VAR Analysis and the Great Moderation*

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Abstract

Most analyses of the U.S. Great Moderation have been based on structural VAR methods, and have consistently pointed towards good luck as the main explanation for the greater macroeconomic stability of recent years. Based on an estimated New-Keynesian model in which the *only* source of change is the move from passive to active monetary policy, we show that VARs may misinterpret good policy for good luck.

First, the policy shift is *sufficient* to generate decreases in the theoretical innovation variances for all series, and decreases in the variances of inflation and the output gap, without any need of sunspot shocks. With sunspots, the estimated model exhibits decreases in both variances and innovation variances for all series. Second, policy counterfactuals based on the theoretical structural VAR representations of the model under the two regimes fail to capture the truth, whereas impulse-response functions to a monetary policy shock exhibit little change across regimes.

Since these results are in line with those found in the structural VAR-based literature on the Great Moderation, our analysis suggests that existing VAR evidence is compatible with the 'good policy' explanation of the Great Moderation.

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1 Introduction

Post-WWII U.S. macroeconomic history is usually divided into two distinct subperiods. The former period, which extends up to the end of the Volcker disinflation, is characterised by a significant extent of macroeconomic turbulence, with highly volatile inflation and output growth. The latter period, from the end of the Volcker disinflation up to the present day, is marked in contrast by significantly smaller volatilities for both inflation and output growth. These dramatic changes in the reduced-form properties of the U.S. economy over the last several decades characterise a phenomenon known as the 'Great Moderation'.[1]

A vast empirical literature has investigated the source(s) of the Great Moderation in an attempt to disentangle the relative contributions of two main explanations: good policy and good luck. Based on (time-varying or Markov-switching) structural VAR methods, the good luck hypothesis has been advocated by a number of authors including Stock and Watson (2002), Primiceri (2005), Sims and Zha (2006), and Gambetti, Pappa, and Canova (2006) (the disaggregated analysis of Mojon (2007), based on a Markov-switching structural VAR, finds however an important role for the unsystematic component of monetary policy in fostering the Great Moderation). Based on estimated sticky-price DSGE models of the U.S. economy, both Lubik and Schorfheide (2004) and Boivin and Giannoni (2006) find, in contrast, support for the good policy explanation originally advocated by Clarida, Gali, and Gertler (2000), according to which a shift in the systematic component of monetary policy has been the driving force behind the recent, greater macroeconomic stability.

This paper tries to reconcile the two conflicting sets of results by asking whether *methodological* differences between the two approaches might account for the differences in their outcomes. In order to investigate the ability of structural VAR methods to correctly identify the sources of the Great Moderation, we use as data-generation process a New Keynesian model in which—in line with Clarida, Gali, and Gertler (2000) and Lubik and Schorfheide (2004)—the *only* sources of change are the move from passive to active monetary policy,[2] and the presence of sunspots under indeterminacy. We estimate the model *via* Bayesian methods, and we explore the theoretical properties of the estimated structure.

1.1 Main results

Our main results may be summarised as follows.

- The shift in the systematic component of monetary policy associated with the

[1]See in particular Kim and Nelson (1999) and McConnell and Perez-Quiros (2000).

[2]As we abstract from the role of fiscal policy, the relationship between the monetary policy stance and equilibrium (in)determinacy in a simple New-Keynesian model is one-to-one, with a passive (active) rule associated with an indeterminate (determinate) equilibrium. As shown by Leeper (1991), in more complex settings this is not the case.

move from indeterminacy to determinacy is *sufficient* to generate, *in population*, (i) decreases in the innovation variances for all series, and (ii) decreases in the variances of inflation and the output gap, as a simple implication of the Lucas (1976) critique, and without any need of sunspot shocks. With sunspot shocks, the estimated model exhibits decreases in both variances and innovation variances in population when moving from indeterminacy to determinacy, thus replicating the key features of the Great Moderation.

- Policy counterfactuals based on the theoretical structural VAR representations of the model under the two regimes fail to capture the truth. In particular, substituting the VAR's structural monetary rule corresponding to the indeterminacy regime into the VAR for the determinacy regime causes a volatility *decrease*—rather than an increase—for two series out of three.

- Impulse-response functions to a monetary policy shock exhibit little change across regimes.

Overall, our results suggests that existing VAR evidence is, in principle, uninformative on the issue of the role played by monetary policy in the Great Moderation, and is compatible with the notion that policy played a crucial role in fostering the greater macroeconomic stability of recent years.

1.2 Explaining the results

We identify two key dimensions along which VAR analysis turns out to be misleading.

First, in general, changes in the coefficients of the monetary policy rule of the DSGE model exert their impact on *both* the coefficients of the VAR representation of the model, *and* the elements of the VAR's covariance matrix of reduced-form innovations. Although this is a well-known implication of the Lucas (1976) critique, this point has generally been overlooked in the structural VAR-based empirical literature on the Great Moderation, which has routinely interpreted changes in the volatilities of the reduced-form innovations, accompanied by weak evidence of changes in the VAR's coefficients, as evidence against good policy, and in favor of good luck. As this paper shows, however, the dominant impact of a change in the systematic component of monetary policy may well turn out to be the one on the elements of the VAR's covariance matrix, with a comparatively milder effect on the VAR's coefficients. As a corollary, this logically implies that this kind of evidence does not allow, in principle, to discriminate among the good policy and good luck explanations, simply because, within our data generation process, they are essentially observationally equivalent.

Second, changes in the interest rate equation (i.e, the monetary policy rule) of a structural VAR bear no clear-cut relationship with changes in the parameters of the monetary policy rule in the underlying DSGE model. To put it differently, there appears to be a fundamental disconnect between what *is* structural within a DSGE model, and what is *defined* as structural based on the structural VAR representation implied by the very same DSGE model. Earlier contributions, on the other hand, have performed counterfactual simulations in structural VARs under the implicit *presumption* that switching the estimated coefficients of the interest rate equations in the structural VAR provides a reasonable approximation to the *authentic* policy counterfactual, i.e. the one you obtain by switching the parameters of the monetary policy rule in the underlying DSGE model. Again, as the present work shows, such a presumption is, in general, unjustified.

Finally, the present work contribues to the literature along another dimension, by identifying a crucial, and previously unnoticed difference between the determinacy and indeterminacy regimes for New Keynesian models. In particular, under indeterminacy the equivalent minimal state-space representation of the DSGE model possesses an additional state variable compared with the determinacy regime.

The paper is organized as follows. Section 2 describes the standard New Keynesian model we use in the paper, and discusses details of both the Bayesian estimation procedure and the specific experiment we construct. Section 3 discusses key theoretical properties of the estimated data-generation process, focusing on the difference between the equivalent minimal state-space representations of the model under the two regimes, which imply that the model possesses a VAR representation under determinacy, and a VARMA one under inderminacy. Section 4 shows hos the estimated structure replicates key aspects of the Great Moderation in population. Section 5 shows how neither structural VAR-based policy counterfactuals, nor impulse-response functions to a monetary policy shock, point towards the authentic cause of changes in the data-generation process. Section 6 concludes.

2 Assessing VAR Studies of the Great Moderation

In order to assess the ability of structural VAR methods to correctly identify the causes of the Great Moderation, we consider the following experiment:

Suppose that the Great Moderation in the United States has been exclusively *due to improved monetary policy, with a passive monetary policy regime in place before October 1979, and an active regime in place thereafter. Would structural VAR techniques be capable of uncovering the authentic causes of the changes in the data-generation process?*

As we will see, the answer is 'No', with structural VAR methods clearly pointing towards 'good luck'—i.e., an exogenous reduction in the variance of the structural shocks—as the true underlying cause of the changes in the DGP, in spite of the fact that, by construction, *everything* is here driven by improved monetary policy.

2.1 The model

The model we use in what follows is given by

$$R_t = \rho R_{t-1} + (1-\rho)[\phi_\pi \pi_t + \phi_y y_t] + \epsilon_{R,t} \tag{1}$$

$$\pi_t = \frac{\beta}{1+\alpha\beta}\pi_{t+1|t} + \frac{\alpha}{1+\alpha\beta}\pi_{t-1} + \kappa y_t + \epsilon_{\pi,t} \tag{2}$$

$$y_t = \gamma y_{t+1|t} + (1-\gamma) y_{t-1} - \sigma^{-1}(R_t - \pi_{t+1|t}) + \epsilon_{y,t} \tag{3}$$

where R_t, π_t and y_t are the nominal interest rate, inflation, and the output gap, respectively; α and γ are price setters' extent of indexation to past inflation[3] and the forward-looking component in the intertemporal IS curve, respectively; κ is the slope of the Philips curve; σ is the elasticity of intertemporal substitution in consumption; ρ, ϕ_π, and ϕ_y are the smoothing coefficient and the long-run coefficients on inflation and the output gap in the monetary policy rule, respectively; and $\epsilon_{\pi,t}$, $\epsilon_{y,t}$, and $\epsilon_{R,t}$ are three structural disturbances following the AR(1) processes $\epsilon_{x,t}$=$\rho_x \epsilon_{x,t-1}$+$\tilde{\epsilon}_{x,t}$, for x=π, y, R, with $\tilde{\epsilon}_{x,t} \sim N(0,\, \sigma_x^2)$.

