Do Energy Prices Respond to U.S. Macroeconomic News? A Test of the Hypothesis of Predetermined Energy Prices

Lutz Kilian
University of Michigan and CEPR

Clara Vega
Federal Reserve Board

October 7, 2008

Abstract: Models that treat innovations to the price of energy as predetermined with respect to U.S. macroeconomic aggregates are widely used in the literature. For example, it is common to order energy prices first in recursively identified VAR models of the transmission of energy price shocks. Since exactly identifying assumptions are inherently untestable, this approach in practice has required an act of faith in the empirical plausibility of the delay restriction used for identification. An alternative view that would invalidate such models is that energy prices respond instantaneously to macroeconomic news, implying that energy prices should be ordered last in recursively identified VAR models. In this paper, we propose a formal test of the identifying assumption that energy prices are predetermined with respect to U.S. macroeconomic aggregates. Our test is based on regressing cumulative changes in daily energy prices on daily news from U.S. macroeconomic data releases. Using a wide range of macroeconomic news, we find no compelling evidence of feedback at daily or monthly horizons, contradicting the view that energy prices respond instantaneously to macroeconomic news and supporting the use of delay restrictions for identification.

Key words: Oil price; Gasoline price; News; Identification; Impulse Responses.

JEL: C32, E37, Q43

Acknowledgements: We thank Christian S. Miller for his research assistance. The views expressed in this paper do not necessarily reflect the views of the Federal Reserve Board or its members. We have benefited from discussions with Roger Gordon, Eric Sims and Daniel Beltran.

Correspondence to: Lutz Kilian, Department of Economics, University of Michigan, 238 Lorch Hall, 611 Tappan Street, Ann Arbor, MI 48109-1220, USA. Email: lkilian@umich.edu. Fax: (734) 764-2769. Phone: (734) 647-5612.

1. Introduction

It is widely accepted that energy prices in general and crude oil prices in particular have been endogenous with respect to U.S. macroeconomic conditions dating back to the early 1970s (see e.g., Barsky and Kilian 2004; Hamilton 2008; Kilian 2008a). Endogeneity in this context refers to the fact that not only do energy prices affect the U.S. economy, but that there is reverse causality from U.S. macroeconomic aggregates to the price of energy. Clearly, both the supply of energy and the demand for energy depend on U.S. macroeconomic aggregates such as real economic activity and interest rates (see Barsky and Kilian 2002). Thus, a correlation between energy prices and U.S. macroeconomic outcomes does not necessarily imply causation.

One response to this problem is the use of instruments for changes in energy prices. While this approach is appealing, the challenge has been to find instruments that are both truly exogenous and strong in the econometric sense (see Stock, Wright and Yogo 2002). For example, Ramsey, Rasche and Allen (1975) and Dahl (1979) use the relative prices of refinery products such as kerosene and residual fuel oil as instrumental variables for the price of gasoline. As noted in Hughes, Knittel and Sperling (2008) the problem with this approach is that the relative prices of other refinery outputs are likely to be correlated with gasoline demand shocks.[1] Davis and Kilian (2008) explore the use of changes in gasoline taxes as instruments for changes in gasoline prices, but note various concerns over the validity of this instrument.

Hamilton (2003) uses measures of exogenous cutbacks in global crude oil production as instruments for changes in the price of crude oil. A similar approach has been taken by Hughes, Knittel, and Sperling (2008) in instrumenting for U.S. gasoline prices.[2] Although these instruments are arguably exogenous, Kilian (2008a) provides evidence that crude oil supply shocks driven by exogenous political events are weak instruments, rendering estimation and inference by standard methods invalid. Finally, Cullen, Friedberg and Wolfram (2004) use weather data as exogenous instruments for home energy costs, but that instrument seems unsuitable for crude oil or gasoline prices.

An alternative response to the lack of exogeneity of energy prices has been to impose the much weaker and hence more defensible assumption that energy prices are predetermined with

[1] Oil is a common input to the production of refined products. Since increased gasoline demand will tend to increase the price of oil, unobserved shocks to gasoline demand are likely to be correlated with the prices of other refinery outputs via the price of oil.
[2] For a detailed discussion of alternative methods of constructing exogenous oil supply shocks driven by political events in the Middle East see Kilian (2008b,c).

respect to U.S. macroeconomic aggregates. The assumption of predetermined energy prices rules out instantaneous feedback from U.S. macroeconomic aggregates to energy prices, but allows energy prices to respond to all past information. In other words, the price of energy responds to changes in U.S. macroeconomic conditions only with a delay. This identifying assumption permits the consistent estimation of the expected response of real U.S. macroeconomic aggregates to an innovation in energy prices. In conjunction with the assumption that there are no other exogenous events that are correlated with the exogenous energy price innovation, these impulse responses can be interpreted as the causal effect of the energy price innovation (see Cooley and LeRoy 1985).

The simplest example of such a model is a recursively identified bivariate vector autoregression (VAR), in which the percent change in real energy prices is ordered first and the macroeconomic aggregate of interest is ordered second. For example, we may assess the response of U.S. real GDP or industrial production to a real energy price innovation by specifying a model in the percent change of the real price of energy and the percent growth in real output. Exactly the same issues arise in larger VAR models, in which the energy prices are treated as predetermined with respect to some or all U.S. macroeconomic aggregates.

The assumption of predeterminedness typically is implausible when working with annual data, but may provide a good approximation when working with quarterly and in particular with monthly data. Although the exactly identifying assumption that energy prices are predetermined with respect to domestic macroeconomic aggregates is not testable within the VAR framework, the working hypothesis that the feedback from shocks to domestic macroeconomic aggregates to the global price of oil is negligible within the same month has been regarded as plausible by many researchers. Models that treat innovations to the price of crude oil as predetermined with respect to U.S. macroeconomic aggregates at high frequency have been prominent in the literature for many years (see, e.g., Rotemberg and Woodford 1996; Davis and Haltiwanger 2001; Lee and Ni 2002; Leduc and Sill 2004; Blanchard and Galí 2007, Kilian and Park 2008). Similar assumptions have been made in studying U.S. retail energy prices such as the price of motor gasoline (see, e.g., Edelstein and Kilian 2007a,b).

A typical assumption is that changes in the price of crude oil are predetermined with respect to U.S. real output, consumption, and investment. Other studies have specifically assumed that oil prices are predetermined with respect to U.S. interest rates in studying the endogenous response of monetary policy to oil price shocks (see, e.g., Bernanke, Gertler and Watson 1997, 2004; Hamilton and Herrera 2004; Balke, Brown and Yücel 2002; Pesavento and Herrera 2007). Similar predeterminedness assumptions have been used implicitly in VAR models that disentangle demand and supply shocks in energy markets (see, e.g., Kilian 2008d; Kilian 2008e; Kilian and Park 2008).

Explicitly or implicitly, the assumption in these studies is that the price of crude oil is determined in global markets and responds instantaneously to global demand and supply shocks, yet does not respond in the short run to U.S. domestic macroeconomic innovations. Imposing the delay restriction on feedback from U.S. macroeconomic aggregates to the price of energy in practice has required an act of faith in the empirical plausibility of the delay restriction used for identification.[3] It is fair to say that this identifying assumption, while popular in academic research, is not universally accepted. If nothing else, we read in the newspaper every day about how oil prices have responded that day to information revealed since the previous day about economic events in the U.S. This alternative view is based on the notion that oil prices behave like asset prices in that they respond immediately to all new information. To the extent that this view is correct, the assumption of no instantaneous feedback may provide a rather poor approximation. In fact, one would want to order oil prices last in recursively identified VAR models of the U.S. macroeconomy rather than first. This distinction matters. Current policy discussions about the causes and effects of higher oil prices are based on empirical results obtained from models that impose the assumption of predetermined energy prices. If the asset price interpretation of oil prices were empirically supported, we would have little confidence in these results and their policy implications.

In this paper, we examine the empirical support for these alternative interpretations. We propose a formal test of the view that oil prices respond without delay to exogenous variation in macroeconomic data. Our approach is based on a methodology pioneered by Andersen Bollerslev, Diebold, and Vega (2003, 2007) in the related, but different context of studying price discovery in asset markets (also see Faust, Rogers, Wang, and Wright 2007). It utilizes daily data on crude oil and gasoline prices in conjunction with daily data on the news component of U.S. macroeconomic data releases. The proposed test is quite simple: Evidence that news about U.S.

