The Power of Long-Run Structural VARs

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Abstract

Can structural vector autoregressions (VARs) discriminate between macro models? In simulation exercises, VARs will only infrequently reject the true data generating process. However, equally important is power: the rejection rate of false hypothesis. For a set of DSGE models, we report power results for both the standard test of the sign on impact and a test of the shape of the response. We find that testing the shape is more powerful than testing the sign and is also can be more powerful than another commonly used non-VAR-based test. Therefore, we conclude that structural VARs are useful for testing macro models.

JEL Codes: C1

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1 Introduction

Are long-run structural vector autoregressions (VARs) useful for discriminating between macro models? Because the long-run identifying assumption holds in a broad class of models, these VARs are potentially a useful tool for evaluating and critiquing alternative structural macro models and by doing so can play an important role in developing structural models with empirically realistic dynamics.[1] Recently, there has been a spirited debate about the role of VARs in achieving this objective. Chari Kehoe and McGrattan (2008) [CKM] argue that structural VARs with long-run restrictions are not useful in developing business cycle models. For a correctly specified VAR[2], they claim that "the

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[1]Several recent papers have identified a technology shock in the U.S. macroeconomic data using a long-run restriction. These papers include Gali (1999); Francis and Ramey (2003); and Altig, Christiano, Eichenbaum, and Linde (2004). Identifying how the economy responds to a technology shock has the potential to be useful to help us determine how to best model the economy.

[2]In the model that CKM studies, hours worked is stationary. In such a case, as is discussed in Christiano Eichenbaum and Vigfusson (2003), hours worked should enter the VAR in levels.

procedure does not allow a researcher to distinguish between promising and unpromising classes of models" (CKM, 2008, p. 1339). In contrast, Christiano Eichenbaum and Vigfusson (2006), (henceforth CEV), show that these VARs are reliable in that they only infrequently reject the true data generating process. Although these researchers reach opposite conclusions, they focus almost exclusively on whether VARs have adequate size properties. However, in assessing a statistical test, we also care about power: the ability of the test to reject a false hypothesis. A key contribution of this paper is to assess the power properties of long-run VARs.

Beyond responding to the concerns of these other papers, we further contribute to the literature by making concrete proposals for how to improve inference. We show that, compared with testing the impact response, testing the shape of the response is much more informative about what models are false. Furthermore, studying other variables such as investment (for flexible price models) or real wages (for sticky price models) can also be more informative. More generally, our results suggest that, to construct more powerful tests that are able to discriminate across models, it is best to examine those responses over which the models differ most, which often means going beyond the sign of the impact response of a particular variable. Given the results on the power and size properties of long-run VARs, we conclude that these VARs are useful for discriminating between the DSGE models that we consider.

These results may seem to contradict Faust and Leeper (1997) who argue that, under the long run assumption, any test of an impulse response will reject false models only at a rate equal to the rejection rate of the true model. (Faust and Leeper 1997 p. 347). However, Faust and Leeper's claim of such weak power is only true when the possible set of data generating processes (DGP) is very general. Faust and Leeper themselves note that these long-run VARs would have better power properties, if the set of DGPs was suitably restricted. For example, assuming that the DGP must be a finite-ordered VAR with maximum lag length K (where K is a fixed and finite number) would be a sufficient restriction to imply that these long-run VARs do have power greater than size. However, most DGE models imply that the data generating process is an infinite-order VAR (see the current paper's Section 4), as such Faust and Leeper's result of a long-run VARs having power for finite-ordered VAR is not applicable. As such, the current paper expands the set of models for which long-run VARs have power. (See Section 4 for more details.)

The next two sections describe how to estimate a long-run VAR and the DSGE models that are used as data generating processes. These sections should be familiar to readers of the Erceg, Guerrieri, and Gust (2005) [EGG], CKM, and CEV papers. Section 4 reviews the challenges resulting from adopting the long-run identification assumption. Section 5 presents the simulation results for flexible price models. Section 6 presents the results for sticky price models. Section 7 reports on an empirical application of the methods to U.S. data. Section 8 concludes.

2 Estimating A Vector Autoregression with A Long Run Identification Assumption

Here, as in Galí (1999), a technology shock is identified as a permanent shock to productivity. These shocks and resulting impulse responses are computed in the following manner. Consider a vector of variables Y_t that consists of n elements. The first element is the growth rate of labor productivity, denoted by Δa_t. The next $n-1$ elements are x_t, the other variables to be studied. As such Y_t can be written as

$$Y_t = \begin{pmatrix} \Delta a_t \\ x_t \end{pmatrix}. \tag{1}$$

The time series for Y_t can be described by the following structural vector autoregression (VAR) :

$$A_0 Y_t = A(L) Y_{t-1} + \begin{pmatrix} \varepsilon_t^z \\ v_t \end{pmatrix} \tag{2}$$

The fundamental shocks ε_t^z and v_t (where v_t has $n-1$ elements) are assumed to be independent, have mean zero, and have variances equal to one. Given this structural VAR, we can invert A_0 to construct the reduced form VAR,

$$Y_t = A_0^{-1} A(L) Y_{t-1} + A_0^{-1} \begin{pmatrix} \varepsilon_t^z \\ v_t \end{pmatrix}, \tag{3}$$

where the reduced form VAR coefficients $A_0^{-1}A(L)$ are denoted by $B(L)$ and the reduced form errors are denoted by u_t. For notational simplicity let C denote A_0^{-1}. The mapping between structural shocks and reduced form errors is

$$u_t = C \begin{pmatrix} \varepsilon_t^z \\ v_t \end{pmatrix}. \tag{4}$$

Denote the variance covariance matrix of u_t, $Eu_t u_t'$ by V and note by assumption that V equals CC'. As such the reduced form is the following:

$$Y_t = B(L) Y_{t-1} + u_t. \tag{5}$$

Galí (1999) identifies the technology shock by assuming that only the technology shock ε_t^z can have a permanent effect on the level of productivity z_t. All other shocks are assumed to have no long-run effect. This restriction is referred to the exclusion restriction as it excludes the other shocks from having any long run effect on the level of productivity. This restriction imposes a restriction on the moving average representation of the data. Denote the moving average representation by:

$$Y_t = [I - B(L)]^{-1} C \begin{pmatrix} \varepsilon_t^z \\ v_t \end{pmatrix}. \tag{6}$$

The exclusion restriction implies that each element in the top row of the sum of moving average coefficients equals zero except for the first element. In other words, we have the following restriction

$$[I - B(1)]^{-1} C = \begin{bmatrix} C_{11} & \underline{0} \\ \text{numbers} & \text{numbers} \end{bmatrix}, \tag{7}$$

where $\underline{0}$ is a row vector. To identify C_{11} requires the additional restriction that a positive technology shock increases labor productivity which implies that C_{11} is positive. No additional restrictions are required.

To compute the dynamic effects of ε_t^z, we require $B_1, ..., B_q$ and C_1, the first column of C. The symmetric matrix, V, and the B_i's can be computed using ordinary least squares regressions. However, the requirement that $CC' = V$ is not sufficient to determine a unique value of C_1. There are many matrices, C, that satisfy $CC' = V$ as well as the exclusion and sign restrictions. However, in all cases, the first column, C_1, of each of these matrices is the same. In particular, we can compute a C that satisfies these restrictions as the following

$$C = [I - B(1)] D \tag{8}$$

where D is the lower triangular matrix such that

$$DD' = [I - B(1)]^{-1} V [I - B(1)']^{-1} = S_Y(0). \tag{9}$$

In equation (9), $S_Y(\omega)$ denotes the spectral density of Y_t at frequency ω that is implied by the q^{th} order VAR. The use of the spectral density at frequency zero to identify a technology shock is closely connected to the critique of Faust and Leeper (1997) and will be discussed in a subsequent section.

3 Models

The DSGE model presented here is very similar to the model presented in Christiano, Eichenbaum, and Vigfusson (2006). The model, however, has two additional features. The first is the addition of habit persistence in the utility function. Thus, the previous period's level of consumption affects current utility. Habit persistence results in a slower response of consumption. The second feature is adding investment adjustment costs to the model. Increasing investment is expensive and therefore an economic agent will have an incentive to smooth out investment. Christiano, Eichenbaum, and Evans (2005) use a similar specification to generate improved dynamics in a sticky price model.

3.1 The Utility Function

The model has a representative agent who chooses consumption C and the fraction of time spent working H to maximize utility, where utility is defined as

$$E_t \sum_{j=1}^{\infty} \left(\beta \left(1 + \mu_p\right)\right)^j \left(\log\left(C_{t+j} - bC_{t+j-1}\right) + \eta \log\left(1 - H_{t+j}\right)\right). \tag{10}$$

The coefficient b describes the degree of habit persistence in the model. The parameter β is the discount rate, μ_p is the growth rate of population, and η controls the trade-off between consumption and leisure. The agent maximizes utility subject to the budget constraint that consumption and investment I_t must equal the return r_t from capital K_t and income from working $(1-\tau_{lt})\, w_t H_t$:

$$C_t + (1+\tau_{x,t})\, I_t \leq (1-\tau_{lt})\, w_t H_t + r_t k_t. \tag{11}$$

The capital accumulation equation is the following

$$\left(1+\mu_p\right) K_{t+1} = (1-\delta)\, K_t + \left(1 - S\left(\frac{I_t}{I_{t-1}}\right)\right) I_t, \tag{12}$$

where S is the function that determines the cost of changing investment. The value of S and its first derivative are zero along a steady state growth path and the parameter γ denotes the second derivative of S evaluated in steady state.

The production function is standard

$$Y_t = K_t^{\alpha} \left(Z_t H_t\right)^{1-\alpha}, \tag{13}$$

where Z_t denotes the level of technology. The economic resource constraint is

$$Y_t = C_t + I_t.$$

There are three shocks.

$$\begin{aligned} \log z_t &= \mu_Z + \sigma_z \varepsilon_t^z & (14)\\ \tau_{lt} &= (1-\rho_l)\, \bar{\tau}_l + \rho_l \tau_{lt-1} + \sigma_l \varepsilon_t^l & (15)\\ \tau_{xt} &= (1-\rho_x)\, \bar{\tau}_x + \rho_x \tau_{xt-1} + \sigma_x \varepsilon_t^x & (16) \end{aligned}$$

where z_t equals the growth in technology Z_t/Z_{t-1}. Each of the shocks $\varepsilon_t^z, \varepsilon_t^l$, and ε_t^x is independent and identically distributed with mean zero and variance equal to one. The values of $\mu_Z, \bar{\tau}_l$,and $\bar{\tau}_x$ are the average values of the shocks. One could describe the shocks τ_{lt} and τ_{xt} as labor and capital tax rates respectively. However, estimation that matches the model to observed non-tax variables implies that these variables τ_{lt+1} and τ_{xt+1} are much more variable than observed labor and capital tax rates. The values of the auto-regressive parameters ρ_l and ρ_x are both constrained to be less than one.