2.1.1 Model solution under determinacy and indeterminacy

By defining the state vector as $\xi_t \equiv [R_t,\, \pi_t,\, y_t,\, \pi_{t+1|t},\, y_{t+1|t},\, \epsilon_{R,t},\, \epsilon_{\pi,t},\, \epsilon_{y,t}]'$, the vector collecting the structural shocks as $\epsilon_t \equiv [\tilde{\epsilon}_{R,t},\, \tilde{\epsilon}_{\pi,t},\, \tilde{\epsilon}_{y,t}]'$, and the vector of forecast errors as $\eta_t \equiv [\eta_t^\pi,\, \eta_t^y]'$—where $\eta_t^\pi \equiv \pi_t$-$\pi_{t|t-1}$ and $\eta_t^y \equiv y_t$-$y_{t|t-1}$, the model can then be put into the 'Sims canonical form'[4]

$$\Gamma_0 \xi_t = \Gamma_1 \xi_{t-1} + \Psi \epsilon_t + \Pi \eta_t \tag{4}$$

where Γ_0, Γ_1, Ψ and Π are matrices conformable to ξ_t, ϵ_t and η_t.

In order to solve the model under both determinacy and indeterminacy, following Lubik and Schorfheide (2003) we exploit the QZ decomposition of the matrix pencil (Γ_0-$\lambda\Gamma_1$). Specifically, given a pencil (Γ_0-$\lambda\Gamma_1$), there exist matrices Q, Z, Λ, and Ω such that QQ'=$Q'Q$=ZZ'=$Z'Z$=I_n, Λ and Ω are upper triangular, Λ=$Q\Gamma_0 Z$, and Ω=$Q\Gamma_1 Z$. By defining w_t=$Q'\xi_t$, and by premultiplying (4) by Q, we have:

$$\left[\begin{array}{c|c} \Lambda_{11} & \Lambda_{12} \\ \hline 0 & \Lambda_{22} \end{array}\right] \left[\begin{array}{c} w_{1,t} \\ w_{2,t} \end{array}\right] = \left[\begin{array}{c|c} \Omega_{11} & \Omega_{12} \\ \hline 0 & \Omega_{22} \end{array}\right] \left[\begin{array}{c} w_{1,t-1} \\ w_{2,t-1} \end{array}\right] + \left[\begin{array}{c} Q_{1\cdot} \\ \hline Q_{2\cdot} \end{array}\right] (\Psi\epsilon_t + \Pi\eta_t) \tag{5}$$

where the vector of generalised eigenvalues, λ (equal to the ratio between the diagonal elements of Ω and Λ) has been partitioned as λ=$[\lambda_1',\, \lambda_2']'$, with λ_2 collecting all the explosive eigenvalues, and Ω, Λ, and Q have been partitioned accordingly. In

[3] See e.g. Smets and Wouters (2003) and Christiano, Eichenbaum, and Evans (2005). The specific formulation we use herein is Smets and Wouters'.

[4] See Sims (2002).

particular, $Q_{j\cdot}$ collects the blocks of rows corresponding to the stable (j=1) and, respectively, unstable (j=2) eigenvalues. The explosive block of (5) can then be rewritten as

$$w_{2,t} = \Lambda_{22}^{-1}\Omega_{22}w_{2,t-1} + \Lambda_{22}^{-1}\left(\Psi_x^*\epsilon_t + \Pi_x^*\eta_t\right) \tag{6}$$

where Ψ_x^*=$Q_{2\cdot}\Psi$, and Π_x^*=$Q_{2\cdot}\Pi$. Given that λ_2 is purely explosive, obtaining a stable solution to (4) requires $w_{2,t}$ to be equal to 0 for any $t \geq$0. This can be accomplished by setting $w_{2,0}$=0, and by selecting, for each $t >$0, the forecast error vector η_t in such a way that $\Psi_x^*\epsilon_t + \Pi_x^*\eta_t$=0.

Under determinacy, the dimension of η_t is exactly equal to the number of unstable eigenvalues, and η_t is therefore uniquely determined. Under indeterminacy, on the other hand, the number of unstable eigenvalues falls short of the number of forecast errors, and the forecast error vector η_t is therefore not uniquely determined, which is at the root of the possibility of sunspot fluctuations. Lubik and Schorfheide (2003), however, prove the following. By defining UDV'=Π_x^* as the singular value decomposition of Π_x^*, and by assuming that for each ϵ_t there always exists an η_t such that $\Psi_x^*\epsilon_t + \Pi_x^*\eta_t$=0 is satisfied, the general solution for η_t is given by

$$\eta_t = \left[-V_{\cdot 1}D_{11}^{-1}U_{\cdot 1}'\Psi_x^* + V_{\cdot 2}M_1\right]\epsilon_t + V_{\cdot 2}M_2 s_t^* \tag{7}$$

where D_{11} is the upper-left diagonal block of D, containing the square roots of the non-zero singular values of Π_x^* in decreasing order; s_t^* is a vector of sunspot shocks; and M_1 and M_2 are matrices whose entries are not determined by the solution procedure, and which basically 'index' (or parameterise) the model's solution under indeterminacy. Concerning M_1 and M_2 we follow Lubik and Schorfheide (2004), first, by setting $M_2 s_t^*$=s_t, where s_t can therefore be interpreted as a vector of 'reduced-form' sunspot shocks. Second, we choose the matrix M_1 in such a way as to preserve continuity of the impact matrices of the impulse-responses of the model at the boundary between the determinacy and the indeterminacy region. Specifically, let θ be the parameters' vector, and let Θ_I and Θ_D be the sets of all the θ's corresponding to the indeterminacy and, respectively, to the determinacy regions. For every $\theta \in \Theta_I$ we identify a corresponding vector $\tilde{\theta} \in \Theta_D$ laying just on the boundary between the two regions.[5] By definition, the two impact matrices for the impulse-responses of the model conditional on θ and $\tilde{\theta}$ are given by

$$\begin{aligned} \frac{\partial \xi_t(\theta, M_1)}{\partial \epsilon_t} &= \Psi^*(\theta)\text{-}\Pi^*(\theta)V_{\cdot 1}(\theta)D_{11}^{-1}(\theta)U_{\cdot 1}'(\theta)\Psi_x^*(\theta)\text{+}\Pi^*(\theta)V_{\cdot 2}(\theta)M_1 \equiv && (8) \\ &\equiv B_1(\theta) + B_2(\theta)M_1 && (9) \end{aligned}$$

[5]Specifically, for any $[\phi_\pi,\ \phi_y]'$ such that $\theta \in \Theta_I$, we choose the vector $[\tilde{\phi}_\pi,\ \tilde{\phi}_y]'$, such that the resulting $\tilde{\theta} \in \Theta_D$ lies just on the boundary between the two regions, by minimising the criterion $\tilde{C}$=$[(\phi_\pi\text{-}\tilde{\phi}_\pi)^2\text{+}(\phi_y\text{-}\tilde{\phi}_y)^2]^{1/2}$. It is important to stress that, in general, there is no clear-cut criterion for choosing a specific vector on the boundary. Minimisation of $\tilde{C}$ is based on the intuitive notion of taking, as the 'benchmark' $\tilde{\theta}$, the one that is closest in vector 2-norm to θ.

and, respectively,

$$\frac{\partial \xi_t(\tilde{\theta})}{\partial \epsilon_t} = \Psi^*(\tilde{\theta}) - \Pi^*(\tilde{\theta}) V_{\cdot 1}(\tilde{\theta}) D_{11}^{-1}(\tilde{\theta}) U_{\cdot 1}'(\tilde{\theta}) \Psi_x^*(\tilde{\theta}) \equiv B_1(\tilde{\theta}) \tag{10}$$

where $\Psi^*(\cdot) \equiv Q\Psi(\cdot)$, and $\Pi^*(\cdot) \equiv Q\Pi(\cdot)$. We minimise the difference between the two impact matrices, $B_1(\tilde{\theta})$-$[B_1(\theta)+B_2(\theta)M_1]$=$[B_1(\tilde{\theta})$-$B_1(\theta)]$-$B_2(\theta)M_1$ by means of a least-squares regression of $[B_1(\tilde{\theta})$-$B_1(\theta)]$ on $B_2(\theta)$, thus obtaining $\widetilde{M}_1$=$[B_2(\theta)'B_2(\theta)]^{-1}\times$ $B_2(\theta)'[B_1(\tilde{\theta})$-$B_1(\theta)]$.

The solution to (1)-(3) is now completely characterised. The forecast error η_t can be substituted into the law of motion for $w_{1,t}$,

$$w_{1,t} = \Lambda_{11}^{-1}\Omega_{11}w_{1,t-1} + \Lambda_{11}^{-1}Q_{1\cdot}(\Psi\epsilon_t + \Pi\eta_t) \tag{11}$$

thus obtaining, under both regimes, a VAR(1) representation for ξ_t,

$$\xi_t = A_0\xi_{t-1} + B_0 u_t \tag{12}$$

where u_t is vector standard white noise. Finally, the state-space representation of the model in terms of the three observable variables, R_t, π_t, y_t, implies the following observation equation

$$Y_t = C_0\xi_t \tag{13}$$

with $Y_t \equiv [R_t,\, \pi_t,\, y_t]'$ and C_0=$[I_3\ 0_{3\times(N_0-3)}]$, where N_0 is the dimension of the state vector. (Notice that, in terms of the canonical '*A*-*B*-*C*-*D*' representation of a state-space form, the matrix D_0 is here equal to D_0=$0_{3\times 3}$.)

