} x_{t-1} + v_{yt}.$$

We consider two samples, both starting in 1980Q3, one ending estimation in 2008Q4, the last data available before QE, the other ending estimation in 2011Q2.[8] With structural instability there is an issue of whether the variance or the mean shifts. When error variances are falling, as occurred during the period before the financial crisis (the so-called great moderation), it is optimal to place more weights on the most recent observations. Pesaran, Pick and Pranovich (2011).

Both model M1 equations pass tests for serial correlation and heteroskedasticity, fail (at the 5% level) tests for normality and functional form. The equation estimated up to 2008Q4, passes predictive failure and parameter stability tests. In all the specifications, it is the change in spread that seems important. The impact of the policy tends to erode quite rapidly (within less than a year) with the long-run effect of the spread on output growth not significantly different from zero. This is apparent from Figure 4, where the model M1 predictions for growth using realised and counterfactual spread converge quite quickly. A similar picture also emerges from Charts 2 and 3 of Kapetanios et al. (2012) where the 100 basis point counterfactual returns to the model prediction within about a year , although they do not highlight this aspect of their results. Notice that as is clear from a comparison of (11) and (13), our distinction between *ex ante* and *ex post* evaluation is not based on the sample used for estimation, but on whether the counter-factual

[8] The estimates tend to be quite sensitive to sample, giving larger effects if shorter sample periods including the crisis are considered.

predictions are compared with predicted (ex ante) or realized (ex post) values of the policy and outcome variables. On this basis Kapetanios et al. conduct an *ex ante* evaluation exercise.[9] The issues of parameter and model uncertainty, and the choice of the estimation sample (whether to include post intervention observations in the sample) will be present irrespective of whether the policy evaluation exercise is carried out *ex post* or *ex ante*.

Model M1: ARDL in UK output growth (y_t) and spread (x_t)

	1980Q3-2008Q4	1980Q3-2011Q2
y_{t-1}	0.3986	0.4773
	(4.75)	(6.69)
x_t	-0.9139	-1.0073
	(-2.83)	(-3.08)
x_{t-1}	1.1571	1.1269
	(3.54)	(3.44)
$\overline{R}^2$	0.290	0.363
LM test Res. Serial corr.	0.747	0.651
$\widehat{\sigma}_{v_y}$	0.0053	0.0053

Bracketed figures are t-ratios

We can improve the efficiency of estimation by conditioning on foreign output growth variables. Over the full sample the correlation between UK growth and US growth is 0.47, in the post 1999 sample it is 0.76. For euro growth, the correlations are 0.36 and 0.73. Thus we augment the ARDL with current euro and US growth rates. In terms of the earlier notation $\mathbf{w}_t = (y_t^{US}, y_t^{Euro})'$. The estimated equation is model M2:

$$y_t = \alpha + \lambda y_{t-1} + \pi_{10} x_t + \pi_{11} x_{t-1} + \gamma_{us} y_t^{US} + \gamma_{euro} y_t^{Euro} + v_{yt}.$$

The fit is rather better than the bivariate ARDL and again the equations pass tests for serial correlation and heteroskedasticity, fail (at the 5% level) tests for normality and functional form. The equation estimated up to 2008Q4, passes predictive failure and parameter stability tests. We cannot reject $\pi_{10} + \pi_{11} = 0$ on the full sample. This restriction is imposed in the simulations reported below. Although the long-run effect is very close to zero in both models, the impact effect is rather smaller when one allows for foreign growth, 0.77-0.82 in model M2, depending on the sample used; which is less than the 0.91-1.0 effect in model M1 which does not allow for the foreign output variables. This difference is understandable considering that the UK economy would have benefited from growth in the US and euro area even if the Bank of England had not adopted QE.

[9] Kapetanios et al. (2012,p22) comment "..these are estimated by comparing the no policy scenario with the policy scenario which is a forecast conditional on the actual path for Bank Rate over the forecast horizon. The effects would be larger if the counterfactual were defined as the no policy scenario relative to the actual data, as the model underpredicts output and inflation over the period."

