Comment on Blanchard-Gali: The Macroeconomic Effects of Oil Price Shocks: Why are the 2000s so different from the 1970's

Julio J. Rotemberg

August 31, 2007

This paper's primary empirical finding is that the effect of oil price increases on output and inflation appears to be more muted after 1984 than it was in the post-war period up to 1984. As it happens, a small response of the economy to oil price increases is more consistent with standard macroeconomic models. Thus, I fear that this paper will be taken to to be an argument for not studying the issue of the effects of oil price increases anymore on the ground that unusual responses to these shocks are now of interest only to antiquarians. My own view, by contrast, is that the study of the effects of oil price increases remains important because it is hard to deny that at least some oil price increases were exogenous with respect to developments in the US economy. Given the impossibility of experimenting with the economy as a whole, the effect of oil price increases thus provides a precious window into the operation of the economy as a whole. This window is all the more precious in that the response of the economy was, in the past, fairly puzzling. It thus seems particularly important to try to ascertain what led to these responses and what the conditions are under which economies could be subject to these problematic responses in the future.

The paper offers three basic stories for the decline in the response to the price of oil, which can be read as three features of the economy which led past responses to be large. These are 1) that "real wage rigidity" was more important in the past than it is today, 2) that "monetary policy credibility" was weaker in the past than it is today and 3) that the share of energy in the economy was larger in the past than it is today. The message of this paper is thus optimistic in that it suggest that institutional changes in the US economy have inoculated the economy against the responses that we saw in the past.

In this discussion, I articulate my doubts about all three mechanisms introduced by Blanchard and Gali. First, I see the "real wage rigidity" emphasized in this paper as being of relatively limited use in explaining the large responses observed in the past. Moreover, I see very little evidence that such rigidities have become less prevalent more recently, as required for this story to explain the reduction in the importance of oil shocks. Second, the model of low credibility of central banks that the authors introduce has counterfactual implications for the response of real interest rates to oil price shocks, so it does not seem compelling as an explanation of past responses. Third, while Blanchard and Gali are correct that the ratio of the value of energy inputs to the value of GDP has fluctuated over time, many of these fluctuations are inconsistent with their model. It is thus difficult to know how the shares that appear in their model should be calibrated. After devoting three sections to spelling out my concerns with the three mechanisms spelled out by Blanchard and Gali, my comment closes with some alternative interpretations for the reduction in the measured effect of oil price increases on the economy.

1 The importance of reductions in real wage rigidity

Blanchard and Gali suppose that, if there were no real wage rigidities, households would be indifferent between consuming one additional hour of leisure and working this extra hour and consuming the proceeds. With log utility for consumption and a Frisch labor supply of 1, this would imply that the log of real wages is equal to the sum of the log of employment plus the log of consumption. As a result the wage should rise by considerably more than 1 percent every time that GDP rises by one percent. In practice, real wages are only slightly procyclical.[1]

It thus seems difficult to disagree with the notion that this model is inaccurate as a description of labor market equilibrium. Moreover, the equation that Blanchard and Gali

[1]My interpretation of the relevant empirical findings can be found in Rotemberg (2007).

propose to use instead is a model of simplicity. They suppose that

$$w_t - p_{c,t} = (1 - \gamma)(c_t + n_t)$$

where w_t, $p_{c,t}$, c_t, and n_t represent respectively the nominal wage, the consumer price index, consumption, and employment at t. What seems much more problematic to me is the claim that the parameter γ that represents real wage rigidity has fallen in the United States. Blanchard and Gali defend this claim by noting that real wage declines after energy price increases were similar in the pre- and post-1984 period while the reductions in $(c_t + n_t)$ were much larger in the earlier period. They interpret this as suggesting that wages are now more (procyclically) sensitive to employment and consumption so that γ is smaller.

While this does constitute a possible interpretation of the change in impulse responses, it is by no means the only one. An obvious alternative is that firms are able to reduce consumption wages when oil prices rise for reasons having nothing to do with this model of wage determination while the response of wages to employment and consumption has remained unchanged. To see which of these alternatives is more valid one could look at other shocks that move consumption and employment and study whether these now have larger procyclical effects on wages.