[3] Kilian (2008d) provides some empirical evidence in support of this assumption, but that evidence does not cover all possible forms of instantaneous feedback and hence is suggestive only.

macroeconomic data affect daily energy prices would contradict the assumption that these prices are predetermined with respect to U.S. macroeconomic aggregates, whereas lack of evidence of such feedback would be supportive of the conventional assumption of predetermined energy prices. The test can be modified easily to assess not only the instantaneous effect of macroeconomic news on the day of the announcement, but to assess its effect on energy prices a month later. This novel approach allows us to test the assumption of predetermined energy prices, despite the fact that this assumption is inherently untestable within the context of an exactly identified econometric model estimated at monthly frequency.

Our first result is that, unlike stock prices, bond prices or exchange rates, the price of WTI crude oil and the U.S. price of gasoline do not respond to U.S. macroeconomic news instantaneously, contradicting the view that oil prices should be thought of as asset prices. This result is based on the longest available sample of daily oil and gasoline prices and 30 different measures of macroeconomic news. Our second result is that there is no compelling evidence against the assumption that oil and gasoline prices are predetermined with respect to monthly U.S. macroeconomic aggregates during 1983-2008, lending support to previous empirical work based on the delay restriction. Specifically, our analysis provides no evidence of feedback at the monthly horizon for any of the U.S. macroeconomic aggregates typically included in regression models used to study the transmission of energy price shocks. Only for a set of forward-looking news variables is there any statistically significant evidence at all of feedback at the monthly horizon. That evidence is stronger for gasoline prices than for crude oil prices. In fact, none of the forward-looking news variables by itself has statistically significant effects on the price of oil. Moreover, the extent of the feedback appears to be small enough to be ignored. Notably, between 98% and 99% of the monthly variation in gasoline and oil prices remain unexplained by all thirty macroeconomic news shocks combined.

The remainder of the paper is organized as follows. In section 2, we describe the data and econometric methodology. Section 3 contains a detailed discussion of the impact response of energy prices to U.S. macroeconomic news. We show that oil prices and gasoline prices differ from commonly studied asset prices. In section 4 we extend the analysis to monthly horizons and test the assumption that energy prices are predetermined with respect to U.S. macroeconomic aggregates at monthly frequency. In section 5 we contrast our methodology with related papers in the literature on the effect of news on oil prices. The concluding remarks are in section 6.

2. Methodology

2.1. Macroeconomic News

We use the International Money Market Services (MMS) real-time data on expected and realized U.S. macroeconomic fundamentals, defining "news" as the difference between ex ante survey expectations and the subsequently announced realizations. The MMS sample covers the period from January, 1983 through April, 2008, but not all the announcements are followed by MMS from the beginning of the sample. Table 1 provides a description of the announcement releases, including the number of observations, the agency reporting the news, and the time of the release. Our data set includes quarterly announcements for GDP; monthly announcements for various measures of real activity, consumption, investment, fiscal and trade balances, prices, the Fed target rate, and forward looking indicators; as well as weekly announcements of initial unemployment claims. The units of measurement obviously differ across the macroeconomic indicators as is apparent from the last column of Table 1 that shows the standard deviations. To allow for meaningful comparisons of the estimated news response coefficients across indicators and asset classes, we follow Andersen et al. (2003) in that we use "standardized news" measures. Specifically, we divide the surprise component of the announcement by its sample standard deviation, defining the standardized news associated with indicator i at time t as

$$S_{it} = \frac{A_{it} - E_{it}}{\hat{\sigma}_i},$$

where A_{it} denotes the announced value of indicator i, E_{it} refers to the market's expectation of indicator i prior to the announcement (represented by the MMS median forecast), and $\hat{\sigma}_i$ is the sample standard deviation of the surprise component, $A_{it} - E_{it}$. Because $\hat{\sigma}_i$ is constant for each indicator i, this standardization affects neither the statistical significance of the estimated response coefficients nor the fit of the regressions compared to the results based on the "raw" surprises.

2.2. Estimating the Effect of U.S. Macroeconomic News on Energy Prices

We model energy prices as daily percent changes, permitting news to have a permanent effect on the level of nominal energy prices. The baseline model in section 3 focuses on the impact effect of news. We fit the model

$$R_{t+1} = \alpha + \beta_i S_{it} + \varepsilon_{t+1},$$

where $R_{t+1} = 100 \times \ln(P_{t+1} / P_t)$ denotes the daily return on holding regular gas or WTI crude oil from the end of day $t-1$ to the end of day t, and S_{it} refers to the standardized news for announcement i, $i = 1,...,30,$ on day t. The regression estimates are based only on data for those days on which a news announcement was made. Inference is based on White standard errors to allow for the possibility of time-varying variances. The parameter β_i measures the response of R_{t+1} to a one-standard deviation news shock. An estimate of $\hat{\beta}_5 = 0.027$, for example, would imply that an unexpected increase of nonfarm payroll employment by 111,153 jobs would cause an increase in the price of oil by 0.027%.

In addition to the regressions involving one news shock predictor at a time, we also consider the joint regression

$$R_{t+1} = \alpha + \sum_{i=1}^{30} \beta_i S_{it} + \varepsilon_{t+1},$$

for all date *t* observations. In that case, inference is based on Newey-West standard errors to allow for the possibility of serial correlation and heteroskedasticity under the null hypothesis.

Focusing on daily asset price changes around the time of the announcement and estimating the immediate news reaction of asset prices helps isolate the effect of the news announcement among the effect of a myriad of other changes in the economy. This strategy has already been applied successfully to numerous financial assets in the literature. If traders are slow to appreciate the significance of news shocks, however, the reaction of oil prices to news shocks may be delayed; hence the focus on daily data may cause us to miss the impact of news on oil prices. In section 4, we will allow for a delayed reaction of oil prices to news, by regressing cumulative daily returns on crude oil for a horizon of one month on daily macroeconomic news (the monthly returns are calculated from the end of day $t-1$ to the end of day $t+h-1$, where h is equal to 20 business days in the monthly regression, and the announcement occurs on day *t*).

Our sample period is dictated by data availability constraints. The full-sample regression results for WTI crude oil prices rely on data from May 1983 to April 2008 (see Table 2) and the full sample regression results for regular gasoline prices are based on data from January 2003 to April 2008 (see Table 3). We report the coefficient estimates, *t*-statistics and *p*-values calculated using robust standard errors. We also report the R^2 of the regression and the number of observations in each regression. In the case of the individual regressions, that sample size corresponds to the number of news announcements over the sample period.

We test $H_0 : \beta_i = 0$ against the one-sided alternative hypotheses suggested by economic theory. In particular, a positive news shock about measures of current or future output (and its components) or about employment should be associated with a positive response. The same is true for unanticipated increases in the price level. In contrast, positive news shocks about the unemployment rate and initial claims should be associated with declining energy prices. Similarly, positive interest rate shocks tend to be associated with a decline of economic activity and hence lower energy prices.[4] Finally, an unanticipated increase of business inventories is interpreted as evidence of an economic slowdown and is associated with a negative sign. The use of one-sided *t*-tests not only makes economic sense in this context, but it is conventional in testing for predictability, and it improves substantially the power of tests of the predictability of energy prices, as discussed in Inoue and Kilian (2004).

3. Should We Think of Oil Prices as Asset Prices?

It is well documented that stock prices and exchange rates fully and systematically respond within the same day to macroeconomic news announcements (see Andersen et al. 2003, 2007). Given the perception that oil prices behave much like asset prices that respond instantaneously to all news, it is natural to contrast the response of oil and gasoline prices to macroeconomic news shocks to that of commonly studied asset prices. A useful starting point is the nonfarm payroll report. Of the thirty macroeconomic news announcements we analyze, the nonfarm payroll report is one of the most closely observed U.S. macroeconomic announcements. Andersen and Bollerslev (1998), among others, refer to this announcement as the "king" of announcements because of the significant sensitivity of most asset prices to its release. Tables 2a and 3a, however, suggest that nonfarm payroll announcements have no effect on retail gas prices and crude oil prices using conventional asymptotic *p*-values. This is a first indication that oil and gasoline prices should not be thought of as asset prices. In fact, even the R^2 estimates of 0.02%

[4] An alternative view is that interest rate cuts may signal weaker-than-expected economic growth to financial markets (see Bernanke and Kuttner 2005, p. 1230). This interpretation would suggest a positive sign for the interest rate coefficient. However, there does not appear to be empirical evidence in support of that alternative view and theoretical models overwhelmingly predict a negative sign (see, e.g., Barsky and Kilian 2002).

and 0.37% for nonfarm payroll employment are strikingly low compared with the estimates that would be obtained for the corresponding sample periods using other asset returns. The latter estimates may be as high as 20% in some cases.

