# Is There a Single Shock that Drives the Majority of Business Cycle Fluctuations?*

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**Abstract**

I estimate the set of models in which one shock drives the majority of business cycle fluctuations. This shock explains the bulk of the long-run variation in the relative price of investment and a significant share of that in TFP and features a boom-bust behavior in the late 1990s-early 2000s period. Based on theory and the common view that the late 1990s-early 2000s episode was driven by overly optimistic expectations about information and communications technology which were thereafter revised downwards, the business cycle shock can be interpreted as a news shock about a general purpose technology represented by investment-specific technology.

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# 1 Introduction

What is the source of business cycles? This question has been the center of attention for macroeconomists for decades but has nevertheless remained a source of debate and disagreement. The list of potential business cycle shocks that have been studied by the macroeconomics literature is quite long. A considerable part of this list pertains to technology related shocks: total factor productivity (TFP) shocks (see, e.g., Kydland and Prescott (1982), Gali (1999), and Basu et al. (2006)); news shocks about future TFP, i.e., shocks that portend future changes in TFP (see, e.g., Beaudry and Portier (2006) and Barsky and Sims (2011)); investment-specific technology (IST) shocks (see, e.g., Greenwood et al. (1988), Fisher (2006), and Justiniano et al. (2010)); and news shocks about future IST (see, e.g., Ben Zeev and Khan (2015) and Ben Zeev (2018)). In recent years researchers have also explored shocks that erroneously move expectations about technology, termed noise or sentiment shocks (see, e.g., Lorenzoni (2009), Blanchard et al. (2013), Angeletos and La'O (2013), and Forni et al. (2017a,b)). And the recent Great Recession has expectedly spawned research on credit supply shocks (see, e.g., Gilchrist et al. (2009), Jermann and Quadrini (2012), Gilchrist and Zakrajšek (2012), and Christiano et al. (2014)).[1]

**What This Paper Does.** In general, all of the above-cited works take the approach of identifying a shock of interest and then examining its potential role as a business cycle driver. A notable exception in this literature is Angeletos et al. (2020), the discussion of which in terms of its relation to my paper is deferred to the next section and Section 7.) I take a largely agnostic approach whose aim is to inform us about the existence and nature of the driving force behind business cycles without needing to identify, ex-ante, any shock. This is done by implementing a Bayesian VAR-based approach that estimates the set of models in which one shock produces business cycle comovement and drives the majority of business cycle fluctuations. Then, I examine this set of models and search for common characteristics that can be informative about the nature of the business cycle shock. This exercise enables me to structurally pin down the type of shock at hand based on macroeconomic theory as well as narrative information from the large macroeconomic

[1] For a much more comprehensive and detailed review of business cycle studies, the reader is referred to Ramey (2016).

event of the late 1990s and early 2000s boom-bust period.

In particular, via estimation of a Bayesian VAR that includes a number of real aggregates, TFP, the relative price of investment (henceforth RPI), inflation and interest rates, I first compute all of the models in which one shock raises output, hours, consumption, and investment on impact and explains over 50% of these real aggregates' business cycle variation. Then, I examine the common features of this shock and find that it encompasses two robust characteristics: *i*) it drives the bulk of the long-run variation in RPI and a significant share of that in TFP, reducing the former and raising the latter; and *ii*) it behaves in a boom-bust manner in the late 1990s and early 2000s period, exhibiting significant positive realizations in the former period while experiencing significant negative realizations in the latter period. The first characteristic allows to determine that the shock can be reasonably interpreted as a general purpose technology (GPT) shock represented by either an unanticipated IST shock or an IST news shock as macroeconomic theory implies that IST is the sole source of the long-run variation in RPI.[2,3] The second characteristic permits me to interpret the shock as an IST news shock given the common view by economists that the late 1990s boom and subsequent early 2000s bust were generally related to overly optimistic expectations regarding information and communications technology (ICT) that were followed by a downward revision of these expectations (see, e.g., Beaudry and Portier (2004), Jaimovich and Rebelo (2009), and Karnizova (2012)). (See Appendix A in Karnizova (2012) for a list of several extracts from academic and government publications that link the boom and subsequent recession to a downward

[2]The concept of GPT has been pioneered in Bresnahan and Trajtenberg (1995) who defined it as technology characterized by the potential for pervasive use in a wide range of sectors and which is expected to bring about and foster generalized productivity gains as it evolves and advances. Information and communications technology (ICT) is commonly considered to constitute a GPT and, since it is an important driver of IST, it is straightforward to view IST as a GPT. Empirical evidence is consistent with the GPT-view of ICT and the associated relation between ICT growth and delayed TFP gains (see, e.g., Basu and Fernald (2007)).

[3]If one allows for IST to be driven by both unanticipated IST shocks as well as IST news shocks, it can be deduced that these two shocks drive the long run variation in RPI. (The reader is referred to section 3.1 for a depiction of the general relation between RPI and IST, as implied by macroeconomic theory, where it is also explained why it is plausible to make the assumption that IST is the sole source of the long-run variation in RPI.) Hence, as shall be elucidated in Section 3.2, I only consider models in which at least 90% of the long-run variation in RPI is driven by two shocks, none of which is restricted upon to be the business cycle shock. (The rather conservative 90% threshold, as opposed to the ideal 100% one, is mainly motivated by the possibility of measurement error in RPI.) Section 4 presents DSGE model based Monte Carlo evidence that stresses the importance of this long-run restriction for obtaining a correct structural interpretation of the business cycle shock.

revision of overly optimistic expectations regarding ICT.)

It is noteworthy that I identify a business cycle shock directly and then ask what this shock looks like and whether it has a clear structural interpretation, as opposed to imposing identifying restrictions on an identified shock and then examining its business cycle implications. My largely agnostic approach lends credence to the results and their structural interpretation because the assumptions used for identification of the business cycle shock are sound and reliable. Business cycle comovement is arguably the most salient feature of economic fluctuations; and for a shock to be considered the main force behind economic fluctuations, it must account for the majority of the business cycle variation in the real aggregates that move in tandem over the business cycle. Accordingly, the shock that drives the business cycle must produce business cycle comovement and explain most of the business cycle variation in the main macroeconomic real aggregates. I simply impose on these two attributes to characterize my identified business cycle shock, thus resulting in a credible identification procedure. That the identified business cycle shock seems to have a clear structural interpretation supports the notion that there is indeed a single economic shock that drives the bulk of economic fluctuations.

From a broader standpoint, the findings of this paper stress that the business cycle is driven by technology related fundamentals rather than noise or policy related shocks. Particularly notable is the rather strong evidence put forward by this paper that goes against the notion that noise shocks play a considerable role in driving the business cycle. The noise-driven business cycle hypothesis, which arguably is a competing hypothesis for the news-driven hypothesis, is inconsistent with this paper's findings although one cannot entirely rule out on their basis the possibility that noise shocks still play some role, albeit limited relative to IST news shocks, in driving the business cycle.

Moreover, while much of the earlier work focused on shocks to TFP or IST that affect only the fundamental to which they are related, this paper assigns an important role in driving economic fluctuations to a GPT news shock that can be interpreted as an IST news shock whose delayed materialization ultimately produces significant TFP gains. As such, the findings of this paper largely accord with and complement those of Schmitt-Grohe and Uribe (2011), who estimate an RBC model where a common stochastic trend in neutral and investment-specific productivity is found to be the main source of business cycles.[4] I find conceptually similar results to theirs, using a relatively model-free identification approach, although I also provide a news-based interpretation of my business cycle driving force.[5]

**Outline.** The remainder of the paper is organized as follows. The next section provides a literature review. In Section 3 the details of the empirical strategy are laid out. Section 4 provides Monte Carlo evidence from a suitable DSGE model aimed at enhancing confidence in my identification procedure's capacity to answer the question posed in the title. Section 5 begins with a description of the data, after which it presents the main empirical evidence followed by a sensitivity analysis section. Section 7 provides evidence on the importance of proper RPI measurement in the context of the results from Angeletos et al. (2020). The final section concludes.

# 2 Related Literature

The general business cycle literature my paper belongs to is very large and is non-exhaustively cited above. Here I focus on describing the literature my paper is related to from a methodological standpoint. The method I use in this paper is based on the inequality restrictions Structural VAR (SVAR) literature which identifies shocks of interest by employing set identification whereby inequality restrictions are imposed so as to generate the set of admissible models. This literature has mainly focused on imposing restrictions on the sign of impulse responses (see, e.g., Uhlig (2005), Dedola and Neri (2007a), Mountford and Uhlig (2009), Peersman and Straub (2009), and Kilian and Murphy (2012)), the sign of the cross correlation function in response to shocks (Canova and De Nicolo (2002)), and inequality restrictions on the contribution of identified shocks of interest to the forecast error variance of certain variables (Dedola and Neri (2007b), Ben Zeev (2018), and

[4] Using a standard Engle-Granger test (Engle and Granger (1987)) for my RPI and TFP measures, I could not reject the null of no cointegration between these two series. Importantly, however, one should keep in mind that the lack of evidence for cointegration between my RPI and TFP measures has no bearing on whether the business cycle shock can drive in tandem the long-run variation in these two variables; the reason for this is that non-cointegrated series can of course still be driven by the same shocks in the long run.

[5] Wagner (2017) estimates a similar model to that used by Schmitt-Grohe and Uribe (2011) but allows for news shocks to the common stochastic trend in TFP and IST, finding an important role for these news shocks in driving the business cycle.

Volpicella (2022)).

The method in this paper incorporates both sign restrictions, i.e., requiring positive impact effects of the business cycle shock on the real aggregates, as well as restrictions on the forecast error variance of the real aggregates, so that the identified shock explains more than 50% of their two-year variation. Moreover, so as to consider models that comply with standard macroeconomic theory and thus facilitate their coming closer to the true data generating process, I also impose that at least 90% of the long-run variation in RPI is driven by two arbitrary shocks (none of which is restricted upon to be the business cycle shock).

I implement the conventional Bayesian inference approach to set-identified SVARs, i.e., I use a uniform prior for the orthonormal rotation matrix determining the mapping between reduced-form impulse responses and structural (identified) ones. This uninformative prior assumption implies nonuniform prior distributions for identified impulse responses, as stressed and criticized by Baumeister and Hamilton (2015, 2018) who claim that this drawback invalidates results based on this uniform prior. And this criticism has recently spawned alternative Bayesian inference approaches which do not suffer from this drawback (Giacomini and Kitagawa (2021) and Volpicella (2022)). However, Inoue and Kilian (2021) have recently highlighted that this drawback has quantitatively negligible effects on the identified impulse responses' posterior distribution in applied work in which the set-identification of impulse responses is tight. This tight set-identification feature is very much a feature of my results, making them unsusceptible to Baumeister and Hamilton (2015, 2018)'s criticism. Hence, I stick to the conventional Bayesian inference approach in my paper, while also highlighting that the fact that my identification procedure works reasonably well when applied to artificial data from a suitable DSGE model (see Section 4 and Appendix B.10 of the online appendix to this paper) further alleviates the concern that the findings from Baumeister and Hamilton (2015, 2018) have significant consequences for my analysis.

The largely agnostic procedure used in this paper is conceptually similar to that employed by Uhlig (2003). Using the Maximum Forecast Error Variance (MFEV) method to identify a set of orthogonal shocks that maximally explain (in decreasing order) output variation over a five-year horizon, Uhlig (2003) found that two shocks explain more than 90% of output variation at all horizons. The rather rich array of short- and long-run restrictions I use in this paper seems to provide for a more useful framework for studying the sources of business cycles than the narrower set of restrictions used by Uhlig (2003). While my richer set of restrictions does make my approach somewhat more restrictive, I still view them as a necessary step toward correctly uncovering the business cycle shock owing to the fact that they are based on rather weak, data- and theory-consistent assumptions: The short-run restrictions are very much consistent with the salient comovement feature of business cycles and the long-run restriction accords well with basic economic theory. Notably, that my methodological approach can directly impose the restrictions that accurately characterize the nature of the business cycle shock is precisely what makes it a more suitable device for studying the question in the title of this paper.

Finally, in recent work that applies Uhlig (2003)'s identification approach more comprehensively by separately applying it to several macro variables and by looking at various truncation horizons, while focusing just on the first shock that moves the most of a particular variable's variation, Angeletos et al. (2020) find that the shock that drives most of economic fluctuations seems to be unrelated to long-run movements in TFP and RPI. On top of the differences already highlighted in the context of the paper by Uhlig (2003), there are two additional noteworthy differences between mine and Angeletos et al. (2020)'s empirical analysis which seem to be driving the differences between this paper's and their paper's results.

First, the measure of RPI used in Angeletos et al. (2020) considers the durable consumption goods sector along with the *total* investment sector, as opposed to the finer and more standard measure covering durable consumption goods and only equipment investment. Considering the entire investment sector is a too coarse measure for properly constructing a price index that corresponds to IST, which in empirical terms is normally thought to represent technology in producing firm and household equipment rather than residential or commercial structures. To confirm the importance of this RPI measurement issue, I took two steps: I applied my estimation procedure to the RPI measure used in Angeletos et al. (2020), finding small effects of the business cycle shock on this coarse RPI measure; and I applied the estimation procedure from Angeletos et al. (2020) to my RPI measure and found meaningful long-run contribution of the business cycle shock to RPI variation. These results, which are discussed in more detail in Section 7, further stress the importance of using a suitable, state-of-the-art RPI measure for properly uncovering the true nature of the business cycle shock.