A crude way of doing this is to run a regression of $(w_t - p_{c,t} - c_t - n_t)$ on $(c_t + n_t)$. According to the Blanchard-Gali interpretation, this coefficient should be more negative in the earlier period when wages were unresponsive to $(c_t + n_t)$ while it should be closer to zero in the more recent one. Using that data that Blanchard and Gali have graciously provided, the earlier period yields a coefficient of $-.917$ while the latter yields -1.057. Thus, by this metric, real wages have become slightly less procyclical and γ has risen over time. This is precisely contrary to the Blanchard-Gali conclusion that "the response of the consumption wage to the marginal rate of substitution ... appears to have increased over time."

In any event, real wage rigidity (and its changes) cannot be the whole story even though the idea that the failure of wages to adjust played an important role in the recessions induced by oil price increases has a distinguished history. As Bruno and Sachs (1985) pointed out, oil

price increases necessitate a reduction in real wages because they lead workers to produce a lower value of "net output." Thus, an unwillingness of workers to reduce their wages would lead firms to curtail their employment. As Rotemberg and Woodford (1996) have stressed, however, the amount by which wages must fall for firms to keep their employment unchanged is extremely small.

To see this, follow Rotemberg and Woodford and consider a general constant returns to scale production function that relates output Y_t to value added $V(H_t)$ and energy inputs E_t, where H_t is the volume of employment

$$Y_t = Q(V(H_t), E_t)$$

Constant returns them implies that

$$Y = Q_{Vt}V_t + Q_{Et}E_t$$

The first order conditions for profit maximization are

$$Q_{Et} = \mu_t p_{Et} \qquad \text{and} \qquad Q_{Vt}V_H(H_t) = \mu_t W_t \tag{1}$$

where μ_t is the markup of price over marginal cost, p_{Et} is the price of energy relative to the price of final output and W_t is the real wage in terms of final output. The left hand side of the second of these equations falls with employment. Thus this equation says that employment will fall if either the real wage or the markup rise.

These equations can be used to study the extent to which the real wage W_t needs to fall to keep employment constant. One way of demonstrating that this change is quite small is to focus on the wage deflated by the value added deflator defined as

$$P_{Vt} \equiv \frac{Y_t - P_{Et}E_t}{V(H_t)}$$

With perfect competition, $\mu_t = 1$ so the equations above imply that $P_{Vt} = Q_{Vt}$. Thus, the second equation in (1) becomes

$$V_H(H_t) = \frac{W_t}{P_{Vt}}$$

This says that employment would stay constant as long as the wage deflated by the value added deflator stayed constant as well. The wage deflated by the price of total output does indeed have to decline, but a one percent increase in the price of energy requires a percentage decrease in this wage that equals only the share of the value of energy in the value of output. Since this is about 3 percent, the amount by which the wage must decline is trivial.

What is more, both the impulse responses in Rotemberg and Woodford (1996) and those in the current paper show that the wage deflated by the value added deflator actually fell after the pre-1984 energy price increases. Thus, the resulting decline in employment and output is inexplicable unless markups rose. Even if they implicitly recognize that markup increases played a central role, Blanchard and Gali argue that employment would have declined by less if real wages had declined further and this is the mechanism by which real wage rigidity plays a role in their model. In effect, Blanchard and Gali are suggesting that, with more flexible wages, firm's marginal costs would have declined further and employment declines would have been smaller.

We do not know, of course what would have happened if real wages had declined in the past. It is worth stressing, however, that the recessions induced by oil price increases may well have been accompanied by much larger reductions in marginal cost than are recognized by Blanchard and Gali. The authors use a production function where the marginal product of labor does not decline very much when employment declines. If, however, some of the labor firms hired were overhead labor, or if labor effort by those that remained employed declined because firms were hoarding labor, marginal cost might well have fallen much further even without declines in real wages.

A second reason to doubt that real wages played much of a role in the pre-1984 recessions is that it appears that wages declines were much larger in some countries than in others. French wages, for example, seem to have increased in the impulse response functions reported in this paper. Yet, the output decline in France does not appear to have been significantly larger than the output decline in the United States.

2 Blanchard and Gali's model of markup increases: non-credible monetary policy

As discussed above, an explanation of why output fell more after the pre-1984 oil price increases is likely to be incomplete without a theory of why these earlier shocks led to larger increases in markups. This paper's theory of this involves a combination of sticky prices and an ingenious model of monetary policy failure.