3.2 Sticky Price Model

The simulation reported in Section 6 are done with the flexible price model described above. In Section 7, we conduct simulations with a sticky price model, and we briefly describe here how it modifies the flexible price model.

We assume that prices are set in Calvo-style staggered contracts similar to Erceg, Henderson, and Levin (2000). In particular, there is a continuum of differentiated goods, Y_{it}, indexed by $i \in [0,1]$ that are combined to produce the "output index" according to:

$$Y_t = \left(\int_0^1 Y_{it}^{\frac{1}{1+\xi}} di \right)^{1+\xi}, \tag{17}$$

with $\xi > 0$. The output index is produced by a representative firm whose demand for each of the differentiated goods is given by:

$$Y_{it} = \left(\frac{P_{it}}{P_t} \right)^{\frac{-(1+\xi)}{\xi}} Y_t, \tag{18}$$

where P_{it} denotes the price of differentiated good i and the aggregate price index, P_t, satisfies

$$P_t = \left(\int_0^1 P_{it}^{\frac{-1}{\xi}} di \right)^{-\xi}. \tag{19}$$

Each of the differentiated producers have the production function:

$$Y_{it} = K_{it}^{\alpha} \left(Z_t H_{it} \right)^{1-\alpha}. \tag{20}$$

Capital and labor are completely mobile across these producers so that all firms have the same marginal cost:

$$MC_t = \frac{W_t H_t}{(1-\alpha) Y_t}. \tag{21}$$

A differentiated goods producer is a monopolist who face the constant probability, $1-\theta$, of being able to reoptimize its price. This probability is assumed to be independent across time and firms. When firm i is able to reoptimize its contract price, the firm maximizes:

$$E_t \sum_{j=0}^{\infty} \psi_{t,t+j} \theta^j \left(P_{it} - MC_{t+j} \right) Y_{it+j}, \tag{22}$$

where $\psi_{t,t+j}$ is the state-contingent discount factor.[3]

The inclusion of sticky prices requires us to specify a monetary policy rule. We assume that the central bank adjusts the quarterly nominal interest rate in response to inflation and the output gap:

$$i_t = \gamma_i i_{t-1} + \gamma_\pi \pi_t + \gamma_y \tilde{y}_t + \tau_{mt}, \tag{23}$$

where $\pi_t = \log(\frac{P_t}{P_{t-1}})$, $\tilde{y}_t$ is the log-level of output expressed as a deviation from steady state, and τ_{mt} is a monetary policy innovation.[4] The monetary policy innovation evolves as

$$\tau_{mt} = \rho_m \tau_{mt} + \sigma_m \varepsilon_t^m$$

[3]For convenience, we have suppressed the state-dependent nature of $\psi_{t,t+j}$. In equilibrium, $\psi_{t,t+j}$ is equivalent to the price a household pays for a claim in period t that pays one dollar if the corresponding state occurs in period $t+j$, normalized by the probability that state occurs.

[4]The constant term in the policy rule has been suppressed for simplicity.

where ε_t^m is independent and identically distributed with mean zero and variance equal to one.

In the sticky price model, we also allow for a shock to government spending. To do so, we modify the resource constraint so that $Y_t = C_t + I_t + G_t$ where $G_t = g_t Z_t$ and g_t evolves according to:

$$g_t = (1 - \rho_g)g + \rho_g g_{t-1} + \sigma_g \varepsilon_t^g. \tag{24}$$

In the above, g denotes the steady state value of g_t and ε_t^g is independent and identically distributed with mean zero and variance equal to one.

4 The Problem with Long-Run VARs.

There are two problems when you estimate a long-run VAR using data simulated from a DSGE model. The first problem is that the true data generating process is not a finite-ordered VAR; rather, it is a infinite-ordered VAR model. This problem is applicable to all VARs and was the focus of the criticism by CKM. The second problem, applicable to the estimation of long-run VARs, is the challenge in estimating the spectrum at frequency zero which is the basis of the Faust and Leeper critique.

To understand the first problem, express the log-linear solution of the DSGE model as the following equations

$$\begin{aligned} \xi_t &= F\xi_{t-1} + G\varepsilon_t & (25) \\ Y_t &= H\xi_t & (26) \end{aligned}$$

where ξ_t are the model's state variables (such as capital), ε_t are the fundamental shocks,

and Y_t are the model's observed variables (such as investment and hours worked). The autoregressive nature of the first equation is without loss of generality as we can stack variables in the state variable vector (such as k_{t+1}, k_t, and k_{t-1}).

Given this system of equations, one can derive the following infinite-ordered VAR for the observed variables Y_t (CEV 2006). The data generating process for Y_t is

$$Y_t = HF\left(I - ML\right)^{-1} GC^{-1}Y_{t-1} + C\varepsilon_t \tag{27}$$

where L is the lag operator and the following matrices are defined

$$\begin{aligned} C &= HG & (28) \\ M &= \left(I - DC^{-1}H\right)F & (29) \end{aligned}$$

Two additional assumptions are required for equation (19) to hold. The first assumption is that the matrix C be square and invertible. For C to be square requires that there must be as many economic fundamental shocks as there are observed variables. If

there were fewer economic shocks than observed variables, then the variance covariance matrix of Y_t would be singular.[5] The second assumption is that M^j converges to zero as j goes to infinity. This assumption rules out explosive solutions. If we assume that $B(L)$ denotes the infinite-ordered polynomial for the autoregressive terms on Y_t, then we have that

$$B(L) = HF(I - ML)^{-1} GC^{-1}. \tag{30}$$

Given this definition, the jth term of $B(L)$, B_j, equals HFM^jGC^{-1}. For the system to be non-explosive, the value of M^j must converge to zero as j goes to infinity. Satisfying this requirement would imply that B_j converges to zero.

This infinite-ordered VAR is typically approximated by a finite order VAR $\hat{B}(L)$ of order p where $\hat{B}_q$ equals zero for all q greater than p. There has been some debate about the ability of a finite ordered VAR to approximate the dynamics of the infinite order VAR.[6] The short-run identification results in CEV (2006), however, suggest that a finite-ordered VAR can do fairly well at capturing the short-run dynamics. The individual estimated VAR coefficients $\left\{\hat{B}_i\right\}_{i=1}^{p}$ are close estimates of the individual population coefficients $\{B_i\}_{i=1}^{p}$ and, with a relatively small value of p, do a good job of minimizing the variance of the one-step ahead forecast errors. However, as was described in Sims (1972) and further discussed in CEV, the sum of the estimated coefficients $\hat{B}(1)$ (or equivalently $\sum_{i=1}^{p}\hat{B}_i$) may not be close to the true sum $B(1)$ ($\sum_{i=1}^{\infty}B_i$).

An inability to match the long-run sum is a particular problem for the long-run identification assumption since determining C, the mapping between reduced form shocks u_t and fundamental shocks ε_t, requires knowing the matrix D defined previously in equation (9),

$$DD' = [I - B(1)]^{-1} V [I - B(1)']^{-1} = S_Y(0).$$

Because the value of D is a function of $B(1)$, the inability of the sum of the finite-order VAR's coefficients $\hat{B}(1)$ to match $B(1)$ is a problem particular to the long-run identifying assumptions.

As was mentioned in the introduction, a discussion of power of long-run VARs may appear pointless given Faust and Leeper's proposition that any test of an impulse response identified with a long-run restriction has significance level greater than or equal to maximum power. (Faust and Leeper 1997 p. 347). The identification of the long-run VAR depends on knowing the matrix D. The matrix D is a function of the spectrum at frequency zero and knowing the spectrum at frequency zero is what underlies Faust

[5] When there are more shocks than variables, one can still derive a VAR. However, identifying all the shocks becomes more difficult. See Sims and Zha (2006) for more details.

[6] The debate over using a finite VAR is present in the aforementioned EGG, CKM and CEV papers. Because of this debate, some have suggested estimating instead models with both autoregressive and moving average components (VARMA models). However, as was described in Kascha and K. Mertens (2009), the difficulties in estimating these VARMA models suggests that they do not offer much improvement.

and Leeper's claim about the problems with long-run VARs. As was discussed in Faust (1999), the confidence interval on a single point on the spectrum is unbounded because, under the assumption of a very general data generating processes, one can not rule out a spike at that single point. If one can restrict the DGP sufficiently to rule out these spikes, then the spectrum and hence the matrix D will be better behaved. For example, Faust and Leeper themselves note that fixing the DGP to be a finite-ordered VAR with maximum lag length K would be a sufficient restriction such that long-run VARs do have better power ptroperties. However, this restriction is not applicable when the DGP is a DSGE model since this class of models generally yields infinite-ordered VARs.

In the following sections, the set of data generating processes (DGP's) is restricted to the set of infinite-ordered VARs that arise from these DSGE models. For these DGPs, we will show that long-run VARs do have power to reject false null hypotheses at a rate greater than the size of the test. Although these DSGE models are not fully descriptive of the data, these models being infinite ordered do offer additional insight beyond that learned from the fixed lag VARs. As such, our paper's results expand the set of DGP's for which the long-run VARs do have power.

5 Model Calibrations and Simulation Experiments

To simulate data from the model requires values for the models parameters. To make the results reported here comparable to CKM (2008) and CEV (2006), most model parameters are set at values that they use. See Table 1 for the values of $\left\{\beta, \theta, \delta, \tau_x, \tau_l, \mu_p, \psi, \mu_z, \tau_l\right\}$. In the first set of simulations, the values of habit parameter b and the investment adjustment costs parameter γ are set equal to zero. For notional convenience, this benchmark flexible price model that has no real rigidities will be referred to as the **RBC model**. In subsequent simulations, we simulate data from a model with the coefficient of habit persistence b and the degree of investment adjustments costs γ fixed at the values ($b = 0.7$ and $\gamma = 3$) that are reported in Christiano, Eichenbaum and Evans (2005).