2.1.2 The experimental design

Our goal is to assess the performance of (structural) VARs conditional on a DGP in which neither luck (i.e., changes in the volatilities of the structural shocks), nor structural change (in the present case, changes in the non-policy parameters, α, γ, κ, σ, and all of the ρ_x's), play *any* role whatsoever.

We therefore estimate (1)-(3)

- imposing indeterminacy for the pre-October 1979 period and determinacy for the period following the end of the Volcker stabilisation, by allowing for different values of ρ, ϕ_π, and ϕ_y across periods;
- imposing that α, γ, κ, σ, all of the ρ_x's, and all of the σ_x^2's, be identical across regimes. This is obtained by jointly estimating the two models for the pre-October 1979 and the post-Volcker stabilisation periods.

By showing that this DGP can replicate the key features of the Great Moderation, our results will illustrate, in the starkest possible way, that existing VAR evidence is *compatible* with the good policy explanation of the Great Moderation.

2.2 Bayesian estimation

We estimate (1)-(3) *via* Bayesian methods. The following two sub-sections describe our choices for the priors, and the Random-Walk Metropolis algorithm we use to get draws from the posterior.

2.2.1 Priors

Following, e.g., Lubik and Schorfheide (2004) and An and Schorfheide (2006), all structural parameters are assumed, for the sake of simplicity, to be *a priori* independent from one another. The third column of Table 1 reports the parameters' prior densities, whereas the fourth and the fifth columns report two key objects characterising them, the mode and the standard deviation. Different from the vast majority of the papers in the literature, we calibrate the Gamma, inverse Gamma, and Beta prior densities in terms of the *mode* of the distribution, rather than in terms of the *mean* (specifically, we calibrate the densities so that our 'preferred values' for the parameters of interest are equal to the mode). The key reason for doing so is in order to give the maximal amount of prior weight to our 'preferred values', which, on the other hand, would not be the case if calibration were performed in such a way as to make the densities' means equal to such values.

2.2.2 Getting draws from the posterior *via* Random-Walk Metropolis

We numerically maximise the log posterior—defined as $\ln L(\theta|Y) + \ln P(\theta)$, where θ is the vector collecting the model's structural parameters, $L(\theta|Y)$ is the likelihood of θ conditional on the data, and $P(\theta)$ is the prior—*via* simulated annealing (for a full description of the methodology, see Appendix A.1) We then generate draws from the posterior distribution of the model's structural parameters *via* the Random Walk Metropolis (henceforth, RWM) algorithm as described in, e.g., An and Schorfheide (2006). In implementing the RWM algorithm we exactly follow An and Schorfheide (2006, Section 4.1), with the single exception of the method we use to calibrate the covariance matrix's scale factor—the parameter c below—for which we follow the methodology described in Appendix D.3 of Benati (2008), which is briefly described in Appendix A.2 below.

Let then $\hat{\theta}$ and $\hat{\Sigma}$ be the mode of the maximised log posterior and its estimated Hessian, respectively.[6] We start the Markov chain of the RWM algorithm by drawing $\theta^{(0)}$ from $N(\hat{\theta}, c^2\hat{\Sigma})$. For $s = 1, 2, ..., N$ we then draw $\tilde{\theta}$ from the proposal distribution $N(\theta^{(s-1)}, c^2\hat{\Sigma})$, accepting the jump (i.e., $\theta^{(s)} = \tilde{\theta}$) with probability $\min\{1, r(\theta^{(s-1)}, \theta|Y)\}$, and rejecting it (i.e., $\theta^{(s)} = \theta^{(s-1)}$) otherwise, where

$$r(\theta^{(s-1)}, \theta|Y) = \frac{L(\theta|Y)\ P(\theta)}{L(\theta^{(s-1)}|Y)\ P(\theta^{(s-1)})}$$

[6] We compute $\hat{\Sigma}$ numerically as in An and Schorfheide (2006).

We run a burn-in sample of 200,000 draws which we then discard. After that, we run a sample of 200,000 draws, keeping every draw out of 100 in order to decrease the draws' autocorrelation, thus ending up with a sample of 2,000 draws.

Table 1 reports the modes and the 90%-coverage percentiles of the distributions of the model's structural parameters.

3 Theoretical Properties of the Estimated Data-Generation Process

In this Section we explore the theoretical properties of the estimated data-generation process under the two regimes, by analysing the structural VAR(MA) representations of the model, the structural innovations' theoretical impact matrices and impulse-response functions, the VAR(MA)'s reduced-form innovation variances, and the series' theoretical variances under determinacy and indeterminacy. By focusing on the theoretical properties of the DGP, we will therefore show that the ability of Clarida *et al.*'s 'indeterminacy hypothesis' to replicate the broad features of the Great Moderation as a consequence of a shift in monetary policy has nothing to do with estimation issues—sample length, choice of the lag order, etc.—but it rather holds *in population.* Figure 2, in particular, plots the theoretical impulse-response functions of the model under the two regimes.

In order to make the exposition clearer, however, it is useful to start with the model's theoretical equivalent minimal state-space representation under the two regimes.

3.1 The equivalent minimal state-space representations of the model under the two regimes

Conditional on the estimates reported in Table 1, the theoretical state-space representations of the model under the two regimes can easily be computed. By applying MATLAB's routine `ss.m` to the two state-space forms we then obtain the two equivalent minimal state-space representations (henceforth, EMSSR) of the model,

$$\xi_t^M = \begin{bmatrix} 0.69 & -0.31 & 0.29 & -0.08 & 0.09 & 0.01 & 0.00 \\ 0.32 & 0.60 & 0.07 & 0.10 & -0.11 & -0.01 & 0.00 \\ 0.44 & 0.03 & 0.58 & -0.11 & 0.03 & 0.00 & 0.00 \\ -0.32 & 0.06 & -0.09 & 0.27 & -0.25 & 0.01 & 0.00 \\ 0.07 & -0.06 & -0.05 & -0.31 & 0.56 & 0.05 & 0.00 \\ 0.42 & -0.23 & -0.04 & -0.02 & 1.05 & 0.45 & 0.01 \\ 0.62 & -0.40 & 0.70 & 0.39 & -0.38 & -0.05 & 0.41 \end{bmatrix} \xi_{t-1}^M +$$

$$
+\begin{bmatrix} 0.55 & 0.23 & 0.35 & 0.12 \\ 0.11 & 0.58 & 0.55 & -0.17 \\ 0.49 & 0.01 & 0.23 & 0.09 \\ 0.02 & -0.16 & -0.28 & -0.12 \\ -0.32 & 0.24 & -0.03 & 0.13 \\ 0.12 & 1.17 & -0.41 & 0.01 \\ 1.51 & -0.17 & -0.21 & -0.03 \end{bmatrix} u_t \tag{14}
$$

$$
Y_t = \begin{bmatrix} 0.08 & -0.67 & -0.68 & 0.20 & -0.22 & -0.02 & -0.00 \\ 0.00 & -0.05 & 0.087 & 0.41 & 0.34 & -0.76 & -0.35 \\ -0.00 & -0.17 & 0.268 & 0.18 & -0.21 & 0.41 & -0.80 \end{bmatrix} \xi_t^M \tag{15}
$$

under indeterminacy, and

$$
\xi_t^M = \begin{bmatrix} 0.79 & 0.15 & 0.21 & -0.20 & -0.02 & 0.00 \\ 0.34 & 0.51 & 0.05 & -0.15 & -0.01 & 0.00 \\ -0.20 & 0.05 & 0.23 & -0.14 & 0.02 & 0.00 \\ 0.13 & -0.12 & -0.29 & 0.42 & 0.04 & 0.00 \\ 0.20 & -0.28 & 0.24 & 0.60 & 0.38 & 0.01 \\ -0.21 & 0.55 & 0.75 & -0.53 & -0.06 & 0.41 \end{bmatrix} \xi_{t-1}^M + \begin{bmatrix} 0.68 & 0.43 & -0.38 \\ 0.12 & -0.29 & 0.00 \\ -0.02 & -0.30 & -0.13 \\ -0.24 & 0.37 & -0.01 \\ -0.26 & 0.50 & -1.05 \\ 0.04 & -1.28 & -0.51 \end{bmatrix} u_t \tag{16}
$$

$$
Y_t = \begin{bmatrix} -0.70 & -0.64 & 0.23 & -0.18 & -0.01 & -0.00 \\ -0.03 & 0.07 & 0.34 & 0.36 & -0.78 & -0.37 \\ -0.18 & 0.28 & 0.18 & -0.15 & 0.42 & -0.81 \end{bmatrix} \xi_t^M \tag{17}
$$

under determinacy, where ξ_t^M is the state vector in the EMSSR,[7] and Y_t is still equal to $Y_t \equiv [R_t, \pi_t, y_t]'$. A comparison between (14)-(15) and (16)-(17) immediately highlights a fundamental difference between the two regimes. Whereas under determinacy the EMSSR has six states, under indeterminacy it has *seven*.[8] An important point to stress is that the presence of an additional state variable under indeterminacy has *nothing* to do with the presence of a sunspot shock. Indeed, first, it can be easily shown that this feature of the DGP remains unchanged even if we set the variance of the sunpot shock to zero. Second, and more fundamentally, given that the sunspot shock is pure white noise, on strictly logical/mathematical grounds it *cannot* belong to the state vector, so that the additional state under indeterminacy *ought* to be something else.[9] Rather, as shown by Benati (2007b), the presence of an additional

[7] In general, the states of the EMSSR generated by MATLAB's routine `ss.m` do not have any intrinsic meaning whatsoever, as they are simply linear combinations of the states of the original state-space form.