The idea is that monetary policy was set by having

$$i_t = \phi \pi_t$$

where i_t and π_t represent the nominal interest rate and inflation at t respectively and where $\phi > 1$. Price setters, on the other hand, believed that the interest rate was set according to

$$i_t = \phi(1 - \delta)\pi_t + v_t$$

where $\delta > 0$ and v_t was believed to be i.i.d. Since price setters correctly perceived both current interest rates and current inflation, they used their beliefs regarding $\phi(1 - \delta)$ to compute v_t, with the result that this was equal to $\phi\delta\pi_t$. So, they correctly perceived that increases in inflation led to increases in interest rates, but were wrong in thinking that these increases were random i.i.d. disturbances rather than systematic responses aimed at curbing inflation.

For the parameters used in this paper the effect of $\delta > 0$ is to make firms raise their prices too much in response to oil price increases. Firms do so because they expect the monetary authority to conduct a fairly loose monetary policy in the future. The monetary authority, is, in fact, raising rates by enough to plunge the economy into recession by reducing aggregate demand. Firms nonetheless fail to be deterred from raising their prices substantially because of their false beliefs that future policy will be loose. Markups are high in the resulting a recession because of the expectation that the currently high interest rates will quickly be rescinded.

While providing a very elegant theory of stagflation, this model seems counterfactual both regarding the behavior of the *ex ante* real rate and of the "real rate" one obtains by subtracting the current inflation rate from the current interest rate. Because $\phi > 1$, the model predicts that this latter "real rate" should have been very high after an inflationary episode induced by an oil price increase. However, while pre-1984 oil shocks did raise inflation and lower output, they were associated with low rather than high values of $i_t - \pi_t$

Blanchard and Gali point out that surveys of expectations show that these shocks were associated with increases in expected inflation. This is consistent with the idea that people expected monetary policy to be relatively loose. The difficulty for the model is that monetary policy was in fact loose as well. Moreover, people seem to have noticed this in the sense that *ex ante* real rates constructed with their own inflation expectations were low. This seems impossible to square with the reductions in output that are observed (because aggregate demand should not have been low). Not being consistent with output declines, this behavior of *ex ante* rates is not consistent with markup increases either. Rotemberg and Woodford's (1996) theory, by contrast, is designed to offer an alternative reason that markups should have increased and thereby reduced output.

My view remains, as in Rotemberg (1983), that implicating tight monetary policy for the output declines that followed oil price increases is difficult. The reason is that oil price increases led to rises in inflation, and these rises in inflation suggest that monetary policy was too loose rather than being too tight. The idea that monetary policy was to blame for these recessions is not unique to Blanchard and Gali's paper, however. Barsky and Kilian (2002) also proposed a model which seeks to explain post-oil shock recessions in this way, though their model of price setting involves more inertial elements than those found in the current paper. Even so, the Barsky and Kilian (2002) model also has trouble explaining how output declines could have been so large in a period of negative real rates.

3 Changes in the share of energy

The last factor considered by Blanchard and Gali is that there has been a change in the economy's underlying structure that has induced a reduction in energy's importance. There is good prima facie evidence for this decline, with the US Energy Information Administration reporting a drop in the "energy intensity" of GDP from an index value of 100 in 1980 to a value of about 62 in 2000.[2] I am thus inclined to believe that this structural transformation could be an important factor. It turns out, however, that calibrating the importance of this factor is nontrivial.

To see this, recall that the Blanchard and Gali model supposes that utility depends on the log of consumption, which is in turn given by

$$c_t = (1 - \chi)c_{q,t} + \chi c_{m,t}$$

where c_t, $c_{q,t}$ and $c_{m,t}$ represent, respectively, the logarithms of total consumption, consumption of domestically produced goods and consumption of energy at t. The share parameter χ is then literally the ratio of the value of energy consumption over the value of total consumption. With these preferences, this share is constant and independent of the price of energy.

Similarly, Blanchard and Gali suppose that the production function for domestic output takes the Cobb-Douglas form

$$q_t = a_t + \alpha_n n_t + \alpha_m m_t$$

where q_t, n_t and m_t represent, respectively, the logarithm of domestically produced output, of employment and of the energy input at t. Here too, the technological parameter α_m ought to equal the ratio of the value of energy inputs to the value of domestic output and this ratio ought to be independent of the real price of energy.