As in CEV(2006), the variance and auto-correlation of the model's shocks are estimated by standard maximum likelihood methods. Define the observed vector of variables to be the following

$$Y_t = \begin{pmatrix} \Delta y_t - \Delta h_t \\ h_t \\ i_t - y_t \end{pmatrix} \tag{31}$$

where $\Delta y_t - \Delta h_t$ is the growth rate of labor productivity, h_t is the level of per capita hours worked and $i_t - y_t$ is the ratio of investment to output expressed in logs. All data are from the United States for the period 1959 to 2001. Labor productivity and hours worked are measured for the business sector. The ratio of investment to output is measured using the nominal share of total investment in GDP. Given these observed variables and the model structure implied by equations (25) and (26). The model can then be estimated by applying the Kalman filter approach in Hamilton (1994, Section

13.4). Estimated model coefficients match those found in CEV (2006) and are reported in Table 1.

5.1 Simulation Evidence With Data Generated from a RBC model

All simulations are done 2000 times with a sample size of 200 observations. For each simulated data set, we estimate a three variable VAR where the three variables are the growth rate of labor productivity, the log level of per capita hours worked and the ratio of investment to output expressed in logs. For each VAR, we fixed the lag length at four. Based on past experience, applying more sophisticated algorithms for choosing lag length does not provide substantially different results. By applying the long-run identifying assumption, for each data set, we identify the responses to a one-standard deviation increase in technology.

For each simulated data set, we estimate a bootstrapped standard error by simulating the estimated VAR one thousand times where the vector of economic shocks at time s are drawn with replacement from the estimated set of residuals and the starting values come from that particular data set. The bootstrap standard deviation is estimated as the sample statistic coming from the distribution of the bootstrapped impulse responses.

Figure 1 reports, for the benchmark VAR estimated using data simulated from a RBC model, the response of hours worked to a permanent shock to labor productivity with size equal to one-standard deviation. The gray area indicates the sampling distribution of the estimated impulse responses. The edges of the gray area indicate the 5th and 95th percentile of all the estimated impulse responses. These intervals are wide which is typical of structural VARs that are identified with a long-run restriction (see CEV 2006).

Figure 1 also reports the true impulse responses from several parameterizations of flexible price DSGE models that have real rigidities in the form of investment adjustment costs and habit persistence. These other responses all lie within the gray area, which, as previously mentioned, indicates sampling uncertainty. One, therefore, might be tempted to conclude that these impulse responses are unable to discriminate between the different parameterizations and, as such, that the statistical test of the hours response has poor power.[7] The rest of this paper will show that a conclusion that these long-run VARs have poor power would be overly pessimistic.

Figure 2 reports a scatter plot of the estimated impact response of hours to a technology shock versus the corresponding estimated bootstrapped standard error for each of the 2000 simulations from the benchmark flexible price model. For any given simulation, we can determine whether an econometrician observing only that simulation would reject

[7] The argument on page 1339 in CKM seems to be a claim of poor power. However, CKM never report the estimated statistical sampling uncertainty that a researcher would estimate when faced with only a single data set. Rather they just report the distribution of the point estimates. In terms of our Figure 2, they report the distribution of the values on the x-axis but do not calculate the values on the y-axis.

the true null hypothesis that the hours response on impact matches the response from a RBC model. If the econometrician had assumed that the estimated impulse response has an asymptotic normal distribution centered around the true response, then she would use a standard critical value of 2 and falsely reject the true null hypothesis 18 percent of the time. Given that the rejection rate is greater than the nominal size of 5 percent, we calculate the critical value (2.8), where an econometrician observing only a single data set would correctly fail to reject the null hypothesis 95 percent of the time and reject the true model only 5 percent of the time.[8]

5.1.1 Looking At Power

Figure 3 reports the same scatter plot of estimated impulse responses and standard errors. However, Figure 3 reports the results for testing whether the estimated impulse response matches the response from a DSGE model with high levels of habit ($b = 0.7$) and investment adjustment cost ($\gamma = 3$). Even with the size-corrected critical values, the false model is correctly rejected 32 percent of the time. Using the standard critical value would lead to an even greater rejection rate of 53 percent. These results illustrate that one can find model parameterizations with much higher rejections rates than the worse case that is analyzed by Faust and Leeper.

Reporting the equivalent of Figure 3 for all possible parameterizations is not feasible, as such, rejection rates are summarized by Figure 4 which report for low degrees of habit (Figure 4a) and high degrees of habit (Figure 4b), the rejection rates for different values of γ. The rejection rates for the test of the impact response are indicated by the solid lines and are labelled *Sign*, reflecting this test's close relationship with previous papers that reported the distribution of the sign of the impact response. When γ equals 0 and b equals 0, then the rejection rate for the test of the impact response is the likelihood of rejecting the true model and, as we are using a size-corrected critical value, equals 5 percent by construction. When γ equals 3 and b equals 0.7, using the same size-corrected critical value, the rejection rate is 32 percent.

As can be seen in Figure 4, the test is much more likely to reject a false null hypothesis than a true null hypothesis. As such, the evidence implies that, by restricting the model to a plausible set of DSGE models, we gain power relative to the worse case scenario of Faust and Leeper. Moreover, the rejection rates increase with γ and b. However, even for parameterizations with a fairly large degree of habit persistence and investment adjustment costs, the test has a size-corrected rejection rate of under 40 percent.

To be able to discuss the usefulness of these long-run VARs, we need to determine whether the rejection rates reported in Figure 4 are high or low. Because statistical power is only infrequently reported, for comparison purposes, it would be useful to have a benchmark from the literature on statistical testing of DSGE models. One such statistic

[8] An alternative approach would be to experiment with the various proposed modifications of how to construct confidence intervals. However, as many different methods have been proposed, we leave explorations of the properties of these different methods for future work.

is the correlation of output growth.[9] The inability of the RBC model to match the correlation of output growth has been an important statistic in casting doubt on the basic RBC model. Papers that discuss the correlation of output growth include Cogley and Nason (1995) and Christiano and Vigfusson (2003).

Results for this unconditional statistic help put the performance of the impulse response analysis in context. Figure 4 also reports results for the output correlation (the dashed lines labelled *Correlation*). Given the use of a size-corrected critical value, the test of the correlation statistic rejects 5 percent of the time when γ and b both equal zero. The power of these correlation statistics are somewhat better than the impulse responses. The rejection rates increase as both γ and b increase. With no habit persistence in consumption, the rejection rates using the correlation are about 20 percent for models with moderate or high degrees of investment adjustment costs γ. For models with a high degree of habit, the correlations reject the false models much more frequently.

Overall, this evidence suggests that the power of testing using the correlation is better than testing using just the impact response of hours worked. The rest of this paper shows that other applications of VARs can be more informative. In particular, Figure 4 has an additional set of lines that, for certain parameterizations, have better rejection rates than the rejection rates from the correlation test. The next section describes these lines.

5.2 Shape of Hours

In this section, we show that, compared to a test of the impact response, a test of the shape of the response of hours worked to a technology shock can more frequently reject false models. Studying the shape of impulse responses has a rich tradition in the empirical VAR literature. For example, based on VAR evidence, many variables respond with a delay to monetary policy shocks. To match these delayed responses, researchers abandoned frictionless, New-Classical models that implied immediate responses and adopted models with rigidities. (For a discussion of these issues, see Woodford 2003, p. 173).

Figure 5 reports, for data generated from a standard RBC model, a scatter plot of the response of hours on impact and the change in the response six periods later.[10] Around

[9] The confidence intervals for the correlation statistic were constructed using the standard method from the Matlab Statistics Toolbox. Confidence intervals were constructed using the result that, for the correlation statistic ρ, the valu e of $\frac{1}{2}\log\left(\frac{1+\rho}{1-\rho}\right)$ is approximately normal with a variance equal to $\frac{1}{N-3}$. As with other statistics reported in this paper, the critical value for testing the correlation was size-corrected.

[10] The choice to looking at the response six periods later is somewhat arbitrary. One could imagine testing instead the period that creates the maximum change. However, a result-driven method such as this could result in exaggerating the response in a fashion analogous to the difference between a standard Chow test for parameter instability and the supremum Wald test of Andrews.

An additional extension would be to test the vector of responses between impact and many periods later. This route was not taken here in order to minimize the size of the V matrix that needed to be stored. However, an additional concern is that this larger test may be overly precisely and reject models that have the same rough shape as the empirical responses but that do not match period by period.

each of these responses, one could construct a confidence ellipse. Assuming that these responses are drawn from a multivariate normal distribution, then the formula for the confidence ellipse can be easily derived from a simple Wald test. Given an estimated vector $\hat{\mu}$ and a variance covariance matrix for $\hat{\mu}$ denoted by $\hat{V}$, then a point x lies in the 95 percent confidence ellipse if x satisfies the following inequality

$$[x - \hat{\mu}]' \hat{V}^{-1} [x - \hat{\mu}] < \xi_{95} \tag{32}$$

where ξ_{95} is the critical value. According to standard asymptotic theory, the statistic is distributed chi-squared with 2 degrees of freedom. To estimate the variance-covariance matrix $\hat{V}$, weuse the same method as we used to estimate the standard error. For each simulated dataset, the variance-covariance matrix $\hat{V}$ is estimated using a bootstrap simulation where the DGP is the estimated VAR using that specific dataset. As was done for the impact responses, the test needs to be size-corrected. In the simulations the value of ξ_{95} is 10 rather than the standard value of 6. To give some degree of the magnitude of the correction, Figure 5 reports, for one single simulation, the estimated confidence intervals using the two different critical values. In either case, one would fail to reject the true null hypothesis that the response matches the response from the RBC model (the triangle). Although, the size-corrected confidence set is much wider than the non-size corrected interval, for this particular simulation, one would reject the false null hypothesis that the response matches the response from a model with high degrees of habit and investment adjustment costs (the square).

Returning to Figure 4, we can compare the power properties of the shape test versus the power properties of the tests of the impact response and the correlation. The rejection rates for tests using the shape of hours are much better than the rejection rates for tests using the sign and are comparable to rejection rates testing the correlation of output growth.

5.3 Investment Response

As was mentioned in CEV, the variance of hours explained by technology shocks is very low. In the benchmark parameterizations studied here, technology shocks account for less than 1 percent of the variance of hours. However, technology shocks do account for 22 percent of the variance of the ratio of investment to output. As such, a natural question to ask is whether looking at the response of investment rather than hours is more informative. We report below that the investment response does appear to be more informative.