[8] The fact that, under indeterminacy, the EMSSR possesses an additional state variable compared with the determinacy regime was first shown in a previous version of this paper that was presented at the New York FED on on September 27th, 2006. The slides of the presentation are available from either of us, while the paper is available either from us, or from the New York FED.

[9] For an extensive discussion of what the additional state variable under indeterminacy exactly is, see Benati (2007b). Benati (2007b), in particular, demonstrates mathematically that—different

state under indeterminacy is a *general property* of New Keynesian models under this regime.

3.2 The VARMA representations

Such finding has the following important implication. Since, once the EMSSR has been appropriately rotated,[10] three of the states in ξ_t^M are equal to the three structural disturbances, this automatically implies that under determinacy, with six states in the EMSSR, the model possesses a VAR representation in R_t, π_t, and y_t. Indeed, applying to the EMSSR of the model under determinacy a MATLAB code for computing the finite-order VAR representation of a state-space form,[11] we obtain the VAR(2) representation

$$Y_t = \underbrace{\begin{bmatrix} 1.22 & 0.01 & 0.14 \\ -0.04 & 0.47 & 0.06 \\ -0.12 & -0.04 & 1.05 \end{bmatrix}}_{B_1^{DET}} Y_{t-1} + \underbrace{\begin{bmatrix} -0.32 & -0.01 & -0.06 \\ 0.02 & -0.02 & -0.03 \\ 0.08 & 0.00 & -0.26 \end{bmatrix}}_{B_2^{DET}} Y_{t-2} + v_t,$$

$$\text{with } \mathrm{Var}(v_t) = \begin{bmatrix} 0.40 & 0.19 & -0.14 \\ 0.19 & 0.97 & 0.08 \\ -0.14 & 0.08 & 0.98 \end{bmatrix} \tag{18}$$

Under indeterminacy, on the other hand, the very same logic implies that, with one additional state variable in the EMSSR, the model does not possess a pure VAR representation in R_t, π_t, and y_t, but rather a VARMA one, with a small moving-average component. Figure 1 illustrates this, by showing the evolution of the coefficients of the theoretical VAR(∞) implied by the VARMA representation of the model under indeterminacy, as a function of the lag. A simple illustration of the speed of decay towards zero of the coefficients of the VAR(∞) representation of the model is provided

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from what claimed by Canova (2006) and Canova and Gambetti (2007)—the additional state is *not* expected inflation, i.e. $\pi_{t+1|t}$.

[10] In general, state-space forms are unique up to a rotation. Specifically, the EMSR

$$s_t = As_{t-1} + Bu_t \tag{I}$$

$$Y_t = Cs_t \tag{II}$$

is, for a given 'input' vector u_t, observationally equivalent, in terms of 'output' vector Y_t, to the rotated state-space form

$$\tilde{s}_t = \tilde{A}\tilde{s}_{t-1} + \tilde{B}u_t \tag{III}$$

$$Y_t = \tilde{C}\tilde{s}_t \tag{IV}$$

with $\tilde{s}_t \equiv Rs_t$, $\tilde{A} \equiv RAR^{-1}$, $\tilde{B} \equiv RB$, and $\tilde{C} \equiv CR^{-1}$. In plain English, this means that for a *given* vector white noise input u_t, we can obtain *exactly* the same identical realisations for the observables *via* an *infinity* of state-space forms, all of which are a rotation of (I)-(II), and all of which are uniquely identified by a specific rotation matrix R.

[11] The code has been kindly supplied by Juan Rubio-Ramirez.

by the evolution of the maximum among the absolute values of the elements of the AR matrices of the VAR(∞). At the first three lags, such maximum is equal to 1.087, 0.271, and 0.055, respectively, whereas at lags 10 and 20 it decreases to 0.012 and 1.0E-3, respectively, and at lags 50, 75, and 100 it further declines to 2.1E-6, 9.6E-9, and 6.3E-11, respectively. Finally, the covariance matrix of reduced-form innovations of the VARMA representation of the model obtained *when setting the sunspot shock to zero* is given by

$$\mathrm{Var}(v_t)= \begin{bmatrix} 0.47 & 0.43 & 0.06 \\ 0.43 & 1.15 & 0.16 \\ 0.06 & 0.16 & 1.32 \end{bmatrix} \tag{19}$$

4 Replicating the Great Moderation

4.1 Volatility decreases in population

A comparison between the diagonal elements of (19) and (18) shows that the shift in the systematic component of monetary policy associated with the move from indeterminacy to determinacy is *sufficient* to generate decreases in the theoretical innovation variances for all series as a simple implication of the Lucas (1976) critique, and without any need of sunspot shocks. Whereas earlier contributions have interpreted decreases in reduced-form innovation variances as *prima facie* evidence in favor of good luck, and against good policy, our results clearly show such interpretation to be unwarrented. Further, such policy shift is associated with decreases in the standard deviations of both inflation and the output gap, from 1.21 to 1.12 and from 2.17 to 1.88 respectively, whereas the standard deviation of the interest rate slightly increases from 1.65 to 1.68. With the estimated standard deviation of sunspot shocks, the model exhibits clear decreases in both variances and innovation variances when moving from indeterminacy to determinacy. In particular, the theoretical standard deviations of R_t, π_t, and y_t under indeterminacy become equal to 7.68, 7.65, 2.87, respectively, thus highlighting the ability of the estimated DGP to broadly replicate the generalised decline in overall macroeconomic volatility associated with the Great Moderation.

4.2 Results from break tests based on stochastic simulations

Given the decline in the three series' theoretical innovation variances associated with the move from indeterminacy to determinacy, structural break tests applied to the simulated data should point towards significant volatility breaks. In this sub-section we therefore stochastically simulate the estimated DGP 10,000 times,[12] we fit VARs

[12] The pseudo-sample length is equal to T=100 for both regimes.

to the simulated data,[13] and for each of the three equations of the VAR we perform Wald tests for a single break across regimes in either the innovation variance or the equation's coefficients. Table 2 reports, for each of the three equations, the medians and the 90%-coverage percentiles of the distributions of the bootstrapped p-values[14] for the Wald tests, together with the fraction of bootstrapped p-values below 10%. As the Table shows, break tests point towards

- weak evidence of breaks in the coefficients of the equations for inflation and the output gap, with the null of no break being rejected 15% and 10% of the times, respectively;
- some evidence of breaks in both the coefficients and the innovation variance in the interest rate equation, with the null of no break being rejected 26% and 21% of the times, respectively; and
- stronger evidence of volatility breaks in the equations for both inflation and the output gap, with the null of no break being rejected 36% and 39% of the times, respectively.

Results qualitatively in line with those reported in Table 2 are typical of the structural VAR-based literature on the Great Moderation. Sims and Zha (2006), for instance, report that

> 'the best fit [of the VAR] is with a version that allows time variation in structural disturbance variances only. Among versions that allow for changes in equation coefficients also, the best fit is for a one that allows coefficients to change only in the monetary policy rule.'

As we already stressed, although such results are routinely *interpreted* as evidence against good policy, and in favor of good luck, such interpretation is unwarranted, and these results should be regarded, in principle, as *uninformative* for discriminating between luck and policy.

4.3 Generating 'Great Inflations' and 'Great Moderations'

Figure 3 provides a stark illustration of the ability of Clarida *et al.*'s 'indeterminacy hypothesis' to replicate the transition from the Great Inflation to the Great Moderation *uniquely* as a result of improved monetary policy, by plotting a single stochastic simulation of length T=100 for both regimes from the estimated model. As the figure shows, the first part of the sample exhibits what, at first blush, clearly looks like a

[13] We select the VAR lag order based on the Akaike information criterion, based on the joint sample of length 200.

[14] Bootstrapping is performed as in Diebold and Chen (1996) applied to the VAR as a whole.

'Great Inflation' episode, with both inflation and the nominal rate displaying wide and persistent swings, reaching peaks just shy of ten per cent.[15] In the second half of the sample, on the other hand, fluctuations are much more subdued, and do not exhibit any persistent deviation from equilibrium.

5 Can Structural VAR Methods Uncover the Truth?

When applied to the estimated model, can structural VAR methods uncover the authentic causes of the changes in the DGP across regimes? As this section shows, the answer is unfortunately 'No'.