Second, while Angeletos et al. (2020)'s estimation approach is broadly similar to mine along the short-run dimension of the two methodologies, there is meaningful added value from imposing my long-run restriction owing to its capacity of reduce the risk of spurious identification (see Section 4.1) as well as its capacity to avoid the large downward bias in the estimation of the long-run contribution to RPI variation stemming from the removal of this long-run restriction (see Appendix B.10 of the online appendix to this paper). Hence, on top of the RPI choice issue, an additional advantage of my approach relative to that from Angeletos et al. (2020) is my introduction of Restriction 2.

# 3 Methodology

Prior to presenting the empirical strategy in detail, I first explain the underlying framework and assumptions of the analysis employed in this paper.

## 3.1 Underlying Framework

While I do take a largely agnostic identification approach in this paper, I also make an attempt to bridge the gap between my set of identified models and the true data generating process in a way that relies on rather weak, theory-consistent assumptions. Such an attempt can have value in advancing a correct structural interpretation of the business cycle shock without needing to directly impose on this shock anything other than forcing it be the shock that both produces business cycle comovement and drives the majority of business cycle fluctuations. To achieve this advancement, I focus on the long-run relation between RPI and IST, which has clear structural discipline that is implied by a wide variety of models.

Specifically, the general relation between RPI and IST can be illustrated by considering a two-sector model structure along the lines outlined in Justiniano et al. (2011) with separate imperfectly competitive investment and consumption sectors. Both sectors are influenced by a common TFP shock and, in addition, the investment sector is affected by an IST shock. In this set up one can derive the following equilibrium equation linking IST with RPI:

$$IST_t = \left(\frac{a_c}{a_I}\right)\left(\frac{mc_{C,t}}{mc_{I,t}}\right)\left(\frac{K_{C,t}}{L_{C,t}}\right)^{-(1-a_C)}\left(\frac{K_{I,t}}{L_{I,t}}\right)^{(1-a_I)}\left(\frac{P_{I,t}}{P_{C,t}}\right)^{-1}, \tag{1}$$

where $a_j$ stands for the capital share in sector $j$ ($j = C, I$); $mc_{j,t}$ is real marginal cost (or the inverse of the equilibrium markup) in sector $j$; $K_{j,t}/L_{j,t}$ represents the capital-labor ratio in sector $j$; $\frac{P_{I,t}}{P_{C,t}}$ is the relative price of investment where $P_{I,t}$ and $P_{C,t}$ represent the prices of investment and consumption goods, respectively; and $IST_t$ corresponds to investment-specific technology. Many one-sector DSGE models (e.g., Smets and Wouters (2007)) can be viewed as equivalent representations of a two-sector model that admits identical production functions across the two sectors, free sectoral factor reallocation, and perfectly competitive sectors. However, recent research (e.g., Basu et al. (2010), Justiniano et al. (2011), and Moura (2018)) has argued that the assumption of equality between RPI and IST which is based on the latter three conditions is too strong. It is clear from Equation (1) that if one of these three conditions is not met there will be a wedge between RPI and IST. Hence, I only make the weak assumption that IST is the sole source of the *long-run* variation in RPI.[6] This is the underlying identifying assumption made by papers that aimed to identify unanticipated IST shocks (see, e.g., Fisher (2006) and Canova et al. (2010)) whereby they conjectured that the only shock that has a long-run effect on RPI is the unanticipated IST shock. Nevertheless, as opposed to just assuming that one shock drives IST, I allow for the possibility that part of the variation in IST is anticipated in advance.

In particular, it is assumed that IST is well-characterized as following a stochastic process driven by two shocks. The first is the traditional unanticipated IST shock, which impacts the

[6]For IST to be the sole source of the unit root in RPI there would need to be equal capital shares across the investment and consumption sectors, free sectoral factor reallocation in the long run, and stationarity of sectoral mark-ups. The latter is implied by macroeconomic theory as standard sectoral Phillips curves imply that mark-ups are roughly the difference between expected inflation rates and current ones (see, e.g., Justiniano et al. (2011)). Moreover, Basu et al. (2010) find that the capital share for the services and non-durables sector is 0.36 whereas that of equipment and software investment and consumer durables is 0.31. Given that the two shares are relatively close, and that it is reasonable to assume that in the long run factor inputs can freely reallocate, it seems sensible to assume that the long-run variation in RPI is driven solely by unanticipated IST shocks and IST news shocks. Notably, this assumption is quantitatively borne out by the elaborate two-sector model from Moura (2018), which uses similarly different sector-specific capital shares (0.36 and 0.30) along with sector-specific nominal frictions as well as labor and capital reallocation frictions (this model serves as the underlying true data generating process for my Monte Carlo experiments from Section 4) and Appendix B.10 of the online appendix to this paper.

level of technology in the same period in which agents observe it. The second is the news shock, which is differentiated from the first shock in that agents observe the news shock in advance and it portends future changes in technology. The following is an example process that incorporates both unanticipated and news shocks to IST:[7]

$$\epsilon_t = \epsilon_{t-1} + g_{t-j} + \eta_t, \tag{2}$$

$$g_t = \kappa g_{t-1} + v_t. \tag{3}$$

Here the log of $IST_t$, denoted by $\epsilon_t$, follows a unit root process where the drift term itself $g_{t-j}$ follows an AR(1) process with $j \geq 1$. $j$ represents the anticipation lag, i.e., the delay between the announcement of news and the period in which the future technological change is expected to occur. Parameter $0 \leq \kappa < 1$ describes the persistence of the drift term. $\eta$ is the conventional unanticipated technology shock. Given the timing assumption, $v_t$ has no immediate impact on the level of IST but portends future changes in it. Hence, it can be defined as an IST news shock.

Given the above underlying theoretical framework, I only consider models that are consistent with Equations (1)-(3) in the empirical analysis below. Specifically, I impose the restriction that at least 90% of the long-run variation in RPI is driven by two shocks, none of which is restricted upon to be the business cycle shock. Ideally, one would want to require that 100% of the long-run variation in RPI is driven by two shocks but given that there could be measurement errors present in my empirical analysis and that the capital shares in the consumption and investment sectors seem to be close but not entirely identical, the 90% restriction seems a reasonable compromise.

One may argue that some restrictions on the behavior of TFP should also be incorporated in my analysis. E.g., if the identifying assumption of Barsky and Sims (2011) that two shocks drive all variation in TFP at all horizons holds (the first shock being a surprise shock that moves TFP on impact and the second being a news shock that moves it with a delay), then it is advisable to restrict the set of identified models to accord with this assumption. However, as stressed by Kurmann and Sims (2017) and Bouakez and Kemoe (2017), measured TFP likely contains measurement errors which in turn lead to a violation of the aforementioned identifying assumption.[8]

[7] A similar process was used by Leeper and Walker (2011), Barsky and Sims (2011, 2012), and Leeper et al. (2013).

[8] The focus in Kurmann and Sims (2017) is on the large revisions in the widely-used series of utilization-

Moreover, even if these measurement errors are transient, restricting the long-run behavior of TFP may be erroneous on the grounds that other shocks, such as GPT-type shocks, could drive some of the long-run variation in TFP. Therefore, I leave TFP behavior unrestricted in my analysis, which ex-post turns out to be a reasonable choice given that the business cycle shock drives the bulk of the long-run variation in RPI and also a considerable share of that in TFP.

## 3.2 Generating the Set of Admissible Models

My methodology is a set identification VAR-based method which generates a set of admissible models that comply with a defined set of restrictions, to be described below in detail. The method is a set identification one because the imposed restrictions admit a system of inequalities that in general will have either no solution or a set of solutions. This set of solutions constitutes the set of models that satisfy my imposed restrictions. I employ Bayesian estimation and inference using a baseline empirical VAR that consists of the real aggregates, TFP, RPI, inflation, and interest rates.

Specifically, Let $y_t$ be a $k$x1 vector of observables of length $T$ and let the VAR in the observables be given as

$$y_t = B_1 y_{t-1} + B_2 y_{t-2} + ... + B_p y_{t-p} + B_c + u_t, \tag{4}$$

where $B_i$s are matrices of size $k$x$k$, $p$ denotes the number of lags, $B_c$ is a $k$x1 vector of constants, and $u_t \sim i.i.d.\ N(\mathbf{0}, \Sigma)$ is the $k$x1 vector of reduced-form innovations where $\Sigma$ is the variance-covariance matrix of reduced-form innovations. For future reference, let the stacked $(kp+1)$x$k$ $B = [B_1, ..., B_p, B_c]'$ matrix represent the reduced form VAR coefficient matrix. Hence, the reduced form VAR parameters can be summarized by the coefficient matrix $B$ and variance covariance matrix $\Sigma$.

adjusted TFP from Fernald (2014) and these revisions' substantial effect on empirical conclusions about the macroeconomic effects of TFP news shocks identified using the Barsky and Sims (2011) method, with identified TFP news shocks found to produce business cycle comovement for newer TFP vintages while failing to do so for older ones. Interestingly, and largely in accordance with the newer TFP vintages issue highlighted by Kurmann and Sims (2017), I find that older TFP vintages such as from 2011 respond to the business cycle shock to a much lesser extent. Nevertheless, since newer TFP vintages likely contain less measurement error than older ones and as such constitute better proxies for true TFP, I utilize the most recent Fernald (2014) TFP vintage in my estimations and place more trust in results based on this series than those based on older TFP vintage series.

It is assumed that there exists a linear mapping between the reduced-form innovations and economic shocks, $e_t$, given by

$$u_t = Ae_t. \tag{5}$$

The impact matrix $A$ must satisfy $AA' = \Sigma$. There are, however, an infinite number of impact matrices that solve the system. In particular, for some arbitrary orthogonalization, $C$ (e.g, the Cholesky factor of $\Sigma$), the entire space of permissible impact matrices can be written as $CD$, where $D$ is a $k$ x $k$ orthonormal matrix (i.e., $D' = D^{-1}$ and $DD' = I$, where $I$ is the identity matrix).

Given an estimated reduced form VAR, standard SVAR methods would try to deliver point identification of at least one of the columns of $A$ whereas set identification methods would generate the set of admissible models. In the set identification approach the aim is to draw a large number of random $B$s, $\Sigma$s, and $D$s from their posterior distributions so as to generate a large set of models (a model here can be represented by the matrix triplet $\{B,\Sigma,D\}$) from which the set of admissible models can be obtained by checking which models comply with the imposed restrictions. I take $10^6$ such posterior draws, while following the conventional Bayesian approach to estimation and inference taken by the sign restrictions literature (see, e.g., Uhlig (2005), Mountford and Uhlig (2009), Peersman and Straub (2009), and Kilian and Murphy (2012)) in assuming a normal-inverse Wishart prior distribution for the reduced-form VAR parameters and a Haar distribution for the orthonormal $D$ matrix.[9] Appendix A of the online appendix to this paper contains a detailed description of the Bayesian estimation procedure used in this paper.

The restrictions that I impose on the set of admissible models are as follows:

1. One shock, belonging to the vector of economic shocks $e_t$, raises on impact the real aggregates, i.e., output, hours, consumption, and investment, and explains at least 50% of the two-year variation of the real aggregates.

2. At least 90% of the long-run variation in RPI is driven by two arbitrary shocks belonging to $e_t$, none of which is restricted upon to be the business cycle shock from Restriction 1.

[9] I follow the efficient method proposed by Rubio-Ramirez et al. (2010) for generating orthonormal matrices and the associated identification, impact matrices.

Imposing Restriction 1 constitutes a necessary step for directly examining the nature of the driving force of business cycle fluctuations. The latter Restriction ensures that the estimated set of admissible models only contains models in which one shock explains the majority of business cycle fluctuations. Note that I also require that this shock is capable of generating business cycle comovement by restricting the real aggregates to rise at the impact horizon in response to the business cycle shock. This is an important restriction given the stylized fact that the real aggregates move in tandem over the business cycle. Hence, the shock that I am trying to capture both generates business cycle comovement and explains the majority of business cycle fluctuations. Notably, however, this initial step in and of itself is not sufficient for providing an answer to the sought after question of this paper as it would also be necessary to examine the common characteristics of the business cycle shock so as to determine if there is truly a single common economic shock that drives the majority of business cycles.

Restriction 2 ensures that I am only considering models that are consistent with Equations (1)-(3) so as to impose some structural discipline on the estimated models in terms of being consistent with standard macroeconomic theory. This in turn facilitates bringing the identified models closer to the true data generating process, which can have much value in advancing a correct structural interpretation of the business cycle shock. Note that Restriction 2 is independent of Restriction 1 in that the two shocks driving the long-run variation in RPI can be any pair of shocks belonging to $e_t$. I.e., I do not restrict upon the business cycle shock to be one of the shocks contained in this pair, effectively letting the data determine if the business cycle shock belongs to this pair. Section 4 and Appendix B.10 of the online appendix to this paper present DSGE model based Monte Carlo evidence that stresses the importance of this long-run restriction for obtaining a correct structural interpretation of the business cycle shock.

