Shocks:
A comment

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This paper presents a broad indictment of the state of our knowledge in macroeconomics. It argues that we do not know the source of most of the fluctuations in GNP. There is a sense in which the paper must be right as a description of our field as a whole. Our obvious lack of consensus on the sources of GNP fluctuations shows that, whether particular individuals know a lot about these issues or not, our collective knowledge is paltry. But Cochrane goes much farther. He argues that he can prove that the two principal suspects, namely technology and monetary shocks, cannot be very important. He then goes on to say that the shocks that really matter may be ones that we will never be able to identify with an observable exogenous variable because they simply represent “news” about the future that is reflected first in current endogenous variables.

In the process of making these points, Cochrane amasses a vast amount of evidence about the aggregate U.S. time series. I learned a great deal from this and expect others to do so as well. My discussion, however, will concern itself first with asking whether the evidence really supports the view that technology and monetary shocks must be unimportant. Then I will talk about the effect of the “news” shock that Cochrane proposes in this paper. Finally, I will close with a general discussion of what sort of model is needed to rationalize the relationship between variables such as $C/Y$ and subsequent movements in output.

There is a vast literature that uses aggregate U.S. data to study whether monetary disturbances have any effect on economic activity. This literature has long grappled with the issue of how to measure monetary disturbances. For a variety of reasons, innovations in standard monetary aggregates are unlikely to be perfect measures of such shocks. One reason for this is that the Fed accommodates changes in money demand. Another reason is that aggregates such as $M2$, whose components have very different time series
behavior and different rates of return, are likely to be very poor measures of the available level of liquidity. Thus, numerous alternatives to the use of innovations in monetary aggregates as measures of random changes in monetary policy have been suggested. In particular, investigators have searched extensively for measures of monetary disturbances that have “plausible” effects on output, interest rates, and prices.\textsuperscript{1} Showing that one can construct a measure of monetary disturbances, call it $n_t$, whose effects are plausible serves a very important role. In particular, it is probably essential if one wants to convince others that there exist true monetary disturbances, call them $m_t$, that matter at all. Without such a demonstration, skeptics are well-positioned to argue that the shocks that are being labeled as monetary really have quite different origins. The demonstration that one can construct a time series $n_t$ that has similar effects than those one would expect on a priori grounds from $m_t$ suggests both that $m_t$ matters and that $n_t$ is correlated with $m_t$.

Cochrane does require that his measures of monetary disturbances have plausible effects and thus the various measures of $n_t$ that he considers are likely to be correlated with $m_t$. What is much less clear to me is that he can use the effects of his constructed $n_t$’s on aggregate activity to obtain an upper bound for the role of monetary disturbances. The problem is that the insistence on plausible effects only implies that $n_t$ is likely to be correlated with $m_t$; it in no way implies that $n_t$ is equal to $m_t$. Thus $m_t$ might have significantly larger effects than $n_t$.

An obvious indication that the search for plausible responses does not immediately deliver the true $m_t$ is that several different specifications lead to plausible responses, even though these responses differ from one another. Thus the impulse response functions Cochrane obtains when he modifies his VAR in the way suggested by Gali (1992) (which involves ensuring that the long-run effects are zero) differ from those he obtains when he modifies it in the way proposed by Christiano, Eichenbaum and Evans (1993). Once it is recognized that none of these specifications is likely to recover the true $m_t$, one must accept that the fraction of the variance of output that is generated by variations in $m_t$ will remain unknown.

Moreover, the effect of monetary policy that are not captured by $n_t$ must be picked up by some other innovation in the VAR. It seems reasonable enough to suppose that consumption would pick up some of the effects of

\textsuperscript{1}Plausibility seems to hinge on having the impulse response trace out points on an unchanging money-demand function of the form

$$M/P = L(Y, i)$$

with $P$ adjusting slowly to ensure that there are no long-run effects on $Y$. If this is indeed the criterion, it might be good to check the degree to which responses fit this pattern.
unmeasured monetary policy. One reason is that, as Cochrane emphasizes, rational consumers would respond to the change in monetary policy as soon as it was seen. Another is that, because the effects of monetary disturbances are likely to be temporary, consumption would be different at the bottom of a recession induced by monetary policy than it would be if the same level of output was the result of a more permanent shock. Thus some of the variance that the VAR attributes to consumption innovations could easily be due to monetary policy.

The second part of the paper deals with technology shocks. I am obviously sympathetic to this part of the paper since my joint work with Michael Woodford, (Rotemberg and Woodford 1994), is used to bolster the case that technology shocks must have a small role in practice. Our work has certainly convinced me that the standard one-sector growth model with random walk disturbances cannot generate the sort of predictable movements in GNP that we observe in the data. This means in particular that one can believe that technology shocks are responsible for long-run growth without having to believe that they cause business cycles. However, it does not follow that technology shocks are intrinsically unable to generate business cycles of the size we observe; these shocks would just have to be different from those usually considered in the real business cycle literature.

Recognizing this, Cochrane pursues a purely empirical method for measuring the contribution of technical change to business cycles. This approach essentially amounts to measuring the degree to which changes in Solow residuals are correlated with output movements. This would be useful as a measure of the role of technological change if measured Solow residuals were identical to changes in technological opportunities. But, as Cochrane recognizes, Solow residuals are correlated with variables that we would normally expect to be orthogonal to technology shocks. Indeed, a variety of mechanisms have been suggested that lead Solow residuals to be affected by demand disturbances. In the presence of these mechanisms, the correlation of Solow residuals with output movements is fairly uninformative about the role of technology shocks in the business cycle.