Figure 6 plots the impulse responses estimated for investment from the same benchmark three variable VAR (with variables: labor productivity growth, hours worked and investment to output ratio). The various model responses again lie within the large gray area. Also of note is that, unlike with hours, the investment response is not monotonically increasing with respect to both habit and investment adjustment costs. For a given degree of habit persistence, the size of the investment response declines as the investment

adjustment costs increase. However, for a high degree of habit persistence, the investment response is larger than would be the case with a low degree of habit. The economics behind this reversal is that, with a high degree of habit, the utility maximizing behavior is to invest more in order to smooth consumption.

Figure 7 reports the power properties of the two tests of investment and also reproduces from Figure 4 the rejection rates when one tests using the correlation of output, (the dashed lines). As indicated by the lines with circles, the shape of the investment response (again measured using the impact response and the response six periods later) seems more useful than the impact response (the solid line). Furthermore, both tests appear to be quite useful in discriminating between models.

5.4 Changing the True Data Generating Process

For the results reported above, the true data generating process is the RBC model. Given data simulated from the RBC model, we reported the rejection rate of the false null hypothesis that the data was generated from models with real rigidities. Of course, we are also interested in the opposite case where the data are simulated from a model with real rigidities and the false null hypothesis of the RBC model is tested. Reversing the role of the two models is particularly relevant as most empirical work favors models with various degrees of adjustment costs. (See ACEL and Smets and Wouters (2003) for examples).

For this exercise, we generate data from the model with high investment adjustment costs and habit persistence with values of γ and b set at the values estimated in Christiano, Eichenbaum and Evans (2005). Using data generated from this model, the statistical tests frequently reject the standard RBC model. The rejection rates are much greater than the rejection rates observed when data are simulated from the RBC model and the test is of the model with high adjustment costs.

Figure 8 reports the impulse response of hours worked. Given the high level of costs associated with adjusting either the level of consumption or investment, a technology shock actually drives down hours. The improvement in productivity causes the consumer to increase leisure rather than increase consumption or investment. The average estimated response is somewhat biased away from the true response. However, using the standard critical value, the rejection rate for testing the impact response compared to the true model impact response is 24 percent which is similar to the results presented above. As can be seen in Table 2, when using the size-corrected critical value, the power of the test to reject the impact response from the now false RBC model is 58 percent. Comparing these results with the results from Figure 4, the test of the impact response is much more powerful when the data are simulated from the model with real rigidities than when the data are simulated from the RBC model. Table 2 reports the power of testing the shape of the hours response using the size-adjusted critical values. By looking at the shape of the hours response, the false RBC model is rejected more than 90 percent of the time. Table 2 also reports the results for testing the investment response. For data

simulated from this model with high real rigidities, as was the case for data simulated from the RBC model, the size and power properties are better for testing investment than testing hours. Using the investment response, one can almost always reject the RBC model. Finally Table 2 also reports a test using the correlation of output growth. Here too the statistic has a high degree of power.

Table 3 and Table 4 summarizes across several different parameterizations to reject the RBC model. For low levels of habit, testing the initial period of the hours response is not very informative but testing the shape of the hours response can be very informative. When there is no habit, the initial investment response is also not very informative. In models with a higher degree of habit, the initial investment response is much more informative.

Table 4 presents the values of the critical values that result in tests with correct size. For each test statistic, the critical values are fairly constant across parameterizations. As such, it seems reasonable to suppose that using the average critical value from this table is a good way to size correct when the true data generating process is unknown.

6 Simulation Results For Sticky Price Models

In this section, we present results for a model with sticky prices with a moderate degree of price stickiness ($\theta = 0.75$), habit (b=0.4), and investment adjustment costs ($\gamma = 2$). Given these model parameters and other calibrated values reported in Table 1, we estimated the sticky price model using the U.S. data. Using these coefficient estimates which are also reported in Table 1, we simulate data from this sticky price model and then look at rejection rates for various models. In this section, we focus on the Calvo adjustment parameter θ which determines the degree of price stickiness.

Table 5 reports results for a four variable VAR of labor productivity growth, hours worked, the investment-to-output ratio and real wages minus labor productivity (i.e. we impose the cointegrated relationship between real wages and labor productivity). As with the previous section, we did have some problems with modest size distortion and, as such, had to increase critical values to get the size of the test right, resulting in wider confidence intervals. The rejection rates are reported for various values of θ, which controls the degree of price stickiness. Low values of θ are associated with less sticky prices.

High values of adjustment costs imply that regardless of the degree of price stickiness, it is hard to differentiate amongst models using data on either hours worked or investment adjustment costs. The responses by hours worked and investment are similar for high values of investment adjustment costs, regardless of the value of θ or b. One however, can get better rejection rates by looking at wages. The rejection rates are much higher for values of θ that are far from the true DGP.

The results from this section support the message from the previous section. In terms of discriminating amongst model parameterizations, it is best to examine those responses over which the parameterizations differ most. As such, to discriminate amongst models

that differ in the degree of habit persistence or in the adjustment costs of investment, the investment response is most informative. To discriminate amongst models that differ to the degree of nominal rigidity, the wage response is most informative.

7 Empirical Application

As an empirical application, we take a VAR similar to that estimated in Christiano Eichenbaum and Vigfusson (2003) and ask what parameterizations can be ruled out and which can be allowed.

The VAR is the same three variable system used above in terms of labor productivity, hours worked and the ratio of investment to output. The difference is that rather than the data being simulated from a RBC model, the data are the empirical data from the United States. In particular, productivity is measured as hourly labor productivity in the business sector, hours worked is per capita business hours worked, and investment is the ratio of private nonresidential investment to GDP. In this section, hours enters the VAR in levels. The discussion of how to treat hours and other low frequency movements in estimating these VARs is beyond the scope of the current paper and is instead addressed in Christiano Eichenbaum and Vigfusson (2003). The sample period is 1954 to 2001.

Empirical responses are reported in Figure 9a, 9b, and 9c. In addition, for comparison sake, model impulse responses are reported for a few model parameterizations. A 95 percent confidence interval is constructed for the empirical responses using the estimated bootstrap error and a size-adjusted critical value that is the average of the values reported in Table 5. Given the width of the confidence interval, we fail to reject most of the models. We can reject the model with large real rigidities. However, models with only slightly less real rigidities would not be rejected. In contrast, Figure 10a, shows that, even when using the large size-adjusted critical value, a test based on the shape of the hours response does lead to a clear rejection of the large real rigidities. Likewise, the investment response leads us to reject a model with no real rigidities. Hence, although the sign is almost uninformative in this application, the shape of the responses is informative.

Based on these results, the most promising way to model the response to a technology shock is to allow for delayed hump-shaped responses to the technology shock. Future work will be directed towards determining whether these delayed responses are best modeled as the result of nominal or real rigidities.

8 Conclusions

In order to discriminate between macro models, a statistical test should infrequently reject true models and frequently reject false models. The earlier literature focused on how frequently one would reject true models. These results are suggestive that long-run VARs have the potential to be useful tools. However, we could not conclude that these

tests are actually useful without answering the equally important question of how often one would reject false models. The current paper does address this question.

Impulse responses from long-run VARs can reject false models. As expected, these rejection rates increase the further away the false model is from the true data-generating model. In addition, these rejection rates vary depending on what variables are studied. Overall, however, this paper shows that these long-run VARs can be informative about which models are to be preferred. For the models studied here, testing the shape is a more powerful test than simply looking at the sign of the response. In addition, relative to an alternative statistical test based on sample correlations, we find that the shape-based tests have greater power.

These results should encourage us to find creative and new ways to test our models. The conclusion is not to abandon our existing tools but to find ways to improve their use. Already several papers have explored methods to improve estimation with the long-run identification assumption including Feve and Guay (2009), Gospodinov (2008), Kascha and K. Mertens (2009), and E. Mertens (2008). Using these alternative methods, researchers may be able to improve on the results reported here. Overall, given these results on the power and size properties of long-run VARs, we conclude that these VARs can be useful for discriminating between macro models and, therefore, should continue to be used in developing and testing business cycle theory.

A Understanding why the size correction is needed

To get the size correct, we need a larger critical value than has been used in applied literature. This section shows that this need for a larger-than-standard critical value is directly related to the persistence of hours worked. As has been discussed in the literature, the sampling properties of the impulse responses identified from a long-run VAR will be non-standard if hours worked has a unit root (CEV 2003) or near-unit root (Gospodinov 2008). Although hours worked is stationary in the RBC model, hours is highly persistent. In particular, consider the results from estimating the following covariate-augmented Dicky Fuller test which was featured in CEV.

$$\Delta h_t = a + \beta h_{t-1} + \sum_{i=1}^{p} \gamma_i \Delta h_{t-i} + \sum_{i=1}^{p} \zeta_i x_{t-i}$$

where x_{t-i} are other covariates. (in the current application it is just productivity growth). An F-test of whether β equals zero is both a unit root test and also a test of whether hours worked h_{t-1} is a weak instrument for hours growth Δh_t [11]

The top panel of Figure A reports the distribution of the f-test.[12] For most of the simulated data sets studied here, an econometrician would actually fail to reject the null hypothesis that hours worked is a weak instrument. The distribution of the F-test illustrates how the RBC model fails to model the dynamics in the actual U.S. data. In the actual U.S. data, the F-test has a value well about 10, which is quite unlikely for the RBC model. Hence, the average simulation from the RBC model is actually a much less suitable data generating process than is actual U.S. data for applying the long-run VAR estimation. Even though hours worked is persistent, first differencing hours does not solve the problem. As was shown before in EGG, CEV (2003) and CKM, first-differencing hours results in a very biased estimate of the hours response. As was documented in Gospodinov (2008), first-differencing hours can remove an important low frequency comovement between hours and labor-productivity growth. As such, even when hours is very persistent, estimating the VAR in levels works better than estimating the VAR with hours in first differences.

In the bottom panel, Figure A reports a scatter plot of the f-statistic for weak instruments relative to the t-statistic of testing the true hours response. Clearly the distribution of the t-statistic depends on the value of the f-statistic. Table A reports the ninety fifth percentile of the absolute value of the t-statistic conditioning on the value of the f-statistic. This table suggests a simple rule to correct the t-statistic. If the F-statistic is less than 10, then use a critical value of 3. If the F-statistic is more than 10, then use a critical value of 2. This simple rule of thumb approximates a more complex procedure that would adjust the confidence interval depending on the strength of the instruments.

[11] For VARs with more than two variables, we will have to use a multivariate test like the Cragg Donald test to assess the weakness of all the instruments.

[12] These results are reported for 40,000 simulated data sets.