As is well known and as the Blanchard-Gali calculations in the Appendix 2 make clear, increases in the price of energy raise the ratio of energy expenditures of households over

[2] See http://www.eia.doe.gov/oiaf/1605/gg05rpt/stopics.html

total expenditures as well as raising the ratio of the value of energy inputs over the value of produced output. This suggests that the elasticity of demand for energy is less than one and that the constant share models are not ideal. If, as is done in Blanchard and Gali, one calibrates the "constant shares" χ and α_n on the basis of particular yearly observations (they choose 1973 for the pre-1984 period and 1997 for the post-1984 period), the year that is chosen for this calibration is important. In future work aimed at obtaining more accurate estimates of the effect of structural changes in the economy that lead to lower energy intensity, it would be appealing to consider explicitly a model with less substitutability between energy and other goods. This would allow one to compute the extent to which changes in energy shares are due to changes in the price of oil and thereby yield estimates of the changes induced by the structural transformation of the economy.

4 Alternatives

Blanchard and Gali's question of why the output response to oil price increases dampened after 1984 is important, and is only made more urgent if one does not find all the explanations offered in this paper entirely convincing. One possible alternative explanation, stressed in Rotemberg and Woodford (1996) is that the earlier movements in the price of oil were exogenous to the behavior of the US economy (and induced either by the Texas Railroad Commission or by developments in the Middle East), the more recent ones were endogenous.

Blanchard and Gali argue that they are considering exogenous changes in the price of oil because they are letting oil be influenced contemporaneously by other variables in their VAR and treating the residuals as the oil price shock. Unfortunately, this technique is not compelling in the case of the price of a durable commodity such as oil. In a free market, the price of such commodities is strongly affected not only by variables that affect current GDP but also by expectations of future demand. Thus, the expectation that China will demand a great deal of oil in the future would drive up the price of oil today. Blanchard and Gali assert that, from the point of view of the United States, an oil price increase fueled by Chinese demand is equivalent to one fueled by a supply disruption. This is questionable, however.

Increases in Chinese demand can also lead to increases in the demand for US output in a way that oil supply disruptions need not. This expansionary effect of Chinese demand may then counteract the negative effect of oil price increases. This point is perhaps best understood at the world level, where an increase in the price of oil that is due to factors that raise world output is much less likely to lead to output reductions than an exogenous increase in the price of oil.

While separation of exogenous from endogenous oil price changes seems essential for progress on this issue, the task appears to be a difficult one. Kilian (2007) proposes a method for detecting the exogenous changes in the quantity of oil sold by particular countries (by comparing the output to the output of other countries that he deems to have been unaffected by the shocks that he focuses on). These exogenous changes turn out to be associated with only small subsequent movements in output and inflation both in the early period and in the Kuwait-Iraq War of 1990. While very attractive methodologically, this approach may well under-estimate the importance (and even the timing) of exogenous disturbances. Between September 1973 and January 1974, the official price of Saudi crude went from $2.59 per barrel to $11.65 per barrel. This seems like a good candidate for an exogenous shock, particularly since it was associated with dramatic meetings by OPEC ministers whose pronouncements made front page news on a daily basis. However, this quadrupling of the price of oil does not show up as an important quantity movement in Kilian's (2007) analysis. This is presumably because the elasticity of the demand for oil is so small that even dramatic price decisions by OPEC were not immediately associated with substantial quantity movements.

In addition to being much more likely to be endogenous, recent changes in the price of oil seem more likely to have been seen as transitory. It is apparent from Figure 1 in their paper that the price of oil experienced several transitory up-and-down movements between 1995 and 2001, and that these movements are of a different character than those that came before. Of course, it is still an open question whether a coherent model can be developed where transitory endogenous movements in the price of oil are less correlated with subsequent movements in output and inflation than are more permanent exogenous movements in the

price of oil. Given the importance of making sense of the pre-1984 correlations between oil prices and the economy, such an effort would seem very worthwhile.

References

Kilian, L. (2007): Exogenous Oil Supply Shocks: How big are they and how much do they matter for the U.S. Economy, *Review of Economics and Statistics,*

Rotemberg, J.J. (1983): Supply Shocks, Sticky Prices and Monetary Policy, *Journal of Money, Credit and Banking,* 15, 489–498.

— (2007): Cyclical Wages in a Search and Bargaining Model with Large Firms, *NBER International Seminar on Macroeconomics,* forthcoming.