A second indication is that the R^2 of the joint regressions in Tables 2b and 3b tends to be very low. In the joint regression, all macroeconomic news shocks combined explain only 0.38% of the variation in oil prices (and only 1.91% of the variation in gasoline prices). Put differently, more than 99% (or more than 98%) of the variation in daily energy prices is driven by factors not correlated with domestic macroeconomic aggregates. These R^2 estimates also tend to be lower than those for similar regressions for other asset prices. For example, for daily S&P500 returns, which are known to be among the least predictable asset returns, the R^2 estimates for the corresponding sample periods and joint regressions are 2.0% and 3.2%, respectively. At the other extreme, for daily 10-year bond returns, we obtain R^2 estimates of 4.9% and 8.2%, respectively, confirming the impression that macroeconomic news are less informative for oil and gasoline prices than for financial asset prices.

The R^2 estimates for the individual regressions, at least for gasoline prices, seem at first sight to paint a more favorable picture for the asset market interpretation.[5] Whereas for the price of crude oil the individual R^2 exceeds 2% only in one case, in the case of gasoline prices, the individual R^2 estimates tend to be higher in general, with nine estimates exceeding 2%, of which two even exceed 5%. There is reason to be cautious in interpreting these individual R^2 results, however. For example, the NAPM index appears to explain 5.58% of the variation in U.S. gasoline prices, but it has a coefficient of the wrong sign. The fact that quite frequently the estimated coefficients are of the wrong sign is a further indication that the regression fit is likely to be spurious. For example, in Tables 2a and 2b, unanticipated increases in retail sales, personal income or consumption, or durables goods orders, should increase the price of oil, not lower it. The same is true for inflation surprises, yet three of four inflation news shocks have negative coefficients.

If we focus on the statistical significance of the one-sided *t*-tests using conventional asymptotic critical values, a somewhat different picture emerges. For the price of crude oil, only six predictors appear statistically significant at the 10% level in the individual regressions (see

[5] In interpreting the results it is useful to keep in mind that the individual regressions are based on a different data set than the joint regressions, so the magnitude of the R^2 estimates is not comparable.

Table 2a). In the joint regression, only three predictors remain statistically significant at the 10% level (see Table 2b). For the price of gasoline, there are three rejections using individual regressions in Table 3a and two rejections for the joint regression in Table 3b. The statistically significant predictors are not the same in both markets, which again suggests that the results are likely to be spurious. For gasoline prices, industrial production and factory orders are most significant (with mixed results for the core CPI), whereas for crude oil the net government purchases, the core CPI and housing starts are selected most often with mixed support for preliminary GDP, the Conference Board's consumer confidence measure, and new home sales.

Although many of these variables are not part of the regression models that have been used to study the transmission of energy prices shocks, it may be tempting to interpret these rejections as evidence that the assumption of predetermined energy prices is suspect. This interpretation, however, is questionable. For one thing it is odd that among news variables that are conceptually closely related only some appear to have predictive power. For example, we would expect GDP and industrial production news to have similar effects on energy prices.

More importantly, that interpretation would ignore that we have conducted not one *t*-test in assessing the evidence against that assumption, but thirty *t*-tests. Conventional critical values do not account for repeated applications of the same test to alternative predictors. The failure to account for such data mining is known to cause spurious rejections of the null of no predictability (see, e.g., Inoue and Kilian 2004). The problem of data mining is well recognized in the literature (see, e.g., Denton 1985). If we investigate whether at least one of many predictors is statistically significant, the probability of rejecting the null hypothesis of no predictability at conventional significance levels increases with the number of predictors considered, resulting in spurious rejections of the null hypothesis of no predictability when that null hypothesis is in fact true. Such data mining problems have been shown to be practically important in a variety of related contexts including the search for calendar effects in stock returns and the search for profitable technical trading rules (see, e.g., White 2000; Sullivan, Timmermann, and White 2001).

Inoue and Kilian (2004) discuss appropriate adjustments to the null distribution of predictability tests in the presence of data mining. The basic idea is to compute data-mining robust critical values for the supremum of the *t*-statistic across the thirty alternative predictors. In practice, this may be accomplished by bootstrap methods. We simulate the finite-sample distribution of the supremum of the t-statistic under the null hypothesis of no predictability. For simplicity, we postulate that returns and news shocks are i.i.d. normally distributed with the variances found in the actual data. We abstract from the possibility of fat tails, heteroskedasticity or serial correlation under the null hypothesis. Accounting for these possible departures from i.i.d. normality, if anything, would tend to increase further the data-mining robust critical values constructed below. We treat the news shocks as mutually independent.[6] The empirical distribution of the supremum t-statistic is constructed by estimating the regression models in question in each bootstrap sample and tabulating the distribution of the largest t-statistic among the 30 alternative predictors. All results are based on 100,000 bootstrap replications. The bootstrap replicates of the individual regressions take account of the differences in sample size across regressions. When bootstrapping the joint regression we treat the timing of the news shocks as exogenously given in repeated sampling. This makes sense because the announcements are pre-scheduled.

After adjusting for data mining, none of the statistically significant results in Tables 2 and 3 remain. For example, the 5% data-mining robust critical value for Table 2a is 2.96 and the 10% critical value rises to 2.72. For the price of crude oil, the lowest p-value is 0.23 in the individual regressions and 0.46 in the joint regression. For gasoline prices, the lowest p-value is 0.88 in the individual regressions and 0.48 in the joint regression. These results suggest that there is no empirical evidence that daily WTI crude oil prices or U.S. gasoline prices respond to macroeconomic news shocks on impact.[7]

4. Testing the Assumption of Predetermined Energy Prices at Monthly Horizons

The preceding analysis demonstrated that energy prices do not behave like stock prices, bond prices, or exchange rates. Neither WTI crude oil prices nor, for that matter, U.S. gasoline prices appear to respond to macroeconomic news on impact. In particular, whereas most asset markets respond significantly to news about nonfarm payroll reports the crude oil and gasoline markets do not. The evidence we presented was based on the reaction of energy prices to news shocks

[6] That assumption is empirically plausible except for news announcements for closely related series that occur on the same day (such as news announcements for the core CPI and the CPI). The latter situation is an exception. We experimented with alternative assumptions that account for the possible dependence of these announcements. The results reported below are robust to these alternative assumptions.

[7] Note that many of the data-mining robust p-values are effectively 1.000. The reason is that we compare all individual t-test-statistics to the null distribution of the maximum t-statistic. Alternatively, one could focus on the largest of the thirty t-statistics only. The substantive interpretation of the results would be the same.

within the day. This approach made sense since financial asset prices are known to adjust fully to news announcements within the day (see, e.g., Andersen et al. 2007) and a rejection of the no predictability null at daily horizons would have sufficed to reject the assumption of predetermined energy prices at monthly frequency. Since we did not reject the null for any news shock, some additional analysis is required. The reason is that, even if energy prices are not asset prices in the same sense as exchange rates or stock prices, they may still respond to macroeconomic news shocks over time, invalidating the assumption of predetermined energy prices in applied and theoretical work on the transmission of energy price shocks. For example, it may take traders time to appreciate the full significance of domestic macroeconomic news announcements for the global crude oil market (and hence the U.S. gasoline market).

In this section, we address this concern by specifying a regression for the percent change in energy prices between close-of-business on the trading day preceding the news shock S_{it} and thirty calendar days later:

$$R_{t+1}^{h} = \alpha + \beta_i S_{it} + \varepsilon_{t+1}^{h},$$

where $R_{t+1}^{h} = 100 \times \ln(P_{t+h} / P_t)$ denotes the monthly return on energy from the end of day $t-1$ to the end of day $t+h-1$, $h = 20$ (since there are five business days per week), and the one-step ahead predictive error ε_{t+1}^{h} is serially correlated under $H_0 : \beta_i = 0$, necessitating the use of Newey-West standard errors. As before, the estimates are based only on data for those dates for which an announcement was made on day *t*. Alternatively, we consider the joint regression:

$$R_{t+1}^{h} = \alpha + \sum_{i=1}^{30} \beta_i S_{it} + \varepsilon_{t+1}^{h},$$

One concern is that one-month-ahead regressions may lack the power to detect predictability, because we need to estimate the effect of news shocks among a myriad of other changes that take place over the course of one month. We address this concern by focusing on the WTI price of crude oil, for which 6214 observations spanning 25 years of data are available. The comparatively large sample size helps increase the power of the test. As Table 4 shows, conventional *p*-values indicate about as many rejections of the null of no predictability at the monthly horizon as in the earlier daily analysis, suggesting that low power is not a concern.