Let me now turn to Cochrane’s innovative combination of random walk and “news” shocks. Cochrane’s assumptions imply that the level of technology at \( t \), \( a_t \) is given by

\[
a_t = a_{t-1} + \epsilon_t + \sum_{i=0}^{13} \sum_{j=1}^{13} \rho^i \delta_{t-i-j}
\]

The \( \epsilon_t \) shock raises technology only at \( t \) while the \( \delta \) shock keeps raising it forever (though it raises it most in the first 13 periods). I am going to argue that this process for technology (or actually this general type of processes) has a very unfortunate consequence. It raises the standard deviation of the
“permanent” change in output relative to the standard deviation of output changes from one period to the next. To see this, define $v_t$ as the level of $a_t$ that is expected at $t$ to prevail in the infinite future neglecting deterministic changes in $a$.

Thus

$$v_t = E_t \lim_{t \to \infty} a_t$$

(2)

These equations imply that

$$v_t - v_{t-1} = \epsilon_t + \frac{13}{1 - \rho} \delta_t$$

(3)

A unit increase in $\epsilon_t$ raises $v_t$ by one. However, a unit increase in $\delta$ raises $z_t$ by one and, with $\rho = 0.8$, it raises $v_t$ by 65. Cochrane does assume that the standard deviation of $\delta$ is only one-twentieth of the standard deviation of $\epsilon$. Still, $\delta$ accounts for 10.56 (65/20)² times as much of the variance of the change in $v$ as $\epsilon$. With $\sigma_\epsilon = 1$ and $\sigma_\delta = 0.05$, the variance of the change in $v$ is 11.56.

With these same parameters, the variance of the change in $a$ can easily be computed to be 1.55. Note that $\epsilon$ contributes one to this variance. The $\delta$ shock contributes very little to the variance of the changes in $a$ because the way that $\delta$ gets incorporated into $a$ is so smooth.

The VAR allows us to obtain direct estimates of the variance of the change in $v_t$. The reason is that it allows us to compute at each point in time expectations of output in the infinite future. But, because output moves one for one with $a$ in the long run, the variance of the change in $v$ is simply the variance of the changes in the expectations of output in the infinite future. Variances for $v_t$ have been constructed by King, Plosser, Stock and Watson among others. Using a VAR that uses Cochrane’s measures of output and consumption, the standard deviation of $v_t$ is 0.006.

The VAR does not produce a standard deviation for the change in $a$ but one can easily compute the standard deviation of the change in $Y$ and, using Cochrane’s data, this equals 0.012 so that it is larger than the standard deviation of the change in the permanent component. Thus the VAR suggests some mean reversion in output. The change in $a$ is not equal to the change in $Y$ though Cochrane’s plots of impulse responses to the two shocks suggest that the two are very close. The hypothesis that the standard deviation of changes in $Y$ is equal to the standard deviation of the change in $a$ is thus not exactly right but a reasonable approximation. With this approximation, it is clear that the model is widely at variance with the facts. In the data the variance of the permanent component is significantly smaller than the variance of the change in $a$ (or $Y$) while in the model the former is substantially larger.
My work with Michael Woodford leads me to expect this to be an endemic problem of models where technology diffuses slowly so that shocks change the expected rate of growth of output for some time. True, the existence of these shocks will rationalize the fact that consumption predicts output growth but at the cost of raising to an implausible extent the variance of changes in the permanent component of output.

Let me now close by talking more generally about what is learned from the fact that $C/Y$ helps forecast output growth. Speaking somewhat vaguely, this could be due to one of two things. The first is that consumption contains information about future changes in exogenous variables. At least one version of this story seems implausible to me, as I explained a minute ago. The second is that consumption reacts sluggishly to shocks that have only a temporary effect on output. Thus, they help to predict the return of output to its normal level. Just to clarify ideas, let me consider a simple model that is purely statistical. Suppose that $v_t$ is a random walk, as is implied by (2). So

$$v_t = v_{t-1} + \epsilon_t$$

Now suppose that output $Y_t$ is given by

$$Y_t = v_t + u_t$$

where $u_t$ is i.i.d. so that

$$Y_t - Y_{t-1} = \epsilon_t + u_t - u_{t-1}$$

Suppose that we have a variable $Z_t$ which is given by

$$X_t = v_t$$

which responds to the permanent shock but not to the transitory shock. Then, $Y_t - Y_{t-1}$ will be highly positively correlated with $X_{t-1} - Y_{t-1}$. Perhaps the lagged consumption output ratio has predictive power for just this reason. In other words, it might capture the correlation of output growth with a state variable that equals the deviation on output from steady state caused by the cumulation of past transitory disturbances. There are a couple of straws in the wind which suggest to me that this is indeed part of the story. The first is that $C/Y$ is so highly correlated with NBER troughs, as Cochrane's figure demonstrates. It is systematically high at the depth of a recession. This suggest that some of $C/Y$'s forecasting power comes from its ability to look back and notice when an economy has become depressed as a result of past disturbances. This is useful for forecasting as long as recessions do eventually end and always end with a fast recovery. One would expect other backward-looking variables to share this property. This is particularly true
of hours per capita. Indeed, hours per capita predict output growth very well as we argue extensively in my paper with Michael Woodford.

In some respects, my conclusion is very similar to Cochrane's. We are collectively uncertain of what causes GNP to vary and, to dispel this uncertainty, we will need to construct model economies where business fluctuations are more substantial. But my uncertainty is deeper than Cochrane's. I am also not sure that, when our understanding improves, we will not end up assigning a large role to monetary and/or technological disturbances.
References

