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Working Paper

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CFS Working Paper Series, No. 661

Provided in Cooperation with:
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Suggested Citation: Kilian, Lutz (2021) : Facts and fiction in oil market modeling, CFS Working Paper Series, No. 661, Goethe University Frankfurt, Center for Financial Studies (CFS), Frankfurt a. M.

This Version is available at:
https://hdl.handle.net/10419/246873

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Facts and Fiction in Oil Market Modeling

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September 18, 2021

Abstract: A series of recent articles has called into question the validity of VAR models of the global market for crude oil. These studies seek to replace existing oil market models by structural VAR models of their own based on different data, different identifying assumptions, and a different econometric approach. Their main aim has been to revise the consensus in the literature that oil demand shocks are a more important determinant of oil price fluctuations than oil supply shocks. Substantial progress has been made in recent years in sorting out the pros and cons of the underlying econometric methodologies and data in this debate, and in separating claims that are supported by empirical evidence from claims that are not. The purpose of this paper is to take stock of the VAR literature on global oil markets and to synthesize what we have learned. Combining this evidence with new data and analysis, I make the case that the concerns regarding the existing VAR oil market literature have been overstated and that the results from these models are quite robust to changes in the model specification.

JEL codes: Q43, Q41, C36, C52

Key words: Elasticity, structural VAR, Bayesian inference, oil price, global real activity, oil inventories.

Acknowledgments: The views expressed in this paper are my own and should not be interpreted as reflecting the views of the Federal Reserve Bank of Dallas or any other member of the Federal Reserve System. I thank Ron Alquist, Brian Prest, Ana Maria Herrera, Atsushi Inoue and Xiaoqing Zhou for helpful discussions. This paper is a substantially rewritten version of an earlier paper with the same title. The author has no conflict of interest to declare.

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1. Introduction

In a series of articles, Baumeister and Hamilton (2019a,b; 2021) and Hamilton (2021), building on Baumeister and Hamilton (2015, 2018, 2020), have called into question the validity of oil market models dating from the pioneering oil market vector autoregressive (VAR) model of Kilian (2008, 2009) to the sign-identified VAR models of Kilian and Murphy (2012, 2014) and their extensions.[1] Baumeister and Hamilton specifically attacked the credibility of the data used, the identifying assumptions, the econometric methodology used in these studies, and the substantive conclusions reached in this literature. They sought to replace these oil market models by a structural VAR model of their own based on different data, different identifying assumptions, and a different econometric approach that they consider superior.

Although their critiques also extend to other research using structural VAR models, one of their main aims has been to revise the consensus in the literature that oil demand shocks are a more important determinant of oil price fluctuations than oil supply shocks. Baumeister and Hamilton (2019a,b) conclude that oil supply shocks are more important drivers of the real price of oil and that they are much more recessionary for the U.S. economy than suggested by earlier oil market studies including Kilian (2008, 2009) and Kilian and Murphy (2014). A number of recent studies including Herrera and Rangaraju (2020), Kilian (2019, 2021) and Kilian and Zhou (2019) have questioned details of their analysis. There has been no comprehensive evaluation of this debate, however.

Substantial progress has been made in recent years in sorting out the pros and cons of the

[1] Recent examples of applications and extensions of these models include Angelini, Cavaliere and Fanelli (2021), Antolin-Diaz and Rubio-Ramirez (2018), Baumeister and Kilian (2014, 2016), Bützer, Habib, and Stracca (2016), Cross, Nguyen and Tran (2021), Herrera and Rangaraju (2020), Herwartz and Plödt (2016), Inoue and Kilian (2013; 2021a,b), Juvenal and Petrella (2015), Kilian (2017), Kilian and Lee (2014), Kilian and Lütkepohl (2017), Kilian and Zhou (2020a,b), Lippi and Nobili (2012), Ludvigson, Ma and Ng (2020), Lütkepohl and Netšunajev (2014), Montiel Olea, Stock and Watson (2020), and Zhou (2020).

underlying econometric methodologies and data, and in separating claims that are supported by empirical evidence from claims that are not. The purpose of this paper is to take stock of this debate and to synthesize what we have learned. Based on new evidence and a careful review of the underlying econometric issues, I make the case that the concerns regarding the existing VAR oil market literature have been overstated and that the results from these models are quite robust to changes in the model specification. The question of how to model oil markets may seem esoteric to many economists at first, but has important implications for how oil-importing and oil-exporting economies respond to global oil price fluctuations and for how policymakers should respond to these oil price fluctuations. The focus in this paper is not only on correcting important misunderstandings in the recent literature, but on the substantive and methodological insights generated by this exchange, which are of broader interest to applied researchers.

The remainder of this paper is organized as follows. Section 2 clarifies the econometric foundations of conventional sign-identified oil market models and the implications of the Haar prior for the rotation matrix for impulse response analysis. I also explain why the alternative econometric method proposed by Baumeister and Hamilton does not address the potential problem of unintentionally informative impulse response priors, I discuss the generality of their approach, and I review under what conditions Bayes estimates and credible sets are useful summary statistics of the impulse response posterior. Section 3 examines the specification of the oil market models discussed in Baumeister and Hamilton (2019a,b; 2021). I emphasize the role of the market clearing condition, I explain why classical measurement error in the change in oil inventories is not a useful modeling device, I examine Baumeister and Hamilton's claim that existing studies have defined the price elasticities of oil demand and oil supply incorrectly, and I discuss the pros and cons of an explicit elasticity prior. Section 4 examines Baumeister and

Hamilton's concern that the impulse response estimates in Kilian and Murphy (2014) are not robust and addresses their claim that this study did not employ narrative sign restrictions. Section 5 addresses Hamilton's (2021) renewed critique of the merits of the Kilian (2009) index of global real economic activity, which is used in many oil market studies, drawing on new results in the literature and related econometric research. Section 6 explains the main source of the substantive disagreement between Baumeister and Hamilton (2019a) and the traditional oil market literature. The concluding remarks are in section 7.

2. The Econometric Foundations of Sign-Identified Oil Market VAR Models

The conventional approach to estimating sign-identified VAR models, as discussed in Uhlig (2005), Rubio-Ramirez, Waggoner and Zha (2010), Arias, Rubio-Ramirez and Waggoner (2018) and Antolin-Diaz and Rubio-Ramirez (2018), involves specifying a Haar prior for the orthogonal rotation matrix Q and a Gaussian-inverse Wishart prior for the parameters A and Σ of the reduced-form VAR model, where A denotes the slope parameters and Σ is the error covariance matrix.[2] The prior for the impulse response vector $\theta = g(A, \Sigma, Q)$ is defined implicitly by the nonlinear function $g(\cdot)$. A number of recent studies have questioned the extent to which the impulse response estimates from these models are driven by the choice of the prior for Q, especially given that Q does not enter the likelihood and its prior cannot be overruled by the data (see, e.g., Baumeister and Hamilton 2015, 2018, 2019a; Watson 2020).

Baumeister and Hamilton in particular have argued for ignoring all empirical evidence from conventional sign-identified oil market VAR models because they see no reason for the posterior impulse response estimates and the credible sets reported in applied work to be more

[2] The Haar prior is a uniform prior in the space of all orthogonal matrices Q.

plausible than other responses in the identified set. This view is based on analysis in Baumeister and Hamilton (2015) who claimed that the Haar prior typically utilized for Q is unintentionally informative about the implied prior for the structural impulse responses and hence invalidates posterior inference. The questions raised in these studies are indeed important for applied work, but Inoue and Kilian (2021a) show that the tools Baumeister and Hamilton use examine this problem are invalid.

2.1. Why the derivations in Baumeister and Hamilton (2015) are misleading

Baumeister and Hamilton (2015) propose characterizing the impulse response prior based on the distribution of the impulse responses conditional on the maximum likelihood estimator (MLE) of the reduced-form parameters A and Σ. This approach does not make sense from a Bayesian point of view. The impulse response prior in Bayesian analysis captures the beliefs the researcher holds about the distribution of the impulse responses before examining the data. Since the impulse response distribution conditional on the MLE depends on the data by construction, it cannot be the prior. In fact, conditioning on the MLE of the reduced-form parameters is logically inconsistent with the conventional approach of postulating a prior distribution for A and Σ, so the results derived in Baumeister and Hamilton (2015) do not address the question of how informative conventional priors for sign-identified models are for the impulse response prior.

One may object that the point of conditioning on the MLE is to isolate the contribution of the prior for Q to the prior for θ. The problem is that this contribution cannot be isolated, when conducting inference conditional on the data. If we restrict attention to the impact responses, for example, these responses in general can be written as products of elements of Q and elements of the Cholesky decomposition of Σ, with the diagonal elements normalized to be positive. By the

change-of-variable method the impulse response prior will differ from the prior for a given element in Q.

Inoue and Kilian (2021a) illustrate this point in more detail. They show that the analytical examples for selected impact responses discussed in Baumeister and Hamilton (2015) are misleading. The actual impulse response prior tends to look quite different from their derivations, as illustrated in Figure 1 based on a diffuse uniform-Gaussian inverse Wishart prior for the VAR parameters with the slope parameters centered on zero. The underlying structural VAR model involves only static sign restrictions. The first column shows the distribution of a selected impulse response conditional on the MLE of A and Σ, while the second column shows the properly derived prior distribution for the same impulse response. The latter prior is roughly centered on zero and allows for considerable prior uncertainty in either direction. This point generalizes to joint inference on vectors of impulse responses, which Baumeister and Hamilton do not consider.

2.2. How informative is the Haar prior?

This result leaves unanswered the question of how widespread unintentionally informative impulse response priors are in applied work based on oil market models and, more importantly, to what extent these implicit impulse response priors affect posterior inference. Baumeister and Hamilton never documented the impulse response priors in conventional oil market VAR models nor did they provide empirical evidence for their claim that these priors distort posterior inference. Appropriate tools to differentiate economically sensible impulse response priors from unintentionally informative priors were only derived in Inoue and Kilian (2021a) who also illustrated the use of these tools in a range of representative empirical applications. Their evidence suggests that unduly informative impulse response priors are the exception rather than

the rule. In all examples they considered, the posterior of the impulse responses is dominated by the data rather than the prior.