To conserve space, Table 4 shows only the *p*-values of tests of no feedback from news announcements to the price of oil. The first two columns of *p*-values refer to the results from the

30 individual regressions, the next two columns to the results from the joint regression. The R^2 estimate from the joint regression is somewhat larger at the monthly horizon than at the daily horizon. It rises from 0.38% to 0.69%. This pattern is consistent with the increasing importance of feedback from macroeconomic news shocks at longer horizons. In absolute terms, however, the feedback continues to be negligible, even abstracting from the dangers of overfitting.

Conventional *t*-tests indicate four rejections at the 5% level in the individual regressions (net government purchases, trade balance, preliminary UM consumer confidence, Conference Board consumer confidence, index of leading indicators) and two additional rejections at the 10% level (GDP final, capacity utilization). In the joint regression, there are five rejections at the 5% level (capacity utilization, net government purchases, preliminary Michigan consumer confidence, Conference Board consumer confidence, and the index of leading indicators) with no additional rejections at the 10% level.

As in the daily analysis, there is reason to distrust these *p*-values. It is not uncommon for the point estimates underlying Table 4 to be of the wrong sign, in some cases even significantly so. For example, GDP (advanced) both in the individual and joint regression has a t-statistic of about -1.9. Using more appropriate data-mining robust critical values constructed along the lines described in section 3, none of *t*-statistics remains statistically significant. The 5% critical value rises to 2.965; the 10% critical value to 2.726. The lowest *p*-value in the joint regression is obtained for the index of leading indicators with 0.17; for the individual regressions it is 0.18 for the Conference Board's index of consumer confidence. There is no evidence of within-the-month feedback from industrial production, consumer expenditures, the unemployment rate, consumer prices, or interest rates, in particular. These are the variables most widely used in monthly regressions aimed at uncovering the effects of energy price shocks on domestic aggregates. The results in Table 4 support the common practice of treating oil prices as predetermined with respect to U.S. macroeconomic aggregates.

An alternative approach to addressing the potential for data mining based on the joint regression is to construct Wald tests for the joint statistical significance of subsets of news shocks related to the same economic concept. For example, the first 10 news shocks jointly with the last shock all represent news about *domestic aggregate real activity*. If we add news shocks 11 through 18 to this set, we obtain the set of all *aggregate and disaggregate measures of domestic real activity*. News shocks 19 through 22 represent *inflation* shocks and news shocks 23 through 28 represent *forward-looking indicators*. Since there are only four sets of predictors in total, the scope for data mining is limited, and conventional critical values are likely to be only mildly downward biased.

Table 5 shows that measures of domestic real activity jointly are not statistically significant at conventional significance levels. Nor are measures of inflation. Only forward-looking variables are statistically significant at the 5% level.[8] The predictive power of this set of news shocks appears to emanate from a combination of news about the index of leading indicators and about consumer confidence, none of which is individually statistically significant, as we showed in Table 4. Even granting that the Wald test evaluated at conventional critical values is likely to overstate the degree of significance somewhat, this result is likely to be at least borderline statistically significant at the 5% level. The possibility of contemporaneous feedback suggested by this alternative test has to be taken seriously. The existence of such feedback would have important implications for the interpretation of innovations to the price of oil even in VAR models that do not include any of these variables as regressors. If there is contemporaneous feedback from any U.S. macroeconomic aggregate to the price of oil (even if that aggregate is not included in the VAR model), we cannot interpret VAR innovations to the price of crude oil as exogenous with respect to the U.S. economy.

On the other hand, it is important to keep in mind that the explanatory power of these news shocks as measured by R^2 is negligible. In fact, only 0.69% of the monthly variation in oil prices is explained by *all* news shocks combined, and hence even less by any subset of these predictors. Even if we restrict ourselves to days on which announcements about forward looking variables took place, which tends to result in larger R^2 estimates, as shown in Table 2, the R^2 of the set of all forward looking variables is only 0.38%. In other words, if there is contemporaneous feedback, it is so weak, that we may ignore it in practice. Thus, the result in the last row of Table 5 does little to overturn our earlier evidence in favor of the assumption that oil prices can be treated as predetermined with respect monthly measures of domestic macroeconomic aggregates.

For completeness, we conducted a similar analysis on U.S. gasoline prices for 2003-2008 at the monthly horizon. Despite the shorter time span, the pattern of rejections of the Wald tests is the same as in Table 5. Only news shocks about forward-looking variables appear to have

[8] The same set of forward-looking variables is not jointly statistically significant at the daily horizon.

predictive power jointly. The latter result appears to be driven exclusively by the index of leading indicators, which (unlike all other predictors) is statistically significant in both the individual and joint regression even after allowing for data mining. Nevertheless, the overall explanatory power of all macroeconomic news shocks is negligible. Only 1.6% of the monthly variation in gasoline prices can be explained by all macroeconomic news shocks combined. Restricting ourselves to announcement dates, the combined R^2 of all forward looking variables is 2.28%. This is much larger than in the case of crude oil prices, but still represents only a small fraction of the month-to-month variability in gasoline prices. As in the case of the price of oil, these results are broadly supportive of the assumption of predetermined energy prices in applied and theoretical work on the transmission of energy price shocks.

Although the likely loss of power suggests caution in extending this analysis to the quarterly horizon, given the lack of feedback at the monthly horizon from news shocks about GDP to energy prices, our analysis suggests that the feedback from innovations to domestic macroeconomic variables such as U.S. GDP growth to the price of oil and the price of gasoline is likely to be weak at the quarterly frequency as well.

5. Related Literature

This is not the first paper to have studied the effect of news on oil prices. For example, Cavallo and Wu (2006) proposed two measures of exogenous oil price shocks based on market commentaries on daily oil-price fluctuations. Their intent was to identify shocks free of endogenous and anticipatory movements. They selected among all major daily oil price changes those that the authors classify as exogenous based on commentaries in two oil industry trade journals, the *Oil Daily* and the *Oil & Gas Journal*. There are several important differences between that paper and ours. First, given our focus on testing the assumption of predetermined energy prices, we focus on macroeconomic news announcements exclusively, whereas Cavallo and Wu focus mainly on supply-side shocks in the crude oil market.

Second, there are important differences in how the news shock is defined. Cavallo and Wu propose two measures of news shocks. Their first measure is the percent change in the one-month oil futures prices around the day of an exogenous event. Implicitly, Cavallo and Wu treat the change in oil futures prices as the change in expected oil prices associated with the news event in question. This approach is questionable since oil futures prices change every day, even in the absence of exogenous events. Moreover, the one-month futures price closely tracks the spot price of oil, so this measure essentially treats all oil price shifts on selected dates as exogenous news about oil prices.

Cavallo and Wu's second measure of news is the unexpected change in the spot price of oil as realized on the day immediately after an exogenous event, where the ex ante expectation is obtained from an oil futures spread regression. The latter news measure is problematic as well in that Alquist and Kilian (2008) have shown that oil futures prices are less accurate predictors of the spot price than simple no-change forecasts, casting doubt on the identification of the news component based on oil futures prices. In contrast, the news shocks in our paper have been identified using explicit measures of market expectations that have been widely used in related studies of asset markets, following the methodology of Andersen and Bollerslev (1998) and Andersen et al. (2003, 2007), among others. Moreover, our approach requires no judgment, and, rather than inferring news from the expectation of the price of oil, we directly measure the news component of macroeconomic announcements.

A third difference is that Cavallo and Wu aggregate their shocks to monthly frequency in order to estimate the responses of monthly U.S. macroeconomic aggregates to these shocks. This requires additional assumptions about the time aggregation of news shocks. In contrast, our analysis is based on daily data throughout, facilitating the measurement of news shocks as well as the estimation of their transmission to energy prices over the subsequent thirty days.

Our methodology is more closely related to Arseneau, Beechey and Vigfusson (2008) who investigate the effect of news on oil inventories on the price of oil. News in that context is measured as the difference between realized oil inventories and ex ante survey expectations of oil inventories. The key difference is that we are concerned with the effect of U.S. macroeconomic news rather than oil market news. This focus reflects our interest in testing the predeterminedness of oil prices with respect to U.S. macroeconomic aggregates rather than modeling the determination of oil prices more generally.

6. Concluding Remarks

Our analysis in this paper established that oil prices, unlike financial asset prices, do not respond instantaneously to domestic macroeconomic news. We showed that there is no evidence of such feedback in daily WTI oil price data for 1983-2008. We found that 99% of the variation in crude oil prices is left unexplained by domestic macroeconomic news. Similar results were obtained for U.S. gasoline prices using a much shorter sample. Again, there was no evidence of a statistically significant response to domestic macroeconomic news at daily horizons.