I search over all drawn models and collect only those models that comply with Restrictions 1 and 2 whereas models that do not comply with these restrictions are discarded. Once all of the models are collected, it is possible to analyze them and try to structurally characterize the business cycle shock.

# 4 Suitability of Methodology for Answering the Question In the Title

One may argue that this paper's identification approach may pick up a combination of shocks, rather than a single one, thus leaving the question posed in this paper's title inconclusively answered. To address this concern, I conduct two Monte Carlo experiments based on an appropriate DSGE model with endogenous RPI. In the first experiment, I apply my identification approach to artificial data generated from a data generating process (DGP) where IST news shocks do not conform to the definition of a business cycle shock in their not producing comovement. In the second experiment I use a DGP where IST news shocks comply with the identifying restrictions from Restriction 1. (I accommodate these two rather different DGPs by utilizing two different parameterizations of the same structural framework, which is based on the elaborate model structure from Moura (2018). The details of the model and its calibration appear in Appendix B of the online appendix to this paper.) Taken together, the evidence from these two experiments bolsters the empirical results shown so far in alleviating the above-mentioned concern about their potential spuriosity and accordingly enhancing confidence in their ability to provide a positive answer to the question in this paper's title. To keep the exposition minimal, I present here only the results from the first experiment and defer the presentation of the second experiment and its results to Appendix B.10 of the online appendix to this paper.[10]

## 4.1 Monte Carlo Experiment: A Model where IST News Do Not Produce Comovement

**Objective.** The objective of the experiment of this Section is to demonstrate what my identification approach yields when the true DGP contains IST news shocks that fail to induce comovement. Specifically, I use a DSGE model where TFP news and monetary policy shocks produce comovement but neither of them explains more than 25% of the two-year variation in output, while IST news shocks explain the majority of the latter variation but fail to produce comovmement. Un-

[10] I also show in Appendix B.11 of the online appendix results from an experiment where news shocks are imperfectly observed from noisy signals, as opposed to the full-information structure underlying the results from the baseline experiments, demonstrating the robustness of the simulation results to the presence of such imperfect-information structures.

derstanding what follows from my identification approach in this type of setting is important for alleviating the concern that this paper's results are merely an outcome of identifying a combination of shocks.

**Data Simulation.** The Monte Carlo experiment is conducted as follows. I generate 100 artificial data sets from the model from Appendix B of the online appendix to this paper with a sample size of 235 observations and apply my identification procedure (based on $10^5$ posterior draws) involving Restrictions 1 and 2 to each artificial data set using a VAR that is identical to the baseline empirical VAR. Since the model is solved via log-linearization around the steady state, I add the model-consistent steady state growth rates to the simulated non-stationary variables as well the steady state values to the simulated stationary variables. To gain an understanding as to the importance of imposing the long-run restriction (Restriction 2) in my analysis, I present results for two cases: *i*) the baseline case, where I impose both Restriction 1 and Restriction 2 when applying my estimation procedure to the artificial data sets and *ii*) an alternative case, where I only impose Restriction 1.

**Baseline Case.** The first row of Table 1 presents the share of simulations in which identification was null along with the average admissibility rate (average number of admissible models divided by total number of posterior draws ($10^5$)) for the simulations that did produce a non-null set of admissible models, where both Restriction 1 and Restriction 2 are imposed in the identification procedure. Ideally, one would want to see that in all simulations a zero admissibility rate obtains, i.e., null set of identified models for all simulations, as this would strongly support the capacity of my procedure to avoid spurious identification. As shown Table 1, the results are very close to ideal: 96 out of the 100 simulations lead to a null set of identified models and for the 4 simulations which do not there is an average admissibility rate that is much lower than its baseline empirical counterpart reported on Page 17 ($1.75 \times 10^{-5}$ compared to $129.7 \times 10^{-5}$).[11] In fact, in three of the

[11] Importantly, one need be careful in considering the size of the set of admissible models as an indication for the validity of the identifying restrictions. As Kilian and Lutkepohl (2017) point out in Chapter 13, the estimates of sign-identified models are conditional on the chosen identifying assumptions which are in turn *not testable* within the SVAR framework. (To see this, consider an asymptotic world where the reduced form VAR is perfectly estimated and also assume that identifying restrictions are correct. In this

non-null four simulations, only one admissible model was identified out of $10^5$ posterior draws with the remaining simulation yielding a set of only 4 admissible models. Overall, the findings from the first row of Table 1 indicate that it is very unlikely that my identification procedure would spuriously identify a business cycle shock if the true model contained several shocks that individually comply with only part of my procedure's identifying restrictions.

**Removing the RPI Long-Run Restriction.** The second row of Table 1 presents the share of simulations in which identification was null along with the average admissibility rate for the simulations that did produce a non-null set of admissible models, only now from only imposing Restriction 1 in the estimations. The risk of spurious identification seems low also in this case, with only 15% of the simulations resulting in non-null identification. However, this risk is still much greater than that observed for the baseline estimation case (nearly 4 times as much). This emphasizes one dimension of the added value from imposing Restriction 2, which is related to the significantly reduced risk of spurious identification when the true DGP does not contain a single business cycle shock. The other dimension, which is related to the added value from doing so when the true DGP *does* contain a single business cycle shock and speaks to the considerable downward bias in the estimation of the shocks's long-run contribution to RPI variation resulting from removing Restriction 2, is discussed in Appendix B.10 of the online appendix to this paper. Lastly, it is also noteworthy that the fact that the admissibility rate observed in actual data when applying my estimation procedure without imposing Restriction 2 (reported on Page 26) is much higher than the very low admissibility rate reported in the second row of Table 1 ($2.67 \times 10^{-5}$) is also not supportive (like that from the first row) of the notion that it is likely that the true DGP behaves similarly to that implied by the DSGE model at hand (i.e., a model where no business cycle shock exists).

kind of world there is only one impact matrix compatible with the reduced form VAR, i.e., upon applying an estimation algorithm such as mine one should get one admissible model.) I am merely using the size of the set of admissible models here to highlight that the stark differences between actual and Monte Carlo based admissibility rates are not supportive of the notion that it is likely that the true DGP corresponds to the DSGE model at hand, i.e., a model where there is no single business cycle shock but a combination of shocks individually complying only in part with my identifying restrictions.

# 5 Empirical Evidence

In this section the main results of the paper are presented. I first provide a brief description of the data used in my analysis, followed by the main empirical results from my baseline VAR.

## 5.1 Data

The baseline VAR includes eight variables: TFP, RPI, output, hours, consumption, investment, inflation, and interest rates. For the TFP series, I employ the quarterly series on total factor productivity (TFP) for the U.S. business sector, adjusted for variations in factor utilization (labor effort and capital's workweek), constructed by Fernald (2014).

RPI is measured in the standard way as a quality-adjusted investment deflator (see, e.g., Greenwood et al. (1997, 2000), Fisher (2006), Canova et al. (2010), Beaudry and Lucke (2010), and Liu et al. (2011)) divided by a consumption deflator. The quality-adjusted investment deflator corresponds to equipment and software investment and durable consumption and is based on the Gordon (1990) price series for producer durable equipment (henceforth the GCV deflator), as later updated by Cummins and Violante (2002), so as to better account for quality changes. More recently, Liu et al. (2011) used an updated GCV series constructed by Patrick Higgins at the Atlanta Fed. I use this updated series for the recent 2017:Q3 vintage, spanning the period 1959:Q1:2017:Q3, as my measure for the quality-adjusted investment deflator.[12] (The ending date of this series dictates that of the sample used in my estimation.) The consumption deflator corresponds to nondurable and service consumption, derived in chain-weighted form from the National Income and Product Accounts (NIPA).

The nominal series for output, consumption, and investment are taken from the Bureau of Economic Analysis (BEA). Output is measured as GDP, consumption as the sum of non-durables and services consumption, and investment is the sum of personal consumption expenditures on durables and gross private domestic investment. The nominal series are converted to per capita terms by dividing them by the civilian non-institutionalized population aged sixteen and over. I use the corresponding chain-weighted deflators to obtain the real series. The hours series is log

[12] I thank Patrick Higgins at the Atlanta Fed for providing me with this series. The reader is referred to the appendix in Liu et al. (2011) for a description of the methods used to construct the series.

of per capita total hours worked in the non-farm business sector. Inflation is measured as the percentage change in the CPI for all urban consumers and the nominal interest rate is the three month Treasury Bill rate. To convert monthly population, inflation, and interest rate series to quarterly series, I take the average over monthly observations from each quarter. The data series span the period 1959:Q1-2017:Q3.

## 5.2 Baseline Results

I first present the impulse responses and forecast error variance (FEV) decomposition results with respect to the business cycle shock after which results pertaining to the shock realizations are presented. Both sets of results enable me to derive a structural interpretation of the shock.

**Impulse Responses and Variance Decompositions.** My empirical VAR includes eight variables: TFP, RPI, output, investment, consumption, hours worked, inflation, and interest rates. Apart from hours, inflation, and interest rates, which are assumed to be stationary and enter the system in levels, all other variables enter the system in first differences. Importantly, Restrictions 1 and 2 are imposed on the cumulative impulse responses of the relevant first-differenced variables so that variables' responses at a particular horizon correspond to the difference between their *levels* in that horizon and their pre-shock level (relative to cumulative trend growth up to that horizon). The system is estimated as a stationary VAR as opposed to a VAR in levels due to the superiority of the former over the latter in terms of the identification of the long-run impulse responses (Phillips (1998)).[13] The Akaike information criterion favors four lags whereas the Schwartz and Hannan-Quinn information criteria favor two and one lags, respectively. As a benchmark, I choose to estimate a VAR with three lags. The results are robust to using a different number of lags.

The set of admissible models consists of 1297 models (out of a total of $10^6$ posterior draws of models). Figures 1a and 1b depict the median and 84th and 16th percentiles of the posterior distributions of impulse responses and FEV contributions at all horizons up to the 10-year one,

[13] Applying the cointegration test developed in Pesaran et al. (2001) to my model, which is a mixture of both non-stationary and stationary variables and thus requires using the cointegration test from Pesaran et al. (2001), I found no evidence for cointeration among the non-stationary variables in my model. Therefore, I resorted to estimations that abstract from cointegration.

respectively.

By construction, the identified shock raises the real aggregates (output, hours, investment, and consumption) on impact and drives the bulk of their business cycle variation. The 16th percentile impact effects of IST news shocks on output, hours, investment, and consumption are 0.32%, 0.19%, 1.1%, and 0.2%, respectively, while the median impact effects are 0.41%, 0.26%, 1.4%, and 0.26%, respectively. All of the latter effects are economically significant and point to the strong business cycle comovement that the business cycle shock generates. It should be noted that these significant effects are not imposed upon by construction as the only restriction imposed on the impact effects is that they are positive. The 16th percentile contributions of IST news shocks to the variation in output, hours, investment, and consumption at the two-year horizon are 59%, 55%, 55%, and 53%, respectively, while the median contributions are 68%, 66%, 63%, and 61%, respectively, all indicating that the identified shocks are the major force behind the business cycle. While the latter contributions were restricted to be at least 50%, it is apparent that a large part of the distribution of contributions clearly contains bigger values.

In terms of the implications of the business cycle shock for inflation and interest rates, the results indicate that the shock is deflationary and raises interest rates. That inflation falls in tandem with the rise in economic activity makes it unlikely that the business cycle shock is a pure demand shock, or at least a shock whose main propagation mechanism is demand driven. This observation allows to argue that it is unlikely that the business cycle shock corresponds to demand-type shocks such as monetary policy shocks, government spending shocks, noise shocks, credit supply shocks, and uncertainty shocks.

So as to obtain information on the structural features of the shock, I now turn to focusing on its long-run implications for RPI and TFP. Table 2 shows the median and 84th and 16th percentiles of the long-run impulse responses and FEV shares of RPI and TFP due to the business cycle shock. The median contributions to the long-run variation in RPI and TFP are 80% and 54%, respectively, with corresponding long-run impulse responses of -2.4% and 1%. These estimates clearly indicate that the business cycle shock has very large effects on both variables, where that on RPI strongly suggests that this shock is likely to be an IST shock. In the presence of the standard assumption that IST shocks are the sole source of the long-run variation in RPI, this 80% FEV contribution estimate implies that the business cycle shock is very unlikely to contain a non-IST shock.

More formally, note that any identified shock is a linear combination of reduced form innovations, each of which is itself a linear combination of structural shocks (under the standard assumption of equality between the number of observables and number of structural shocks); this, in turn, renders the identified business cycle shock representable as a linear combination of shocks that include both IST and non-IST shocks. Hence, the fact that it explains 80% of the long-run variation in RPI implies that the weight on the non-IST shock portion of this linear combination is $1 - 0.8^{0.5}$, or 9%.[14,15] This emphasizes the importance of the RPI FEV results in facilitating the structural interpretation of the business cycle shock as an IST shock. And, importantly, it rules out the interpretation of the business cycle shock as a TFP shock.