5.1 Theoretical structural policy counterfactuals

We start by analysing *theoretical* policy counterfactuals, i.e. counterfactuals based on the theoretical VAR representations of the model under the two regimes. By showing that structural VAR methods fail to correctly capture the truth *in population*, we will therefore illustrate in the starkest possible way that the problems discussed in the present work have nothing to do with *estimation* issues—i.e, lag order selection and the like—but rather point towards fundamental weaknesses of the structural VAR approach for the present purposes.

As we pointed out in Section 3.2, the model possesses a VAR(2) representation under determinacy, and a VARMA one with a small moving-average component under indeterminacy. We start by approximating the VAR(∞) implied by the VARMA representation under indeterminacy with a VAR(100),[16] and we augment the VAR(2) under determinacy with 98 further AR matrices equal to $0_{3\times3}$. Based on the structural shocks' theoretical impact matrices for the two regimes, we then put the two theoretical VARs into the corresponding structural VAR forms,

$$A_{0,IND}^{-1}Y_t = \tilde{B}_1^{IND}Y_{t-1}... + \tilde{B}_{100}^{IND}Y_{t-100} + \epsilon_t \tag{20}$$

$$A_{0,DET}^{-1}Y_t = \tilde{B}_1^{DET}Y_{t-1}... + \tilde{B}_{100}^{DET}Y_{t-100} + \epsilon_t \tag{21}$$

where $\tilde{B}_j^x = A_{0,x}^{-1}B_j^x$, with $x = IND$, DET (with IND for 'indeterminacy' and DET 'determinacy'), $A_{0,x}^{-1}$ being the impact matrix of the three structural shocks ($\tilde{\epsilon}_{R,t}$, $\tilde{\epsilon}_{\pi,t}$, $\tilde{\epsilon}_{y,t}$) at zero, and j = 0, 1, ..., 100. A crucial point to stress is that, since we are here working with the impact matrices of the three structural shocks, we are implicitly setting the variance of the sunspot shock to zero—to put it differently, the version of the model we are working in this sub-section is the one *without* sunspots. By

[15] This is conceptually in line with Clarida *et al.*'s (2000) Figure V on page 172.

[16] As we pointed out in Section 3.2, at lag 100 the maximum among the absolute values of the elements of the AR matrix of the VAR(∞) is of an order of magnitude of 10^{-11}, which implies that all lags beyond the 100^{th} can safely be ignored.

illustrating the failure of structural VAR-based policy counterfactuals to capture the truth even in the absence of sunspot shocks, we will therefore show that the problems discussed in this sub-sections have nothing to do with the presence of sunspot shocks, and rather pertain to a fundamental problem of VAR-based policy counterfactuals.

After switching the structural monetary rules in the two structural VARs—i.e., the first equations in (20) and (21)—we convert the counterfactual structural VARs we thus obtain into corresponding counterfactual reduced-form VARs, from which theoretical counterfactual standard deviations for the three series can trivially be computed. As we pointed out in Section 4.1, under determinacy the true theoretical standard deviations are equal to 1.68, 1.12, and 1.88 respectively. Quite strikingly, the corresponding theoretical counterfactual standard deviations are equal to 1.19, 1.10, and 1.92, thus implying a volatility *decrease* for two series out of three. Results for the indeterminacy regime are even more troubling: whereas the largest AR root of the true VAR representation of the model under this regime is equal to 0.998, very close to a unit root, the largest root of the counterfactual VAR representation is actually *explosive*, being equal to 1.0012. This implies that, when plugging the structural monetary rule for the determinacy regime into the structural VAR representation of the model corresponding to the indeterminacy regime, the variance of the three series becomes *infinite*, thus highlighting the failure of structural VAR-based policy counterfactuals to capture the truth.

What is going on here? Why do structural-VAR based policy counterfactuals fail so badly in population? The reason is not difficult to grasp, and is the following. When performing counterfactual simulations in structural VARs, the *implicit presumption* is that switching the VAR's estimated structural policy rules should provide a reasonable approximation to the *authentic* policy switch, i.e. the one between the Taylor rules in the underlying DSGE model. As our results show, however, such presumption is, in general, unwarranted, the key reason being that what is structural as defined by the underlying DSGE model bears no clear-cut connection with what is *defined* as 'structural' by the structural VAR form of the very same DSGE model, i.e. equations (20) and (21). The difference between these two notions of what is structural is at the root of the problem here, and cannot therefore be 'fixed', being rather a fundamental shortcoming of policy counterfactuals based on structural VARs.[17]

5.2 Impulse-response functions

Let's now turn to impulse-response functions (henceforth, IRFs). Little change over time in estimated IRFs to an identified monetary policy shock has been traditionally

[17] This problem of policy counterfactuals based on structural VARs is extensively analysed by Benati (2007a). Taking a standard New Keynesian model as DGP, he explores the conditions under which SVAR-based policy counterfactuals may provide a reasonably good approximation to the 'authentic' policy switch, i.e. the one between the Taylor rules in the New Keynesian model. As he shows, such conditions are extremely restrictive.

regarded as evidence in favor of good luck, and against good policy. As we will now show, such evidence is, once again, uninformative for the issue of deciding the role played by monetary policy in fostering the Great Moderation.

Figure 4 shows, for the two regimes, the medians and the 90%-coverage percentiles of the distributions of the estimated IRFs to a 100 basis points monetary policy shock, based on 1,000 stochastic simulations. Specifically, for each of the 1,000 simulations

(*i*) we generated artificial data under the two regimes from the estimated DGP and we estimated reduced-form VARs exactly as in Section 4.2, choosing the lag order based on the AIC.

(*ii*) Based on the VARs' estimated covariance matrices, we estimate the structural impact matrices for the two regimes by imposing the true theoretical sign restrictions *via* the procedure introduced by Rubio-Ramirez, Waggoner, and Zha (2005). We integrate out rotation uncertainty by computing, for each of the 1,000 stochastic simulations, 1,000 impact matrices satisfying the sign restrictions, and then taking the average among them.

(*ii*) Based on the estimated VARs and the structural impact matrices, we compute the IRFs to a 100 basis points shock to the interest rate.

Two main findings emerge from the Figure. First, the distributions of the estimated IRFs under indeterminacy are much wider than those under determinacy. This is especially clear for inflation and the nominal rate, much less so for the output gap, and finds its origin in the much greater persistence exhibited by the system under indeterminacy. Second, for none of the series it is possible to reject the null that the IRFs have remained unchanged across regimes.

Although little change in estimated IRFs to a monetary policy shock have routinely been interpreted as evidence in favor of good luck and against good policy, these results show that this interpretation is unwarranted.

6 Some Criticisms of Our Analysis, and Our Rebuttals

6.1 Canova's criticism

Fabio Canova has circulated a note which is critical of the present work.[18] He summarises his main objections as follows:[19]

> '[N]one of the problems highlighted by Benati and Surico have to do with VAR methods, *per se*. It is the choice of experimental design, failure to recognize the presence of omitted variables in one regime and the choice of relatively small sample size which drive the results they obtain.'

[18] See Canova (2006).

[19] See Canova (2006, page 12, second paragraph).

In what follows we show that most of Canova's arguments are simply wrong, and none of them affects our conclusions.

6.1.1 The omitted state variable under indeterminacy

As we pointed out, the minimal state-space representation of the model has six state variables under determinacy, and *seven* under indeterminacy. Canova (2006) states that the additional state variable under indeterminacy is $\pi_{t+1|t}$ (i.e. expected inflation at time $t+1$, conditional on information available at time t),[20] and based on this conjecture makes two points.

- *If you augment the VAR in inflation, the output gap and the nominal interest rate with expected inflation, the augmented VAR may uncover the true causes of change in the DGP.*
- *If you integrate out the presence of the unobserved state variable under indeterminacy, the variances of the series are much lower, so that the model cannot replicate the Great Moderation* (see footnote 1, and the variances reported in Table 1 of Canova's comment).

Concerning the first point, Canova and Gambetti (2007) take Canova's conjecture as the rationale for including measures of expected inflation in their (fixed-coefficient and time-varying parameters) VARs. Given that, overall, their results are largely unaffected by the inclusion of expected inflation measures, they conclude that their evidence runs against Clarida *et al.*'s (2000) indeterminacy hypothesis about the sources of the Great Moderation.

The problem with Canova's and Canova and Gambetti's position is that, as it has been demonstrated mathematically by Benati (2007b), the VAR representation they consider is *only one among an infinity* of admissible VAR representations for the economy, so that their evidence is ultimately uninformative for the issue at hand. Another admissible representation, for example, has expected inflation replaced by the expected output gap, or by any linear (convex or non-convex) combination of the two. As a simple corollary, this automatically implies that a *negative* result—like the one obtained by Canova and Gambetti (2007)—is not telling us, in principle, anything: to be informative, the negative outcome should be obtained for *all* the infinite, possible representations of the economy. What truly is informative under these circumstances, on the other hand, is a *positive* result, i.e. the finding that a particular variable which is admissible as the fourth element in the VAR representation of the model beyond

[20] To be precise, Canova (2006) is not explicit about what he actually means by 'expected inflation', which he just labels as π_t^e. Although, in principle, he could mean either $\pi_{t+1|t}$ or $\pi_{t|t-1}$, the latter can be ruled out on strictly logical grounds. Given that $\pi_{t|t-1}$ does not belong to the state vector of the *original* state-space form, ξ_t, there is simply *no way* that it can belong to the state vector of the EMSR, s_t. The only possibility left is therefore $\pi_{t+1|t}$.