Model M2: ARDL in UK growth (y) and spread (x) augmented with US and Euro area growth rates

	1980Q3-2008Q4	1980Q3-2011Q2
y_{t-1}	0.3217	0.3822
	(3.71)	(5.12)
x_t	-0.7693	-0.8197
	(-2.47)	(-2.63)
x_{t-1}	1.1116	1.0092
	(3.58)	(3.29)
y_t^{US}	0.1349	0.1465
	(1.83)	(2.00)
y_t^{Euro}	0.1546	0.1636
	(2.58)	(3.047)
$\overline{R}^2$	0.362	0.446
LM test Res. Serial Corr.	0.721	0.332
$\widehat{\sigma}_{v_y}$	0.0050	0.0050

Bracketed figures are t-ratios

Figures 4-7 examine the effect of a 100 basis points increase in spreads on growth, imposing the restriction that the long-run effect is zero. We have two models (M1: bivariate ARDL and M2: ARDL conditional on US and euro area output growth) and two estimation samples: ending in 2008Q4 or 2011Q2. The choice of the estimation sample does not make much of a difference. Figure 4 compares the predictions for output growth using realized and counterfactual spreads based on model M1, the bivariate ARDL. Figure 5 compares the predictions for output growth using realized and counterfactual spreads based on model M2, the ARDL model that includes US and euro output growth. Figure 6 compares the counterfactual forecasts using the two models, M1 and M2, with and without foreign output growth. Figure 7 also includes the actual UK output growth rate. Conditional on US and euro growth rates the impact effect of QE is a little smaller. Overall, a 100 basis points increase in spreads reduces growth by somewhat less than 1% on impact, but while the effect on the spread is assumed permanent, the effect on growth is temporary and gets reversed quite quickly.

We also considered the application of the policy ineffectiveness test statistic given by (14) to the current problem, but due to the rapid reversal of the policy effects we found the test not to be statistically significant, suggesting that the average effect of the policy computed even over a 2-3 years time horizon will be zero. This is compatible with the policy having a statistically significant impact effect without the average policy effect being statistically significant if computed over a longer time period.

5 Conclusion

For evaluation of treatments or policy interventions structural identification is not required. What is needed is identification of the parameters of the policy equation where the effects of the co-variates that are influenced by the policy are solved out. Strong parameter and error invariance assumptions are also needed. We distinguish between *ex ante* evaluation, which uses predicted policy and outcomes, and *ex post evaluation,* which uses realizations of policy and outcomes, and highlight the importance of conditioning on the variables that explain the outcomes but are invariant to policy interventions. We also consider the differences between the micro treatment literature and the time series policy evaluation exercises. Although we do not discuss it, the approach adopted here naturally extends to panel data where one has time series for a number of units some of whom are subject to the policy intervention, with all the units observed both before and after the policy intervention.

We illustrate some of the issues that arise in counterfactual policy evaluation with an empirical application to Quantitative Easing which was introduced in the UK in March 2009. The UK QE involved exchanging one liability of the state - government bonds (gilts) - for another - claims on the central bank. That change in the quantities of the two assets would cause a rise in the price of guilts, decline in their yields, but also cause a rise in the prices of substitute assets such as corporate bonds and equities. We estimate two models explaining UK output growth over two sample periods, one ending in 2008Q4 (before QE), and the other ending in 2011Q2. Model M1 is a bivariate dynamic equation between growth and the spread of long interest rates over short interest rates, model M2 adds US and euro area output growth to model M1.

Although there is some dispute about the size of the effect of QE on interest rate spreads, we follow the Bank of England in assuming that QE caused a permanent 100 basis points reduction in the spread of long interest rates over short interest rates after March 2009. Both models and both sample periods indicate that it is the change in spread that matters: there is a significant impact effect of QE but this effect tends to disappear quite quickly, certainly within a year. In all cases the long-run effect is not significantly different from zero. Although the long-run effect of the change in the spread on output growth is not emphasized by Kapetanios et al. (2012), the estimates they provide for the time profiles of the effects of the QE tell very much the same story, namely the beneficial effects of QE are rather short-lived. The size of the impact effect of the 100 basis points reduction in the spread on the output growth rate is between three quarters of a percentage point and one percentage point, with the lower estimate coming from the model that includes foreign output growth. Thus while the estimated change in the spread caused by QE, if sustained, would have permanent distributional effects between savers and borrowers, the change in spread only generated a temporary stimulus to growth. This raises a number of policy issues. These include the costs and benefits of a permanent change in spread relative to a temporary stimulus to growth, the optimal timing for a temporary stimulus and the effects of the eventual reversal of QE.