Inoue and Kilian (2021a) show, first, that in models identified based on static sign restrictions only, standard uniform-Gaussian inverse Wishart priors tend to imply an uninformative prior for θ in the sense that the impulse response prior for responses not restricted by sign restrictions tends to be centered near zero and is fairly diffuse. This result depends only on the prior and not on the data. Moreover, the corresponding impulse response posterior for θ is driven largely by the data rather than the prior. The last column of Figure 1 illustrates this point by example. Compared to the prior distribution of the impulse response, the center of the impulse response posterior distribution has shifted far to the right and the distribution has become more concentrated.

Second, in models with both static and dynamic sign restrictions the implied prior for θ is necessarily informative. Based on the example of the Kilian and Murphy (2014) oil market VAR model, Inoue and Kilian (2021a) illustrate that even in the latter class of models the prior for θ need not be unintentionally informative, directly refuting Baumeister and Hamilton's claims about this model. Moreover, they show that in this case as well the impulse response posterior is driven largely by the data, not by the prior. Thus, the problem asserted by Baumeister and Hamilton (2019a, 2021) does not exist in the Kilian and Murphy (2014) model.

2.3. Why the alternative econometric method proposed by Baumeister and Hamilton does not address the problem of unintentionally informative impulse response priors

Moreover, unbeknownst to Baumeister and Hamilton, their proposal of postulating priors on the structural VAR model parameters rather than on (A, Σ, Q) implies an impulse response prior of

unknown form much like the conventional approach and hence does nothing to address the concerns they raised about the conventional approach.

The reason is simple. Imposing explicit prior distributions on the parameters $B_0,\ldots,B_p$ in the structural VAR representation $B_0 y_t = B_1 y_{t-1} + ... + B_p y_{t-p} + w_t,$ where y_t denotes the $n \times 1$ vector of data and w_t the $n \times 1$ vector of mutually uncorrelated structural errors, as proposed by Baumeister and Hamilton (2015), is not equivalent to specifying an explicit prior on the vector of structural impulse responses, which is defined by the nonlinear transformation $\theta = f\left(B_0,...,B_p\right)$. Even if a prior on $B_0,\ldots,B_p$ may be defended on economic grounds, after applying the change-of-variable method, the prior on θ may become unintentionally informative. This conclusion remains true, if one is specifying an additional prior on one or more elements of B_0^{-1}. Thus, there is nothing to choose between their approach and the conventional approach in this regard.

When evaluating the Baumeister and Hamilton (2019a) oil market model using the tools developed in Inoue and Kilian (2021a), it can be shown that the implied impulse response prior is highly economically implausible. For example, they postulate that an exogenous increase in global real economic activity lowers the real price of oil (see Figure 2). This pattern is clearly at odds with conventional views about the relationship between economic expansions and the real price of oil. The nature of this prior was neither intended by the authors nor has it been discussed in the literature. Because Baumeister and Hamilton never derived the implied impulse response prior, they remained unaware of how strong and economically unreasonable their prior is. In light of this evidence, there is no support for the claim that their methodology is inherently superior to more conventional approaches. In fact, in practice, it may be less appealing.

2.4. Should we report the Bayes estimator and joint credible sets or the identified set?

Baumeister and Hamilton suggest that researchers using conventional priors should only report the identified set of the impulse responses rather than the Bayes estimate and credible sets. Their point is not that there is anything wrong with reporting Bayes estimates and credible sets from an econometric point of view. Indeed, posterior inference about θ is valid from a Bayesian point of view, given any prior for Q. Baumeister and Hamilton's point is that constructing posterior estimates will not be economically sensible, unless the underlying impulse response prior can be shown to be reasonable. There is no disagreement on this point, but, as noted earlier, the priors in standard sign-identified oil market models such as Kilian and Murphy (2014) are either uninformative or not unintentionally informative and the impulse response posterior is largely determined by the reduced-form parameters rather than the Haar prior. This fact renders Baumeister and Hamilton's insistence on reporting only the identified set moot. Unless for a specific oil market model there is evidence that the impulse prior is unreasonable and is driving the impulse response posterior, there is no reason to depart from conventional practice.

Regardless of whether one uses the conventional methodology for estimating structural VAR impulse responses or the alternative approach proposed by Baumeister and Hamilton (2015, 2019a), care must be taken to properly evaluate the posterior of the impulse responses. One concern is that so-called median (or mean) response functions that have been widely used in applied work including Baumeister and Hamilton (2019a) in general are not the Bayes estimate of the vector of impulse responses (see Inoue and Kilian 2021b). Another concern is that the pointwise error bands reported in much of applied work including Baumeister and Hamilton (2019a) are invalid measures of the estimation uncertainty about the vector of impulse responses and tend to understate the estimation uncertainty by a factor of 3 or 4. Appropriate tools for

evaluating the joint posterior distribution of the impulse responses are readily available under a range of alternative loss functions (see Inoue and Kilian 2021b).

2.5. The lack of generality of Baumeister and Hamilton's (2019a) approach

Whether we restrict elements of B_0 or B_0^{-1} when identifying structural VAR models in general depends on the economic rationale of these restrictions (see Kilian and Lütkepohl 2017). The standard approach in the oil market VAR literature has been to put sign restrictions on the elements of B_0^{-1}. Baumeister and Hamilton (2019a, 2021) make the case for specifying priors on the elements of B_0 instead. Contrary to their assertion, this approach is not more natural in the context of modeling oil markets. That's why no one has used this approach before them. Effectively, Baumeister and Hamilton are asking applied users to abandon existing models in favor of alternative models with priors defined at least in part over the elements of B_0.

They emphasize that their approach is perfectly general in that it accommodates priors on both the elements of B_0 and the elements of B_0^{-1}. This claim ignores the important difference between being able to impose restrictions on *some* elements of B_0^{-1} and being able to impose all relevant restrictions on B_0^{-1} that an applied user would have imposed in existing oil market models. This difference is illustrated by the fact that Baumeister and Hamilton (2019a) are unable to replicate the Kilian and Murphy (2012) oil market model within their framework. The "replication code" for Kilian and Murphy (2012) posted on their homepage actually is not for the Kilian and Murphy (2012) model at all. It is for a different model specification that was designed to resemble the original model specification as closely as possible using the Baumeister-Hamilton methodology, but imposes additional restrictions that were not part of the original model specification.

Baumeister and Hamilton (2019a, 2021) did not even attempt to derive a prior that captures the identifying assumptions in the Kilian and Murphy (2014) oil market model, even though this exercise would have been natural in a study questioning the econometric validity of this model. The reason is presumably that the resulting prior would be intractable. Thus, their approach is merely one among several approaches, rather than encompassing all other approaches.

3. The Specification of the Oil Market Models in Baumeister and Hamilton (2019)

Baumeister and Hamilton (2019a) claim that the oil market VAR models of Kilian (2009) and Kilian and Murphy (2012) are misleading because they imply an unrealistically large impact price elasticity of oil demand. Much of the confusion in the arguments of Baumeister and Hamilton relates to the role of crude oil inventories in defining the impact price elasticity of oil demand, as discussed in Kilian (2021). It is useful to elaborate on this point.

3.1. The role of the market clearing condition

Consider the three-variable monthly global oil market model of Kilian (2009) and Kilian and Murphy (2012). Let $y_t = \left(\Delta q_t, a_t, p_t\right)'$, where Δq_t is the growth rate of global oil production, a_t is an appropriately chosen measure of global real economic activity, and p_t is the log real price of oil in global markets. Baumeister and Hamilton (2019a) propose a structural re-interpretation of these oil market models, where lagged values have been dropped for expositional purposes and $w_t = \left(w_{1t}, w_{2t}, w_{3t}\right)'$ denotes the structural shocks. Specifically, they interpret

$$p_t = \alpha_{pq}\Delta q_t + \alpha_{pa}a_t + ... + w_{3t} \tag{1}$$

as an inverted oil demand curve with α_{pq} denoting the reciprocal of the impact price elasticity of demand. Implicit in this specification is the assumption that the quantity of oil produced in the

global market equals the quantity of oil consumed every month. As discussed in Kilian and Murphy (2014) and Kilian and Lee (2014), however, in reality, oil production differs from oil consumption by the change in crude oil inventories. This means that equation (1) is misspecified and the interpretation of α_{pq} as the reciprocal demand elasticity is invalid. As a result, the incredibly large oil demand elasticity estimates reported in Baumeister and Hamilton are spurious and do not undermine the credibility of the Kilian (2009) and Kilian and Murphy (2012) model.

Interestingly, Baumeister and Hamilton (2019a) recognize the role of oil inventories in their preferred four-variable model specification, which states that

$$\Delta c_t \equiv \Delta q_t - \Delta i_t = \beta_{ca} a_t + \beta_{cp} p_t + ... + w_{3t}, \quad (2)$$

where $\Delta q_t \equiv 100 \ln(Q_t / Q_{t-1})$ is the growth rate of global oil production and $\Delta i_t \equiv 100 \Delta I_t / Q_{t-1}$ with ΔI_t denoting the change in global oil inventories, as measured in Kilian and Murphy (2014), and Q_t denotes global oil production. Baumeister and Hamilton interpret equation (2) as an oil demand curve with β_{cp} representing the impact price elasticity of oil demand..[3] Their key assumption is that $\Delta q_t - \Delta i_t$ is the monthly growth rate of global oil consumption. From equation (5) we have that $C_t = Q_t - \Delta I_t$, so $\Delta c_t = 100(C_t - C_{t-1}) / C_{t-1} \approx 100 \ln(C_t / C_{t-1})$. It turns out that the correctly measured Δc_t differs from $\Delta q_t - \Delta i_t$, as defined in Baumeister and Hamilton (2019a), since $\Delta q_t - \Delta i_t = 100(C_t - Q_{t-1}) / Q_{t-1} \neq 100(C_t - C_{t-1}) / C_{t-1}$, unless $\Delta I_t = 0 \forall t$.

Baumeister and Hamilton suggest that $\Delta q_t - \Delta i_t$ provides a good approximation to oil consumption growth because the change in oil inventories is small on average relative to oil

[3] I ignore for now the additive measurement error in Δi_t postulated by Baumeister and Hamilton, which is immaterial for the discussion of the identification. This point is deferred to section 3.2.

production, but that argument ignores that we are not interested in average consumption growth, but in month-to-month consumption growth rates. Figure 3 shows that Baumeister and Hamilton's consumption growth measure overstates oil consumption growth by as much as 3.3 percentage points and understates it by as much as 4.2 percentage points, calling into question their oil demand elasticity estimate.