R_t, π_t, and y_t does indeed Granger-cause the first three variables, and/or produces significant changes in the results.

As for the second issue raised by Canova (2006), the crucial point to stress is that the presence of an additional state variable under indeterminacy is an integral part of the intrinsic dynamics of the system under that regime, so that it is simply not possible to meaningfully talk of the dynamics of inflation, the interest rate, and the output gap under indeterminacy 'controlling' for the effect of this additional state. To make this point even clearer, consider the following simple example. Under determinacy the dynamics of the economy only depends on three states, inflation, the output gap, and the interest rate. Following Canova's logic, we could say that 'if we control for (i.e, integrate out) the influence of the interest rate, the variance of inflation under determinacy would be lower'. This is tautologically true, but the key problem is that 'controlling for the influence of the interest rate' does not have any meaning whatsoever, for the simple reason that the interest rate—exactly like the additional unobserved state variable under indeterminacy—*is not exogenous*, but it is rather *endogenous*. You might legitimately want to control for fluctuations in exogenous driving processes, but controlling for the influence of endogenous variables is just wrong.[21]

6.1.2 The experimental design

Our paper simulates a world in which the Great Moderation is exclusively driven by improved monetary policy, and then asks: *'When applied to the simulated data, are the VAR methods used in earlier contributions capable of delivering the true answer of good policy?'*. Canova (2006), on the other hand, is concerned with a completely different question: *'Is it possible to specify a DSGE model and a policy shift for which VAR analysis would uncover the true change in the DGP?*[22] Unfortunately, this is irrelevant for the issue at hand: the fact that I can conceive circumstamces under which a specific econometric methodology performs well does not tell me anything about its performance conditional on the only DGP that truly matters, i.e,. the one which is out there. The reason is very simple: reality is what it is, and you can't 'choose' it to suit the econometric methodologies that you like. As a consequence, in order to be reasonably confident that the results produced by a specific econometric

[21] Finally, it is worth stressing that the Great Moderation has been identified as the decline in the *overall* volatilities of the series, and Kim and Nelson (1999), McConnell and Perez-Quiros (2000), Stock and Watson (2002), and Kim, Nelson, and Piger (2004) have *not* netted out the impact of inflation expectations when computing the variances of output and inflation. So our paper is consistent with all previous studies documenting the Great Moderation.

[22] 'If one is interested in measuring the ability of VAR methods to answer the questions of interest, one should also design an experiment where only the lagged coefficients change. [...] To have a design with the required features, one should study within regime changes when the nominal interest rate reacts to *lagged* output gap and *lagged* inflation since, by construction, the matrix of impact coefficients is fixed across regimes while the matrix of lagged coefficients changes' (Canova, 2006, page 7, third paragraph).

methodology are sufficiently reliable, such methodology must be shown to perform well conditional on a wide range of plausible data-generation processes. As this paper has shown, however, if the truth for the post-WWII United States is reasonably well described, to a first approximation, by Clarida *et al.*'s (2000) 'indeterminacy hypothesis', VAR methods as they have been implemented so far may well point towards the incorrect conclusion of good luck. And this automatically implies that existing VAR evidence is uninformative for the issue at hand: if I always get good luck irrespective of what the truth is, such result is quite obviously uninformative for the issue at hand. The notion that a researcher may therefore pick and choose a specific DGP in order to show that, conditional on that DGP, his/her favorite econometric methodology performs well does therefore not appear to us a meaningful way of discriminating between what works and what doesn't work.[23]

6.1.3 The sample size

In this paper we simulate the New-Keynesian model using 100 observations per regime, which, at the quarterly frequency, correspond to 25 years of data. VAR analyses of the Great Moderation begin the sample period either at the end of the 1950s or at the beginning of the 1960s and typically split the full sample around October 1979. The sample selection implies that the inference drawn on the VAR estimates is based on 20 years of data for the 'bad policy' regime, and 20-25 years for the 'good policy' regime. Our choice of 100 observations is therefore perfectly in line with the number of data points available to the econometrician—indeed, this is precisely the reason why we chose it!

Interestingly, while investigating the minimal sample size that would allow a researcher to reject the (incorrect) null hypothesis of stability of the diagonal elements of the matrix of VAR autoregressive coefficients, Canova (2006) reports that across the two regimes

> '[...] differences would be detectable only if at least 120 data for the interest rate equation, 410 for the inflation equation and 2300 data points for the output gap equation would be available in each regime'

Canova's own results therefore represent a serious challenge for existing VAR studies of the Great Moderation as, with quarterly data, they correspond *to 30, 102.5, and 575 years* for each regime. These results imply that, even assuming that Canova's conceptual position is entirely correct—on which, as we previously discussed, we definitely disagree—an econometrician would need *several decades of data* in order to

[23] Finally, a DGP should be internally consistent. In the effort of specifying a DGP that may put VARs under the best of possible lights, Canova (2006) proposes a model in which the private sector is fully forward-looking, but the monetary authority is fully backward-looking. What is the reason for this asymmetry? How can we square such a modeling choice with the strong emphasis that many central banks across the world have given on the forward-looking nature of their policy making?

reliably identify the causes of the Great Moderation *via* VAR methods. Given that, in reality, none of us enjoys such luxury, it is not clear why this should be regarded as a *limitation* of our analysis, rather than a needed element of *realism*.

6.2 A further criticism

A further criticism has been offered, so far, of the present analysis. The work of Clarida, Gali, and Gertler (2000) and Lubik and Schorfheide (2004), upon which the present analysis is based, treats the shift from the first to the second regime as 'once-and-for-all', rather than as an ongoing process. As pointed out, e.g., by Davig and Leeper,[24] '[o]nce-and-for-all shifts, by definition, are unanticipated, yet once the shift occurs, agents are assumed to believe the new regime is permanent and alternative regimes are impossible. But if regime *has* changed, then regime *can* change; knowing this, private agents will ascribe a probability distribution to regimes. Expectations formation and, therefore, the resulting equilibria will reflect agents' beliefs that regime change is possible.' The key point to understand, here, is that the problems highlighted in this paper have *nothing* to do with the specific way in which policy changes are modelled—'once-and-for-all', as opposed to an ongoing process—and as a result should be expected to carry over, as a simple matter of logic, even within more realistic settings like those investigated by Davig and Leeper (2007).

7 Conclusions

Most analyses of the U.S. Great Moderation have been based on structural VAR methods, and have consistently pointed towards good luck as the main explanation for the greater macroeconomic stability of recent years. Based on an estimated New-Keynesian model in which the only sources of change are the move from passive to active monetary policy, and the presence of sunspots under indeterminacy, we show that VARs may misinterpret good policy for good luck. In particular, the estimated DGP exhibits decreases in population in both variances and innovation variances for all series. Policy counterfactuals based on the theoretical structural VAR representations of the model under the two regimes fail to capture the truth, whereas impulse-response functions to a monetary policy shock exhibit little change across regimes. Since these results are in line with those found in the structural VAR-based literature on the Great Moderation, our analysis suggests that existing VAR evidence is compatible with the 'good policy' explanation of the Great Moderation.

[24] See Davig and Leeper (2007).

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A Two Technical Aspects of the Bayesian Estimation Procedure

This appendix discusses in detail two technical aspects the Bayesian estimation procedure.

A.1 Numerical maximisation of the log posterior

We numerically maximise the log posterior—defined as ln $L(\theta|Y)$ + ln $P(\theta)$, where θ is the vector collecting the model's structural parameters, $L(\theta|Y)$ is the likelihood of θ conditional on the data, and $P(\theta)$ is the prior—*via* simulated annealing. Following Goffe, Ferrier, and Rogers (1994) we implement simulated annealing *via* the algorithm proposed by Corana, Marchesi, Martini, and Ridella (1987), setting the key parameters to T_0=100,000, r_T=0.9, N_t=5, N_s=20, ϵ=10^{-6}, N_ϵ=4, where T_0 is the initial temperature, r_T is the temperature reduction factor, N_t is the number of times the algorithm goes through the N_s loops before the temperature starts being reduced, N_s is the number of times the algorithm goes through the function before adjusting the stepsize, ϵ is the convergence (tolerance) criterion, and N_ϵ is number of times convergence is achieved before the algorithm stops. Finally, initial conditions were chosen stochastically by the algorithm itself, while the maximum number of functions evaluations, set to 1,000,000, was never achieved.