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UK Output Growth (blue) and Spread (red)

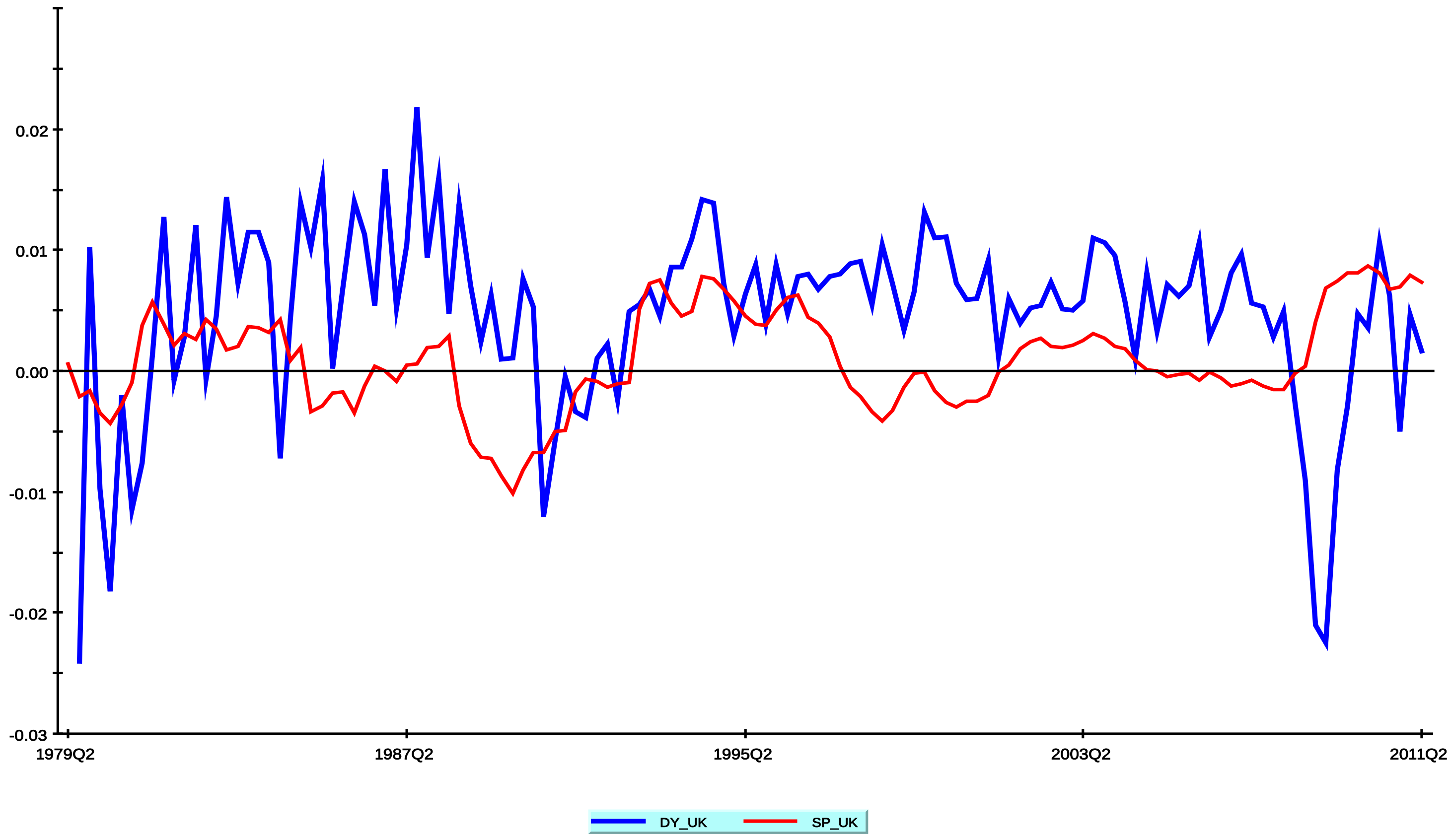

Figure 1

UK (blue) and US (red) Output Growths
Corr(UK,US) = 0.47 (full sample), 0.76 (post 1999)