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Tables

Table 1: Model Parameter Values

Calibrated			
β	$0.98^{1/4}$	ψ	2.5
θ	0.33	μ_p	$1.01^{1/4} - 1$
δ	$1 - (1 - .06)^{1/4}$	μ_z	$1.016^{1/4} - 1$
τ_x	0.3	τ_l	0.242
ξ	0.2	θ	0.75
γ_i	0.75	γ_π	1.5
γ_y	0.125		
Estimated	for flexible price model		
σ_z	0.00968		
σ_l	0.00631	ρ_l	0.9994
σ_x	0.00963	ρ_x	0.9923
Estimated	for sticky price model		
σ_z	0.0125		
σ_l	0.0195	ρ_l	0.9663
σ_x	0.0382	ρ_x	0.7195
σ_m	0.0026695	ρ_m	0.19437
σ_g	0.0512	ρ_g	0.8543

Table 2: Size and Power when DGP is DSGE Model with High Adjustment Costs

Test	Rejection Rate		Critical
	RBC Model Size Adjusted*	True Model Not-size Adjusted	Value
Impact Hours Response	58	23	3.2
Shape of Hours Response	99	29	16.5
Impact Investment Response	100	10	2.4
Shape of Investment Response	100	13	9.84
Correlation of Output Growth	100	10	2.55

*Rejection Rates are for the size-adjusted critical values given in column (iii)

Table 3: Rejection Rates of the RBC Model (Percent)

Model Parameters		Test Statistic				
		Hours		Investment		Output
b	γ	Sign	Shape	Sign	Shape	Correlation
0	0.5	5	81	12	100	0
0	1.5	8	100	21	100	1
0	3	11	100	24	100	0
0.5	0.5	20	43	70	88	79
0.5	1.5	33	100	85	100	92
0.5	3	41	100	87	100	92
0.7	0.5	27	25	83	58	95
0.7	3	58	100	89	100	100

Results are reported for tests done on data simulated using the macro model described in the paper with parameters in the first two columns on the left. For each set of model parameters, 2000 simulations are done. For each parameterization, size-adjusted critical values are used to test the false null hypothesis that the data were generated by an RBC model.

Table 4: Size Adjusted Critical Values

Model Parameters		Test Statistic				
		Hours		Investment		Output
b	γ	Sign	Shape	Sign	Shape	Correlation
0	0	3.20	16.5	2.4	9.84	2.55
0	0.5	2.96	10.80	2.36	8.38	2.96
0	1.5	2.81	10.60	2.27	8.36	3.11
0	3	2.75	9.93	2.31	8.78	3.06
0.5	0.5	2.77	10.56	2.36	8.12	2.64
0.5	1.5	2.83	12.55	2.32	9.06	2.64
0.5	3	2.87	13.42	2.40	9.34	2.63
0.7	0.5	2.87	11.65	2.52	9.13	2.56
0.7	3	3.20	16.47	2.39	9.85	2.55

Table 5: Rejection Rates (in Percent) For Various Degrees of Price Stickiness θ

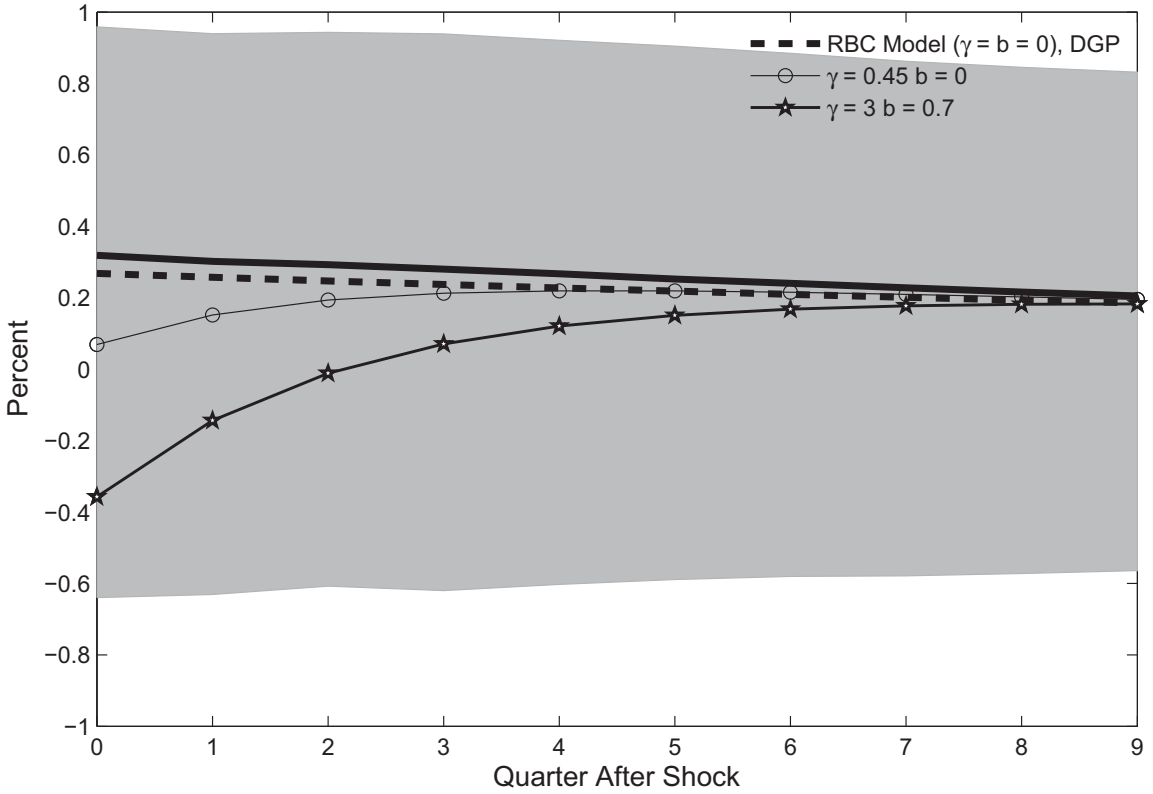
Other parameters	b = 0.4	$\gamma = 2$			
θ	0.15	0.35	0.55	0.75*	0.9
Hours Impact	2	2	3	5	8
Hours Shape	2	2	3	5	8
Investment Impact	5	5	5	5	8
Investment Shape	17	12	9	5	10
Wage Impact	32	20	11	5	4
Wage Shape	22	13	8	5	4
Other parameters	b = 0.4	$\gamma = 0$			
θ	0.15	0.35	0.55	0.75	0.9
Hours Impact	9	3	11	100	100
Hours Shape	13	3	13	100	100
Investment Impact	73	27	19	100	100
Investment Shape	61	23	26	100	100
Wage Impact	24	17	6	29	94
Wage Shape	17	11	6	43	100
Other parameters	b = 0.8	$\gamma = 4$			
θ	0.15	0.35	0.55	0.75	0.9
Hours Impact	5	5	6	8	9
Hours Shape	5	5	6	7	9
Investment Impact	5	5	5	5	6
Investment Shape	19	13	10	8	9
Wage Impact	37	23	14	6	5
Wage Shape	27	16	10	6	4

* denotes DGP parameterization. Critical Values chosen to insure correct size.

Table A: Critical Values of The T-statistic of Impact Response of Hours Conditional on a Test of Hours Being a Weak Instrument

F-statistic of Hours Being A Weak Instrument	Average Bias	Absolute Value of T-statistic 95th Percentile	Fraction of Observations (percent)
(0,1)	1.31	3.12	15
(1,2)	1.12	3.15	14
(2,3)	0.87	2.96	15
(3,4)	0.74	2.90	11
(4,5)	0.56	2.75	9
(5,6)	0.45	2.52	7
(6,7)	0.37	2.43	5
(7,8)	0.26	2.30	3
(8,9)	0.21	2.26	2
(9,10)	0.12	2.10	2
(10,11)	0.13	1.93	2
(11,12)	-0.06	1.90	1
>12	-0.13	1.97	2

Figure 1: The response of hours worked to a technology shock estimated using data simulated from a RBC Model



Note Thick solid line is average response over 2000 estimated responses using data simulated from a RBC model. Edges of grey area indicate 5th and 95th percentiles of all estimated responses to a one−standard−deviation technology shock

Figure 2: Testing The Impact Response of Hours.