A.2 Calibrating the covariance matrix scale factor

A key problem in implementing Metropolis algorithms is how to calibrate the covariance matrix's scale factor—the parameter c in subsection _._—in order to achieve an acceptance rate of the draws close to the ideal one (in high dimensions) of 0.23. Typically the problem is tackled by starting with some 'reasonable' value for c, and adjusting it after a certain number of iterations during the initial burn-in period. Specifically, given that the draws' acceptance rate is decreasing in c, c gets increased (decreased) if the initial acceptance rate was too high (low). A problem with this approach is that it does not guarantee that after the adjustment the acceptance rate will be reasonably close to the ideal one. The approach for calibrating c used in this paper, on the other hand—which is the same used in Benati (2008)—is based on the idea of *estimating* a reasonably good approximation to the inverse relationship between c and the acceptance rate by running a pre-burn-in sample. Specifically, let C be a grid of possible values for c—in what follows, we consider a grid over the interval [0.1, 1] with increments equal to 0.05. For each single value of c in the grid—call it c_j—we run n draws of the RWM algorithm as described in section 2.2.2, storing, for each c_j, the corresponding fraction of accepted draws, f_j. We then fit a third-order polynomial to the f_j's *via* least squares, and letting $\hat{a}_0$, $\hat{a}_1$, $\hat{a}_2$, and $\hat{a}_3$ be the estimated coefficients, we choose c by solving numerically the equation $\hat{a}_0+\hat{a}_1c+\hat{a}_2c^2+\hat{a}_3c^3=0.23$. As the fraction of accepted draws reported in Table 1 shows, the procedure works quite well.

Table 1 Bayesian estimates of the structural parameters

Parameter	Domain	Density	Prior distribution		Posterior distribution: mode and 90%-coverage percentiles	
			Mode	Standard deviation	Before October 1979	After the Volcker stabilisation
σ_R^2	$\mathbb{R}^+$	Inverse Gamma	0.25	0.25	0.468 [0.415; 0.596]	
σ_π^2	$\mathbb{R}^+$	Inverse Gamma	0.5	0.5	0.311 [0.227; 0.457]	
σ_y^2	$\mathbb{R}^+$	Inverse Gamma	0.1	0.25	0.053 [0.043; 0.083]	
σ_s^2	$\mathbb{R}^+$	Inverse Gamma	0.25	0.25	0.213 [0.144; 0.442]	—
κ	$\mathbb{R}^+$	Gamma	0.05	0.01	0.043 [0.036; 0.057]	
σ	$\mathbb{R}^+$	Gamma	2	1	8.209 [6.589; 10.577]	
α	[0, 1]	Beta	0.75	0.2	0.053 [0.027; 0.091]	
γ	[0, 1]	Beta	0.25	0.2	0.723 [0.668; 0.795]	
ρ	[0, 1)	Beta	0.75	0.2	0.580 [0.503; 0.670]	0.838 [0.788; 0.880]
ϕ_π	$\mathbb{R}^+$	Gamma	1.0	0.5	0.867 [0.775; 0.901]	1.653 [1.139; 2.711]
ϕ_y	$\mathbb{R}^+$	Gamma	0.15	0.25	0.513 [0.417; 0.701]	1.220 [0.759; 1.683]
ρ_R	[0, 1)	Beta	0.25	0.2	0.412 [0.301; 0.526]	
ρ_π	[0, 1)	Beta	0.25	0.2	0.426 [0.332; 0.530]	
ρ_y	[0, 1)	Beta	0.25	0.2	0.795 [0.725; 0.842]	
Fraction of accepted draws					0.261	

Table 2 Testing for stability in the VAR equations: bootstrapped *p*-values for the Wald tests[a]

	Innovation variance		*Coefficients*	
Equation:	Median and 90% percentiles	Fraction below 0.1	Median and 90% percentiles	Fraction below 0.1
interest rate	0.441 [0.011; 0.905]	0.212	0.276 [0.010; 0.876]	0.263
inflation	0.210 [0.002; 0.863]	0.357	0.499 [0.016; 0.968]	0.151
output gap	0.155 [0.003; 0.864]	0.386	0.523 [0.054; 0.961]	0.100

[a] Medians and 90% percentiles of the p-values distributions. 1,000 replications

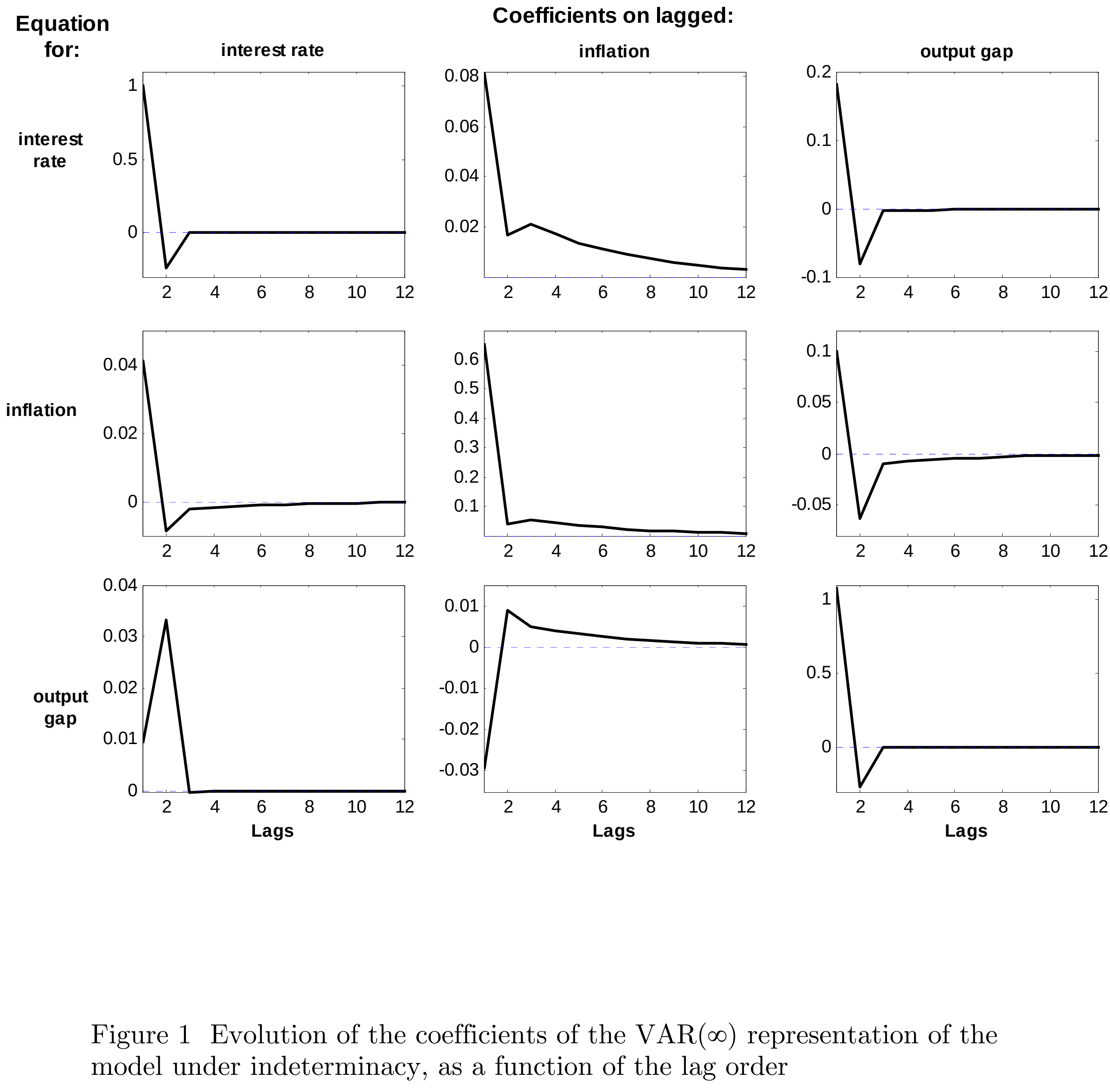

Figure 1 Evolution of the coefficients of the VAR(∞) representation of the model under indeterminacy, as a function of the lag order