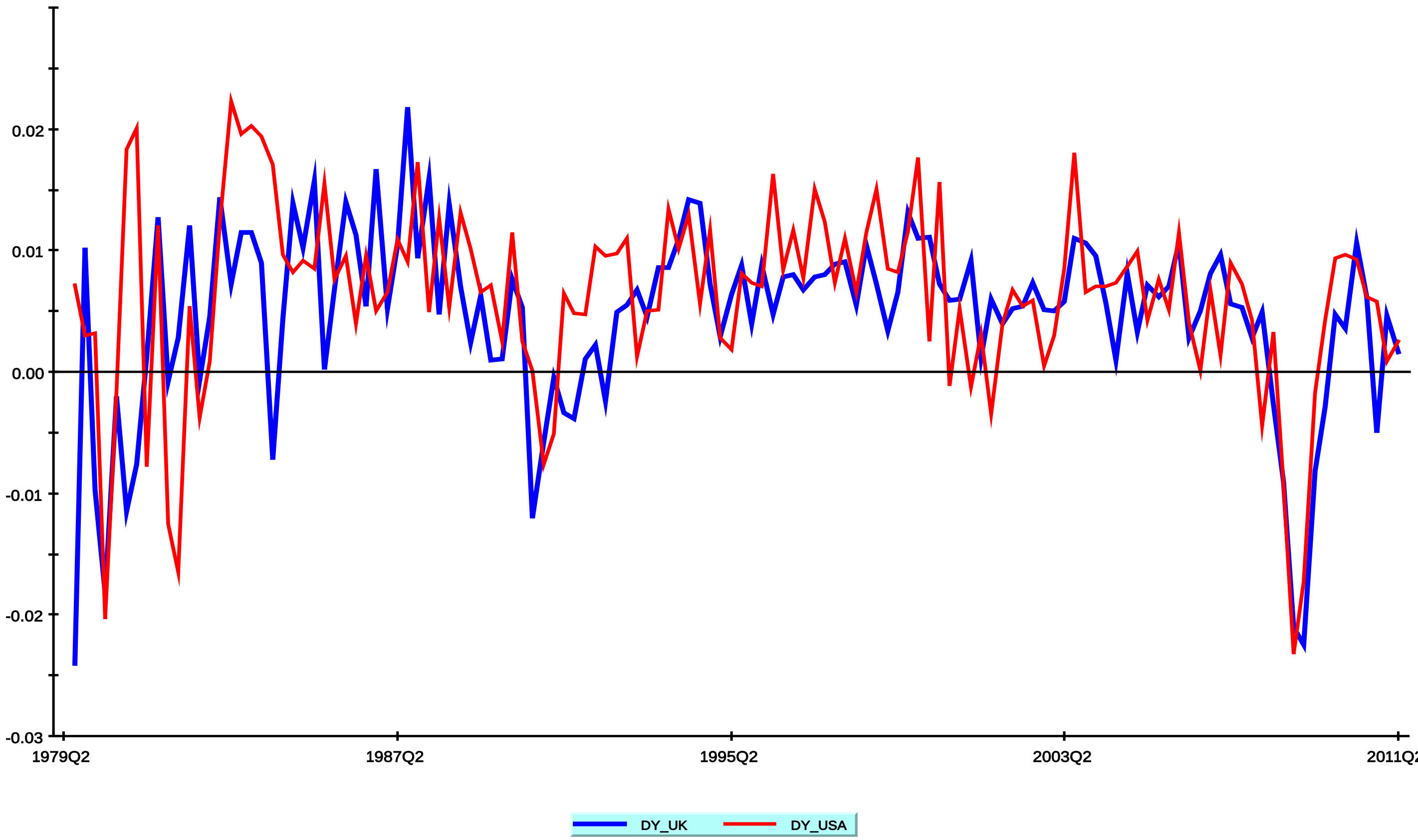

Figure 2

UK (blue) and Euro (red) Output Growths
Corr(UK,Euro)= 0.36 (full sample), 0.73 (post 1999)