How, then, can one interpret the strong long-run effect of the business cycle shock on TFP? Notably, the effect on TFP only becomes really noticeable at medium- to long-run horizons. E.g., we see from Figure 1b that only 10% of TFP variation is accounted for by the business cycle shock at the five-year horizon. This TFP behavior is consistent with a GPT-based interpretation of IST where gains in the latter lead to medium- and long-run gains in TFP by inducing long-term fundamental changes in the production process of the sectors using the new IST-related goods. (These results are consistent with those from Chen and Wemy (2015), who find that IST changes are an important source of long-run TFP movements.) Taken together, the results on the long-run behavior of RPI and TFP indicate that the business cycle shock is either an unanticipated IST shock or an IST news shock, as macroeconomic theory implies that IST is the long run driver of RPI, and that owing to their general-purpose nature IST improvements lead to long-term gains in TFP. I now turn to demonstrating how additional information on the shock series itself can help distinguish

[14] To see this, denote the identified business cycle shock by $\epsilon_t^{bs}$ and let it be represented as a weighted average of IST and non-IST shock components, $\epsilon_t^{bs} = \omega_1 \epsilon_t^{ist} + \omega_2 \epsilon_t^{non\_ist}$. (The $\epsilon_t^{ist}$ component can be taken to be the sum of surprise and anticipated IST shocks, while $\epsilon_t^{non\_ist}$ can be taken to be the sum of all remaining non-IST shocks.) Since in the long run only the first component should have a non-negligible contribution to RPI variation and since the long-run RPI FEV attributable to the business cycle shock is 0.8, we can deduce that $\omega_1^2 = 0.8$. But since $\omega_1 + \omega_2 = 1$, we obtain that $\omega_2 = 1 - 0.8^{0.5} = 0.09$.

[15] Note that the long-run estimates are not directly shown in Figures 1a and 1b as these figures pertain to only the first 10 years following the shock whereas the long-run estimates are computed from the permanent responses of the non-stationary variables. Given the rather strongly gradual nature of the impulse response of RPI and TFP, the 10 year estimates are downward biased estimates of the long-run response estimates.

between the two IST shocks and provide an interpretation of the shock as an IST news shock.

**Boom-bust Behavior of the Shock in the Late 1990s-Early 2000s Period.** The real economy and stock market experienced a significant boom in the late 1990s which was followed by a bust in the early 2000s. In particular, in the period 1997-1999 Shiller's cyclically adjusted price-earnings ratio, computed as the ratio of the real price of the S&P 500 index to average real earnings over the previous 10 years, reached its highest levels in the sample peaking at the end of 1999 from which point it began its bust period bottoming out in February 2003. The common view by economists is that the boom and subsequent bust were generally related to overly optimistic expectations about IST that were followed by a downward revision of these expectations (see, e.g., Beaudry and Portier (2004), Jaimovich and Rebelo (2009), Karnizova (2012) (see also references therein), and Ben Zeev (2018)).

The first two rows of Table 3 present the median and 84th and 16th percentiles of the average value of the 1997:Q1-1999:Q4 and 2000:Q1-2003:Q1 shock sub-series for both the business cycle shock and the other shock driving the long-run variation in RPI, respectively.[16] It is apparent that a clear boom-bust pattern is prevalent in the business cycle shock series where the average shock realization is significantly positive in the boom period while being significantly negative during the bust period. The median mean realization for the business cycle shock in the boom period is 0.49 standard deviations compared to 0.04 standard deviations for the corresponding counterpart of the other long-run RPI shock. The median mean realization of the bust period is -0.38 for the business cycle shock compared to -0.06 for the other long-run RPI shock. And the posterior bands around these median estimates clearly indicate that one can be fairly confident in inferring that the business cycle shock strongly exhibits a boom-bust type behavior in the late 1990s and early 2000s period, whereas the other long-run RPI shocks exhibits no such clear pattern.

[16]In 1233 models out of the set of 1297 admissible models the business cycle shock is also one of the two IST shocks, i.e., the shocks driving long-run RPI variation. Moreover, out of these 1233 models, the other long-run RPI shock explains at least 5% of the long-run variation in RPI in 982 models. Hence, the results on the other long-run RPI shock are based on these 982 models so as to only consider models where the other long-run RPI shock is a true IST shock rather than possible measurement error. Notably, I also show the results on the other long-run shock as it is important to check that the other shock that explains the long-run variation in RPI does not display this boom-bust pattern given that this would undermine my ability to obtain a structural interpretation of the business cycle shock based on the boom-bust feature.

While the above-reported results demonstrate that the business cycle shock exhibits an apparent boom-bust behavior in the late 1990s and early 2000s period, it seems worthwhile to also compute the historical decomposition of this boom-bust period in terms of the contribution of the two shocks to movement in investment over this period given that this period is considered to have been an investment-driven episode. The first two rows of Table 4 present the median and 84th and 16th percentiles of the relative contribution of the business cycle shock and the other long-run shock to the movement in investment in the boom-bust period, respectively. In particular, the results from Table 4 show, in percentage terms, how much of the movement in investment in the boom and bust periods is accounted for by the two shocks.[17] It is clear from Table 4 that the business cycle shock accounts for a very significant share of both the investment boom in the late 1990s as well as the subsequent investment bust in the early 2000s. The median shares in the boom and bust periods explained by the business cycle shock are 97% and 161%, respectively, while those explained by the other long-run RPI shock are very negligible and statistically insignificant. The 16th percentile shares explained by the business cycle shock for the boom and bust periods are also large, amounting to 62% and 94%, respectively. These are very strong results which indicate that the business cycle shock is the main force behind the boom-bust investment episode of 1997-2003, whereas the other long-run RPI shock is a negligible one.

Taken together, the results presented so far indicate that the business cycle shock can be interpreted as an IST news shock whose GPT-based properties lead to long-term gains in TFP. I now turn to showing that this shock has also played an important role in driving the actual recessions that have taken place in my sample period.

**Historical Decomposition.** My use of the FEV restriction in defining the business cycle shock in this paper is based on the notion that such a shock should have a major contribution to economic fluctuations *on average*. But one additional property such a shock should desirably posses is having an important role in driving *actual*, historical economic downturns. To test whether my business

[17]The relative contribution is computed as $\frac{contribution\ of\ shock}{percentage\ change\ in\ investment\ in\ deviation\ from\ steady\ state\ growth}$, where the annual steady state growth rate for investment is assumed to be 2.8%, which is the average growth rate in the sample period. Note that a relative contribution of one implies that all of the gain or loss in investment is accounted for by the shock. Investment increased relative to its steady state growth by 17% in the boom period while it declined by 11% in the bust period relative to its steady state growth rate.

cycle shock posses this property, I have computed the historical contribution of this shock to the eight NBER-determined U.S. recessions since 1959.

Table 5 shows the results from doing this historical decomposition. In particular, for each recession the median contribution of the business cycle shock to the peak-to-trough percentage change in each of the four real aggregates' per capita levels (in deviation from trend growth) is estimated. Trend growth rates are computed from the average growth rates of corresponding per capita real aggregates over the sample. The results indicate that the business cycle shock was an important driving force behind seven of the last eight U.S recessions. The only recession in which the business cycle shock played a limited role was the 1981-1982 recession, which is commonly thought of as having been driven by aggressively contractionary monetary policy. Apart for this recession, the business cycle shock contributed to all recessions in an economically and statistically significant manner.

The most recent recession (2007-2009), in which output loss was 7.9%, seems to have been driven in large part by the business cycle shock which contributed 5.5% to that accumulated decline.[18] The business cycle shock has also contributed 2.6%, 3.9%, and 1.5% to the accumulated 2.6%, 5.6%, and 2.6% output losses during the 1960-1961, 1973-1975, and 1990-1991 recessions, respectively. Moreover, that 1.2% of the 1.7% output loss in the 2001 recession is attributed to the business cycle shock is consistent with the IST-news-based interpretation of this shock advanced in this paper, which draws on the notion that a downward revision of expectations about future IST took place after the IST news driven boom of the late 1990s.

Overall, the historical decomposition results emphasize that the business cycle shock is not only a dominant driver of U.S. business cycles *on average*, but also a dominant driver of actual historical recessions that have taken place in my sample period.

[18]Importantly, the results of this paper are not driven by the inclusion of the recent recession in the sample as I have confirmed that stopping the sample at 2007:Q4 yields similar results to the baseline ones. These results are presented in Appendix D.4 of the online appendix to this paper.

# 6 Robustness Analysis

I have examined the robustness of the baseline results along ten dimensions. The first deals with showing that the stationary hours specification is superior to a non-stationary hours one, where the latter is demonstrated to be an erroneous modeling choice that likely leads to misguided inference. The second confirms that the business cycle shock's IST-news-based structural interpretation is robust to removing the long-run restriction (Restriction 2). The third speaks to the possibility that there may not exist a perfect linear mapping between VAR innovations and economic shocks. The fourth is that over the entire sample period VAR innovations may not be homoscedastic and VAR coefficients may not be stable. The fifth relates to the inclusion of stock prices in the VAR. The sixth concerns the potential implications of the financial crisis and zero lower bound (ZLB) periods for my results. The seventh pertains to the stationary specification choice used in my baseline VAR. The eighth and ninth concern the robustness of the results to using Fernald (2014)'s investment TFP measure and a PCE-based inflation measure, respectively. And the last deals with only imposing that consumption rise on impact following the business cycle shock.

To keep the exposition minimal, I present here only the first two dimensions of my robustness analysis. The remaining eight, whose results continue to support the main message of the paper, are shown in Appendix D of the online appendix to this paper.

## 6.1 Hours Stationarity and the Low-Frequency Comovement between Hours and RPI and TFP Growth Rates

The results presented above were obtained from a VAR in which hours worked were assumed to be stationary and thus entered the system in levels. However, entering hours in differences in the VAR results in a negligible contribution of the business cycle shock to the variation in both RPI and TFP. (The impulse responses and FEV contribution results for the differenced hours specification appear in Figures 2a and 2b, respectively.) The contributions of the shock to the long-run variation in RPI and TFP, not directly shown in Figure 2b, are 2% and 5%, respectively. While the differenced hours specification results in a permanent effect of the business cycle shock on hours (again, not directly shown in Figure 2a, but is clearly indicated by the leveling-off of the response at medium-run horizons), which is at odds with standard macroeconomic theory and thus limits the credibility of this specification as a suitable way for modeling hours, the results from the differenced hours specification are still a concern whose source is worth exploring and understanding.

Motivated by the controversy regarding how hours should be entered in the VAR when trying to identify technology shocks, Gospodinov et al. (2011) highlighted that even a small low-frequency correlation between hours and productivity growth can account for the difference in results on technology shocks between levels and first-differenced specifications, as the latter correlation is allowed for in the levels specification but is implicitly shut down in the differenced specification. I shall now demonstrate that the low-frequency correlation of hours with the growth rates of RPI and TFP is very large, making it all the more important to enter hours in the VAR in levels so as to allow for this low-frequency correlation rather than to erroneously shut it down via the differenced hours specification.

**Low-Frequency Correlations.** Table 6 shows the correlations between the HP trends of log and log-first-differences of hours worked and HP trends of log-first-differences of RPI and TFP. While the low-frequency correlations of hours in levels with RPI and TFP growth rates are very high (-0.74 and 0.52, respectively), they are negligible and even oppositely signed when hours are considered in log-first-differences. This stresses the importance of entering hours in levels so as to allow for its strong low-frequency comovement with RPI and TFP growth rates, as opposed to wrongly eliminating it via a first-difference specification. Since Gospodinov et al. (2011) have reported significant biases from a first-differenced specification in the presence of even a small low-frequency component, it is likely that the strong correlations reported in Table 6 would lead to significant biases for my setting if I were to estimate a VAR with log-first-differenced hours.

**Monte Carlo Experiment.** To formalize the argument that the correlations from Table 6 can lead to significant estimation bias from first-differencing hours, I now present evidence from the following Monte Carlo experiment. I generate 100 artificial data sets from VARs that are identical to my empirical VAR, i.e., with hours worked in levels and which comply with Restrictions 1 and

2, and apply my identification procedure (based on $10^5$ posterior draws) to each artificial data set using a VAR that includes hours in first-differences. The objective of this experiment is to study the long-run estimation bias from erroneously entering hours in first-differences in the VAR.[19]

Figures 3a and 3b show the mean estimated median and 84th and 16th percentile impulse responses and FEV contributions, along with the corresponding mean true responses and contributions from the true model. The mean *estimated* impulse responses and FEV contributions are averages over monte carlo simulations; the mean *true* impulse responses and FEV contributions are averages over the 100 DGPs. It is apparent that the mean estimated median responses and FEV contributions for RPI and TFP are significantly downward biased. E.g., while the true FEV contribution to RPI 10-year variation is 57%, the average estimated median contribution is 23%. The numbers for the long-run horizon (not directly shown in the figures) are similarly far apart at 80% and 38%. Similar discrepancies hold for TFP also.