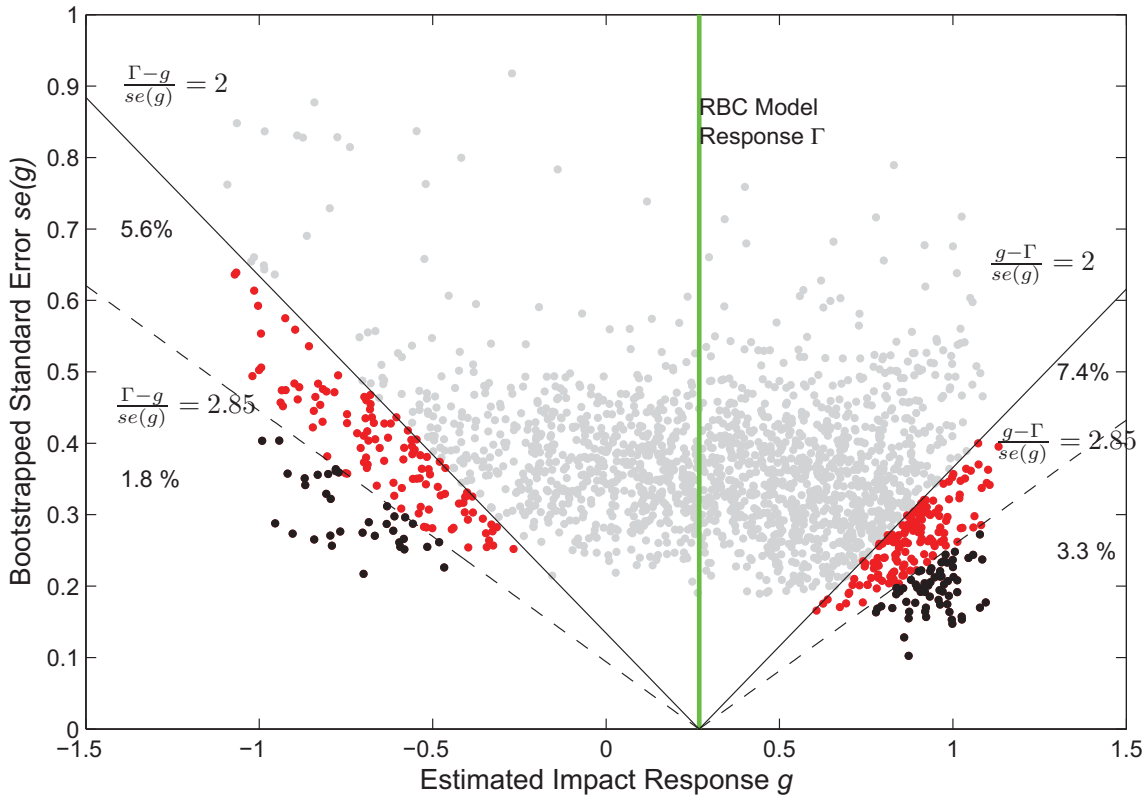


Figure 3: Testing The Impact Response of Hours using a false null hypothesis

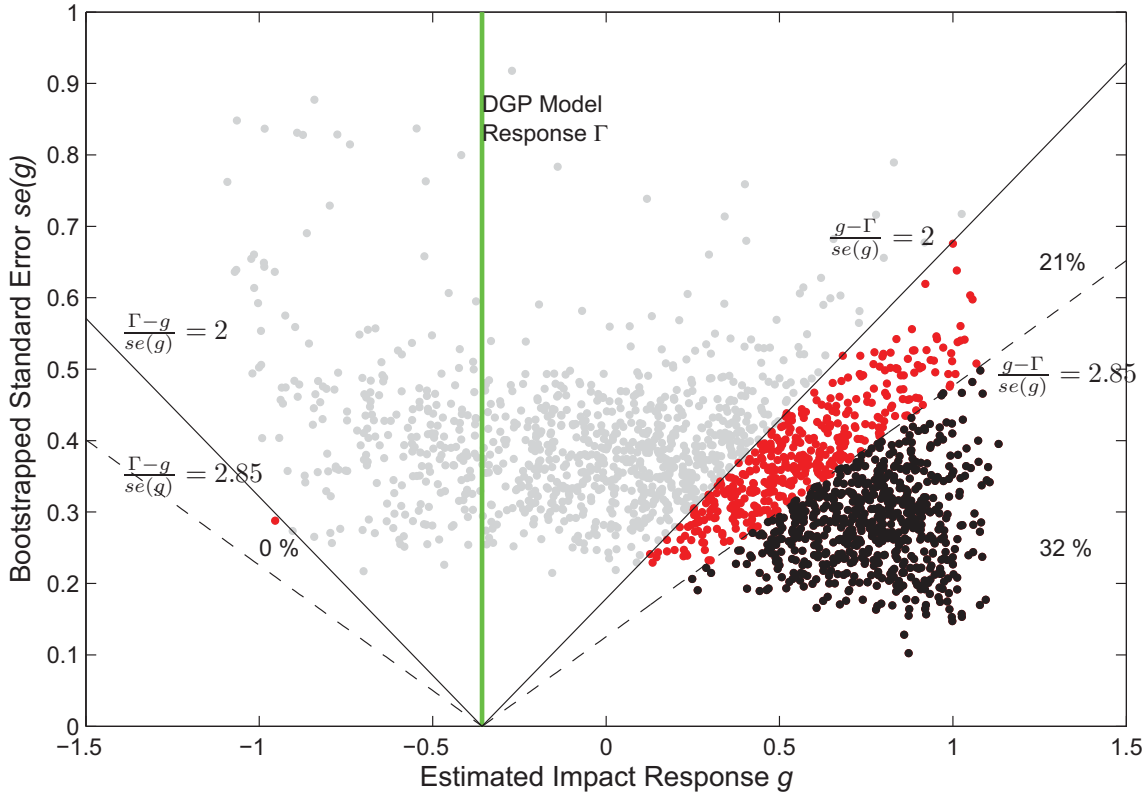


Figure 4 Rejection Rates For Different Tests when True DGP is RBC Model.

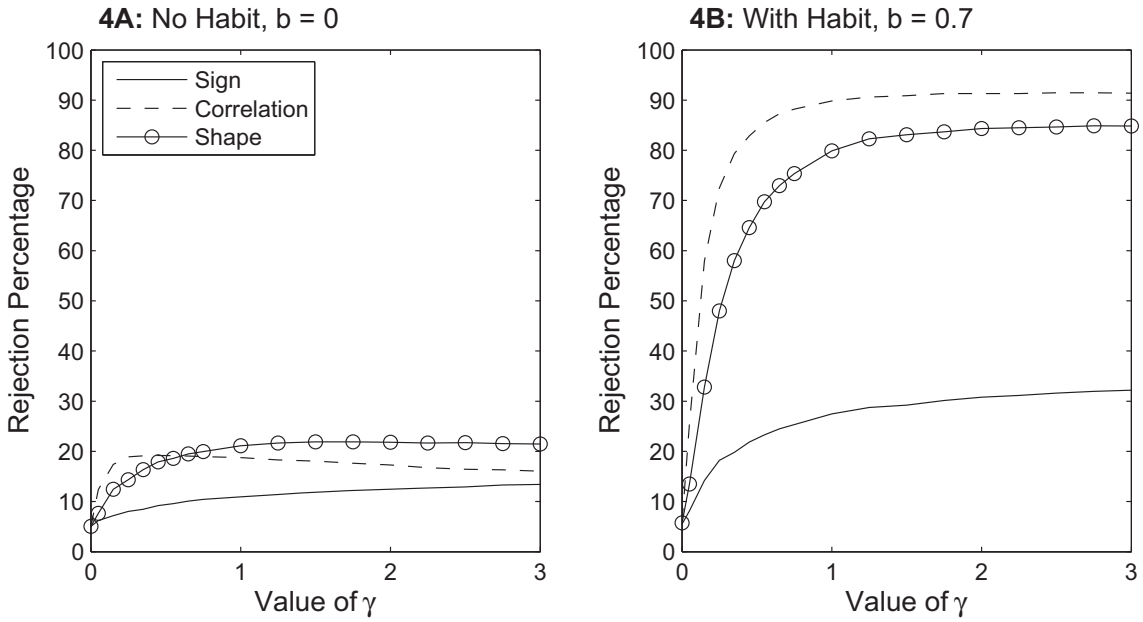
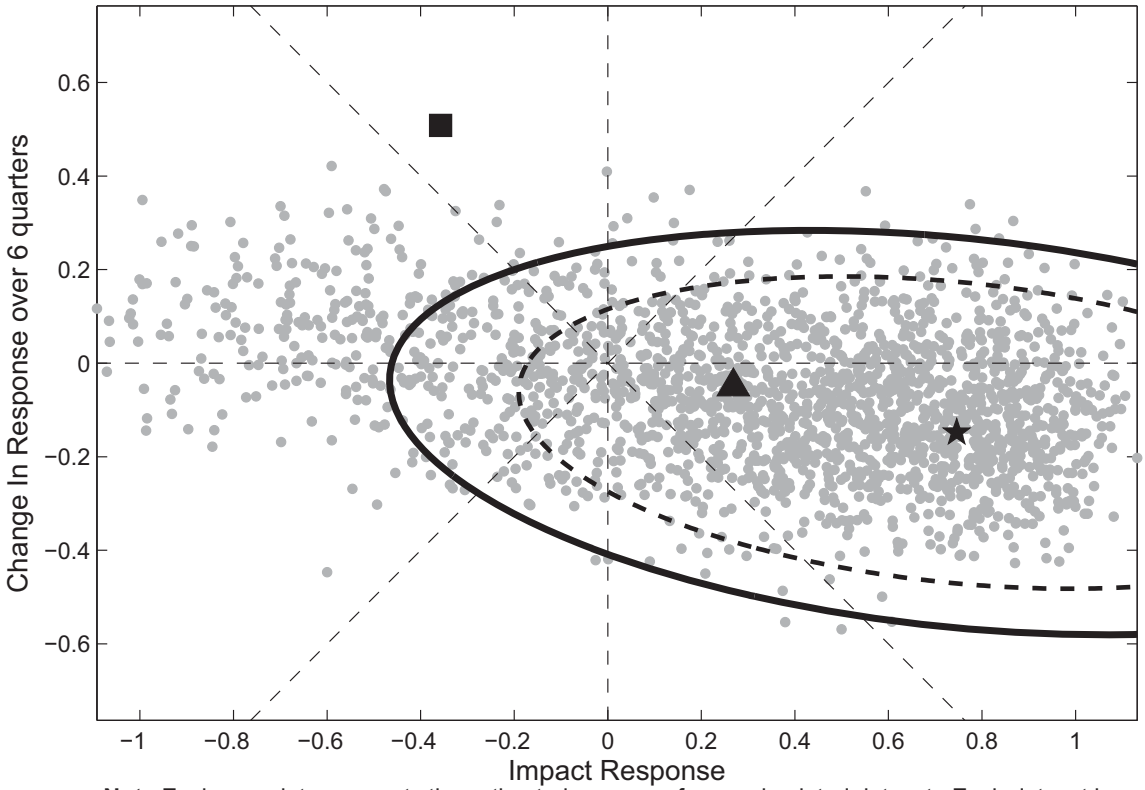
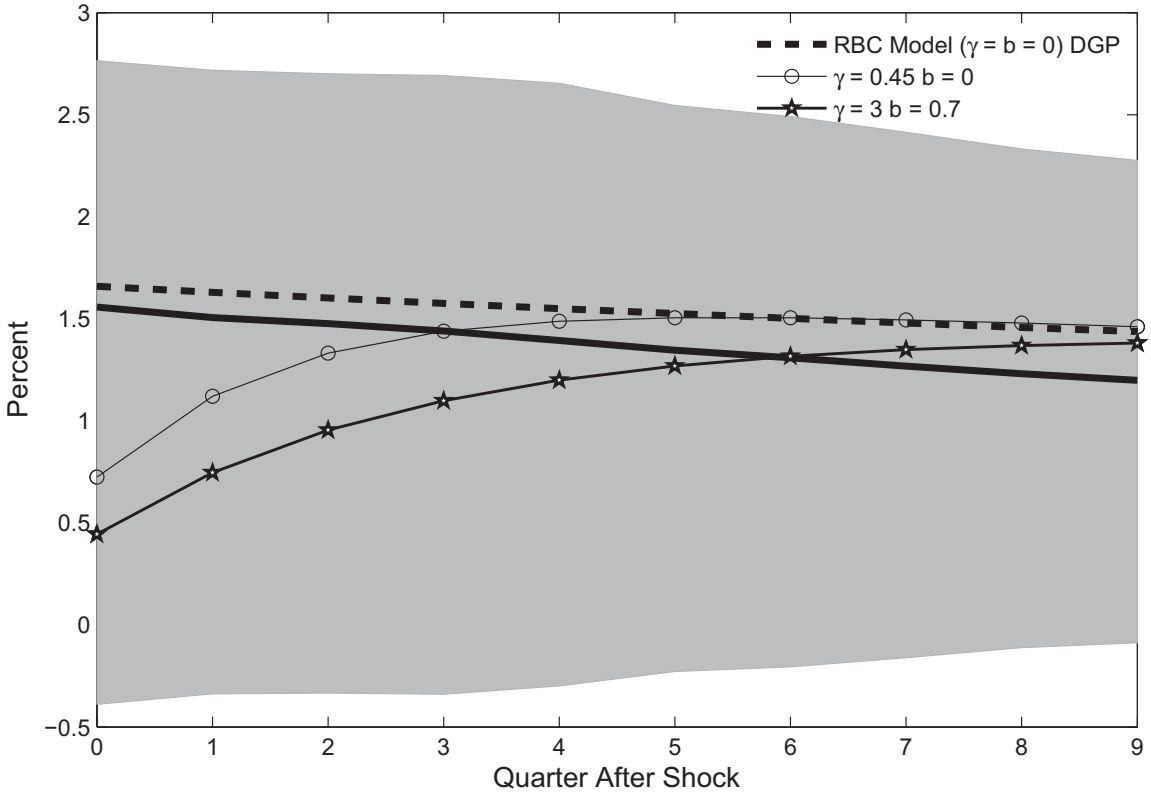