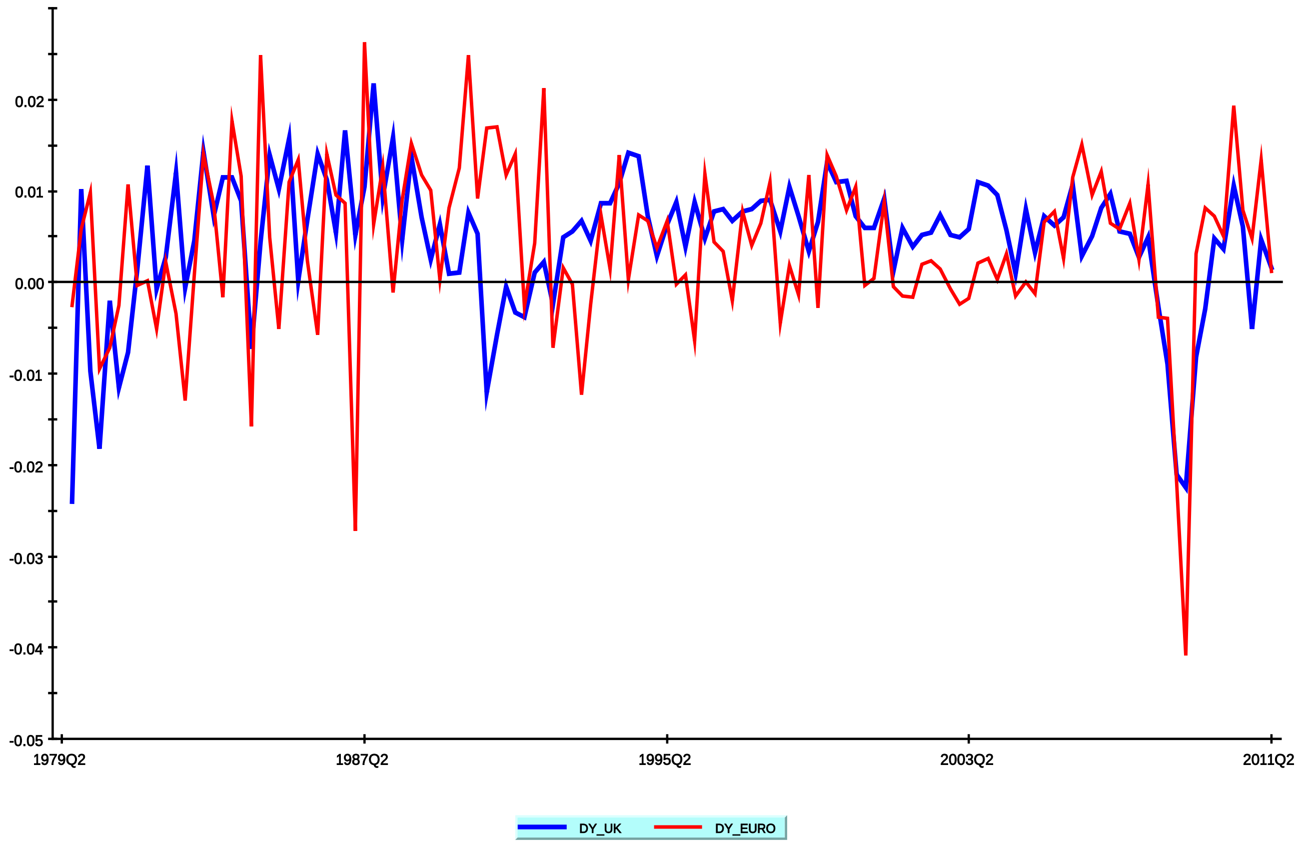

Figure 3

UK Output Growth Forecasts using Realized (blue) and Counterfactual (red) Spreads – Model 1 without US and Euro Output Growths