Our analysis also shed light on the validity of the commonly used identifying assumption that energy price shocks are predetermined with respect to domestic macroeconomic aggregates. Testing this assumption is complicated by the fact that exactly identifying assumptions are inherently untestable. We overcame that problem by estimating the response of daily WTI crude oil prices and U.S. gasoline prices at monthly horizons to U.S. macroeconomic news shocks. Since these shocks are exogenous by construction, we were able to estimate their effect on energy prices and to test for feedback from U.S. macroeconomic aggregates to energy prices within the month.

For a wide range of macroeconomic aggregates commonly used in studies of the transmission of energy price shocks (including U.S. real output and consumption, interest rates and inflation), we found no evidence of statistically significant feedback within thirty calendar days from exogenous macroeconomic news to the price of crude oil or the price of gasoline. The results most favorable to the hypothesis that there is feedback from the U.S. economy to energy prices within the month were not obtained with any of the macroeconomic aggregates used in the literature, but with selected forward-looking news variables such as the index of leading indicators. While none of the forward-looking news variables (including the index of leading indicators) were individually significant in predicting the price of crude oil at monthly horizons, a broader set of forward looking predictors was jointly statistically significant at the 5% level. Considering the low overall explanatory power of all news shocks combined of less than 1%, the extent of the feedback to the price of crude oil seems minimal, however. Similar, if somewhat stronger, evidence of feedback from forward-looking news variables was obtained for gasoline prices. The latter results are necessarily more tentative, given the much smaller sample size. In any case, the overall explanatory power of all macroeconomic news shocks combined for gasoline prices is below 2% at the monthly horizon, suggesting that the assumption of no contemporaneous feedback provides a good approximation at monthly frequency, even for gasoline prices.

We concluded that the widely used assumption that energy prices are predetermined at monthly frequency is broadly consistent with the data, lending support to empirical as well as theoretical models of the transmission of energy price shocks based on that assumption. At the same time, our results cast doubt on empirical work based on the alternative assumption that energy prices should be ordered below domestic macroeconomic aggregates in recursively identified VAR models.

References

Alquist, R., and L. Kilian (2008), "What Do We Learn from the Price of Crude Oil Futures?" mimeo, Department of Economics, University of Michigan.

Andersen, T.G., and T. Bollerslev (1998), "DM-Dollar Volatility: Intraday Activity Patterns, Macroeconomic Announcements, and Longer Run Dependencies," *Journal of Finance,* 53, 219-265.

Andersen, T.G., Bollerslev, T., Diebold, F.X., and C. Vega (2007), "Real-Time Price Discovery in Stock, Bond, and Foreign Exchange Markets," *Journal of International Economics*, 73, 251-277

Andersen, T.G., Bollerslev, T., Diebold, F.X., and C. Vega (2003), "Micro Effects of Macro Announcements: Real-Time Price Discovery in Foreign Exchange," *American Economic Review*, 93, 38-62.

Arsenau, D., M. Beechey, and R.J. Vigfusson, R.J. (2008), "The Response of Oil Prices to Inventory Surprises," mimeo, Federal Reserve Board.

Balke, N.S., Brown, S.P.A., and M.K. Yücel (2002), "Oil Price Shocks and the U.S. Economy: Where Does the Asymmetry Originate?" *Energy Journal*, 23, 27-52.

Barsky, R.B., and L. Kilian (2002), "Do We Really Know that Oil Caused the Great Stagflation? A Monetary Alternative," in: *NBER Macroeconomics Annual 2001*, B.S. Bernanke and K. Rogoff (eds.), MIT Press: Cambridge, MA, 137-183.

Barsky, R.B., and L. Kilian (2004), "Oil and the Macroeconomy Since the 1970s," *Journal of Economic Perspectives,* 18(4), 115-134.

Bernanke, B.S., and K.N. Kuttner (2005), "What Explains the Stock Market's Reaction to Federal Reserve Policy," *Journal of Finance,* 60, 1221-1257.

Blanchard, O.J., and J. Galí (2007), "The Macroeconomic Effects of Oil Shocks: Why are the 2000s so Different from the 1970s?" NBER Working Paper No. 13368.

Cavallo, M., and T. Wu (2006), "Measuring oil price shocks using market-based information," Working Paper 2006-28, Federal Reserve Bank of San Francisco.

Cooley, T.F., and S. LeRoy (1985), "Atheoretical Macroeconometrics: A Critique," *Journal of Monetary Economics*, 16, 283-308.

Cullen, J.B., L. Friedberg and C. Wolfram (2004), "Consumption and Changes in Home Energy Costs: How Prevalent is the 'Heat or Eat' Decision?", mimeo, Department of Economics, University of California at San Diego.

Dahl, C. A. (1979), “Consumer Adjustment to a Gasoline Tax,” *Review of Economics and Statistics*, 61, 427-432

Davis, L., and L. Kilian (2008), “Estimating the Effectiveness of a Carbon Tax on Gasoline,” mimeo, Department of Economics, University of Michigan.

Davis, S.J., and J. Haltiwanger (2001), “Sectoral Job Creation and Destruction Responses to Oil Price Changes,” *Journal of Monetary Economics*, 48, 465-512.

Denton, F. T. (1985), “Data Mining as an Industry,” *Review of Economics and Statistics*, 67, 124-127.

Edelstein, P., and L. Kilian (2008), “Retail Energy Prices and Consumer Expenditures,“ mimeo, Department of Economics, University of Michigan.

Faust, J., J. Rogers, S.-Y. Wang, and J. Wright (2007), "The High Frequency Response of Exchange Rates and Interest Rates to Macroeconomic Announcements," *Journal of Monetary Economics*, 54, 1051-1068.

Hamilton, J.D. (2003), “What is an Oil Shock?” *Journal of Econometrics*, 113, 363-398.

Hamilton, J.D. (2008), “Oil and the Macroeconomy,” forthcoming in: S. Durlauf and L. Blume (eds), The New Palgrave Dictionary of Economics, 2nd ed., Palgrave MacMillan Ltd.

Hamilton, J.D., and A.M. Herrera (2004), “Oil Shocks and Aggregate Economic Behavior: The Role of Monetary Policy: Comment,” *Journal of Money, Credit, and Banking*, 36, 265-286.

Herrera, A.-M., and E. Pesavento (2007), “Oil Price Shocks, Systematic Monetary Policy and the ‘Great Moderation’,” forthcoming: *Macroeconomic Dynamics*.

Herrera, A-M. (2007), “Inventories, Oil Shocks and Macroeconomic Behavior,” mimeo, Department of Economics, Michigan State University.

Hughes, J.E., C.R. Knittel, and D. Sperling (2008), “Evidence of a Shift in the Short-Run Price Elasticity of Gasoline Demand,” *Energy Journal*, 29, 113-134.

Inoue, A., and L. Kilian (2004), “In-Sample or Out-of-Sample Tests of Predictability: Which One Should We Use?” *Econometric Reviews*, 23, 371-402.

Kilian, L. (2008a), “The Economic Effects of Energy Price Shocks,” forthcoming: *Journal of Economic Literature.*

Kilian, L. (2008b), “Exogenous Oil Supply Shocks: How Big Are They and How Much Do They Matter for the U.S. Economy?” *Review of Economics and Statistics*, 90, 216-240.

Kilian, L. (2008c), “A Comparison of the Effects of Exogenous Oil Supply Shocks on Output and Inflation in the G7 Countries,” *Journal of the European Economic Association*, 6, 78-121.

Kilian, L. (2008d), “Not All Oil Price Shocks Are Alike: Disentangling Demand and Supply Shocks in the Crude Oil Market,” forthcoming: *American Economic Review*.

Kilian, L. (2008e), “Why Does Gasoline Cost so Much? A Joint Model of the Global Crude Oil Market and the U.S. Retail Gasoline Market,” mimeo, Department of Economics, University of Michigan.

Kilian, L., and C. Park (2008), “The Impact of Oil Price Shocks on the U.S. Stock Market,” forthcoming: *International Economic Review*.

Leduc, S., and K. Sill (2004), “A Quantitative Analysis of Oil-Price Shocks, Systematic Monetary Policy, and Economic Downturns,” *Journal of Monetary Economics*, 51, 781-808.

Lee, K., and S. Ni (2002), “On the Dynamic Effects of Oil Price Shocks: A Study Using Industry Level Data,” *Journal of Monetary Economics*, 49, 823-852.