Notably, the proportion of Monte Carlo simulations where estimated median long-run contributions to RPI and TFP FEVs are both below 0.1 is 36% (i.e., for 36 out of the 100 considered artificial data sets, my identification produces an estimated median long-run RPI and TFP FEV contribution of less than 0.1); the proportion for the contributions being both below 0.05 is 24%. These significant proportions indicate that it is very much possible that applying an erroneous differenced hours VAR specification to the actual data could result in the negligible long-run FEV shares I find when specifying hours in first-differences, supporting the view that the actual data is likely generated by a stationary hours based DGP. In sum, these proportions stress the strong likelihood of erroneously inferring that the business cycle shock is unrelated to long-run movements in RPI and TFP when using a VAR with differenced hours.

[19] Appendix C of the online appendix to this paper also presents evidence from an additional experiment that is identical to the first only that I apply there my identification procedure to each artificial data set using a VAR that includes hours in levels, rather than first-differences. The objective of that experiment is to examine the identification precision from correctly specifying hours in levels. Further details on the DGP used for these two experiments are also deferred to the online appendix to this paper so as to save space. Appendix C.2 of the online appendix also presents simulation evidence from removing hours from the VAR, which was found to produce similar results to those from first-differencing hours. These Monte Carlo results are also consistent with the general message of this paper.

## 6.2 Lifting the Long-Run Restriction.

The structural discipline Restriction 2 puts on the long-run behavior of RPI is valuable for the structural interpretation of the business cycle shock as it uses rather weak assumptions to make the set of admissible models be more theory-consistent and hence facilitates their coming closer to the true data generating process. (DSGE model based Monte Carlo evidence supporting this argument is shown in Section 4 and Appendix B.10 of the online appendix to this paper.) Notwithstanding the merit of including Restriction 2 in the analysis, one may argue that showing that the structural interpretation of the business cycle shock advanced in this paper holds also in the absence of this restriction can serve to increase this interpretation's validity.

Toward this end, I now present results from estimating the baseline VAR without imposing Restriction 2. The impulse responses and FEV contributions are shown in Figures 4a and 4b, respectively, while the first two rows of Table 7 depict the long-run impulse response and FEV contributions of the business cycle shock for RPI and TFP and the first two rows of Table 8 present its mean realizations for the boom-bust period and contribution to the variation in investment over this period. The results are based on $10^6$ randomly generated models from which a total of 17176 admissible models were collected.

It is clear that the business cycle shock still has a significant and rather large effect on both RPI and TFP, explaining 44% and 38% of their long-run variation, respectively. While these numbers are lower than their baseline counterparts, they are still sufficiently large on their own to make a valid case that there is likely to be an important IST shock component in the business cycle shock. Notably, the difference between the baseline 80% number and the 44% number accords well with the Monte Carlo results from Appendix B.10 of the online appendix to this paper, which indicate a large downward bias in the estimation of the long-run contribution to RPI variation resulting from removing Restriction 2. Turning to the boom-bust based results from Table 8, it becomes apparent that the business cycle shock continues to exhibit a very clear boom-bust pattern over the late 1990s-early 2000s period in tandem with explaining most of the variation in investment over this period. Taken together with the long-run based results, and drawing again on the IST-news-based narrative of this period, these findings indicate that the business cycle shock is likely to be an IST news shock.

# 7 The Importance of Properly Measuring RPI

In recent work that applies Uhlig (2003)'s identification approach more comprehensively by separately applying it to several macro variables and by looking at various truncation horizons, Angeletos et al. (2020) find that the shock that drives most of economic fluctuations seems to be unrelated to long-run movements in TFP and RPI. As argued on Page 6, an important source of the differences between mine and Angeletos et al. (2020)'s results lies in the measure of RPI being used. Specifically, the RPI measure in Angeletos et al. (2020) considers all investment sub-sectors (along with consumer durables), including residential and commercial structures, which the literature has not regarded as possessing the same type of underlying technology as the commonly considered sub-sectors of investment equipment and consumer durables do. (Their measure also does not account for quality adjustment as previous literature has because of their not using the GCV deflator series.)

To confirm the importance of this RPI measurement issue, I conduct the following three estimation exercises. The first consists of applying my baseline estimation procedure to the RPI measure used in Angeletos et al. (2020) while the second and third apply the estimation procedure from Angeletos et al. (2020) to the baseline RPI measure and their RPI measure, respectively. I now turn to discussing the results from these estimation exercises.

**Applying the Baseline Estimation to Angeletos et al. (2020)'s RPI Measure.** Figures 5a and 5b present the impulse responses and FEV contributions from replacing the baseline RPI measure with that used by Angeletos et al. (2020). Results are based on $10^6$ randomly generated models from which a total of 482 admissible models were collected. The FEV share for the Angeletos et al. (2020) RPI measure is small for all considered horizons, only reaching 6% in the long run. This result stresses the importance of using a suitable RPI measure for properly uncovering the true nature of the business cycle shock. Nevertheless, to further reinforce this argument, it is also important to examine the effect of RPI measure choice on the results from using the identification approach from Angeletos et al. (2020), which is what I turn to next.

**Applying Angeletos et al. (2020)'s Estimation to the Baseline RPI Measure.** Angeletos et al. (2020)'s baseline estimation is conducted in the frequency domain and for a levels-VAR. Nevertheless, since they have shown that their results are robust to doing the analysis in the time domain and to using stationary specifications, I proceed by applying their methodology in the time domain to the same stationary baseline specification I consider throughout my analysis. Specifically, I identify the business cycle shock as the shock that contributes maximally to the 8-quarter FEV of output for two different VARs:[20] one that includes the baseline RPI measure and one that includes the RPI measure from Angeletos et al. (2020). The impulse responses and FEV contributions for the former specification are shown in Figures 6a and 6b while those for the latter specification appear in Figures 7a and 7b. Results for these two estimations are based on 2000 posterior draws of the point-identified max-share based impulse responses and FEV contributions.

The results clearly manifest the importance of RPI measure choice for the contribution of the business cycle shock to RPI variation. At the 10-year horizon, the shock from the VAR with Angeletos et al. (2020)'s RPI measure accounts for only 7% of the variation in RPI while accounting for 32% of RPI variation when the baseline RPI measure is used. In the long run this difference grows even further apart with the long-run contribution for the VAR with Angeletos et al. (2020)'s RPI measure being merely 13% compared to a meaningful 46% for the VAR with the baseline RPI measure. Notably, the 46% number is similar to the 44% number from the estimation that removes the long-run restriction (see Section 6.2), indicating that Angeletos et al. (2020)'s estimation approach is broadly similar to mine along the short-run dimension of the two methodologies. But, importantly, the merit of imposing my long-run restriction has been shown both in the context of its capacity of reduce the risk of spurious identification (see Section 4.1) as well as its capacity to avoid the large downward bias in the estimation of the long-run contribution to RPI variation stemming from the removal of this long-run restriction (see Appendix B.10 of the online appendix to this paper). Hence, on top of the RPI choice issue, an additional advantage of my approach relative to that from Angeletos et al. (2020) is my introduction of Restriction 2.

In sum, the results from Figures 6a-7b indicate that Angeletos et al. (2020)'s structural inter-

[20] The results for the 8-quarter output FEV targeting based estimation from Angeletos et al. (2020) appear in their Figure 13b and last row of Table 14.

pretation of their business cycle shock is sensitive to the choice of RPI measure being used.[21] Taken together with the fact that there is a sound basis for the RPI literature's focus on the durable consumption and equipment investment sub-sectors and the associated GCV-based quality adjustment when measuring RPI, these results support the notion that it is important to use a proper RPI measure for correctly ascertaining the type of the business cycle shock and its potential relation to IST.

# 8 Conclusion

This paper has provided robust evidence in favor of GPT news shocks being the major driver behind business cycle fluctuations, where the manifestation of these anticipated GPT shocks takes place in the investment-specific goods sector through IST news shocks. To obtain this evidence, I first computed the set of models in which one shock generates business cycle comovement, i.e., raises output, hours, consumption, and investment on impact, and explains over 50% of the business cycle variation in the latter real aggregates. Then, I examined the common features of this business cycle shock across the models and found that this shock encompasses two robust characteristics: *i*) it drives the bulk of the long-run variation in RPI and has a significant long-run effect on TFP, reducing the former and raising the latter; and *ii*) it behaves in a boom-bust manner in the late 1990s and early 2000s period, exhibiting significant positive realizations in the former period while experiencing negative realizations in latter period. The first characteristic allows to determine that the shock is likely a GPT shock, represented by either an unanticipated IST shock or an IST news shock, which leads to long-term TFP gains by generating delayed fundamental changes in the production process of the sectors using the new IST-related goods, whereas the second feature allows to deduce that it is an IST news shock given the common view of the late 1990s and early 2000s as having been driven by favorable expectations about IST that were later

[21] It is also noteworthy that the business cycle shock drives a significant share of the long-run variation in TFP regardless of the RPI measure being used and in contrast to the general message of Angeletos et al. (2020)'s paper. In Table 14 of their paper, Angeletos et al. (2020) only report the business cycle frequency FEV for TFP rather than medium- and long-run frequency contributions. In my results from Figure 7b the business cycle shock indeed accounts for only 11% and 13% of the two- and three-year variation in TFP, in accordance with the FEV estimates reported by Angeletos et al. (2020), but at the 10-year and long-run horizons this share reaches 44% and 60%, respectively.

revised downwards.

Taken together with the results from Ben Zeev (2018), this paper's results suggest an empirical "if and only if" connection between the following two statements: 1) an IST news shocks is the main business cycle driving shock and 2) the main business cycle driving shock is an IST news shock. Ben Zeev (2018) shows robust empirical support for the first statement based on identification of the IST news shocks as being one of the two shocks (the other being the unanticipated IST shock) driving the long-run variation in RPI and whose realizations follow a boom-bust pattern in the late 1990s and early 2000s period. And this paper provides robust empirical support for the second statement by showing that the identified 'business cycle shock' - which is not restricted upon to be one of the two (unidentified) shocks driving RPI's long-run variation - shares the properties defining the identified IST news shock from Ben Zeev (2018), as manifested by an extremely high correlation of 94.5% between the two (apriori unrelated) shock series. In other words, there seems to be a convincing case for empirical equivalence between the IST news shock and 'business cycle shock' objects.

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Table 1: **DSGE Model Based Monte Carlo Experiment.**

| | Null Identification | Admissibility Rate |
|---|---|---|
| With Long-Run RPI Restriction | 96% | $1.75 \times 10^{-5}$ |
| Without Long-Run RPI Restriction | 85% | $2.7 \times 10^{-5}$ |

*Notes*: This table presents the share of simulations in which identification was null (first column) along with the average admissibility rate (average number of admissible models divided by total number of posterior draws ($10^5$)) for the simulations that did produce a non-null set of admissible models (second column). A total of 100 simulations were conducted (corresponding to 100 artificial data sets from the DSGE model described in Appendix B of the online appendix to this paper) with the first row of the table providing results from applying my baseline identification procedure to each data set using the baseline calibration; and the second row providing results from applying the baseline procedure but without imposing the long-run RPI restriction (Restriction 2) while using the baseline calibration.

Table 2: **Long-Run Implications of Business Cycle Shock for RPI and TFP.**

| | Impulse Response | Forecast Error Variance Contribution |
|---|---|---|
| RPI | -2.4% [-5.2%,-1.5%] | 80% [61%,88%] |
| TFP | 1% [0.5%,2.4%] | 54% [21%,78%] |

*Notes*: This table presents the median and 16th and 84th percentiles of the long-run impulse responses and FEV shares of RPI and TFP due to the business cycle shock in the baseline VAR. The 16th and 84th percentiles appear in squared brackets next to the median estimate.

Table 3: **Mean Realization of Business Cycle Shock and Other Long-Run (Non-Business-Cycle) RPI Shock in Boom-Bust Period.**

| | Business Cycle Shock | Other Long-Run Shock |
|---|---|---|
| Boom Period Mean Realization | 0.49 [0.34,0.65] | 0.04 [-0.22,0.31] |
| Bust Period Mean Realization | -0.33 [-0.57,-0.18] | -0.06 [-0.35,0.27] |

*Notes*: This table presents the median and 16th and 84th percentiles of the mean realization of the business cycle shock and the other shock driving long-run RPI variation in the boom (1997:Q1-1999:Q4) and bust (2000:Q1-2003:Q1) periods. Results for the baseline VAR are shown, where in 1233 models out of the set of 1297 admissible models the business cycle shock is also one the two IST shocks, i.e., the shocks driving long-run RPI variation. To avoid inclusion of non-IST shocks that nonetheless, when coupled with the business cycle shock, drive more than 90% of long-run RPI variation, I only consider for the other long-run RPI shock models where this shock drives at least 5% of the long-run RPI variation, leaving me with 982 such models. Hence, the results on the other long-run RPI shock are based on these 982 models, or 76% of the total number of admissible models (a roughly similar share applies to the corresponding results from the other model specifications considered in Appendix D of the online Appendix to this paper and shown in Table D.2).