3.2. Why classical additive measurement error is not the answer

One feature that sets Baumeister and Hamilton (2019a) apart from other global oil market models is their insistence on treating the change in global oil inventories as subject to measurement error. The question here is not whether there is measurement error. There undoubtedly is error in all observed data, including all variables in Baumeister and Hamilton's model.[4] The question is whether this measurement error is well approximated by classical additive measurement error. The answer is no.

In the case of global oil inventories the main concern is that the proxy for global oil inventories proposed by Kilian and Murphy (2014) and used by Baumeister and Hamilton (2019a) excludes data from non-OECD countries. This omission matters little for much of the estimation period, since non-OECD oil inventories traditionally have been negligible. Since some non-OECD countries such as China have greatly increased their oil inventory holdings since the 2000s, however, one would expect this proxy to become increasingly inaccurate late in the estimation period. Data compiled by the Energy Intelligence Group illustrate the problem. Whereas prior to 2002 the sum of private and public crude oil stocks in China was only about 100 million barrels, by the end of 2019, it had increased to almost 1300 million barrels (see

[4] The importance of measurement error in the real price of oil was emphasized in Hamilton (2011), for example, and the existence of measurement error in global oil production and global real activity is self-evident.

Figure 4).[5] It follows immediately that allowing for classical measurement error in the change in oil inventories over the entire sample will not be able to capture the systematic increases in non-OECD oil inventories since the 2000s. Thus, Baumeister and Hamilton's modeling approach fails to address the root of the problem and needlessly complicates the analysis.

One solution to this problem is the use of improved global inventory data. This point was first made in Kilian and Lee (2014) using an alternative global oil inventory series provided by the Energy Intelligence Group (EIG). Kilian and Lee found that for their estimation period, the estimates of the Kilian and Murphy (2014) model are remarkably similar using either oil inventory measure. Since then EIG has developed even more accurate measures of global crude oil inventories including Chinese commercial and strategic oil inventories that allow us to re-examine this question. It is useful to contrast the evolution of oil stocks in OECD countries and non-OECD countries in recent years. Figure 5 illustrates the extent to which non-OECD oil stocks have increased relative to OECD oil stocks since 2008 in particular. It also shows an increase in global publicly controlled strategic crude oil inventories until 2014, which mainly reflects the creation of strategic crude oil stocks in a number of non-OECD countries (see Kilian and Zhou 2020b).

A good case can be made that in estimating global oil market models using an inventory proxy constructed by combining the Kilian and Murphy (2014) data for the pre-1985 era with EIG data for the more recent period, as shown in Figure 6, would be preferable to the ad hoc assumption of additive classical measurement error, which ignores the increased importance of non-OECD stocks in recent years. Figure 7 shows updated impulse response estimates based on the same specification of the Kilian and Murphy (2014) model as in Inoue and Kilian (2021a,b),

[5] In addition to commercial stocks, this estimate includes strategic crude oil stocks held by companies as well as estimates of strategic stocks controlled by the government.

but with the original inventory data spliced to the EIG global inventory series that starts in 1985.2. Figure 8 shows the same model estimated only on the EIG global oil inventory data since 1985.2.[6] We terminate the estimation period in 2018.6 to allow direct comparisons with a number of alternative impulse response estimates in the literature.

The impulse response estimates are not only qualitatively similar, but also similar to the responses reported in Inoue and Kilian (2021a,b), Herrera and Rangaraju (2020), and Zhou (2020). The main difference is that the estimation uncertainty surrounding the inventory responses and global real activity responses is larger in Figure 7, which is to be expected given the much shorter estimation period. The punchline is that the results from the original specification are remarkably similar to the results based on the alternative global oil inventory data.

Another way of assessing the effect of using the EIG's global inventory data is to compare the impact price elasticities of oil demand and oil supply across model specifications. Table 1 show posterior median estimates of each of these elasticities for three model specifications. It shows that the elasticities implied by the specifications underlying Figures 6 and 7 are virtually identical, confirming the robustness of the estimates. Compared with the specification estimated in Inoue and Kilian (2021a,b) based on the original Kilian and Murphy (2014) proxy for global inventories, the one-month demand elasticity drops from -0.29 to about -0.20, while the one-month oil supply elasticity remains unchanged at 0.02. This demand elasticity estimate is about half of state-of-the-art estimates of the one-month U.S. price elasticity of gasoline demand (see Kilian 2021).

[6] In the latter case, all but one of the narrative restrictions are outside of the estimation period. We follow the recent literature in imposing additional dynamic sign restrictions. The implied level of the inventory response is assumed to be of the same sign as the impact response for the first year after a shock. For consistency, we impose the same restriction in Figure 5, although similar results are obtained on the full sample without that additional restriction.

3.3. The definition of the impact price elasticities of oil supply and oil demand

Baumeister and Hamilton (2019, 2021) insist that the impact price elasticities of oil demand and oil supply should be defined as functions of the parameters in the matrix B_0 in the structural VAR representation $B_0 y_t = B_1 y_{t-1} + ... + B_p y_{t-p} + w_t$, if we are interested in capturing the slopes of short-run oil demand and oil supply curves. They point out correctly that this elasticity definition in general differs from that underlying the work of Kilian and Murphy (2012, 2014) and related studies, who define the elasticity in terms of the parameters of the structural impact multiplier matrix B_0^{-1}. The key difference is that Kilian and Murphy (2012, 2014) allow model variables other than oil consumption (or oil production) to respond to the real oil price change associated with exogenous shifts in the short-run supply (or demand) curve, whereas Baumeister and Hamilton (2019a) hold these variables constant.[7]

The key advantage of the approach taken by Kilian and Murphy is that their elasticity definition corresponds to the way elasticities are typically estimated at the micro level and the way oil supply elasticity bounds have been constructed from aggregate data, facilitating the use of these estimates in specifying and evaluating oil market models. In contrast, these extraneous estimates are not suitable in general for motivating elasticity priors in the Baumeister and Hamilton (2019) framework because they are constructed without holding constant the remaining model variables. This point is important because it contradicts Baumeister and Hamilton's claim that extraneous information about elasticities is naturally represented as restrictions on B_0.

[7] A practical difference is that Baumeister and Hamilton's approach ensures by construction a unique estimate of the oil supply elasticity, whereas Kilian and Murphy's approach produces two estimates of the oil supply elasticity that need not be identical, one in response to the flow demand shock and one in response to the storage demand shock. Given that these estimates in practice tend to differ only by the second decimal point, however, there is little loss in generality in reporting an average supply elasticity estimate.

As discussed in Kilian (2021), either of these approaches in principle can be used to rule out structural models that are economically implausible, provided the underlying model is correctly specified and the elasticities are defined in an internally consistent manner. While the elasticity estimates generated by Kilian and Murphy's approach should not be interpreted as estimates of the slopes of the short-run oil demand or oil supply curve, bounding these elasticities helps rule out empirically implausible draws of structural models and facilitates comparisons with most extraneous elasticity estimates. The challenge in implementing the approach of Baumeister and Hamilton, in contrast, is that there is no readily available extraneous information compatible with their definition of the impact price elasticities of oil demand and supply, it is not straightforward to correctly specify the structural model, and care must be taken not to specify an unintentionally informative impulse response prior, as illustrated earlier.

3.4. The pros and cons of an explicit elasticity prior

Baumeister and Hamilton (2019a,b) make much of the point that an upper bound on the oil supply elasticity such as 0.026 in Kilian and Murphy's work or 0.04 in the more recent literature treats a value at the bound as perfectly reasonable and a value just beyond this bound as entirely unreasonable. Of course, this example is missing the point that, in reality, one would expect values approaching the upper bound to have negligible probability mass. The conventional approach to estimating sign-identified VAR models subject to elasticity bounds does not allow us to incorporate this information. It is not clear how much of a concern this is, however, given that the elasticity posterior largely depends on the data.

The advantage of expressing elasticities in terms of the elements of B_0, as proposed by Baumeister and Hamilton. is that it allows us, in principle, to directly specify a prior for a given elasticity. For example, one could specify an exponential prior for the supply elasticity that starts

at zero and is truncated at the upper bound, as suggested by Kilian and Zhou (2019). This specification would assign more probability mass to values close to the benchmark of zero provided by economic theory (see Anderson et al. 2018). Baumeister and Hamilton (2019a) do not explore this possibility, but instead specify a truncated Student-t prior for the one-month oil supply elasticity that make minimal use of economic reasoning and extraneous evidence. Herrera and Rangaraju (2020) show that when imposing the same prior upper bound on the supply elasticity in this model as used by other studies, the results are overall quite similar to those based on the Kilian and Murphy (2014) model, suggesting that the choice of the prior distribution is less important than that of the upper bound. This conclusion is also consistent with the evidence in Baumeister and Hamilton (2019a) that it is the presence or absence of an upper bound that matters more than the nature of the supply elasticity prior.[8]

Where does this upper bound come from? Kilian and Murphy (2012) used the natural experiment of the invasion of Kuwait in August 1990 to construct an upper bound of 0.026 for the global one-month oil supply elasticity that has been widely used in the literature. Details of this thought experiment can be found in Kilian (2021). As Kilian and Murphy showed, even with a supply elasticity as high as 0.08, oil supply shocks would explain only 10% of the variation in the real price of oil. This does not mean that a bound of 0.08 is empirically plausible, but that the empirical estimates are quite robust to tripling the value of this bound.[9] More recently, Zhou

[8] Braun (2021) finds that exploiting the non-Gaussianity of the reduced-form VAR errors in Baumeister and Hamilton's (2019a) model for the identification produces results much more in line with those in Kilian and Murphy (2014), even without imposing a conventional lower bound on the oil supply elasticity.