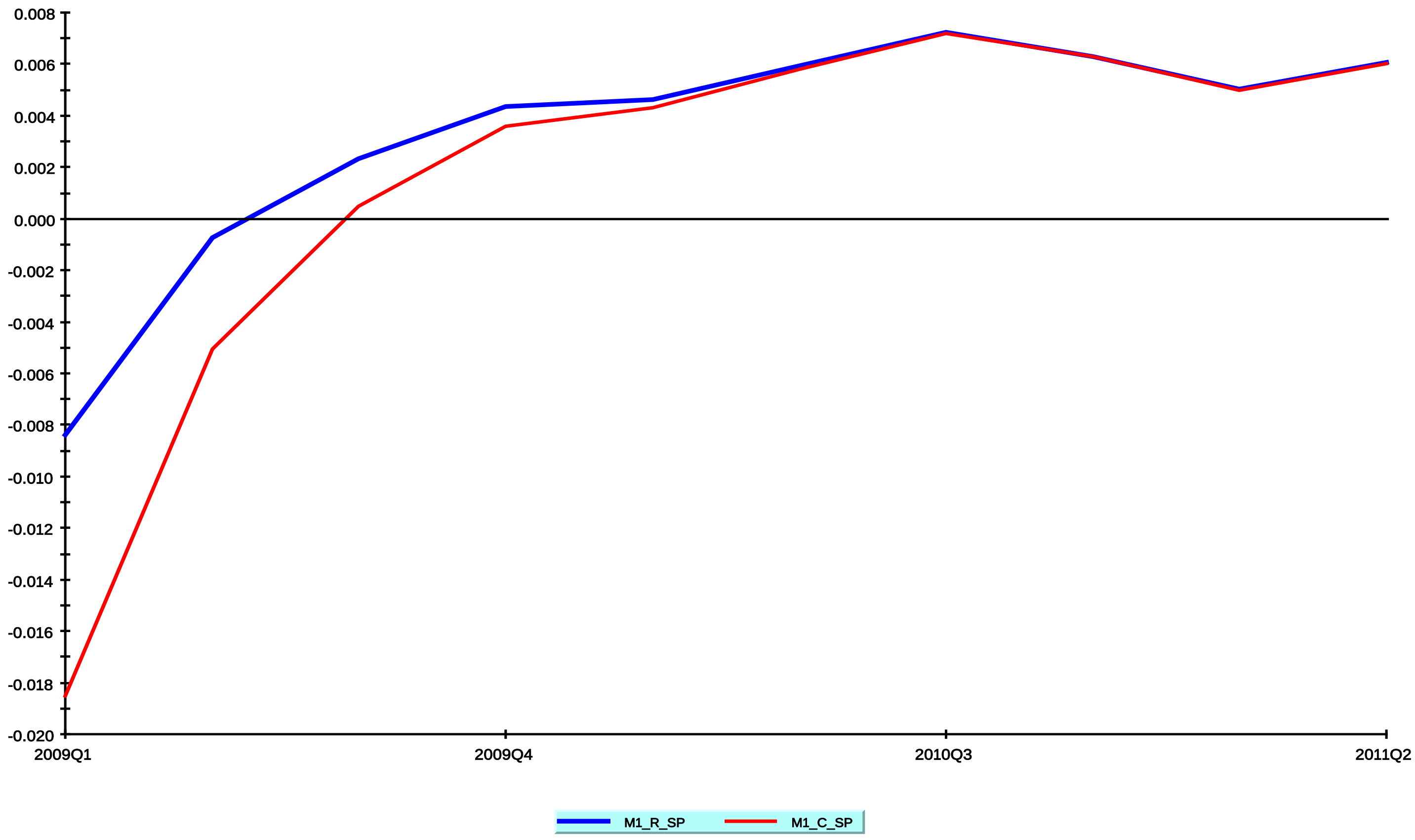

Figure 4

UK Output Growth Forecasts using Realized (blue) and Counterfactual (red) Spreads – Model 2 with US and Euro Output Growths