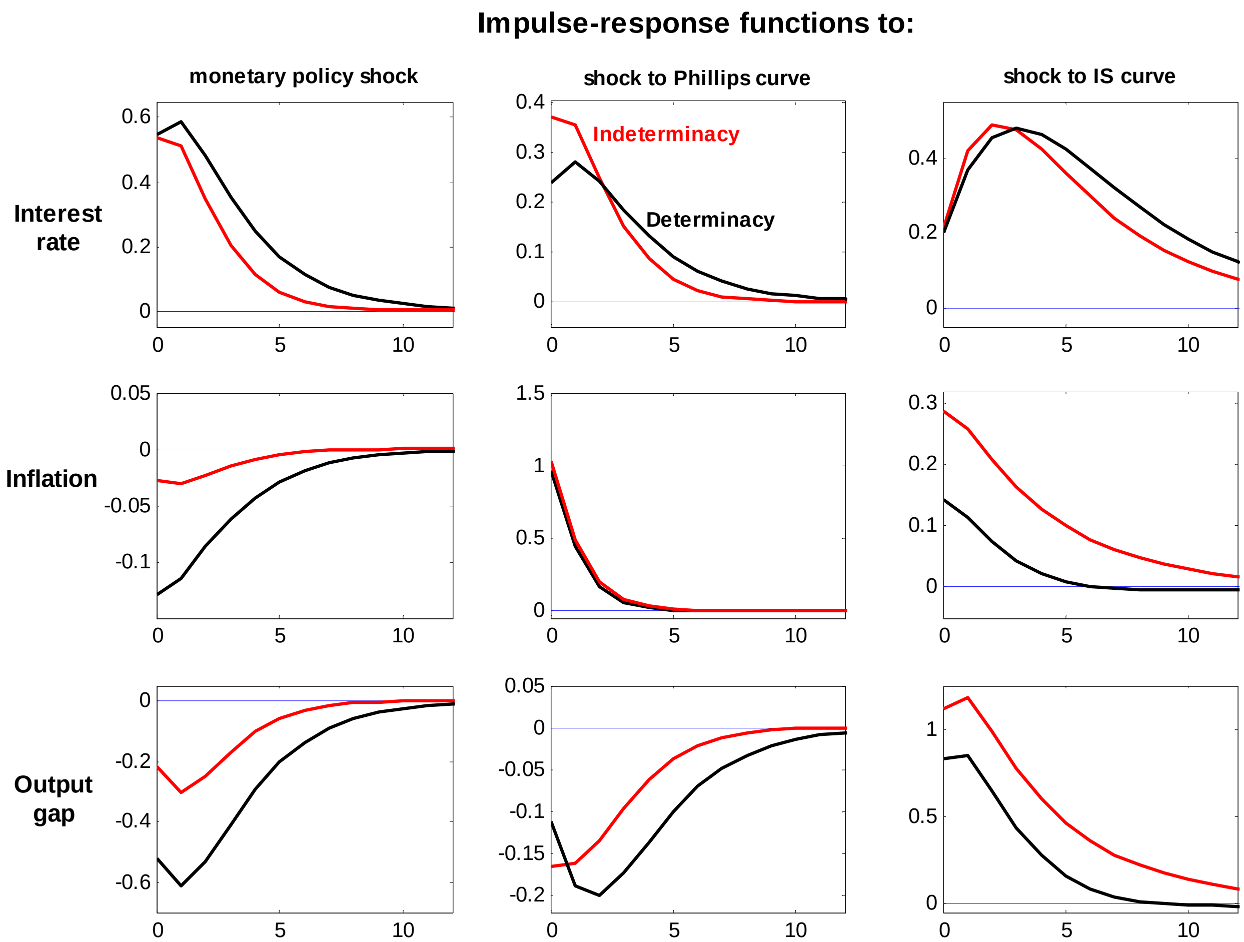

Figure 2 Theoretical impulse-response functions under the two regimes

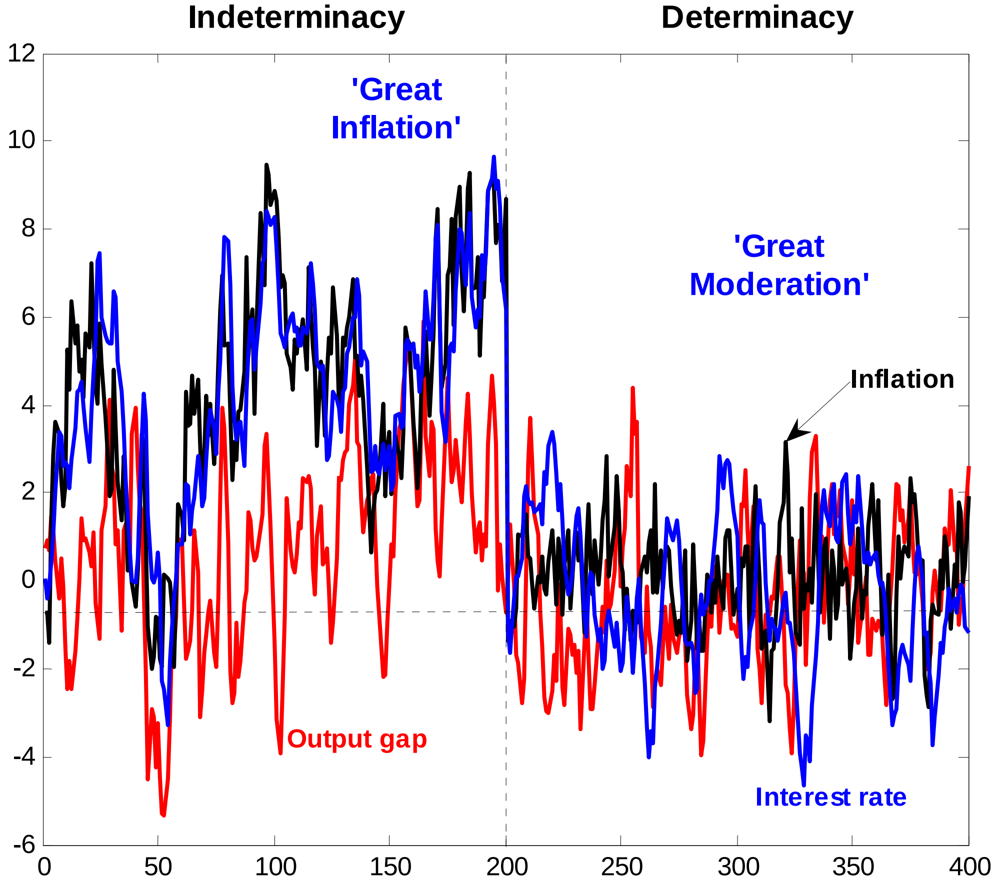

Figure 3 Replicating the Great Moderation: a typical stochastic simulation of the estimated DGP

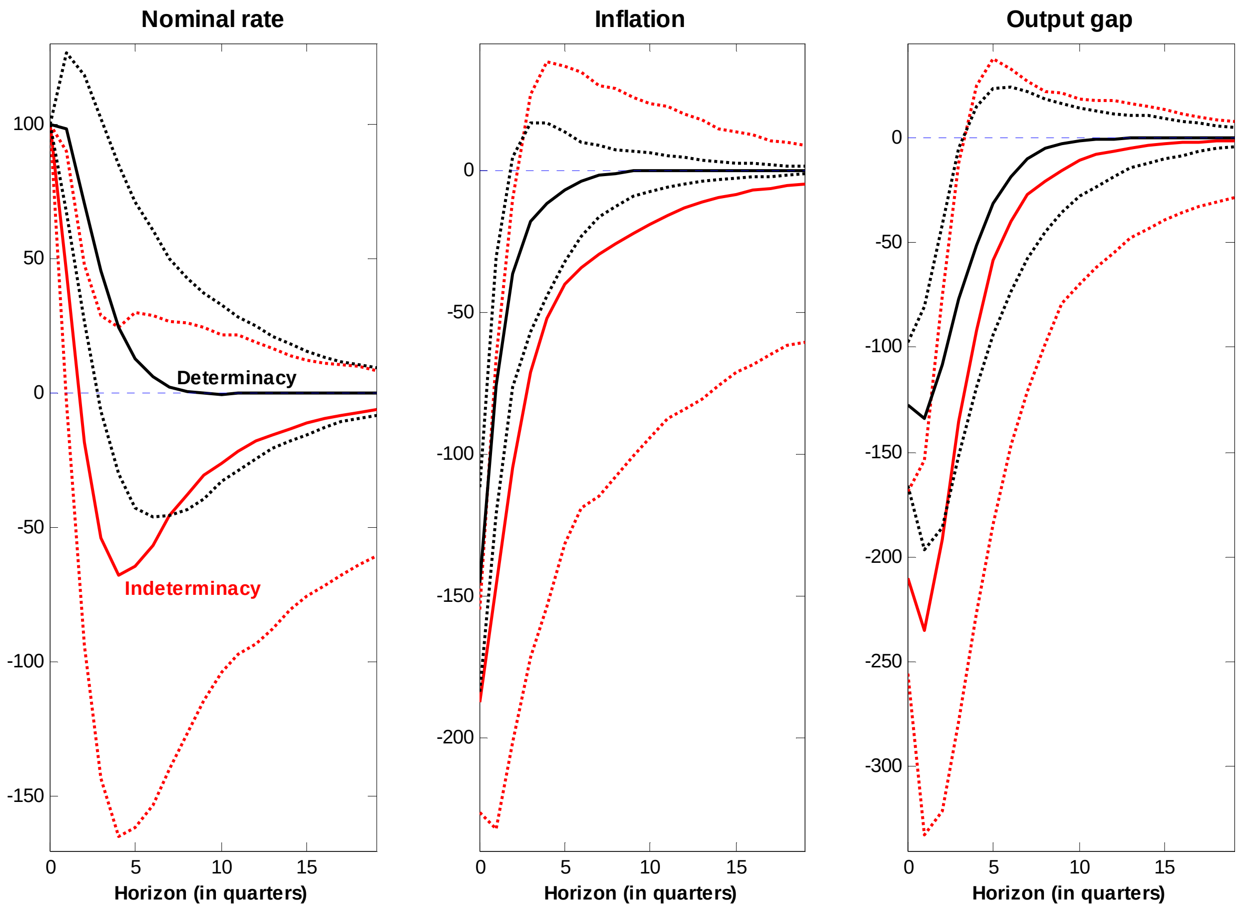

Figure 4 Medians and 90%-coverage percentiles of the distributions of the estimated impulse-response functions to a 100 basis points increase in the nominal rate, under determinacy and indeterminacy (based on 1,000 replications)