Table 4: **Contribution of Business Cycle Shock and Other Long-Run (Non-Business-Cycle) RPI Shock to Investment Boom-Bust Episode.**

| | Business Cycle Shock | Other Long-Run Shock |
|---|---|---|
| Boom Period Contribution | 97% [62%,131%] | 7% [-13%,37%] |
| Bust Period Contribution | 161% [94%,226%] | 1% [-38%,39%] |

*Notes*: This table presents the median and 16th and 84th percentiles of the contribution (in %) of the business cycle shock and the other long-run RPI shock to the change in investment in the boom (1997:Q1-1999:Q4) and bust (2000:Q1-2003:Q1) periods. Results for the baseline VAR are shown, where the contribution is computed as $\frac{contribution\ of\ shock}{percentage\ change\ in\ investment\ in\ deviation\ from\ steady\ state\ growth}$, with the annual steady state growth rate being the average growth rate for the 1959:Q1-2017:Q3 sample. Note that a relative contribution of 100% implies that all of the gain or loss in investment is accounted for by the shock.

Table 5: **Historical Contribution of Business Cycle Shock to Real Aggregates' Per Capita Loss in U.S. Recessions (In %).**

| | Output | | Investment | | Consumption | | Hours | |
|---|---|---|---|---|---|---|---|---|
| Recession | Data | Contribution | Data | Contribution | Data | Contribution | Data | Contribution |
| 1960:2-1961:1 | -2.6 | -2.6<br>[-3.3,-1.8] | -12.4 | -9.4<br>[-12.6,-6.5] | -1.2 | -1.5<br>[-1.9,-1.2] | -3 | -2.3<br>[-3.2,-1.4] |
| 1969:4-1970:4 | -4 | -1.8<br>[-3,-0.5] | -11.7 | -6.4<br>[-10.2,-2.7] | -0.7 | -0.9<br>[-1.7,-0.1] | -5.1 | -2.2<br>[-3.3,-0.8] |
| 1973:4-1975:1 | -5.6 | -3.9<br>[-5.3,-2.3] | -15.5 | -12.2<br>[-16.5,-6.8] | -4.5 | -2.6<br>[-3.6,-1.5] | -4.1 | -4<br>[-5.4,-2.4] |
| 1980:1-1980:3 | -3.8 | -1<br>[-1.7,-0.3] | -15.3 | -3.7<br>[-5.9,-1.5] | -1.9 | -0.6<br>[-1.1,-0.1] | -3 | -1.1<br>[-1.7,-0.6] |
| 1981:3-1982:4 | -6.1 | -1.2<br>[-3,0.7] | -20.8 | -3.7<br>[-10.3,2.8] | -0.4 | -0.8<br>[-1.8,0.3] | -4.7 | -0.2<br>[-2.4,2.1] |
| 1990:3-1991:1 | -2.6 | -1.5<br>[-2.1,-1] | -9.8 | -6<br>[-7.8,-4.2] | -1.7 | -0.9<br>[-1.3,-0.6] | -1.9 | -1.6<br>[-2,-1.1] |
| 2001:1-2001:4 | -1.7 | -1.2<br>[-1.9,-0.4] | -4.8 | -5.5<br>[-8.3,-3] | -1 | -0.4<br>[-0.9,0.1] | -4.2 | -2.1<br>[-2.8,-1.4] |
| 2007:4-2009:2 | -7.9 | -5.5<br>[-7.3,-3.9] | -34 | -18.7<br>[-25.2,-12.8] | -4.8 | -3.4<br>[-4.6,-3.3] | -10.2 | -5.8<br>[-7.8,-3.8] |

*Notes*: This table presents the estimates of the contribution of the business cycle shock to each of the recessions in my sample period. The first column ('Data') for each variable presents the percentage change from peak to trough of the corresponding real aggregate per capita, relative to trend growth, in every recession. The second column reports the median contribution of the business cycle shock to the corresponding real aggregate's loss with the numbers in squared brackets below it representing the 16th and 84th posterior percentiles of the contribution. Trend growth rates are computed from the average growth rates of each real aggregate per capita over the sample.

Table 6: **Low-Frequency Correlation of Hours Worked in Levels and Differences with RPI and TFP Growth Rates.**

| | HP-Trend of RPI Growth | HP-Trend of TFP Growth |
|---|---|---|
| HP-Trend of Hours Worked | -74% | 52% |
| HP-Trend of Hours Worked Growth | 8% | -4% |

*Notes*: This table presents the correlations (in %) of the HP trends of hours worked in logs and log-first-differences with the HP trends of the log-first-differences of RPI and TFP.

Table 7: **Lifting the Long-Run Restriction: Long-Run Implications of Business Cycle Shock for RPI and TFP.**

| | Impulse Response | Forecast Error Variance Contribution |
|---|---|---|
| RPI | -1.5% [-3.2%,-0.7%] | 44% [12%,69%] |
| TFP | 0.8% [0.3%,1.7%] | 38% [11%,65%] |

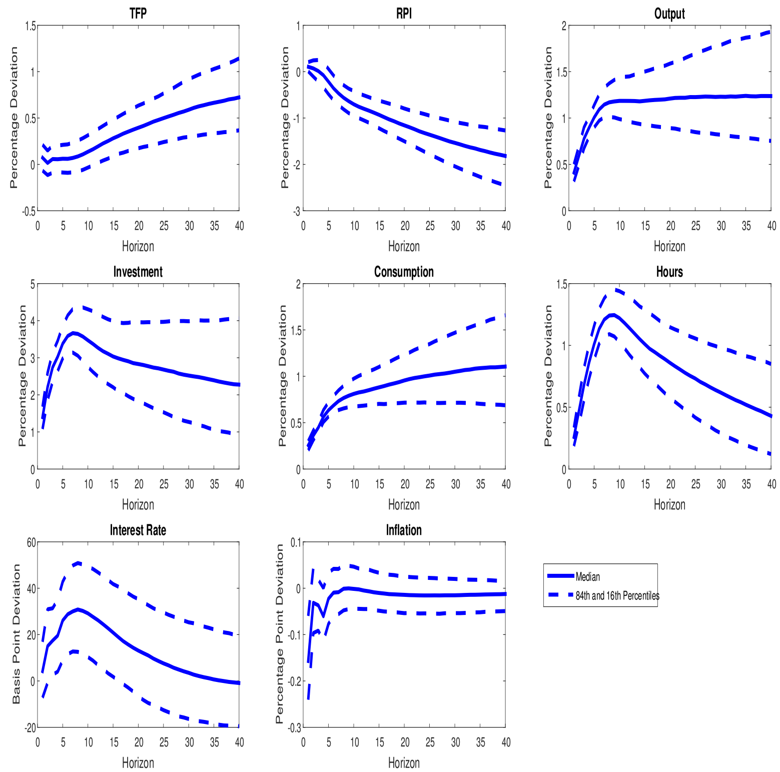
*Notes*: This table presents the median and 16th and 84th percentiles of the long-run impulse response and FEV share of RPI and TFP due to the business cycle shock from an estimation that only imposes Restriction 1 (excluding Restriction 2). The 16th and 84th percentiles appear in squared brackets next to the median estimate.

Table 8: **Lifting the Long-Run Restriction: Mean Realization of Business Cycle Shock and Contribution to Investment Variation in Boom-Bust Period.**

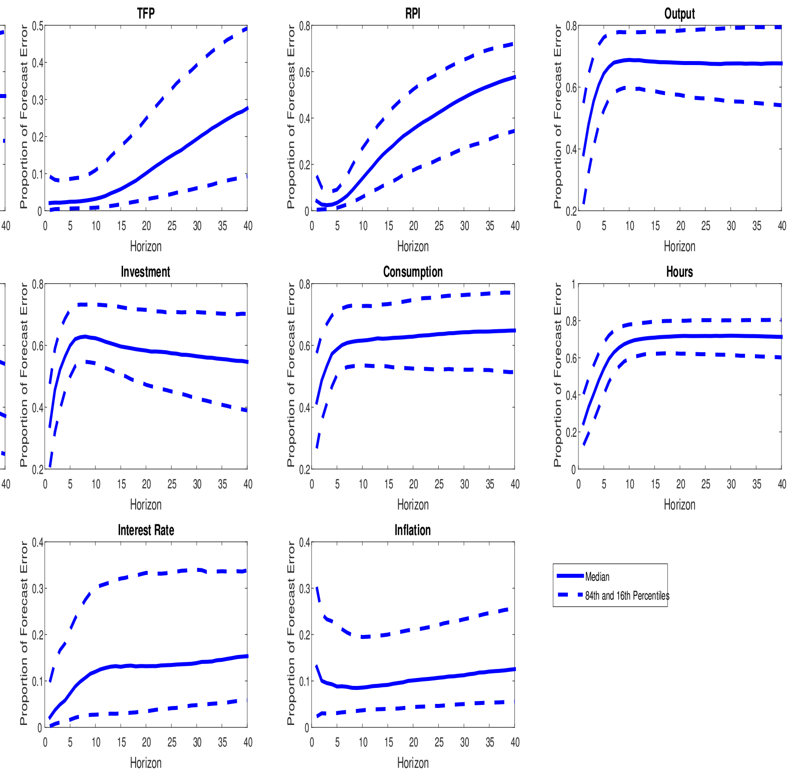
| | Mean Realization | Contribution to Investment Variation |
|---|---|---|
| Boom Period | 0.51 [0.38,0.64] | 85% [53%,120%] |
| Bust Period | -0.35 [-0.54,-0.17] | 166% [102%,229%] |

*Notes*: This table presents the median and 16th and 84th percentiles of the mean realization of the business cycle shock and the contribution (in %) of this shock to the change in investment in the boom (1997:Q1-1999:Q4) and bust (2000:Q1-2003:Q1) periods from an estimation that only imposes Restrictions 1 (excluding Restriction 2). The contribution is computed as $\frac{contribution\ of\ shock}{percentage\ change\ in\ investment\ in\ deviation\ from\ steady\ state\ growth}$, where the annual steady state growth rate for investment is assumed to be 2.8%, which is the average growth rate for the sample period. Note that a relative contribution of 100% implies that all of the gain or loss in investment is accounted for by the shock.

Figure 1: **Baseline VAR: (a) Impulse Responses; (b) Contribution to FEV.**



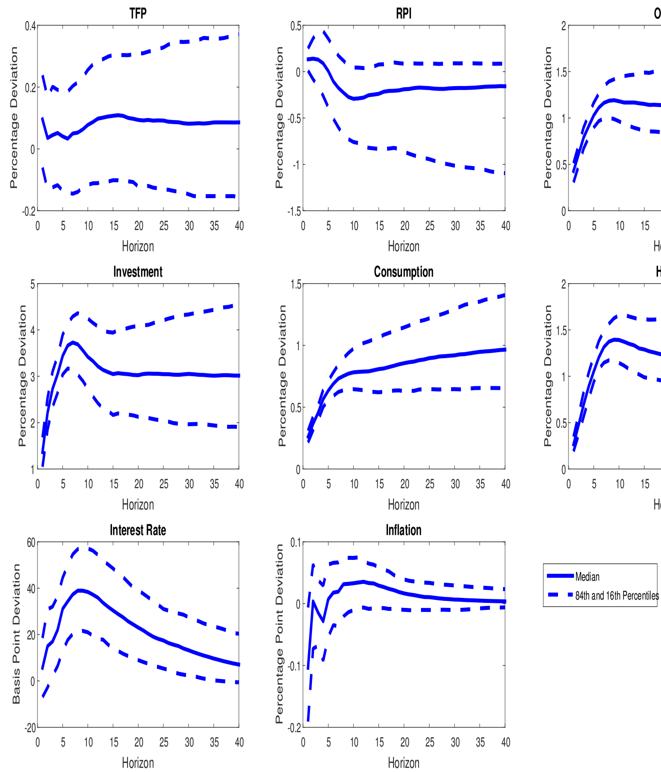

**(a) The Median and 84th and 16th percentiles of the Impulse Responses to the Business Cycle Shock.**



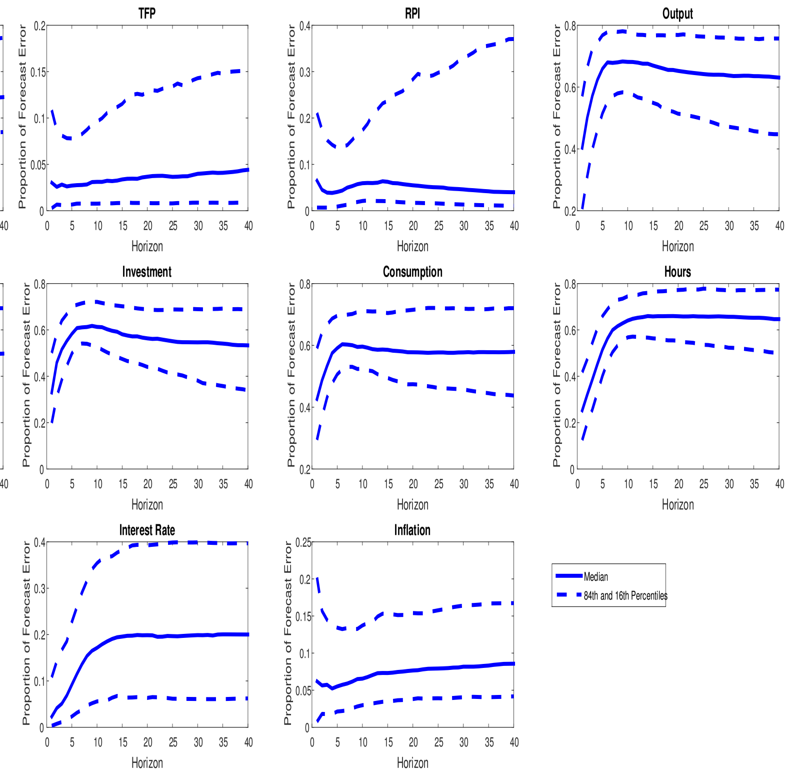

**(b) The Median and 84th and 16th Percentiles of the Contribution of the Business Cycle Shock to the FEV of the Variables.**

*Notes*: Panel (a): The solid line is the median impulse response and the dashed lines are the 84th and 16th percentiles of the posterior distributions of impulse responses from the baseline VAR. Responses are in terms of deviations from pre-shock values. Panel (b): The solid line is the median FEV contribution and the dashed lines are the 84th and 16th percentiles of the posterior distribution of FEV contributions from the baseline VAR.