Figure 5: The Shape of The Hours Response



Note Each gray dot represents the estimated response from a simulated dataset. Each dataset is simulated from an RBC model. The triangle is the response implied by the RBC model (the DGP). The square is the response implied by a DSGE model with high real adjustment costs (ie b =0.7 γ =3). The star and the ellipses indicate the point estimate and confidence interval for an illustrative simulation.

Figure 6: The response of investment to a technology shock estimated using data simulated from a RBC Model



Note Thick solid line is average response over 2000 estimated responses using data simulated from a RBC model. Edges of grey area indicate 5th and 95th percentiles of all estimated responses to a technology shock

Figure 7 Rejection Rates For Testing Investment Response.

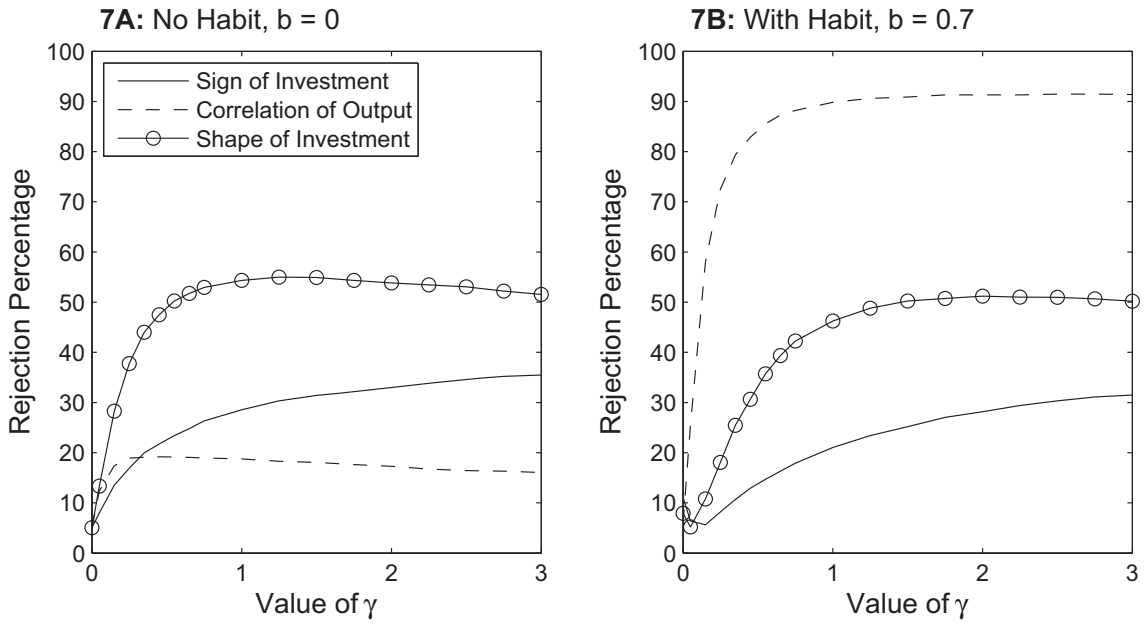
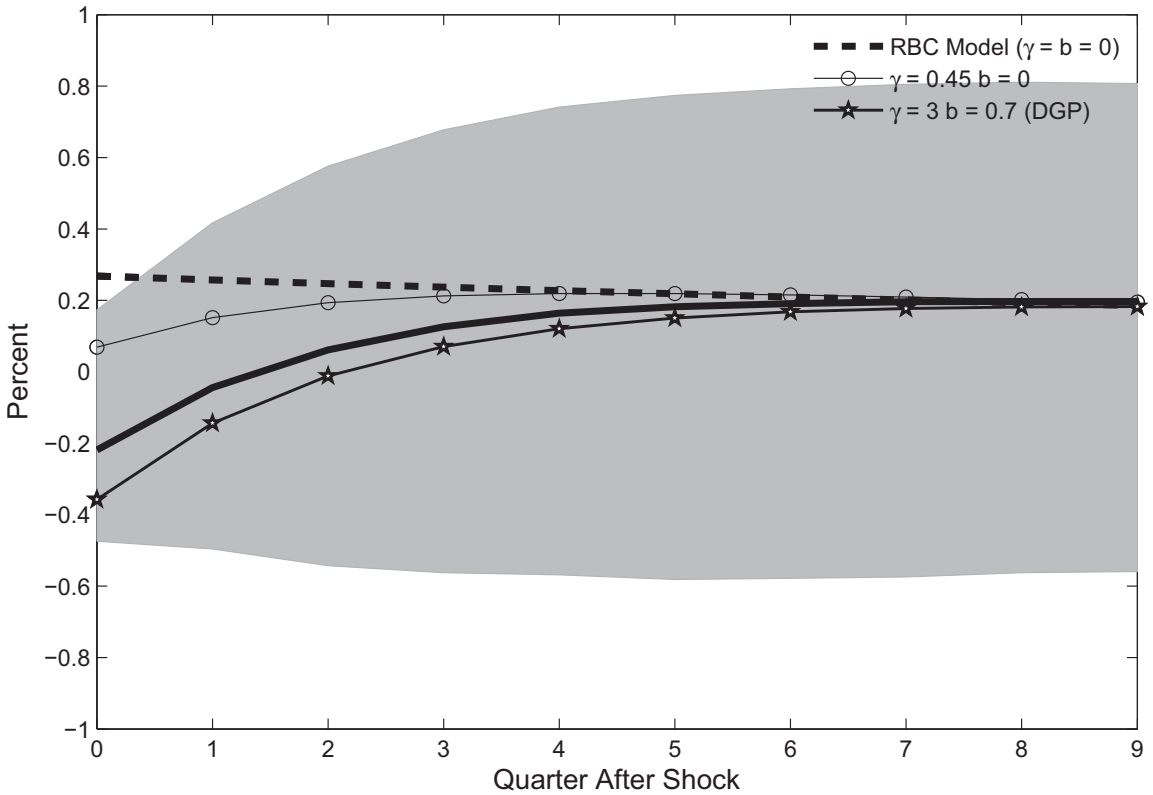
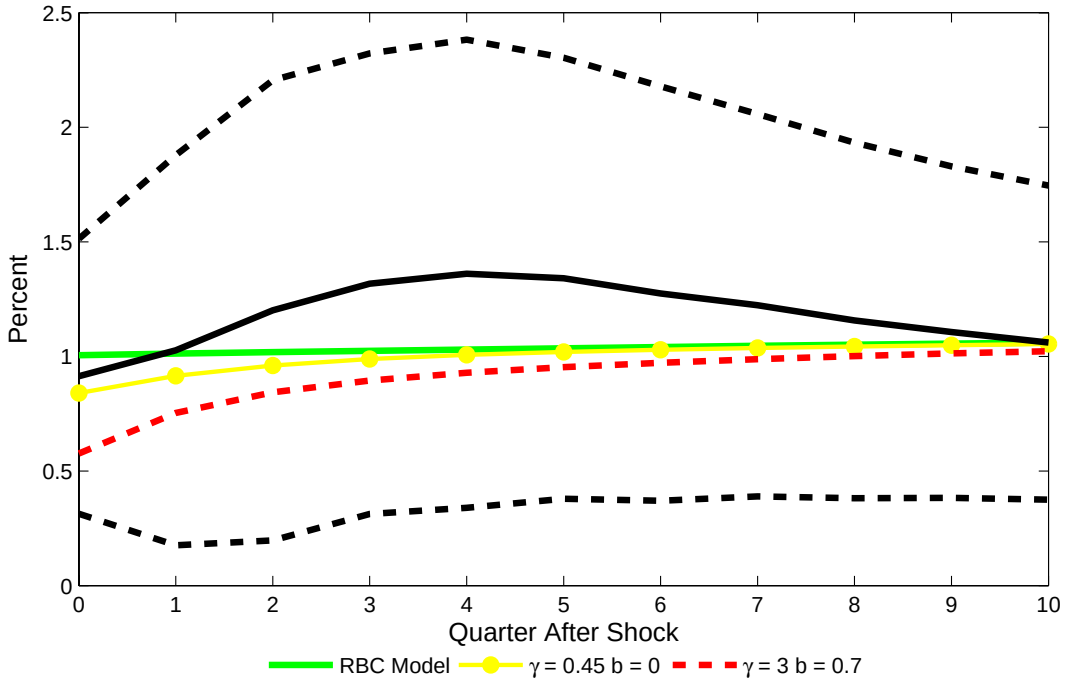


Figure 8: The response of hours worked to a technology shock estimated using data simulated from a Model with High Investment Adj. Costs and Habit