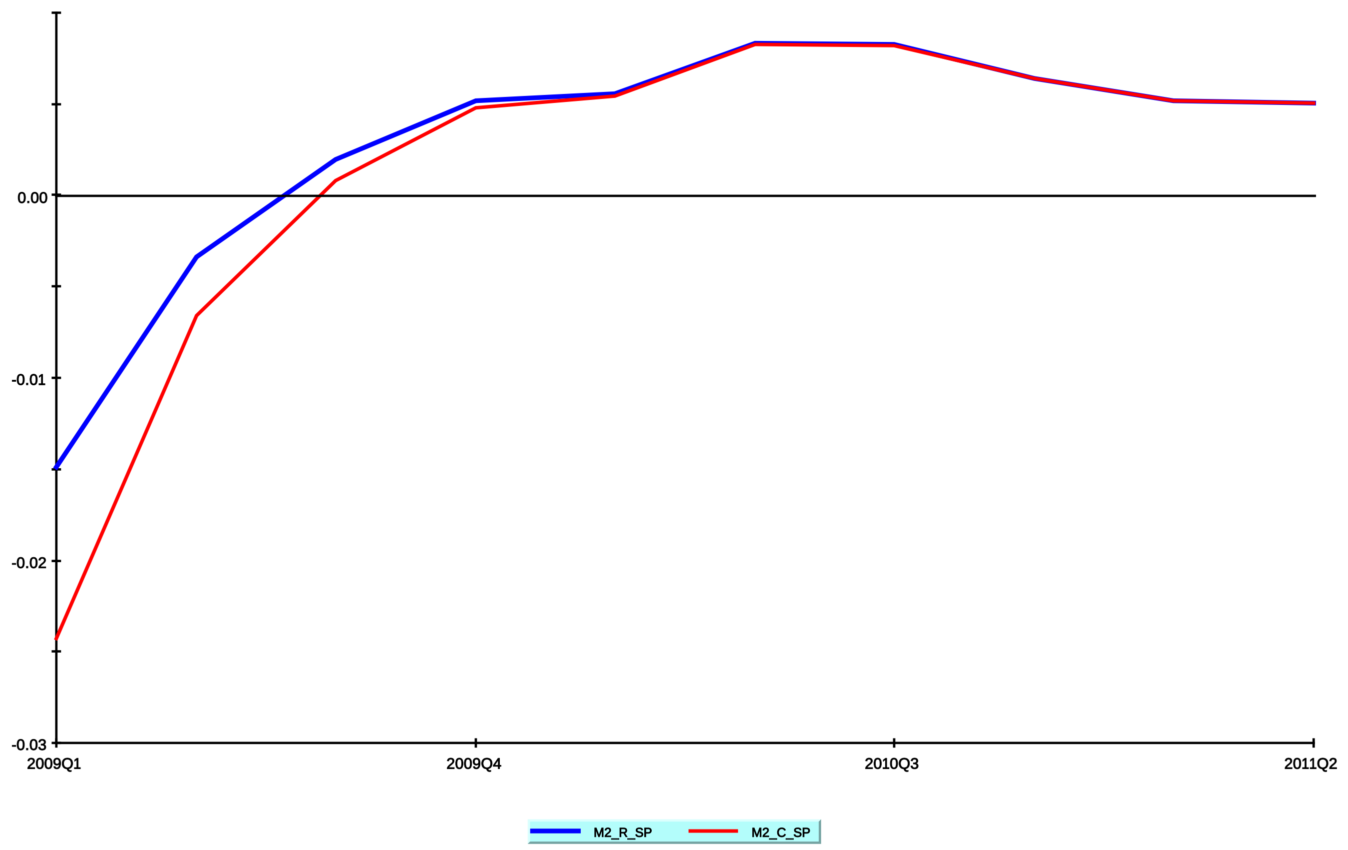

Figure 5

Counterfactual Output Growths based on Models without (blue) and with (red) US and Euro Output Growths

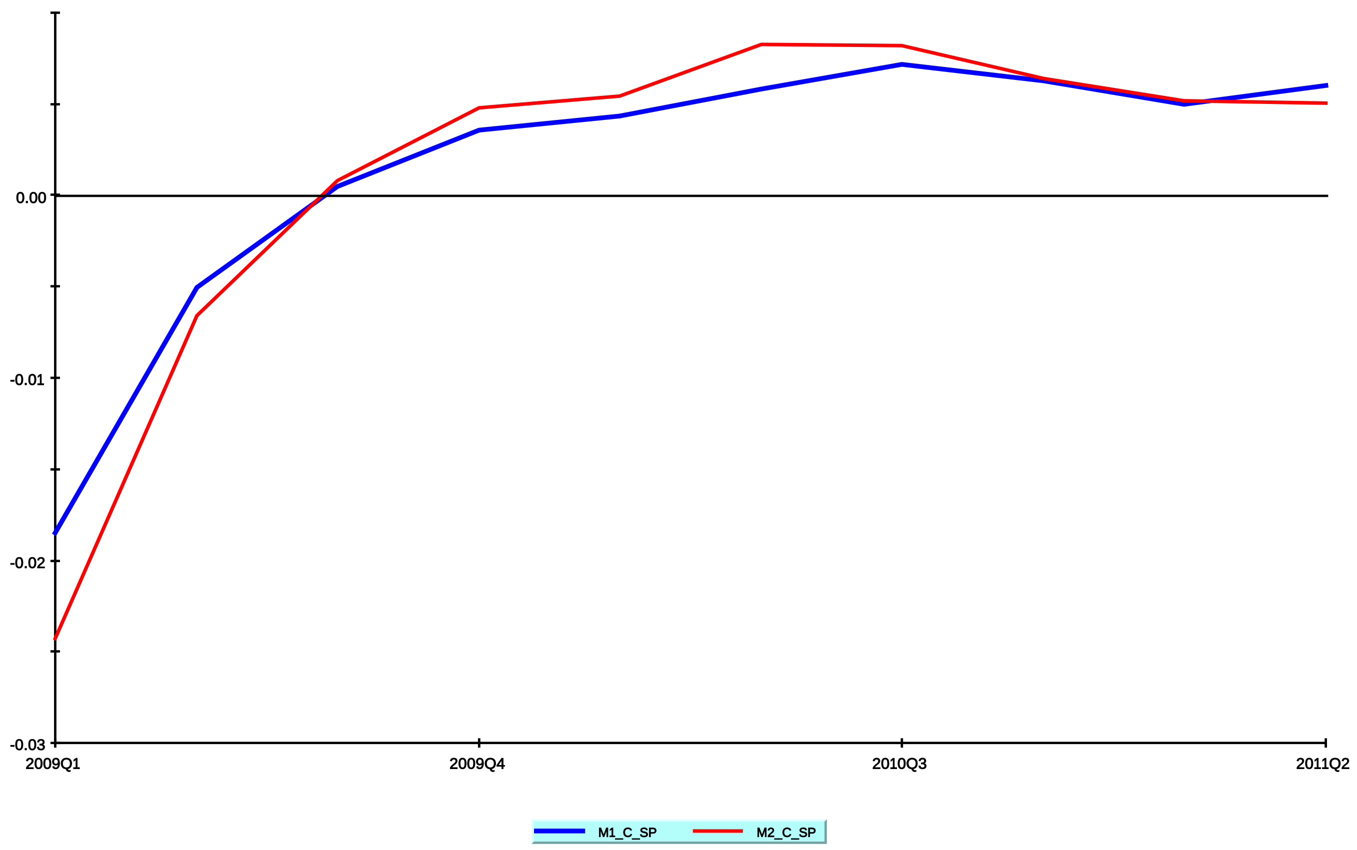

Figure 6

Realized (Blue) and Counterfactual UK Output Growths (Models M1 and M2)