Ramsey, J., Rasche, R., and B. Allen (1975), “An Analysis of the Private and Commercial Demand for Gasoline,” *Review of Economics and Statistics*, 57, 502-507.

Rotemberg, J., and M. Woodford (1996), “Imperfect Competition and the Effects of Energy Price Increases on Economic Activity,” *Journal of Money, Credit, and Banking*, 28, Part 1, 550-77.

Stock, J.H., J.H. Wright, and M. Yogo (2002), “A Survey of Weak Instruments and Weak Identification in Generalized Method of Moments,” *Journal of Business and Economic Statistics*, 20, 518-529.

Sullivan, R., Timmermann, A., White, H. (2001), “Dangers of Data Mining: The Case of Calendar Effects in Stock Returns,” *Journal Econometrics*, 105, 249-286.

White, H. (2000), “A Reality Check for Data Snooping,” *Econometrica*, 68, 1097-1126.

Table 1. U.S. News Announcements

Announcement	Obs.[1]	Source[2]	Dates[3]	Release Time[4]	Std. Dev.[5]
Quarterly Announcements					
1. GDP Advance	83	BEA	4/1987-4/2008[6]	8:30	0.771
2. GDP Preliminary	82	BEA	4/1987-4/2008[7]	8:30	0.418
3. GDP Final	83	BEA	4/1987-4/2008[8]	8:30	0.310
Monthly Announcements					
Real Activity					
4. Unemployment Rate	304	BLS	1/1983-4/2008[9]	8:30	0.156
5. Nonfarm Payroll Employment	279	BLS	2/1985-4/2008[10]	8:30	111.153
6. Retail Sales	258	BC	12/1986-4/2008	8:30	0.604
7. Industrial Production	257	FRB	12/1986-4/2008	9:15	0.273
8. Capacity Utilization	240	FRB	4/1988-4/2008[11]	9:15	0.320
9. Personal Income	253	BEA	12/1986-4/2008[12]	10:00/8:30[13]	0.252
10. Consumer Credit	241	FRB	4/1988-4/2008[14]	15:00[15]	4.243
Consumption					
11. New Home Sales	239	BEA	3/1988-4/2008[16]	10:00/8:30	62.946
12. Personal Consumption Exp.	256	BC	12/1986-4/2008[17]	10:00[18]	0.208
Investment					
13. Durable Goods Orders	299	BC	4/1983-4/2008[19]	8:30/9:00/10:00[20]	2.906
14. Construction Spending	240	BC	4/1988-4/2008[21]	10:00	1.007
15. Factory Orders	240	BC	3/1988-4/2008[22]	10:00	0.714
16. Business Inventories	240	BC	4/1988-4/2008[23]	10:00/8:30[24]	0.273
Fiscal Balance					
17. Net government purchases	236	FMS	4/1988-4/2008[25]	14:00	8.646
Net Exports					
18. Trade Balance	256	BEA	12/1986-4/2008[26]	8:30	2.337
Prices					
19. Producer Price Index	257	BLS	12/1986-4/2008	8:30	0.399
20. Core PPI	195	BLS	1/1992-4/2008[27]	8:30	0.266
21. Consumer Price Index	304	BLS	1/1983-4/2008	8:30	0.127
22. Core CPI	195	BLS	1/1992-4/2008[28]	8:30	0.214
Forward-Looking					
23. Michigan CCI Preliminary	110	UM	5/1999-7/2008[29]	10:00	10.677
24. Michigan CCI Final	110	UM	5/1999-6/2008	10:00	10.843
25. Board CCI Index	200	CB	7/1991-4/2008	8:30	4.960
26. NAPM Index	220	NAPM	2/1990-4/2008	10:00	2.008
27. Housing Starts	303	BC	1/1983-4/2008[30]	8:30	0.135
28. Index of Leading Indicators	304	CB	1/1983-4/2008	8:30	0.243
Six-Week Announcements					
FOMC					
29. Target Federal Funds Rate	185	FRB	1/1983-4/2008	14:15[31]	0.089
Weekly Announcements					
30. Initial Unemployment Claims	870	ETA	7/1991-4/2008	8:30	12.996

Notes to Table 1: We partition the U.S. monthly news announcements into seven groups: aggregate real activity, the GDP components (consumption, investment, fiscal balance and net exports), prices, and forward-looking. Within each group, we list U.S. news announcements in chronological order of their release. CCI denotes the consumer confidence index.

1. Total number of observations in our announcements and expectations data sample.
2. Bureau of Labor Statistics (BLS), Bureau of the Census (BC), Bureau of Economic Analysis (BEA), Federal Reserve Board (FRB), National Association of Purchasing Managers (NAPM), Conference Board (CB), Financial Management Office (FMO), Employment and Training Administration (ETA), University of Michigan (UM).
3. Starting and ending dates of our announcements and expectations data sample.
4. Eastern Standard Time. Daylight savings time starts on the first Sunday of April and ends on the last Sunday of October.
5. Standard deviation of the macroeconomic news surprise before we standardize it.
6. 7/87 and 1/88 are missing observations.
7. 11/87 and 11/95 are missing observations.
8. 12/87 is a missing observation.
9. 7/93 is a missing observation.
10. 4/85 and 10/98 are missing observations.
11. 11/03 is a missing observation.
12. 11/95, 2/96, 3/97, and 12/07 are missing observations.
13. In 01/94, the personal income announcement time moved from 10:00 EST to 8:30 EST.
14. 11/03 is a missing observation.
15. Beginning in 01/96, consumer credit was released regularly at 15:00 EST. Prior to this date the release times varied.
16. 4/88, 1/89, and 12/95 are missing observations.
17. 11/95 and 2/96 are missing observation.
18. In 12/93, the personal consumption expenditures announcement time moved from 10:00 EST to 8:30 EST.
19. 12/95 and 1/96 are missing observations.
20. Whenever GDP is released on the same day as durable goods orders, the durable goods orders announcement is moved to 10:00 EST. On 07/96 the durable goods orders announcement was released at 9:00 EST.
21. 1/96, 10/98, 12/03, and 12/07 are missing observations.
22. 1/96 and 11/03 are missing observations.
23. 11/03 is a missing observation.
24. In 01/97, the business inventory announcement was moved from 10:00 EST to 8:30 EST.
25. 5/88, 6/88, 11/89, 12/89, 1/90, and 1/96 are missing observations.
26. 3/87 is a missing observation.
27. 11/92 is a missing observation.
28. 11/92 and 12/98 are missing observations.
29. 7/99 is a missing observation.
30. 12/95 is a missing observation.
31. Beginning in 3/28/94, the fed funds rate was released regularly at 14:15 EST. Prior to this date the release times varied.