[9] Baumeister and Hamilton's (2021) recent critique of this bound is not supported by the facts. They note that the value of this bound hinges on whether one includes a decline in oil production in the United Arab Emirates (UAE) in the control group. Baumeister and Hamilton claim that the UAE in August 1990 lowered its oil production in response to the threat of being invaded by Iraq rather than in response to pressure from OPEC to adhere to its production quota. Not only would it have been impossible for Iraq to invade the UAE without first conquering Saudi Arabia, which even Saddam Hussein would have recognized as practically infeasible, but the narrative evidence clearly shows that the UAE's decision in July 1990 to lower its oil production came well before the speech by Saddam Hussein that Baumeister and Hamilton want to attribute this decision to. In any case, it can be shown that

(2020) argued for a higher oil supply elasticity bound of 0.04 based on a review of the highest credible monthly oil supply elasticity estimates reported in literature. This value corresponds to adding four standard errors to the quarterly U.S. oil supply elasticity estimate in Newell and Prest (2019), which itself is an upper bound on the one-month elasticity, and has been adopted in a number of recent studies (e.g., Kilian and Zhou (2020a,b); Inoue and Kilian (2021a,b); Cross et al. (2021)). The substance of the findings in Kilian and Murphy (2014) is not affected by this change.

4. On the Robustness of the Estimates of the Kilian and Murphy (2014) Model

A central message of Baumeister and Hamilton (2021) is that the original estimates of the Kilian and Murphy (2014) model cannot be replicated. This claim is astonishing, given how many studies have confirmed the substantive findings of Kilian and Murphy (2014) using a variety of different econometric methods, different data sets, different elasticity bounds, different estimation periods and even extensions of the original model (e.g., Kilian and Lee 2014; Baumeister and Kilian 2014; Kilian 2017; Herrera and Rangaraju 2020; Zhou 2020; Kilian and Zhou 2020a,b; Inoue and Kilian 2021b; Cross, Nguyen and Tran 2021).

4.1. The impulse response estimates in Kilian and Murphy (2014) can be replicated

Baumeister and Hamilton (2019b) report impulse responses constructed using the original data and code of Kilian and Murphy (2014), as posted in the Journal of Applied Econometrics data and code archive, for two different random seeds. Whereas the impulse response estimates based on the original seed match exactly those reported in Kilian and Murphy (2014), those based on the alternative seed in some cases differ in magnitude from the response estimate focused on by

raising the elasticity bound from 0.026 to 0.043, as suggested by Baumeister and Hamilton, does not materially change the substance of the findings in Kilian and Murphy (2014).

Kilian and Murphy, although the responses have the same sign. Baumeister and Hamilton leave the reader with the impression that the empirical results in Kilian and Murphy (2014) are not robust and cannot be replicated with a different seed.

One reason why Baumeister and Hamilton have difficulties reproducing the original impulse response estimates is that they do not implement the full estimation procedure employed by Kilian and Murphy (2014). In particular, they fail to impose the additional narrative sign restrictions on the historical decomposition of the real price of oil that Kilian and Murphy used to ensure the external validity of their preferred model estimate. In contrast, Zhou (2020), for example, using the same data, but imposing these narrative sign restrictions, was able to replicate the impulse responses and historical decompositions in Kilian and Murphy (2019) without difficulty. Zhou also showed that Kilian and Murphy's key result about the relative importance of oil supply and oil demand shocks as drivers of the real price of oil is invariant to which admissible model solution one focuses on.

Closer inspection reveals that Baumeister and Hamilton's alternative estimate appears to be roughly within the range of the conventional posterior quantile error band reported in Figure 1 of Kilian and Murphy (2014). Because this error band is based on code that does not incorporate the additional narrative sign restrictions, it may be used to assess the variability of Baumeister and Hamilton's impulse response estimator (subject to the usual caveats about the construction of pointwise error bands discussed earlier). Figure 1 in the original paper suggests that the magnitude of the alternative response estimates encountered by Baumeister and Hamilton is within the range of what we would expect in the absence of narrative sign restrictions. Thus, their alternative estimate in no way invalidates the analysis in Kilian and Murphy (2014).

4.2. Kilian and Murphy (2014) employed narrative sign restrictions

Baumeister and Hamilton's response has been to deny that Kilian and Murphy (2014) employed narrative sign restrictions on the historical decomposition to select the most credible model among the set of models that satisfy the sign restrictions on the impulse responses. Baumeister and Hamilton insist that they did not find the expression "narrative sign restriction" in the paper or in the replication code provided by the authors and suggest that Kilian and Murphy must have changed their mind about their procedure without telling anyone. They further claim that nothing resembling narrative sign restrictions was implemented anywhere in Kilian and Murphy (2014). These claims are misleading.

Of course, the original paper did not use the term "narrative sign restrictions", which did not exist at the time, but it discussed how the draws for the admissible models were "externally validated" by verifying that the model estimates match external evidence about what has been driving the real price of oil during selected episodes. This point was discussed both in Kilian and Murphy (2014) and in the companion paper by Kilian and Lee (2014). For example, Kilian and Lee (2014), in reviewing the support for Kilian and Murphy's (2014) preferred model, note that:

> "… one can externally validate the fit of the model. There are several episodes for which we have extraneous evidence from industry specialists such as Terzian (1985) or Yergin (1992) that speculation took place in physical oil markets. A natural joint test of the structural model and of the inventory data is to compare its historical decomposition against this external evidence. The model passes this test. For example, it detects surges in speculative demand in 1979 following the Iranian Revolution, in 1990 around the time of the invasion of Kuwait, and in late 2002 in anticipation of the Iraq War, as well as large declines in speculative demand in 1986 after the collapse of OPEC and in late 1990 when the U.S. had moved enough troops to Saudi Arabia to forestall an invasion by Iraq" (p. 74)

Similar statements can be found in Kilian and Murphy (2014, p. 460, 469). Given that Baumeister and Hamilton's candidate solution based on their alternative seed has not been externally validated, Kilian and Murphy (2014) would not have considered it a legitimate estimate.

Baumeister and Hamilton are correct that the external validation procedure discussed in these papers was not contained in the code we provided. Since the number of models satisfying the sign restrictions in Kilian and Murphy (2014) is small, this procedure was originally implemented manually by inspecting the historical decompositions for the real price of oil for each draw. This approach is obviously infeasible when considering a much larger number of model draws, but Zhou (2020) shows how one can incorporate this external validation procedure into the code. Zhou demonstrates that the original findings in Kilian and Murphy (2014) can be replicated, whether on the original data or on extended data. Zhou (2020) describes how to operationalize this procedure:

> "Motivated by the reasoning in Kilian and Murphy (2014, p. 460, 469) and Kilian and Lee (2014, p. 74), I postulate (1) that storage demand shocks cumulatively raised the log real price of oil by at least 0.2 (or approximately 20%) between May and December 1979, consistent with anecdotal evidence of a dramatic surge in inventory building in the oil market during that time, (2) that storage demand cumulatively lowered the log real price of oil by at least 0.15 between December 1985 and December 1986, after OPEC collapsed, and (3) that storage demand shocks raised the log real price of oil by at least cumulatively between June 1990 and October 1990, reflecting market expectations that Iraq would invade its neighbors. Flow supply shocks are assumed to have raised the log real price of oil cumulatively by at least 0.1 between July and October of 1990, reflecting the invasion of Kuwait and the cessation of Iraqi and Kuwaiti oil production in early August. Finally, the cumulative effect of flow demand shocks on the log real price of oil between June and October of 1990 is bounded by 0.1, given that the oil price spike of 1990 was not associated with the global business cycle."

Zhou (2020) also clearly explains how to impose these inequality restrictions, stressing that the external validation procedure in Kilian and Murphy (2014) was an early example of narrative sign restrictions, as recently proposed by Antolin-Diaz and Rubio-Ramirez (2018). Antolin-Diaz and Rubio-Ramirez explicitly note that "narrative information in the context of the oil market was used by Kilian and Murphy (2014) to confirm the validity of their proposed identification" (p. 2803) and that Kilian and Murphy (2014) "impose[d] sign restrictions on the historical decompositions" (p. 2807). The same point is discussed in Kilian and Lütkepohl (2017, section

13.6.5). While the external validation procedure was perhaps not as clearly explained in the original paper as it should have been, owing in part to space constraints imposed by the journal, Baumeister and Hamilton can hardly claim to have had no knowledge of the link between external validation and narrative sign restrictions.[10]

4.3. Bayesian inference in the Kilian and Murphy (2014) model

Baumeister and Hamilton (2019b) stress that there are only 16 admissible models reported in Kilian and Murphy (2014), before imposing the narrative sign restrictions. They raise the concern that the number of admissible draws in Kilian and Murphy (2014) may not be large enough to ensure reliable estimates. At the time they raised this issue, this question had already been addressed by several other studies that showed that the results are robust to substantially increasing the number of admissible draws (e.g., Zhou 2020).

Baumeister and Hamilton also insinuate that the small fraction of admissible models casts doubt on the identification of the model. This point is misguided, as discussed in Kilian and Lütkepohl (2017) and Uhlig (2017). The main reason for the low fraction of admissible models instead is that the identifying restrictions are more restrictive than those in Baumeister and Hamilton (2019a) model. In the words of Uhlig (2017):

> "If one rejects many draws, one may feel that something is wrong. But … the opposite is … true. […] When a lot of draws are rejected, the identification is sharp. […] This is an important insight that is often misunderstood" (p. 110).

Thus, a higher fraction of admissible draws is not desirable in and of itself. The number of admissible models would have skyrocketed, for example, had we dropped the oil supply

[10] Interestingly, even without these narrative restrictions, Baumeister and Hamilton could have replicated the substance of the Kilian and Murphy (2014) results, if they had used state-of-the-art econometric methods for evaluating the posterior model draws rather than conditioning on the MLE, as demonstrated in Herrera and Rangaraju (2020).

elasticity bound, but that would have undermined the identification rather than strengthened it.

Another reason for the low fraction of admissible models is that Kilian and Murphy's original code was computationally inefficient. More efficient code that generates more admissible models for the same number of draws was readily available to Baumeister and Hamilton, but they chose not to mention this fact because it would have undermined their point.

Finally, we must keep in mind that Kilian and Murphy (2014), unlike subsequent studies, conditioned on the MLE of the reduced-form parameters. Comparing the number of admissible draws conditional on one value of the reduced-form parameters to the number of admissible draws obtained when drawing from the posterior of the reduced-form coefficients is obviously misleading. One reason that Kilian and Murphy (2014) conditioned on the MLE and did not conduct Bayesian inference for their preferred model is that they were well aware of the limitations of the econometric methods for evaluating sign-identified VAR models available at the time. The problem of how to conduct Bayesian inference in this class of models was only solved in a series of recent studies (see Antolin-Diaz and Rubio-Ramirez 2018; Inoue and Kilian 2021a,b).