Figure 2: **Differenced Hours VAR: (a) Impulse Responses; (b) Contribution to FEV.**



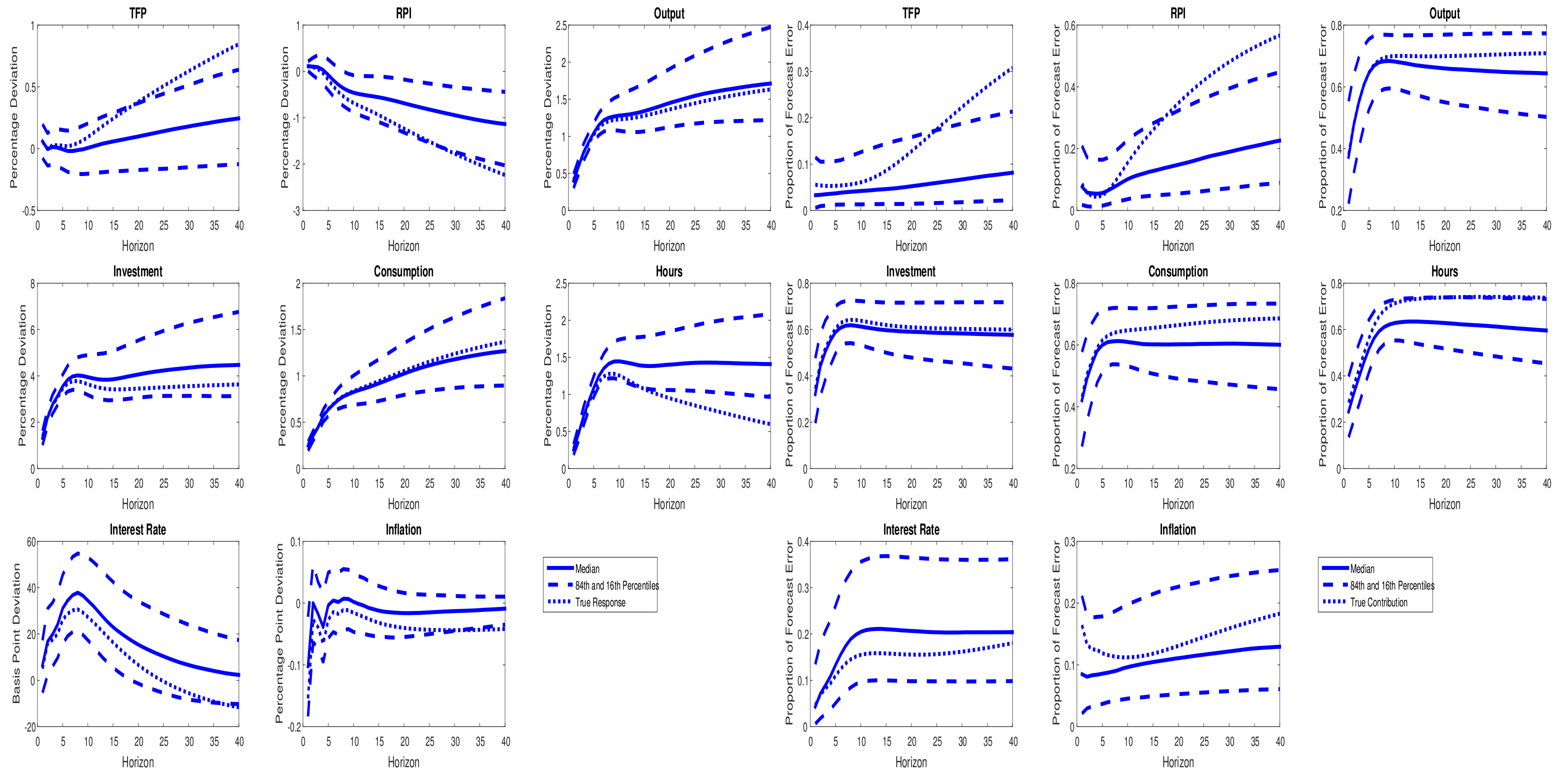

**(a) The Median and 84th and 16th percentiles of the Impulse Responses to the Business Cycle Shock.**




**(b) The Median and 84th and 16th Percentiles of the Contribution of the Business Cycle Shock to the FEV of the Variables.**

*Notes*: Panel (a): The solid line is the median impulse response and the dashed lines are the 84th and 16th percentiles of the posterior distributions of impulse responses from a VAR where hours worked are specified in log-first-differences rather than in log-levels. Responses are in terms of deviations from pre-shock values. Panel (b): The solid line is the median FEV contribution and the dashed lines are the 84th and 16th percentiles of the posterior distribution of FEV contributions from a VAR where hours worked are specified in log-first-differences rather than in log-levels.

Figure 3: **Monte Carlo Evidence from a Non-Stationary Hours Specification: (a) Impulse Responses; (b) Contribution to FEV.**

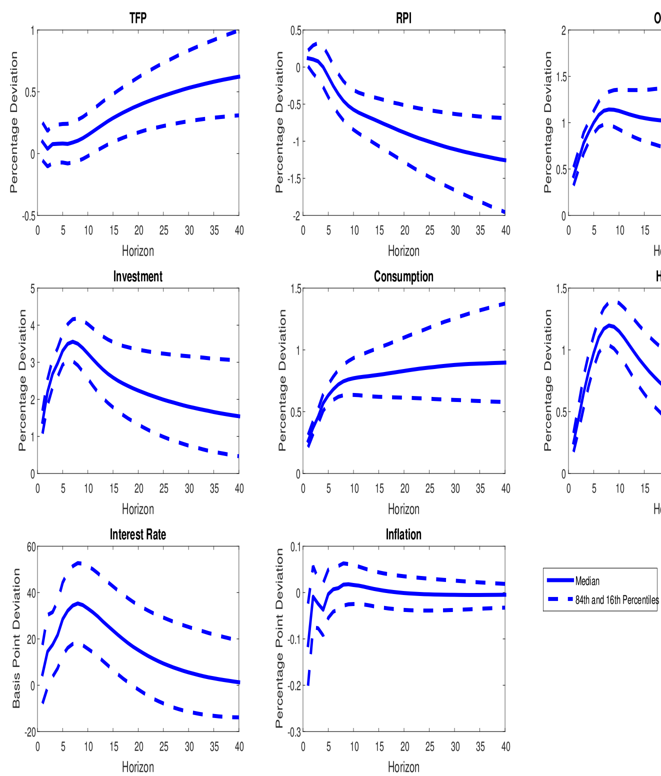



**(a) The Mean Estimated Median and 84th and 16th percentile Impulse Responses and the Mean True Impulse Responses.**

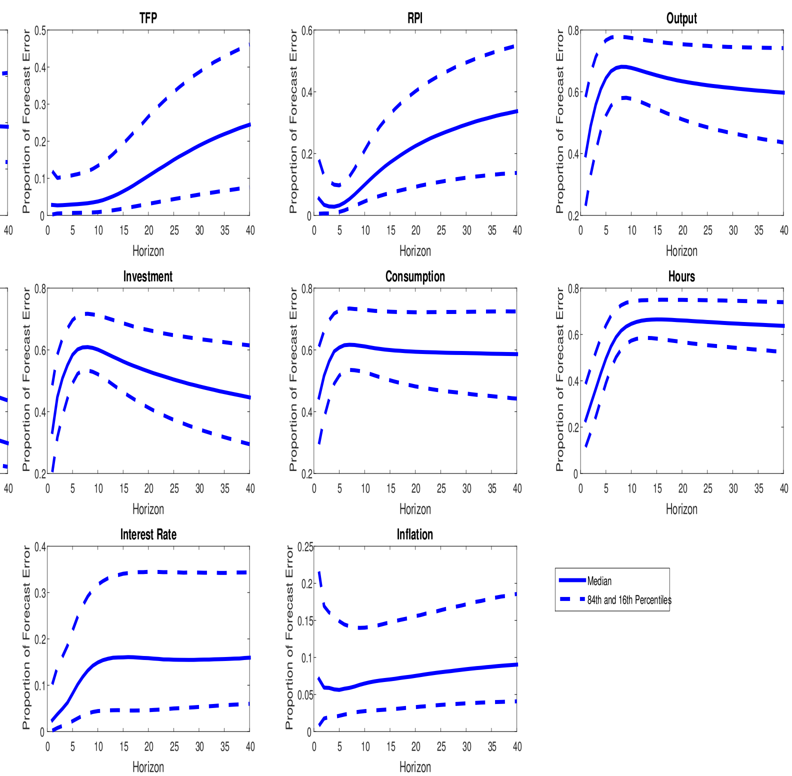
**(b) The Mean Estimated Median and 84th and 16th Percentiles FEV Contributions and the Mean True FEV Contributions.**

*Notes*: This figure presents Monte Carlo evidence on the identification of the business cycle shock from estimating a differenced hours VAR specification with artificial data sets generated from a levels hours VAR. Panel (a): The solid line is the average estimated median impulse response across monte carlo simulations, the dashed lines are the 84th and 16th mean estimated percentiles, and the dotted line represents the average true impulse response across the data generating processes. Responses are in terms of deviations from pre-shock values. Panel (b): The solid line is the average estimated median FEV contribution across monte carlo simulations, the dashed lines are the 84th and 16th mean estimated percentiles, and the dotted line represents the average true contribution across the data generating processes.

Figure 4: **Lifting the Long-Run Restriction: (a) Impulse Responses; (b) Contribution to FEV.**