Table 2a. Daily WTI Crude Oil Prices: Individual Regressions for 1983 – 2008

Announcement	$\hat{\beta}_i$	$\hat{t}_i$	Standard *p*-value	Robust *p*-value	R^2 Percent	Obs.	Alternative Hypothesis
GDP Advanced	-0.224	-1.08	0.86	1.00	0.89	83	$H_1 : \beta_i > 0$
GDP Preliminary	0.348	1.54	**0.06**	0.86	3.22	82	$H_1 : \beta_i > 0$
GDP Final	0.166	0.50	0.31	1.00	0.46	82	$H_1 : \beta_i > 0$
Unemployment Rate	-0.087	-0.63	0.27	1.00	0.19	288	$H_1 : \beta_i < 0$
Nonfarm Payroll	0.027	0.19	0.42	1.00	0.02	268	$H_1 : \beta_i > 0$
Retail Sales	-0.276	-1.06	0.86	1.00	1.86	257	$H_1 : \beta_i > 0$
Industrial Production	0.011	0.08	0.47	1.00	0.00	255	$H_1 : \beta_i > 0$
Capacity Utilization	0.056	0.38	0.35	1.00	0.07	238	$H_1 : \beta_i > 0$
Personal Income	-0.120	-0.82	0.79	1.00	0.23	247	$H_1 : \beta_i > 0$
Consumer Credit	0.057	0.49	0.31	1.00	0.10	238	$H_1 : \beta_i > 0$
New Home Sales	0.202	1.36	**0.09**	0.94	0.93	237	$H_1 : \beta_i > 0$
Personal Consumption	-0.116	-0.51	0.70	1.00	0.23	249	$H_1 : \beta_i > 0$
Durable Goods Orders	-0.102	-0.75	0.77	1.00	0.20	298	$H_1 : \beta_i > 0$
Construction Spending	0.005	0.04	0.49	1.00	0.00	237	$H_1 : \beta_i > 0$
Factory Orders	-0.008	-0.04	0.52	1.00	0.00	239	$H_1 : \beta_i > 0$
Business Inventories	-0.035	-0.20	0.42	1.00	0.03	238	$H_1 : \beta_i < 0$
Government Budget Deficit	0.329	2.40	**0.01**	0.23	1.37	232	$H_1 : \beta_i > 0$
Trade Balance	-0.026	-0.19	0.57	1.00	0.01	255	$H_1 : \beta_i > 0$
PPI	-0.205	-1.66	0.95	1.00	1.04	257	$H_1 : \beta_i > 0$
Core PPI	-0.09	-0.62	0.73	1.00	0.19	195	$H_1 : \beta_i > 0$
CPI	-0.045	-0.30	0.62	1.00	0.03	299	$H_1 : \beta_i > 0$
Core CPI	0.190	2.39	**0.01**	0.24	0.87	194	$H_1 : \beta_i > 0$
CCI Preliminary (Michigan)	0.246	1.07	0.14	0.99	1.34	107	$H_1 : \beta_i > 0$
CCI Final (Michigan)	0.061	0.41	0.34	1.00	0.12	108	$H_1 : \beta_i > 0$
CCI (Board)	0.181	1.36	**0.09**	0.94	0.77	199	$H_1 : \beta_i > 0$
NAPM Index	-0.017	-0.11	0.55	1.00	0.00	218	$H_1 : \beta_i > 0$
Housing Starts	0.243	2.17	**0.02**	0.38	0.55	298	$H_1 : \beta_i > 0$
Index of Leading Indicators	-0.053	-0.27	0.61	1.00	0.03	298	$H_1 : \beta_i > 0$
Target Rate Surprises	0.112	0.67	0.75	1.00	0.20	185	$H_1 : \beta_i < 0$
Initial Claims	0.038	0.50	0.69	1.00	0.03	869	$H_1 : \beta_i < 0$

NOTES: All regressions include a constant. Data mining robust *p*-values were computed based a parametric bootstrap approach under the null hypothesis of no predictability. Standard *p*-values based on N(0,1) distribution. Boldface indicates statistical significance at 10% level.

Table 2b. Daily WTI Crude Oil Prices: Joint Regression for 1983 – 2008

Announcement	$\hat{\beta}_i$	$\hat{t}_i$	Standard *p*-value	Robust *p*-value	Alternative Hypothesis	Obs.	R^2 Percent
GDP Advanced	-0.225	-0.85	0.80	1.00	$H_1:\beta_i>0$	6214	0.38
GDP Preliminary	0.332	1.25	0.11	0.96	$H_1:\beta_i>0$		
GDP Final	0.117	0.43	0.33	1.00	$H_1:\beta_i>0$		
Unemployment Rate	-0.099	-0.69	0.24	1.00	$H_1:\beta_i<0$		
Nonfarm Payroll	0.048	0.33	0.37	1.00	$H_1:\beta_i>0$		
Retail Sales	-0.266	-1.76	0.96	1.00	$H_1:\beta_i>0$		
Industrial Production	-0.056	-0.27	0.61	1.00	$H_1:\beta_i>0$		
Capacity Utilization	0.098	0.46	0.32	1.00	$H_1:\beta_i>0$		
Personal Income	-0.114	-0.73	0.77	1.00	$H_1:\beta_i>0$		
Consumer Credit	0.051	0.32	0.37	1.00	$H_1:\beta_i>0$		
New Home Sales	0.199	1.28	0.10	0.96	$H_1:\beta_i>0$		
Personal Consumption	-0.103	-0.68	0.75	1.00	$H_1:\beta_i>0$		
Durable Goods Orders	-0.104	-0.74	0.77	1.00	$H_1:\beta_i>0$		
Construction Spending	-0.000	0.00	0.50	1.00	$H_1:\beta_i>0$		
Factory Orders	-0.003	-0.02	0.51	1.00	$H_1:\beta_i>0$		
Business Inventories	-0.004	-0.03	0.49	1.00	$H_1:\beta_i<0$		
Government Budget Deficit	0.321	2.02	**0.02**	0.48	$H_1:\beta_i>0$		
Trade Balance	-0.009	-0.06	0.52	1.00	$H_1:\beta_i>0$		
PPI	-0.190	-1.10	0.86	1.00	$H_1:\beta_i>0$		
Core PPI	-0.004	-0.02	0.51	1.00	$H_1:\beta_i>0$		
CPI	-0.098	-0.59	0.72	1.00	$H_1:\beta_i>0$		
Core CPI	0.234	1.32	**0.09**	0.94	$H_1:\beta_i>0$		
CCI Preliminary (Michigan)	0.162	0.70	0.24	1.00	$H_1:\beta_i>0$		
CCI Final (Michigan)	0.088	0.38	0.35	1.00	$H_1:\beta_i>0$		
CCI (Board)	0.172	1.00	0.16	0.99	$H_1:\beta_i>0$		
NAPM Index	0.029	0.18	0.43	1.00	$H_1:\beta_i>0$		
Housing Starts	0.250	1.79	**0.04**	0.68	$H_1:\beta_i>0$		
Index of Leading Indicators	0.028	0.14	0.44	1.00	$H_1:\beta_i>0$		
Target Rate Surprises	0.126	0.73	0.77	1.00	$H_1:\beta_i<0$		
Initial Claims	0.040	0.49	0.69	1.00	$H_1:\beta_i<0$		

NOTES: The regression includes a constant. Data mining robust *p*-values were computed based a parametric bootstrap approach under the null hypothesis of no predictability. Standard *p*-values based on N(0,1) distribution. Boldface indicates statistical significance at 10% level.

Table 3a. Daily U.S. Gasoline Prices: Individual Regressions for 2003 – 2008

Announcement	$\hat{\beta}_i$	$\hat{t}_i$	Standard *p*-value	Robust *p*-value	R^2 Percent	Obs.	Alternative Hypothesis
GDP Advanced	0.024	0.33	0.37	1.00	0.25	21	$H_1 : \beta_i > 0$
GDP Preliminary	-0.039	-0.18	0.57	1.00	0.04	21	$H_1 : \beta_i > 0$
GDP Final	0.073	0.51	0.31	1.00	1.38	21	$H_1 : \beta_i > 0$
Unemployment Rate	-0.096	-0.74	0.23	1.00	1.05	64	$H_1 : \beta_i < 0$
Nonfarm Payroll	0.002	0.02	0.49	1.00	0.00	63	$H_1 : \beta_i > 0$
Retail Sales	-0.079	-1.16	0.88	1.00	2.02	64	$H_1 : \beta_i > 0$
Industrial Production	0.076	1.49	**0.07**	0.89	3.53	64	$H_1 : \beta_i > 0$
Capacity Utilization	-0.016	-0.23	0.59	1.00	0.10	63	$H_1 : \beta_i > 0$
Personal Income	-0.135	-1.02	0.84	1.00	2.51	62	$H_1 : \beta_i > 0$
Consumer Credit	-0.052	-1.00	0.84	1.00	1.40	63	$H_1 : \beta_i > 0$
New Home Sales	-0.041	-1.13	0.87	1.00	2.11	64	$H_1 : \beta_i > 0$
Personal Consumption	0.023	0.34	0.37	1.00	0.06	63	$H_1 : \beta_i > 0$
Durable Goods Orders	-0.010	-0.14	0.56	1.00	0.03	64	$H_1 : \beta_i > 0$
Construction Spending	-0.063	-0.54	0.71	1.00	0.27	63	$H_1 : \beta_i > 0$
Factory Orders	0.105	1.35	**0.09**	0.94	2.68	63	$H_1 : \beta_i > 0$
Business Inventories	0.029	0.29	0.61	1.00	0.17	63	$H_1 : \beta_i < 0$
Government Budget Deficit	-0.058	-0.88	0.81	1.00	1.30	63	$H_1 : \beta_i > 0$
Trade Balance	0.002	0.05	0.48	1.00	0.00	64	$H_1 : \beta_i > 0$
PPI	0.018	0.66	0.25	1.00	0.38	64	$H_1 : \beta_i > 0$
Core PPI	0.053	1.51	**0.07**	0.88	2.27	64	$H_1 : \beta_i > 0$
CPI	-0.126	-2.28	0.99	1.00	5.05	64	$H_1 : \beta_i > 0$
Core CPI	-0.003	-0.18	0.57	1.00	0.01	64	$H_1 : \beta_i > 0$
CCI Preliminary (Michigan)	-0.099	-1.64	0.95	1.00	3.56	64	$H_1 : \beta_i > 0$
CCI Final (Michigan)	-0.031	-0.60	0.73	1.00	0.52	64	$H_1 : \beta_i > 0$
CCI (Board)	-0.005	-0.06	0.53	1.00	0.01	60	$H_1 : \beta_i > 0$
NAPM Index	-0.187	-1.25	0.89	1.00	5.68	63	$H_1 : \beta_i > 0$
Housing Starts	0.008	0.13	0.45	1.00	0.02	64	$H_1 : \beta_i > 0$
Index of Leading Indicators	-0.036	-0.26	0.60	1.00	0.09	64	$H_1 : \beta_i > 0$
Target Rate Surprises	-0.018	-0.57	0.28	1.00	0.14	43	$H_1 : \beta_i < 0$
Initial Claims	-0.022	-0.54	0.29	1.00	0.12	277	$H_1 : \beta_i < 0$

NOTES: All regressions include a constant. Data mining robust *p*-values were computed based a parametric bootstrap approach under the null hypothesis of no predictability. Standard *p*-values based on N(0,1) distribution. Boldface indicates statistical significance at 10% level.