It is straightforward to verify that even based on thousands of admissible posterior draws, generated using state-of-the-art methods of Bayesian inference, the substantive results reported in Kilian and Murphy (2014) remain unchanged. The fact is that the impulse response estimates in Kilian and Murphy (2014) can be replicated and are extremely robust to extensions and changes in the model, as once again illustrated in Section 3 of the current paper.

5. The Merits of the Kilian Index of Global Real Economic Activity

An additional concern raised by Baumeister and Hamilton is about the choice of the data in existing oil market VAR models. Notably, Baumeister and Kilian (2019a,b) criticize Kilian and

Murphy (2014) for having discussed the reliability of their oil inventory data, but not that of their global real activity data. Since Kilian and Murphy (2014) first introduced changes in oil inventories into global oil market models, it is not surprising that they devoted a section to discussing these data. In contrast, their measure of global real activity had been introduced in Kilian (2008, 2009) and was well established by 2014, so there was no need to have a section on this index.[11] It is useful, however, to address the additional concerns about this index raised by Hamilton (2021) after the publication of Kilian (2019) because using an appropriate measure of the global business cycle is a pre-condition for identifying the role of demand and supply shocks in industrial commodity markets (see Kilian and Zhou 2018).

Specifically, Baumeister and Hamilton reiterate four claims recently made by Hamilton (2021), namely (1) that the Kilian index reaching its lowest level in 2016 implies a deeper recession in 2016 than at any other time in history; (2) that the cyclical component of the OECD global industrial production index has a higher correlation with world real GDP than the Kilian index; (3) that the Kilian index is not helpful in forecasting real commodity prices; and (4) that the linear trend specification underlying the construction of the Kilian index is rejected by statistical tests. I will briefly address each of these claims.

5.1. How to interpret the Kilian index of global real economic activity

The fact that the level of the Kilian business cycle index in early 2016 briefly dropped below its level in late 2008, for reasons discussed in Kilian and Zhou (2018), does not imply a bigger recession in 2016 than in 2008. The NBER business cycle dating committee identifies the months when the economy reaches a peak of activity and later months when the economy

[11] Further discussion of this index and alternative proxies for global real activity can be found in Kilian (2009), Kilian (2019) and Kilian and Zhou (2018). The index is available in the Haver and FRED darabases.

reaches a trough. The time in between is a recession, defined as a period when economic activity is contracting. Thus, the depth of a recession is measured by the extent to which real activity declines from peak to trough, not by the lowest level of real activity during the recession. Using the NBER definition of a recession, the decline in 2016 is too short to be called a recession at all, and its magnitude is only about one third of the decline in late 2008.[12]

Hamilton (2021) counters this evidence by suggesting that Kilian's (2009) philosophy of measuring the business cycle precludes defining recessions as done by the NBER. In support of this odd argument, he cites Kilian (2009) as stating that the index is proportionate to deviations of the level of real activity from trend. This statement was intended to draw attention to the fact that numerical values of the Kilian index have no inherent meaning, only its relative changes over time. This fact, however, in no way precludes applying the NBER definition of a recession. For example, the discussion of global booms and global recessions in Kilian (2009, p. 1057) is fully consistent with the NBER definition. There is no disconnect between the analysis in Kilian (2009) and in Kilian (2019).

5.2. The Kilian index is a leading indicator for world industrial production

As discussed in Kilian and Zhou (2018), the Kilian index was designed for modeling the business cycle in industrial commodity markets. It is a proxy for changes in the volume of shipping of industrial raw materials. It is well known that changes in trade volumes need not line up with changes in real output. The Kilian index was, in fact, constructed as an alternative to world real GDP because world real GDP is not only poorly measured, but is an inappropriate

[12] Hamilton (2021) also drew attention to the decline in the index from December 2013 to February 2016, ignoring that the sharp drop in early 2016 does not reflect cyclical variation, but represents an outlier that was quickly reversed. When excluding this outlier, there is indeed a sustained decline in the index in 2014 and 2015, consistent with a wide range of other indicators, as discussed in Kilian and Zhou (2018), but this decline is only half as large as that in late 2008.

measure of global real activity in industrial commodity markets. Thus, the validity of the Kilian index does not depend on being a good proxy for (or predictor of) world real GDP or, for that matter, a good proxy for Hamilton's preferred measure of global industrial production.

Hamilton suggests that the Kilian index is questionable because it lacks predictive power for coincident indicators of the global business cycle. This conclusion is based on ad hoc mixed frequency regressions. These regressions not only suppress the lags of the Kilian index, but they are akin to relating an annual growth rate to a monthly output gap, rendering the regression unbalanced. Not surprisingly, Hamilton finds no predictive relationship. His empirical findings are contradicted by studies such as Ravazzolo and Vespignani (2019), however, who work with balanced regressions. Similarly, Funashima (2020) recently confirmed that the Kilian index is a leading indicator for world industrial production, but industrial production is not a leading indicator for the Kilian index, exactly as hypothesized by Kilian and Zhou (2018). This result is robust to whether the raw data are expressed in growth rates or deviations from a log-linear trend, as long as the regression is balanced. Thus, there is no support for Hamilton's claim that the Kilian index is unsuitable for studying industrial commodity markets.

5.3. The index has predictive ability for real commodity prices

Similarly, Hamilton's claim regarding the lack of predictive power of the Kilian index for real commodity prices is inconsistent with several other studies. Closer inspection shows that Hamilton (2021) did not conduct a forecasting exercise at all, but only reported the in-sample fit of some ad hoc regressions. As Funashima (2020) points out, the predictive regressions reported in Hamilton (2021) are unbalanced in that Hamilton predicts the growth in real commodity prices based on the lagged level of the Kilian index. Hamilton then compares the predictive accuracy to a regression relating the growth of real commodity prices to lagged industrial production growth.

Not surprisingly, the latter regressions show superior predictive accuracy. When the regressand and regressor are transformed in the same way, however, both activity measures have predictive power for real commodity prices. This result is also consistent with predictive evidence in favor of the Kilian index in Alquist, Bhattarai and Coibion (2019). In closely related work, Nonejad (2020) shows that the Kilian (2019) index of global real economic activity predicts real commodity prices as well as (or more accurately than) the OECD world industrial production index favored by Hamilton (2021).

5.4. There is no statistical evidence against the Kilian (2019) index

Finally, the statistical tests used by Hamilton (2021) to reject the linear trend specification underlying the Kilian index are invalid. Hamilton presents results of tests of the I(1) null and tests of the I(0) null. He reports being unable to reject the unit root null using the ADF test, but being able to reject the null of stationarity about a linear deterministic time trend using the KPSS test of Kwiatkowski et al. (1992). He concludes that the Kilian index is invalid. This type of confirmatory analysis was fashionable in the 1990s, but has been shown to be misleading.

The intuition is simple. It is evident that the data underlying the Kilian index are highly persistent. The apparent existence of long and persistent cycles in these data is, in fact, what motivated the analysis in Kilian (2009). For such data, the finite-sample power of tests of the unit root null based on autoregressions is negligible. Thus, the fact that Hamilton cannot reject the unit root null is not surprising. Since the null distribution and the distribution under the alternative of this test overlap to a large extent, we cannot discriminate between these hypotheses based on the data. Because the null hypothesis is protected from rejection in classical hypothesis testing, we necessarily fail to reject the null in this case. This does not mean that the data support the null hypothesis, but that the data are not informative about the hypothesis of interest.

This raises the question of how the KPSS test can yield such decisive results simply by reversing the null of the test. After all, it is still true that the null distribution and the distribution under the alternative of this test largely overlap. Caner and Kilian (2001) trace the tendency of tests of the I(0) null to reject in such situations to the fact that asymptotic critical values for these tests have been constructed under the null of white noise. If these critical values are applied to stationary, but persistent time series, the KPSS test will suffer from potentially severe size distortions. Caner and Kilian demonstrate that rejection rates under the null as high as 70% are not uncommon in applied work, when using asymptotic critical values.

Addressing these finite-sample distortions requires the user to bootstrap the regression model under the null of the best fitting stationary, but persistent process (possibly with bias corrections as in Kilian (1999)). The resulting bootstrap critical values are invariably higher, resulting in non-rejections of the null of trend stationarity, consistent with the evidence from tests of the unit root null. In fact, Caner and Kilian (2001) show by simulation that the power of the size-corrected bootstrap version of the KPSS test is even lower than the already low power of the standard augmented Dickey-Fuller test.

Subsequently, this problem has been studied in more depth from a theoretical point of view in Müller (2005) who used the device of a local-to-unity framework to represent situations in which conventional tests are uninformative. To quote from the abstract of Müller's paper:

> "Tests of stationarity are routinely applied to highly autocorrelated time series. Following Kwiatkowski et al. (J. Econom. 54 (1992) 159), standard stationarity tests employ a rescaling by an estimator of the long-run variance of the (potentially) stationary series. This paper analytically investigates the size and power properties of such tests when the series are strongly autocorrelated in a local-to-unity asymptotic framework. It is shown that the behavior of the tests strongly depends on the long-run variance estimator employed, but is in general highly undesirable. Either the tests fail to control size even for strongly mean reverting series, or they are inconsistent against an integrated process and discriminate only poorly between stationary and integrated processes compared to optimal statistics."

In fact, Müller (2008) proves the impossibility of statistically discriminating between the I(0) and I(1) hypothesis, even with an infinite amount of data, so what Hamilton claims to have done is plainly impossible. Thus, Hamilton's assertion that statistical tests show that the construction of the Kilian index is invalid is without merit.

6. The Importance of Bounds on the One-Month Oil Supply Elasticity

As shown in Herrera and Rangaraju (2020) and Zhou (2020), among others, the key substantive difference between Baumeister and Hamilton (2019a) and other studies is not about bringing additional prior information on the oil demand elasticity to bear. For example, Baumeister and Hamilton's rhetorical question of whether we really know nothing about the impact price elasticity of oil demand falsely implies that previous studies failed to impose any further identifying information about this elasticity. It ignores that bounds on the impact price elasticity of oil demand have been standard in the literature, ever since Kilian and Murphy (2014) proposed bounding this elasticity by zero from above and by extraneous microeconomic estimates of the long-run elasticity from below.