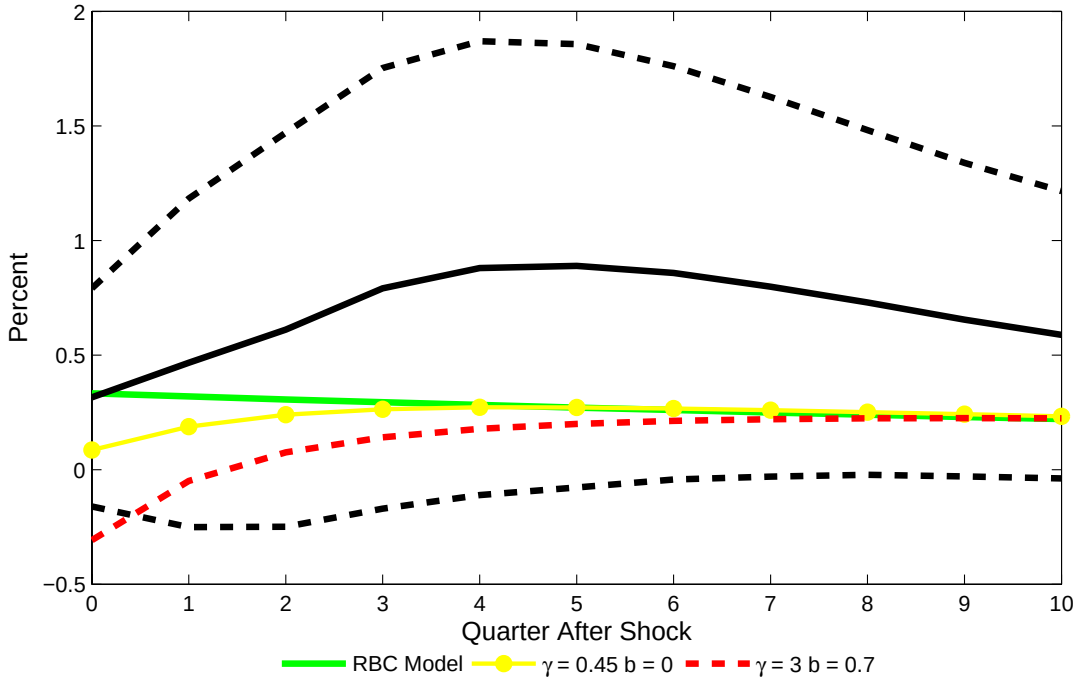
Note Thick black line is average estimated response across 2000 simulations from a DSGE model with high real rigidities. Edges of grey area indicate 5th and 95th percentiles of all estimated responses to a one−standard−deviation technology shock

Figure 9a: The estimated response of output to a technology shock



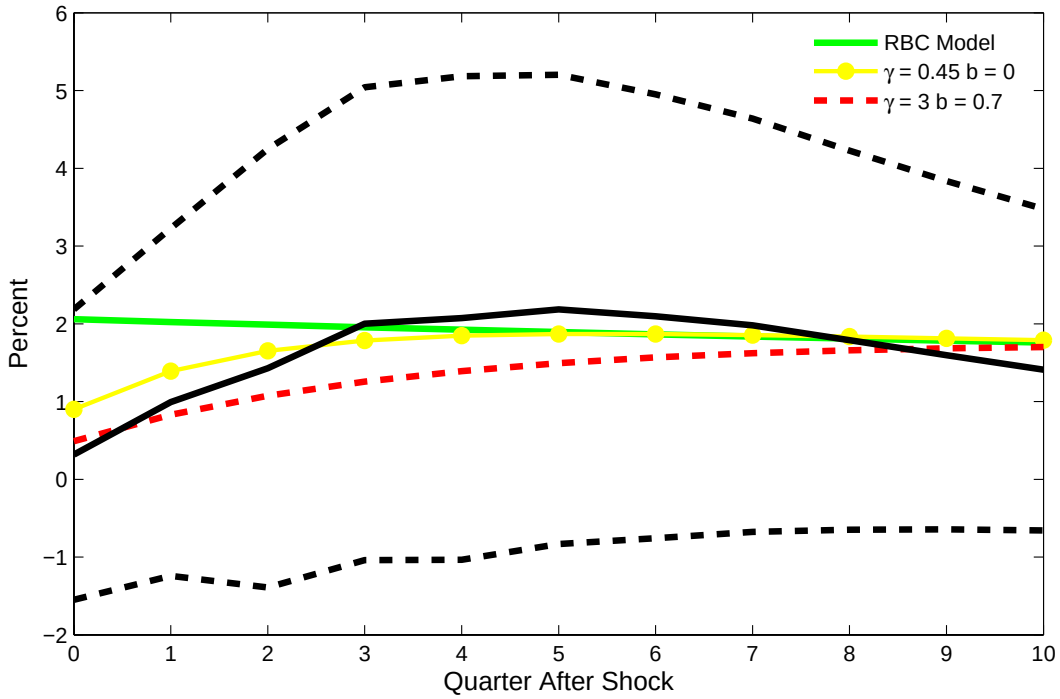
Note Thick black line is estimated response using a three variable VAR using U.S. data between 1954 to 2001. Edges of dashed areas indicate confidence interval of 2.8 standard deviations.

Figure 9b: The estimated response of hours worked to a technology shock



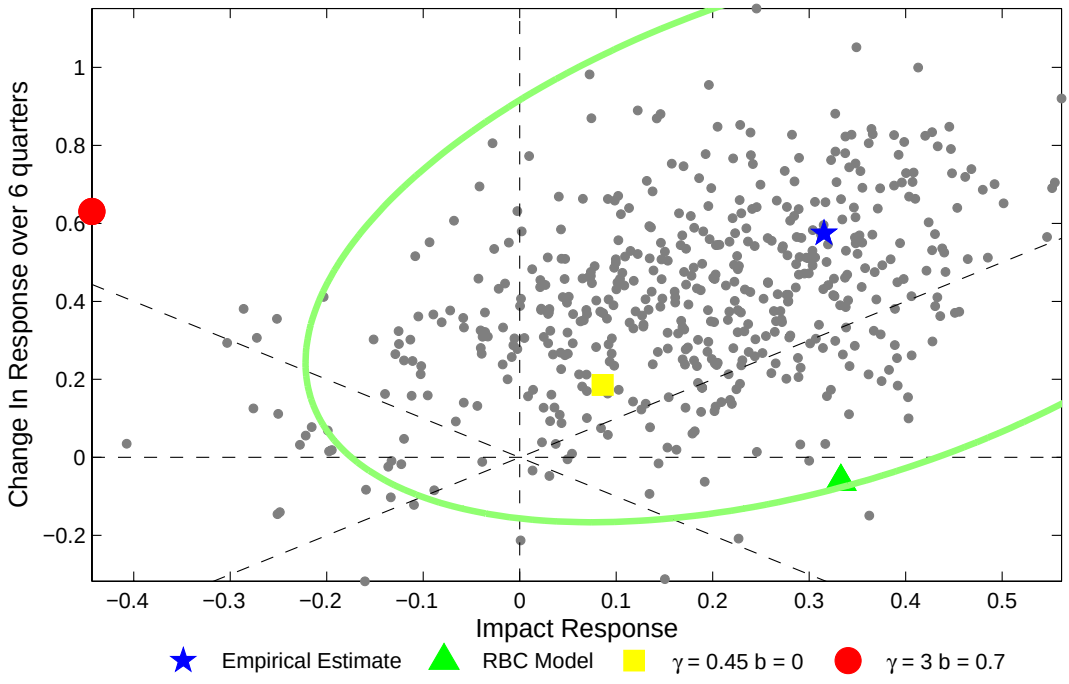
Note Thick black line is estimated response using a three variable VAR using U.S. data between 1954 to 2001. Edges of dashed areas indicate confidence interval of 2.8 standard deviations.

Figure 9c: The estimated response of Investment to a technology shock



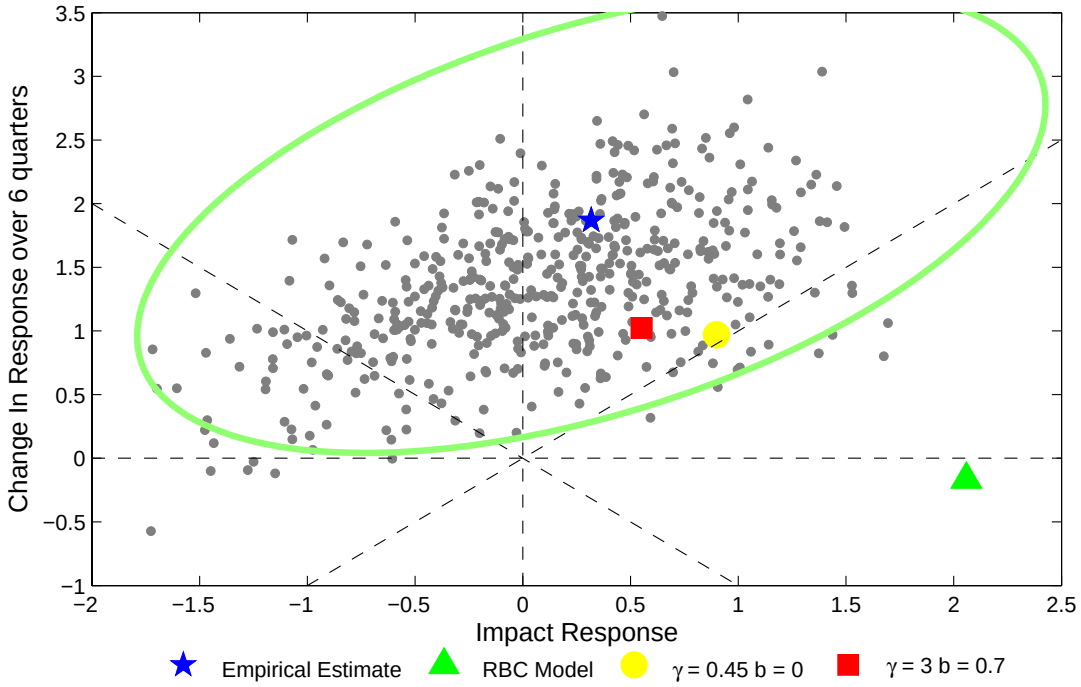
Note Thick black line is estimated response using a three variable VAR using U.S. data between 1954 to 2001. Edges of dashed areas indicate confidence interval of 2.8 standard deviations.

Figure 10a: The Shape of The Hours Response In An Estimated VAR



NoteGrey dots indicate responses from bootstrap simulations using empirical VAR. Blue ellipse indicates confidence interval around point estimate.

Figure 10b: The Shape of The Investment Response In An Estimated VAR



Note: Grey dots indicate responses from bootstrap simulations using empirical VAR.
Blue ellipse indicates confidence interval around point estimate.

Figure A: Implications of Weak Instruments on Sampling Uncertainty