Table 3b. Daily U.S. Gasoline Prices: Joint Regression for 2003 – 2008

Announcement	$\hat{\beta}_i$	$\hat{t}_i$	Standard *p*-value	Robust *p*-value	Alternative Hypothesis	Obs.	R^2 Percent
GDP Advanced	0.003	0.02	0.49	1.00	$H_1:\beta_i>0$	1385	1.91
GDP Preliminary	-0.042	-0.22	0.59	1.00	$H_1:\beta_i>0$		
GDP Final	-0.007	-0.06	0.52	1.00	$H_1:\beta_i>0$		
Unemployment Rate	-0.123	-1.07	0.14	0.99	$H_1:\beta_i<0$		
Nonfarm Payroll	-0.023	-0.22	0.59	1.00	$H_1:\beta_i>0$		
Retail Sales	-0.102	-1.22	0.89	1.00	$H_1:\beta_i>0$		
Industrial Production	0.190	2.02	**0.02**	0.48	$H_1:\beta_i>0$		
Capacity Utilization	-0.184	-1.63	0.95	1.00	$H_1:\beta_i>0$		
Personal Income	-0.125	-1.62	0.95	1.00	$H_1:\beta_i>0$		
Consumer Credit	-0.046	-0.78	0.78	1.00	$H_1:\beta_i>0$		
New Home Sales	-0.047	-0.83	0.80	1.00	$H_1:\beta_i>0$		
Personal Consumption	0.050	0.57	0.28	1.00	$H_1:\beta_i>0$		
Durable Goods Orders	-0.013	-0.14	0.56	1.00	$H_1:\beta_i>0$		
Construction Spending	-0.068	-0.58	0.72	1.00	$H_1:\beta_i>0$		
Factory Orders	0.112	1.46	**0.07**	0.89	$H_1:\beta_i>0$		
Business Inventories	0.023	0.23	0.59	1.00	$H_1:\beta_i<0$		
Government Budget Deficit	-0.071	-0.85	0.80	1.00	$H_1:\beta_i>0$		
Trade Balance	-0.009	-0.15	0.56	1.00	$H_1:\beta_i>0$		
PPI	-0.002	-0.04	0.52	1.00	$H_1:\beta_i>0$		
Core PPI	0.068	0.98	0.16	1.00	$H_1:\beta_i>0$		
CPI	-0.137	-1.68	0.95	1.00	$H_1:\beta_i>0$		
Core CPI	0.010	0.21	0.42	1.00	$H_1:\beta_i>0$		
CCI Preliminary (Michigan)	-0.106	-1.47	0.93	1.00	$H_1:\beta_i>0$		
CCI Final (Michigan)	-0.023	-0.34	0.63	1.00	$H_1:\beta_i>0$		
CCI (Board)	0.006	0.07	0.47	1.00	$H_1:\beta_i>0$		
NAPM Index	-0.169	-2.39	0.99	1.00	$H_1:\beta_i>0$		
Housing Starts	-0.005	-0.05	0.52	1.00	$H_1:\beta_i>0$		
Index of Leading Indicators	-0.042	-0.27	0.61	1.00	$H_1:\beta_i>0$		
Target Rate Surprises	-0.013	-0.13	0.45	1.00	$H_1:\beta_i<0$		
Initial Claims	-0.017	-0.43	0.33	1.00	$H_1:\beta_i<0$		

NOTES: The regression includes a constant. Data mining robust *p*-values were computed based a parametric bootstrap approach under the null hypothesis of no predictability. Standard *p*-values based on N(0,1) distribution. Boldface indicates statistical significance at 10% level.

Table 4. Monthly WTI Crude Oil Prices: Regressions for 1983-2008

Announcement	Individual Regression		Joint Regression		
	Standard *p*-value	Robust *p*-value	Standard *p*-value	Robust *p*-value	Alternative Hypothesis
GDP Advanced	0.97	1.00	0.97	1.00	$H_1 : \beta_i > 0$
GDP Preliminary	0.16	1.00	0.17	1.00	$H_1 : \beta_i > 0$
GDP Final	**0.10**	0.95	0.12	0.98	$H_1 : \beta_i > 0$
Unemployment Rate	0.21	1.00	0.18	1.00	$H_1 : \beta_i < 0$
Nonfarm Payroll	0.55	1.00	0.43	1.00	$H_1 : \beta_i > 0$
Retail Sales	0.63	1.00	0.53	1.00	$H_1 : \beta_i > 0$
Industrial Production	0.29	1.00	0.81	1.00	$H_1 : \beta_i > 0$
Capacity Utilization	**0.05**	0.79	**0.04**	0.72	$H_1 : \beta_i > 0$
Personal Income	0.40	1.00	0.15	0.99	$H_1 : \beta_i > 0$
Consumer Credit	0.69	1.00	0.67	1.00	$H_1 : \beta_i > 0$
New Home Sales	0.38	1.00	0.46	1.00	$H_1 : \beta_i > 0$
Personal Consumption	0.68	1.00	0.66	1.00	$H_1 : \beta_i > 0$
Durable Goods Orders	0.17	1.00	0.23	1.00	$H_1 : \beta_i > 0$
Construction Spending	0.36	1.00	0.33	1.00	$H_1 : \beta_i > 0$
Factory Orders	0.28	1.00	0.27	1.00	$H_1 : \beta_i > 0$
Business Inventories	0.15	0.99	0.17	1.00	$H_1 : \beta_i < 0$
Government Budget Deficit	**0.03**	0.64	**0.04**	0.74	$H_1 : \beta_i > 0$
Trade Balance	**0.10**	0.96	0.13	0.98	$H_1 : \beta_i > 0$
PPI	0.98	1.00	0.93	1.00	$H_1 : \beta_i > 0$
Core PPI	0.83	1.00	0.48	1.00	$H_1 : \beta_i > 0$
CPI	0.42	1.00	0.41	1.00	$H_1 : \beta_i > 0$
Core CPI	0.37	1.00	0.64	1.00	$H_1 : \beta_i > 0$
CCI Preliminary (Michigan)	**0.01**	0.21	**0.02**	0.39	$H_1 : \beta_i > 0$
CCI Final (Michigan)	0.45	1.00	0.41	1.00	$H_1 : \beta_i > 0$
CCI (Board)	**0.01**	0.18	**0.01**	0.26	$H_1 : \beta_i > 0$
NAPM Index	0.42	1.00	0.47	1.00	$H_1 : \beta_i > 0$
Housing Starts	0.82	1.00	0.78	1.00	$H_1 : \beta_i > 0$
Index of Leading Indicators	**0.01**	0.25	**0.01**	0.17	$H_1 : \beta_i > 0$
Target Rate Surprises	0.63	1.00	0.60	1.00	$H_1 : \beta_i < 0$
Initial Claims	0.41	1.00	0.44	1.00	$H_1 : \beta_i < 0$

NOTES: See Tables 2a and 2b. The R^2 of the joint regression is 0.69%.

Table 5. Monthly WTI Crude Oil Prices: Joint Significance Tests for 1983-2008

Announcement	Joint Regression	
	Wald Test Statistic	Standard p-value
Aggregate real activity $i = 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 30$	12.98	0.29
Aggregate and disaggregate real activity $i = 1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 18, 30$	21.09	0.33
Inflation $i = 19, 20, 21, 22$	2.76	0.60
Forward-looking variables $i = 23, 24, 25, 26, 27, 28$	13.62	**0.03**

NOTES: See Table 2b. The index i refers to the news shocks in the order listed in Table 2b.