Instead, the key substantive difference is about how large the one-month price elasticity of oil supply is allowed to be in estimating the oil market model. Rather than bringing to bear more information about this oil supply elasticity, Baumeister and Hamilton (2019a) actually remove a key identifying restriction in existing oil market models by insisting on the support for this elasticity being unbounded. Based on an oil supply elasticity prior that allows for arbitrarily large supply elasticities, Baumeister and Hamilton (2019a) conclude that oil supply shocks are a much more important determinant of the real price of oil than earlier studies such as Kilian and

Murphy (2012, 2014) suggested.[13] This result is shown to be robust to an alternative supply elasticity prior that is equally unbounded from above, but assigns much more probability mass to lower elasticity values, illustrating that the upper bound on the oil supply elasticity matters more than its prior probability distribution.

As shown by Herrera and Rangaraju (2019), when imposing any reasonably tight oil supply elasticity bound, the effect of oil supply shocks on the real price of oil in Baumeister and Hamilton's model is not larger than in other oil market models. Thus, the debate launched by Baumeister and Hamilton is not about relaxing the assumption of a one-month oil supply elasticity of zero made in Kilian (2009). That restriction had already been relaxed in numerous earlier studies (see Kilian and Zhou 2021). Nor is it about whether we should allow for uncertainty in the elasticity value. Rather, at its core, the debate is about whether one-month oil supply elasticity values of 0.15, of 0.9, or of ∞, for example, all of which Baumeister and Hamilton consider a priori plausible, can be defended from an economic point of view.

It goes without saying that allowing for an infinite elasticity in the prior specification makes no economic sense, no matters how one defines the oil supply elasticity. Even if the global one-month oil supply elasticity is only 0.15 (0.3), however, this implies that a 10% unexpected price increase caused by higher demand is associated with an increase of 1.5% (3%) in global oil production within one month. Such increases seem unrealistically large. Oil producers may be able to announce plans to increase production, but materially changing actual production on such short notice tends to be difficult. This is a question where knowledge of the oil industry can help immensely in understanding what is feasible and what is not (e.g., Golding

[13] In fact, their baseline prior for the oil supply elasticity can be shown to resemble the posterior of this elasticity obtained from the Kilian and Murphy (2012) model, when imposing no bounds at all on the one-month oil supply elasticity.

2019). As discussed in Newell and Prest (2019, p. 16), "once a well has been drilled, its flow rate is determined primarily by geology and is therefore largely beyond the operator's control."

Extraneous microeconomic estimates and the theoretical results in Anderson et al. (2018) are consistent with the view that the one-month price elasticity of oil supply is very close to zero.[14] Baumeister and Hamilton dispute this evidence, but a recent exhaustive study by Kilian (2021) shows that the alternative elasticity studies invoked by Baumeister and Hamilton (2019a) including Caldara et al. (2019) suffer from a range of conceptual and econometric problems that call into question their oil supply and oil demand elasticity estimates. For example, the IV estimator of the global one-month oil supply elasticity favored by Caldara et al. (2019) violates the exclusion restriction and appears based on a weak instrument, while their corresponding one-month oil demand elasticity is also incorrectly defined as a cross-price elasticity rather than the own price elasticity. The alternative VAR-based estimator in Caldara et al. (2019) not only inherits the flaws of these IV estimators, but is derived from a global market clearing condition that equates oil production with oil consumption in every period, ignoring that oil is storable, which further invalidates this estimator.

In addition, it must be pointed out that the price elasticity definition in Caldara et al. (2019) differs from that employed in Baumeister and Hamilton (2019a), as discussed in section 4, in that Caldara et al. do not control for changes in other variables such as oil inventories. Thus, Baumeister and Hamilton's (2019a,b; 2021) claim that the evidence in Caldara et al. supports their specification of the oil supply elasticity prior is not correct. In fact, the posterior median

[14] Even for U.S. shale oil producers the one-month price elasticity of supply is essentially zero. For further discussion of why some recent studies arrived at a different conclusion, the reader is referred to Kilian (2021). In addition, it has to be kept in mind that the U.S. shale oil supply elasticity differ from the global oil supply elasticity in global oil market models. Not only was practically all of global oil production from the 1970s until about 2010 conventional, but even more recently the share of shale oil production in global oil production has been small.

elasticity estimates reported in Baumeister and Hamilton (2019a) are logically inconsistent with the theoretical relationship postulated in Caldara et al. (2019), as pointed out in Kilian (2021).

In short, the evidence provided by Baumeister and Hamilton in favor of one-month oil supply elasticities far in excess of conventional oil supply elasticity bounds does not hold up to scrutiny. None of the empirical evidence helps specify the supply elasticity prior in Baumeister and Hamilton. Nor is there a way of evaluating their supply elasticity estimate of 0.15 against extraneous elasticity estimates, given the way they define the elasticity. All we can say is that imposing standard one-month oil supply elasticity bounds on their model corroborates the substance of the conclusions of Kilian and Murphy (2014) about the quantitative importance of oil supply shocks, even disregarding numerous other differences in the data, in the model specification, and in the econometric approach.

7. Conclusion

The new evidence presented in this paper and related evidence compiled by other recent studies support the consensus view that oil demand shocks are the main driver of oil price fluctuations. The reason that Baumeister and Hamilton (2019a) reached a different conclusion was not that their econometric methodology is superior or that they brought additional identifying information to bear, but that they relaxed a key identifying assumption in conventional oil market models about the range of admissible values for the one-month price elasticity of oil supply. As discussed in this paper, there is no empirical support for this change in assumptions.

More generally, there is no support for Baumeister and Hamilton's claim that the conventional approach to estimating sign-identified VAR models, as discussed in Uhlig (2005), Inoue and Kilian (2013, 2019), Rubio-Ramirez et al. (2010), Arias et al. (2018) and Antolin-Diaz and Rubio-Ramirez (2018) is misguided. As we discussed, the econometric evidence they

presented is not relevant to this claim and there is no support for the conclusion that applied users of this approach should only report the identified set. Appropriate tools for summarizing the posterior of the impulse responses under alternative loss functions have been developed in Inoue and Kilian (2021b).

In fact, the alternative approach favored by Baumeister and Hamilton (2015, 2018, 2019a,b, 2020, 2021) suffers from the exact same problem of unintentionally informative impulse response priors that they attribute to the conventional approach. Notably, the oil market model of Baumeister and Hamilton (2019a) implies an unintentionally informative and economically implausible prior for the impulse responses, calling into question their estimates. We also clarified the pros and cons of estimating price elasticities of oil demand and oil supply using this approach compared to the conventional approach.

Finally, we addressed concerns raised by Baumeister and Hamilton regarding the validity of the global real activity and global crude oil inventory data used in earlier oil market models and regarding the robustness of the estimates of these models. We also drew attention to measurement error in the definition of oil consumption growth in Baumeister and Hamilton (2019a) that undermines the interpretation of their model estimates, and we highlighted the importance of correctly specifying the market clearing condition when estimating the price elasticity of oil demand. Specifically, we explained why the conclusion reported in Baumeister and Hamilton (2019a) that conventional oil market models that exclude oil inventories imply a one-month price elasticity of oil demand of near -2 is erroneous. We then showed that when using state-of-the-art global oil inventory data in modeling oil markets, this elasticity is estimated as -0.2, which is consistent with extraneous estimates of the gasoline demand elasticity.

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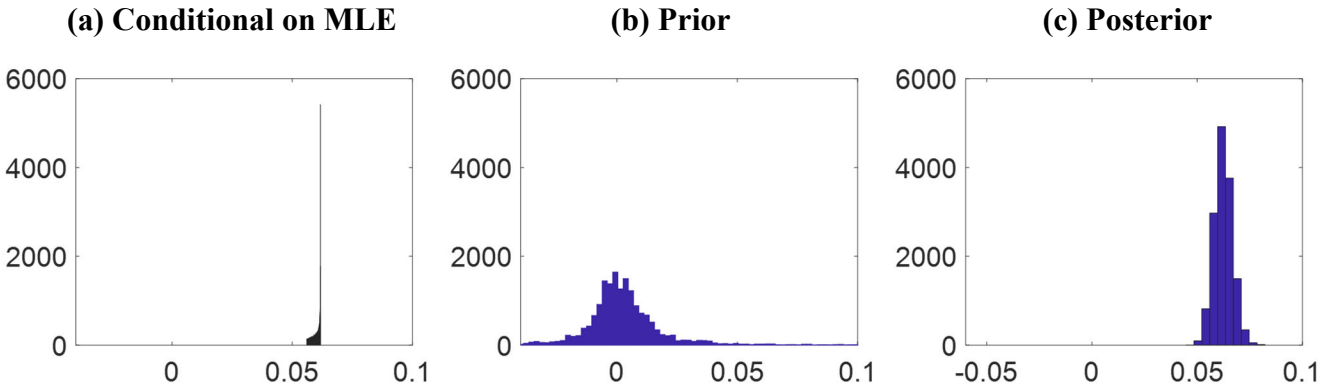
Table 1: Posterior median estimates of impact price elasticities of oil demand and oil supply

	η^d	η^s
1973.2-2018.6 [a]	-0.29	0.02
1973.2-2018.6 [b]	-0.19	0.02
1985.2-2018.6 [c]	-0.20	0.02