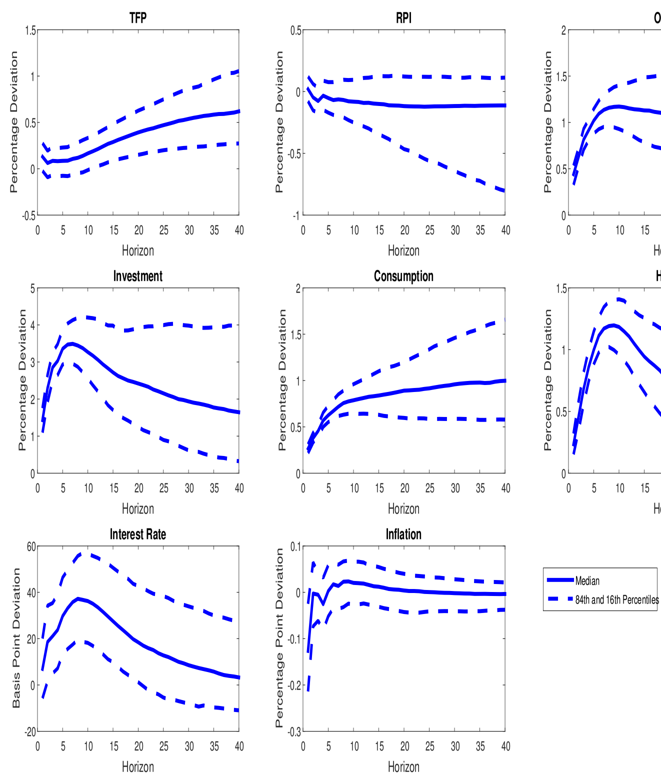

**(a) The Median and 84th and 16th percentiles of the Impulse Responses to the Business Cycle Shock.**



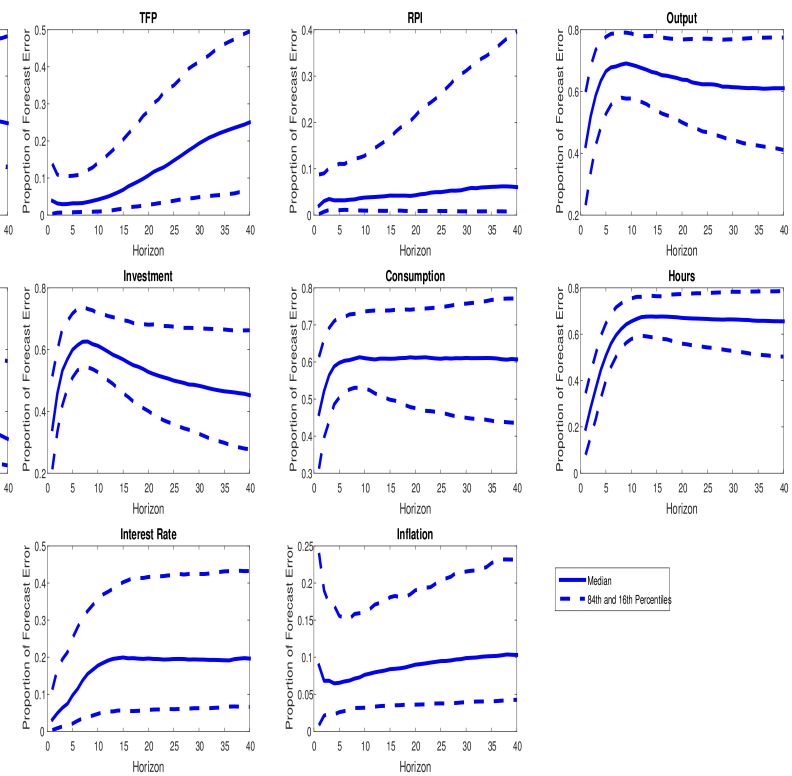

**(b) The Median and 84th and 16th Percentiles of the Contribution of the Business Cycle Shock to the FEV of the Variables.**

*Notes*: Panel (a): The solid line is the median impulse response and the dashed lines are the 84th and 16th percentiles of the posterior distributions of impulse responses from an estimation that does not impose the long-run restriction (Restriction 2). Responses are in terms of deviations from pre-shock values. Panel (b): The solid line is the median FEV contribution and the dashed lines are the 84th and 16th percentiles of the posterior distribution of FEV contributions from an estimation that does not impose the long-run restriction (Restriction 2).

Figure 5: Angeletos et al. (2020)'s RPI Measure: (a) Impulse Responses; (b) Contribution to FEV.



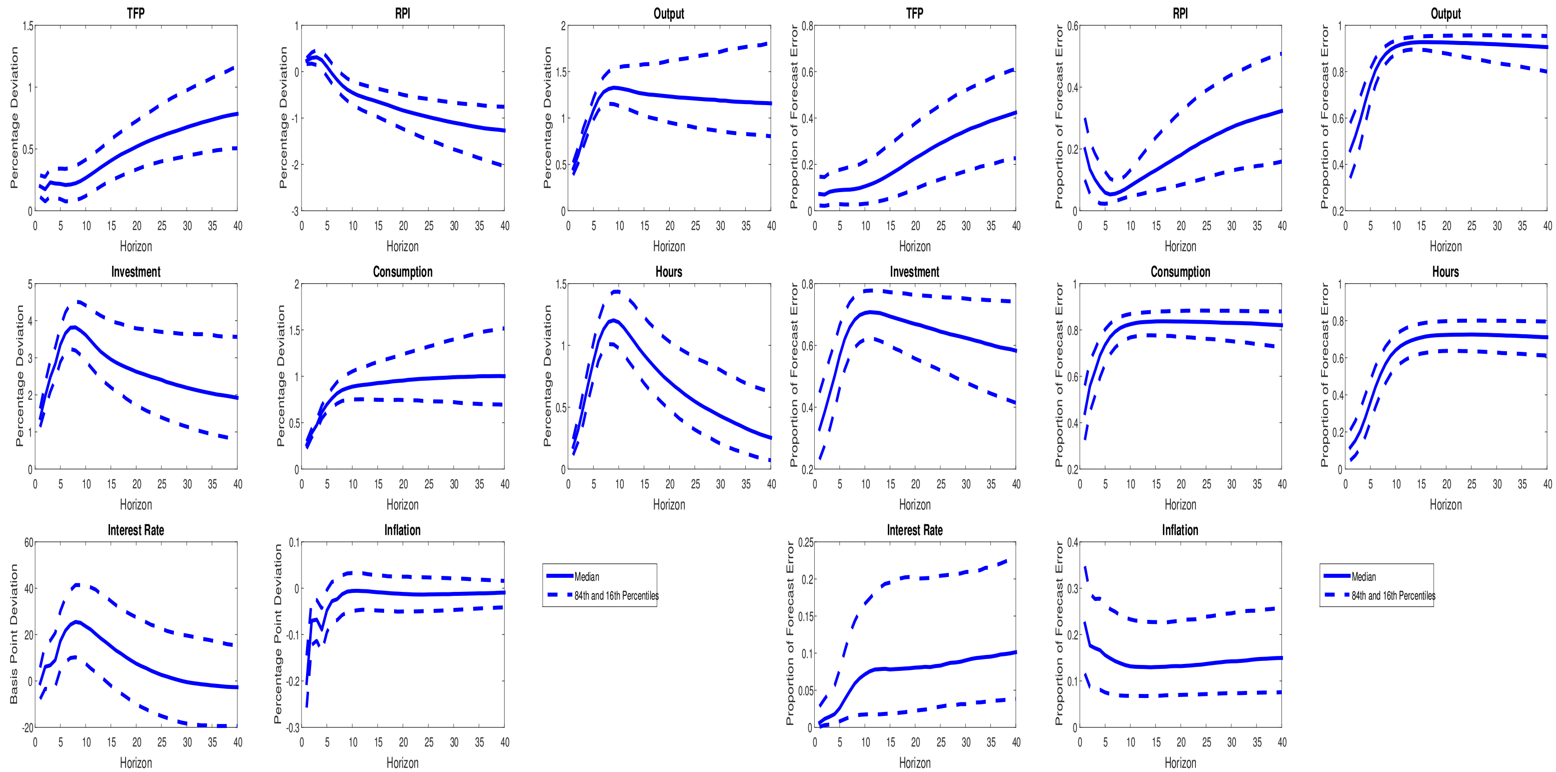

**(a) The Median and 84th and 16th percentiles of the Impulse Responses to the Business Cycle Shock.**




**(b) The Median and 84th and 16th Percentiles of the Contribution of the Business Cycle Shock to the FEV of the Variables.**

*Notes*: Panel (a): The solid line is the median impulse response and the dashed lines are the 84th and 16th percentiles of the posterior distributions of impulse responses from applying the baseline estimation procedure to a VAR where the baseline RPI measure is replaced with Angeletos et al. (2020)'s RPI measure. Responses are in terms of deviations from pre-shock values. Panel (b): The solid line is the median FEV contribution and the dashed lines are the 84th and 16th percentiles of the posterior distribution of FEV contributions from applying the baseline estimation procedure to a VAR where the baseline RPI measure is replaced with Angeletos et al. (2020)'s RPI measure.

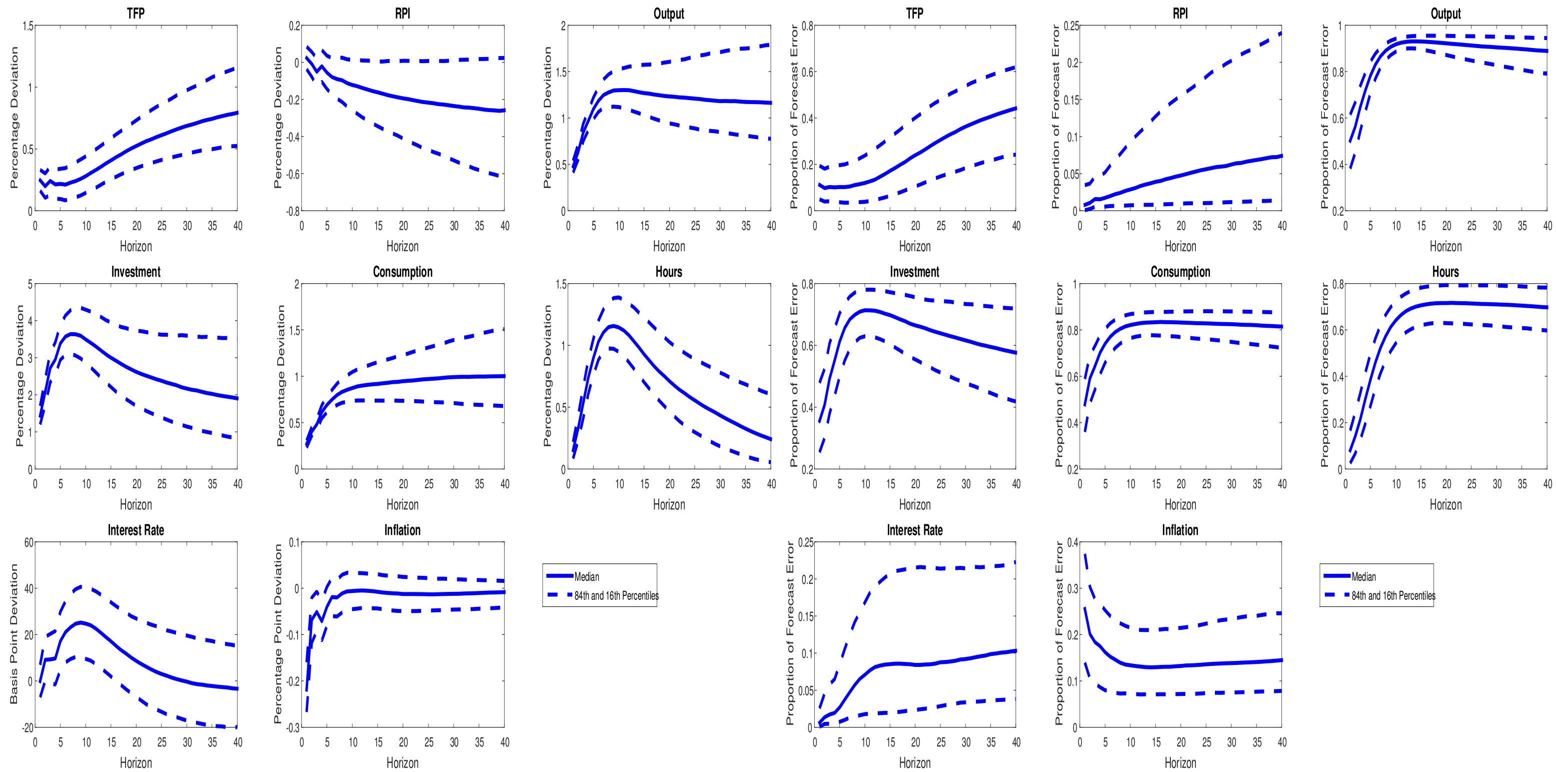
Figure 6: **Applying Angeletos et al. (2020)'s Estimation Procedure to the Baseline VAR: (a) Impulse Responses; (b) Contribution to FEV.**




**(a) The Median and 84th and 16th percentiles of the Impulse Responses to the Business Cycle Shock.** **(b) The Median and 84th and 16th Percentiles of the Contribution of the Business Cycle Shock to the FEV of the Variables.**

*Notes*: Panel (a): The solid line is the median impulse response and the dashed lines are the 84th and 16th percentiles of the posterior distributions of impulse responses from applying the estimation procedure from Angeletos et al. (2020) to the baseline VAR. Responses are in terms of deviations from pre-shock values. Panel (b): The solid line is the median FEV contribution and the dashed lines are the 84th and 16th percentiles of the posterior distribution of FEV contributions from applying the estimation procedure from Angeletos et al. (2020) to the baseline VAR.

Figure 7: **Applying Angeletos et al. (2020)'s Estimation Procedure to a VAR with Angeletos et al. (2020)'s RPI Measure: (a) Impulse Responses; (b) Contribution to FEV.**




**(a) The Median and 84th and 16th percentiles of the Impulse Responses to the Business Cycle Shock.**

**(b) The Median and 84th and 16th Percentiles of the Contribution of the Business Cycle Shock to the FEV of the Variables.**

*Notes*: Panel (a): The solid line is the median impulse response and the dashed lines are the 84th and 16th percentiles of the posterior distributions of impulse responses from applying the estimation procedure from Angeletos et al. (2020) to a VAR where the baseline RPI measure is replaced with Angeletos et al. (2020)'s RPI measure. Responses are in terms of deviations from pre-shock values. Panel (b): The solid line is the median FEV contribution and the dashed lines are the 84th and 16th percentiles of the posterior distribution of FEV contributions from applying the estimation procedure from Angeletos et al. (2020) to a VAR where the baseline RPI measure is replaced with Angeletos et al. (2020)'s RPI measure.