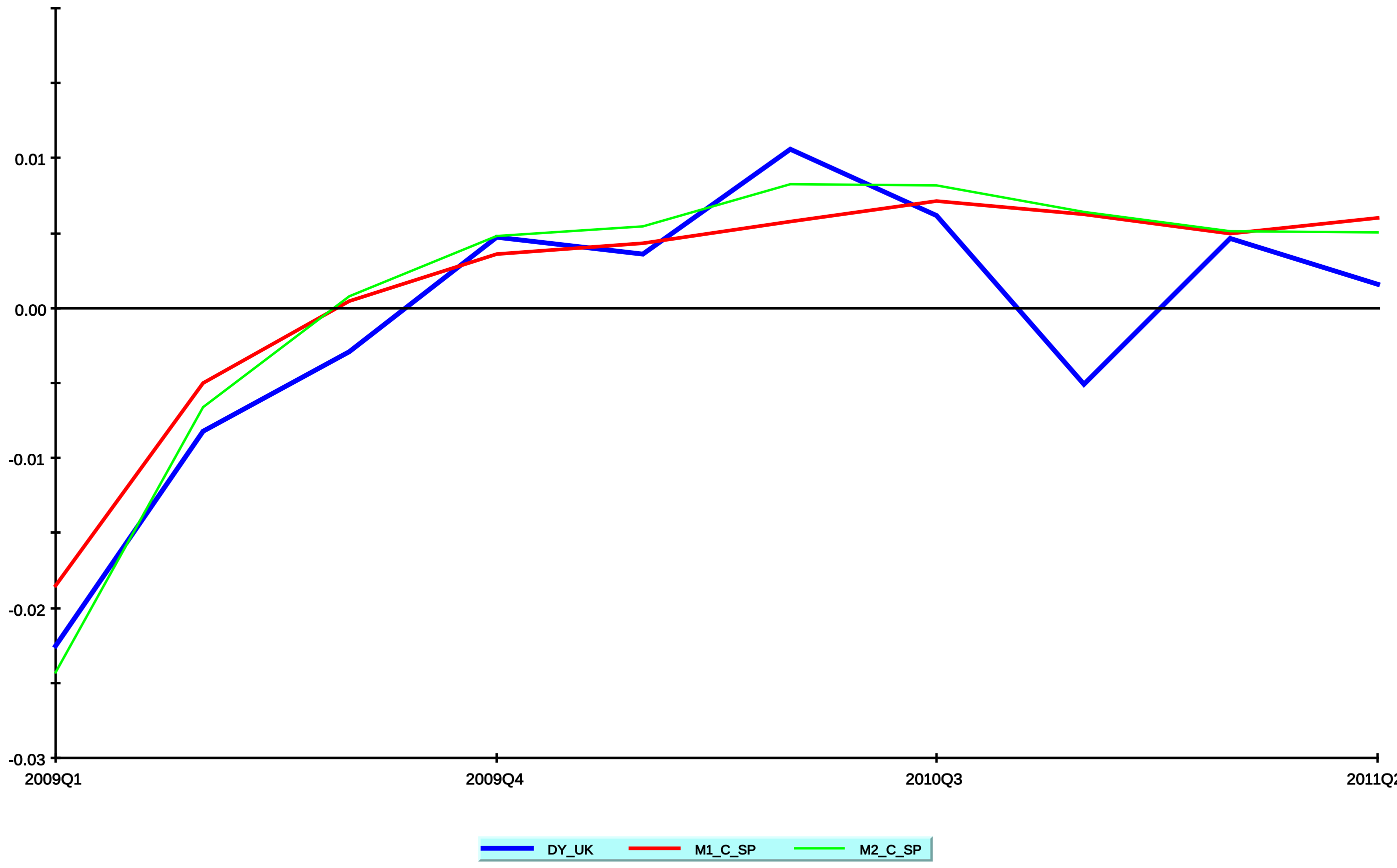

Figure 7