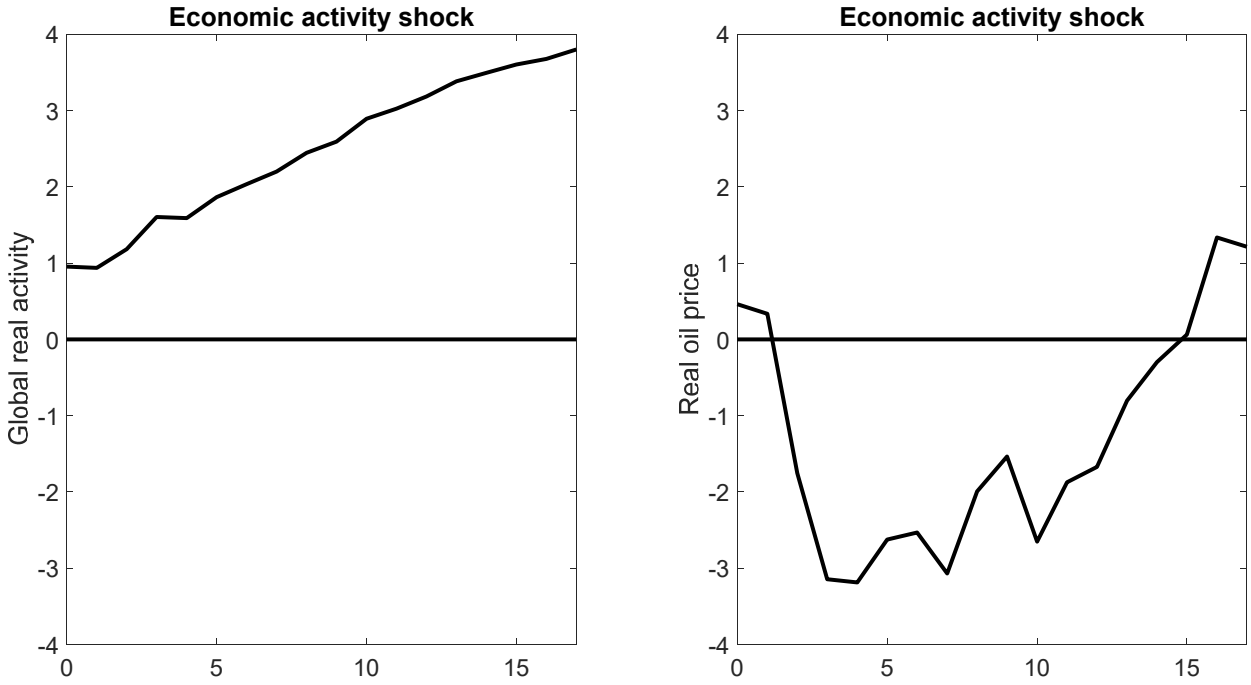
NOTES: The oil demand elasticity is defined as in Kilian and Murphy (2014) to account for the response of oil inventories. The oil supply elasticity is the average of the two oil supply elasticities in the model. The upper bound on the impact oil supply elasticity is 0.04.
[a] Based on oil inventory proxy in Kilian and Murphy (2014) as in Inoue and Kilian (2021a,b).
[a] Based on oil inventory proxy in Kilian and Murphy (2014) spliced with global EIG data.
[a] Based on EIG global oil inventory data.

Figure 1: Illustrative example of histograms over identified sets of an impulse response



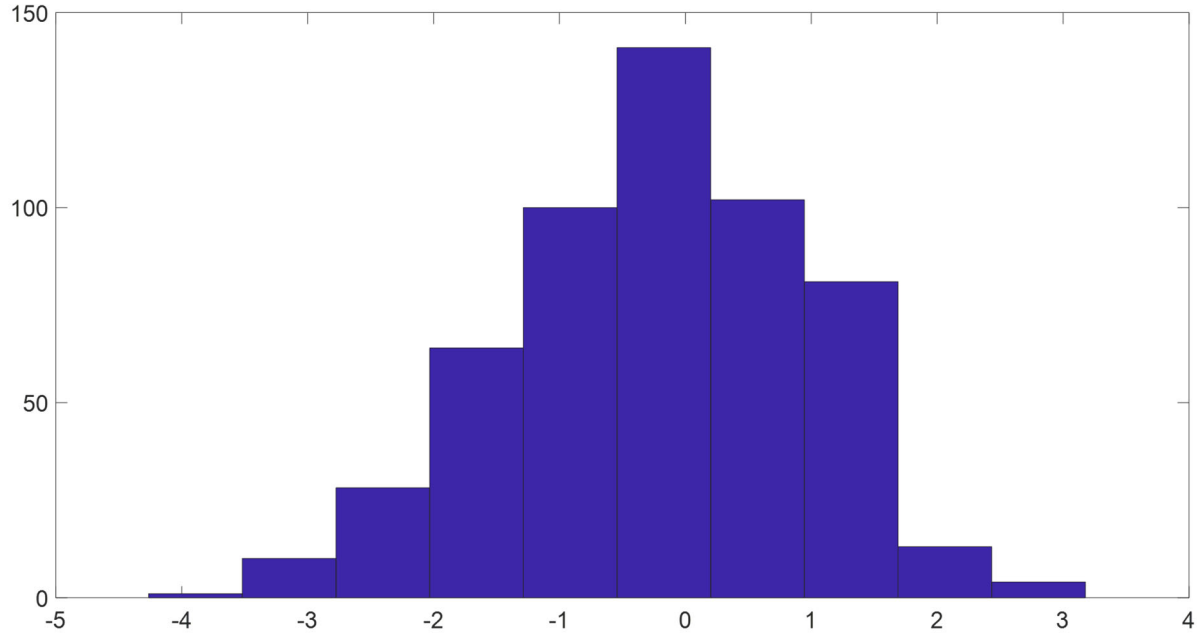
NOTES: The example is based on the sign-identified structural VAR model in Kilian and Zhou (2021), as reported in Inoue and Kilian (2021a).

Figure 2: Impulse response estimator under absolute loss based on data generated from the prior in Baumeister and Hamilton (2019a)



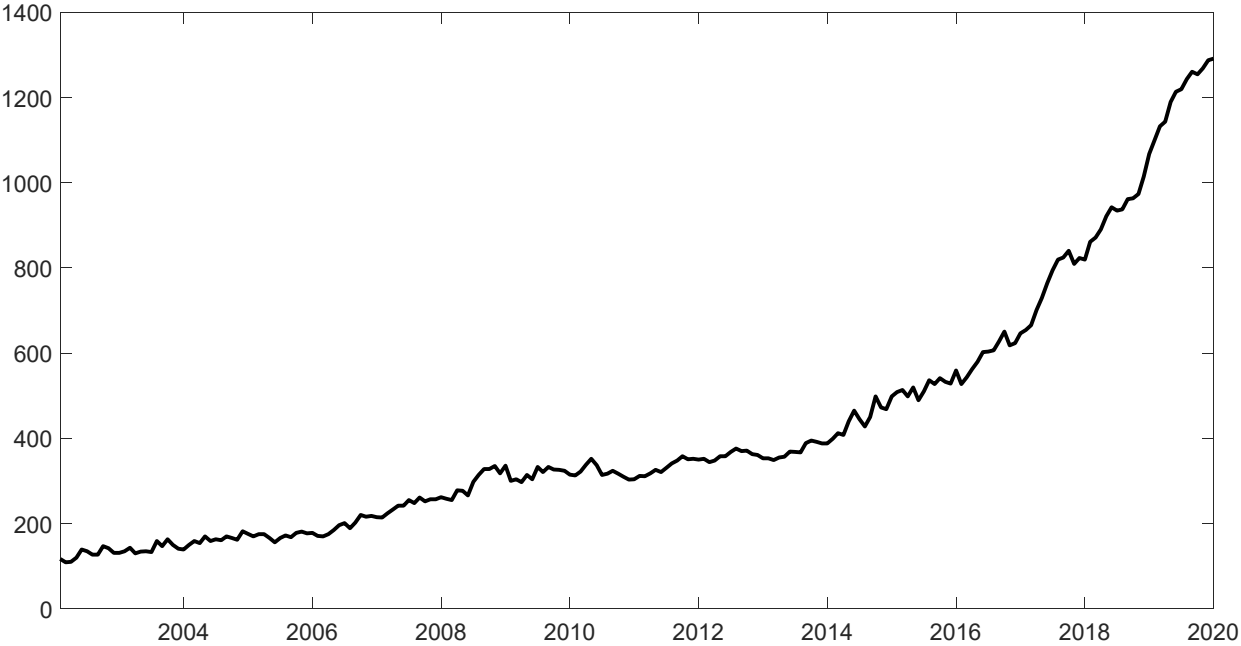
NOTES: The black lines represent the central tendency of the prior under absolute loss, as discussed in Inoue and Kilian (2021a). They show that the prior specified by Baumeister and Hamilton (2019a) implies that a sustained global economic expansion is associated with a decline in the real price of oil, which is at odds with conventional economic reasoning.

Figure 3: Measurement Error in Baumeister and Hamilton's (2019a) Measure of Global Oil Consumption Growth



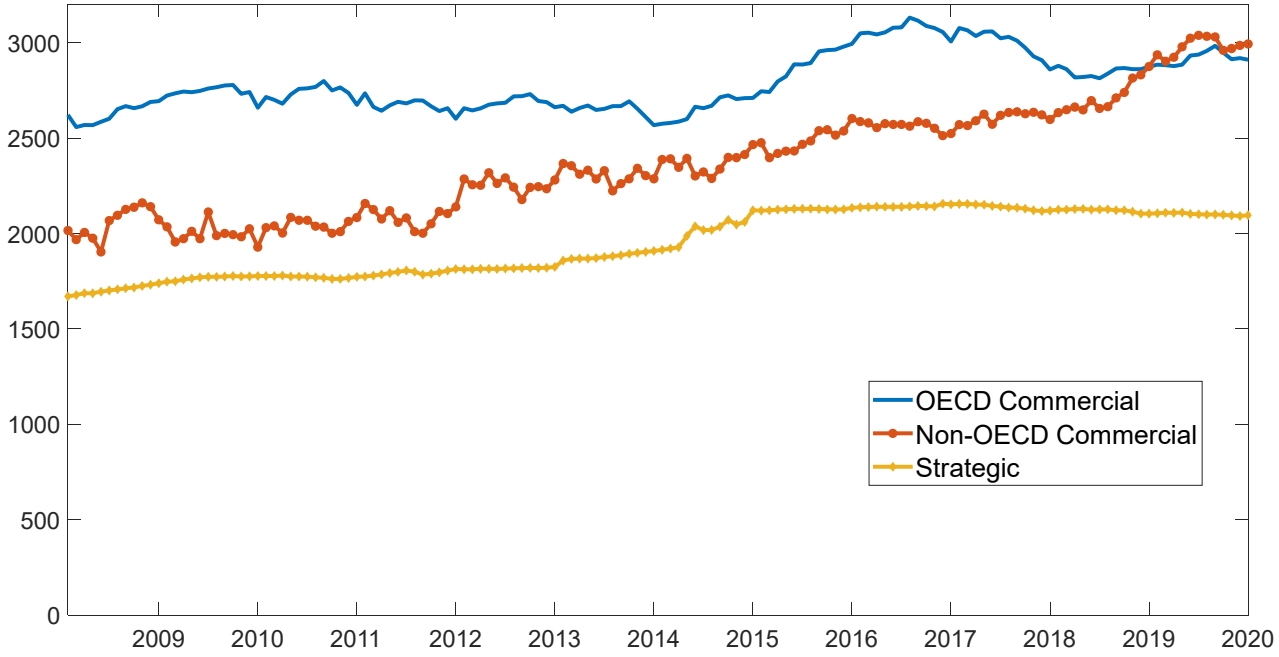
NOTES: Monthly growth rates, not annualized. The figure shows the histogram of the difference between the consumption growth rate based on the formula used in Baumeister and Hamilton (2019a) and the correctly computed consumption growth rate, as described in the text. Source: Kilian (2021).

Figure 4: The Evolution of Chinese Inventories of Crude Oil, 2002.1-2019.12



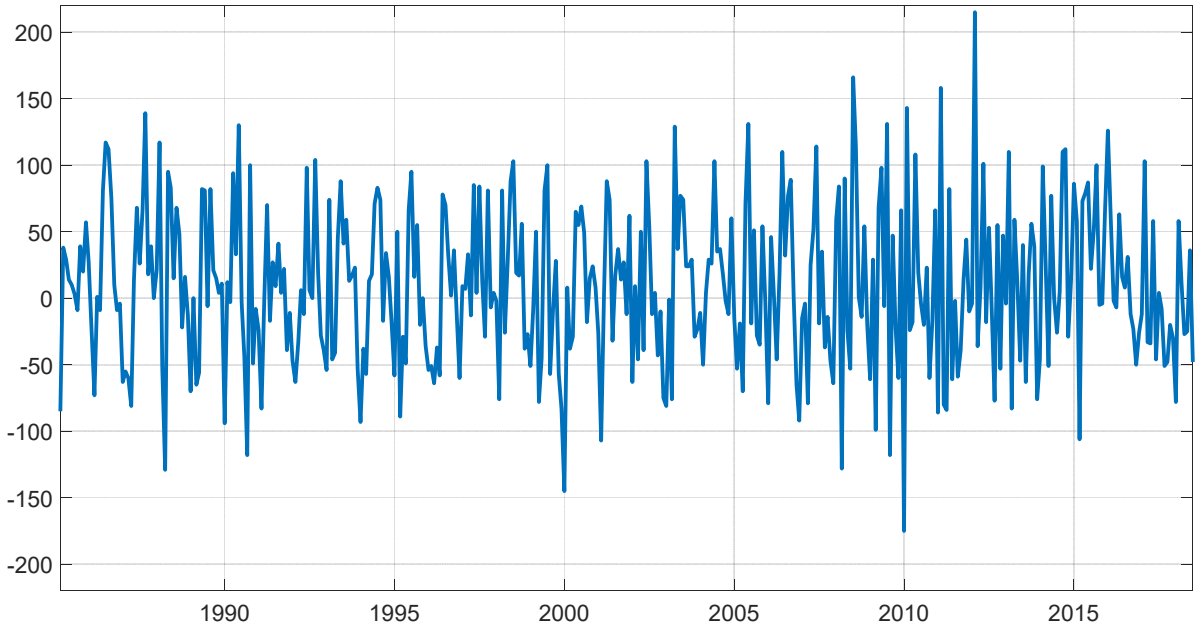
NOTES: Million barrels. Constructed from crude oil inventory data compiled by the Energy Intelligence Group (EIG).

Figure 5: Non-OECD Crude Oil Inventories, 1985.2-2019.12



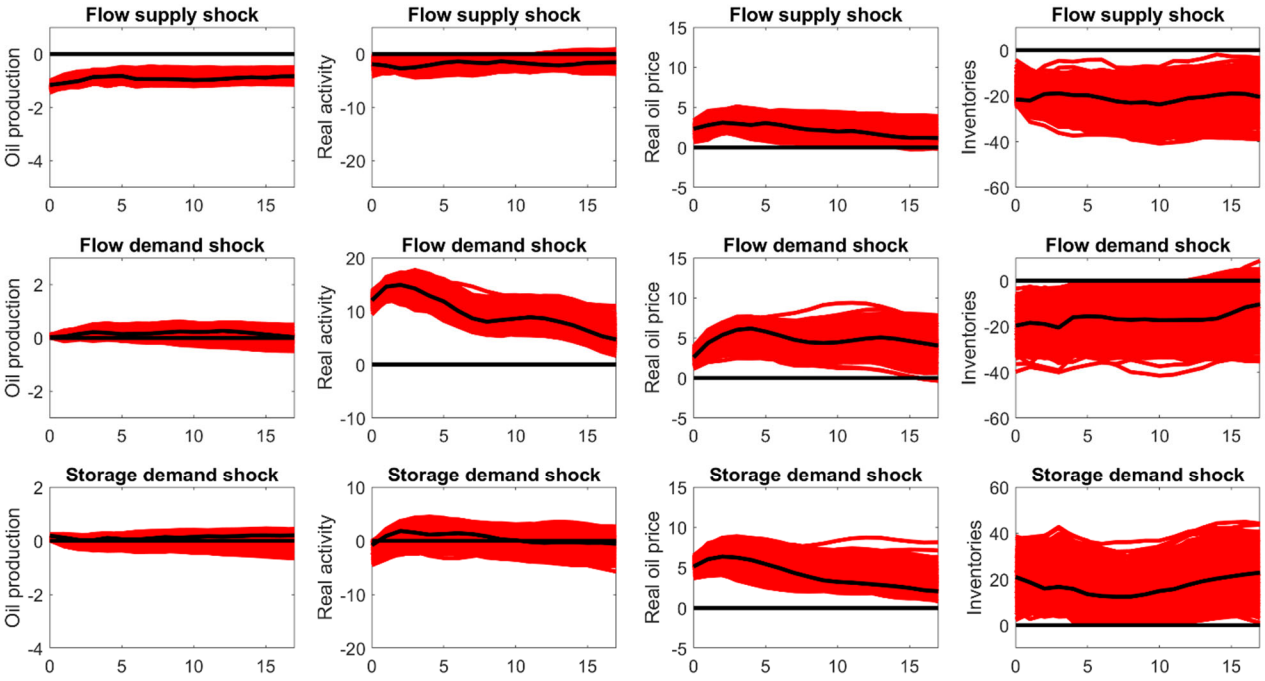
NOTES: Million barrels. Constructed from crude oil inventory data compiled by the Energy Intelligence Group (EIG).

Figure 6: Change in Global Crude Oil Inventories Based on EIG Data, 1985.2-2018.6



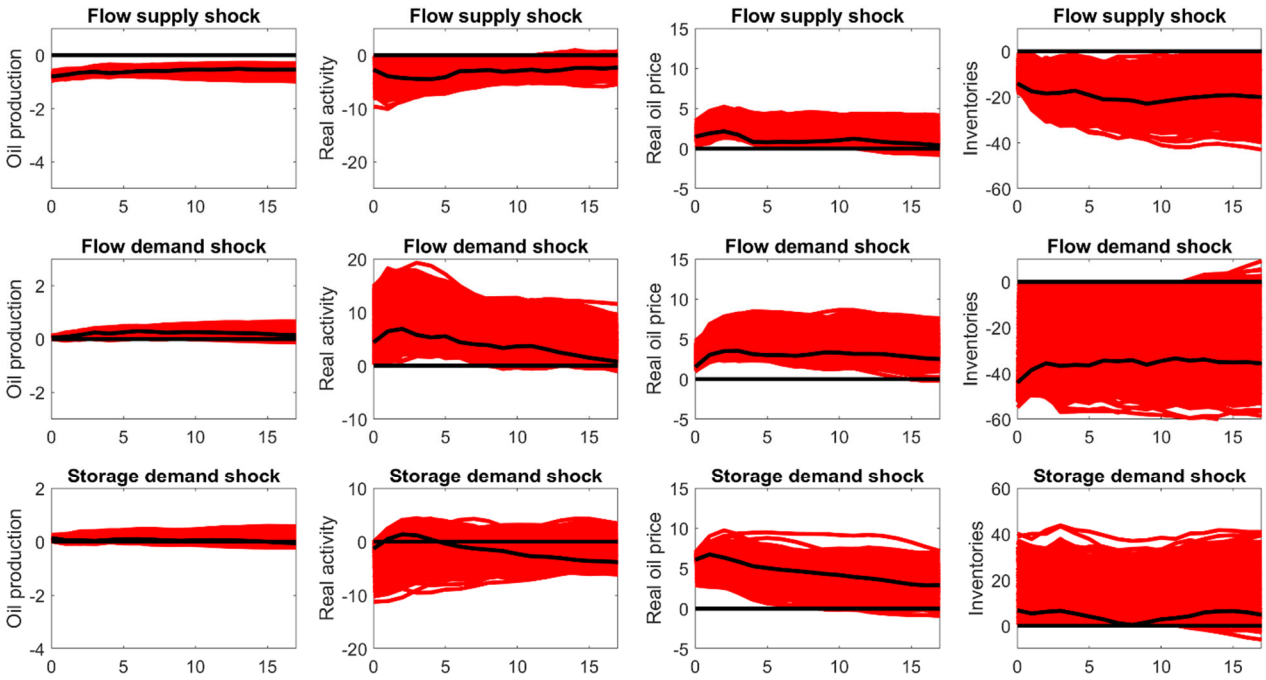
NOTES: Million barrels. Constructed from global crude oil inventory data compiled by the Energy Intelligence Group (EIG). The series incorporates strategic and commercial stocks as well as oil at sea and in transit in the OECD and in the rest of the world, including China.

Figure 7: Kilian and Murphy (2014) Model Fit to Data for 1973.2-2018.6 with Global Oil Inventory Data from the Energy Intelligence Group Spliced with Kilian and Murphy Proxy



NOTES: Bayes estimator of responses under absolute loss and the corresponding 68% joint credible set. Based on 1,383 admissible posterior draws.

Figure 8: Kilian and Murphy (2014) Model Fit to Data for 1985.2-2018.6 with Global Oil Inventory Data from the Energy Intelligence Group



NOTES: See Figure 7. Based on 4,000 admissible posterior draws.

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