Rejoinder

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I am indebted to George Evans for discussions and analysis of a relevant model.

I am pleased that John Cochrane feels that we are in agreement on the most important points. I hope in this note, nevertheless, to considerably extend the area of agreement. In his comment, Cochrane (2009) mentions two major aspects of disagreement. First, he claims that my paper goes wrong by treating the model's monetary policy shock as observable. I partly agree with this but contend that my conclusion regarding the theoretical viability of New-Keynesian (NK) models is not thereby affected. Second, he suggests that even if the learnability[1] results are as I have argued, the NK analysis nevertheless fails to provide a satisfactory model of inflation determination. I will take up these points in turn.

Cochrane's most important objection to the analysis in my paper (McCallum, 2009) is that the monetary policy shock $u_t = -ae_t$ is unobservable, in contrast to the paper's assumption. In this regard I fully agree that his concern for observability is in principle of great importance. Actually, in the monetary policy case of concern, we should probably both be including the private-sector technology shock as the relevant one in the stripped-down, one-shock model in which the current discussion is being conducted. It is a more appropriate shock to include because we are concerned with economies in which technology shocks certainly occur, whereas exogenous policy shocks should not appear at all in a well-designed monetary policy rule. Then the shock in the system being analyzed would be the technology shock for the typical agent (v_t in my paper's equation (1)) and would be observable.

But, putting aside that matter, the main point is that it transpires that the absence of observability does not invalidate my paper's argument regarding learnability vs. non-learnability in the analysis at hand. Let us again consider the model in (5′) of McCallum (2009), namely, $\pi_t = aE_t\pi_{t+1} + u_t$ with $u_t = -ae_t$ and $e_t = \rho e_{t-1} + \varepsilon_t$ (ε_t is white noise and a<1). Thus the model can

[1] Throughout, "learnability" will refer to the specific concept termed "least-squares learnability" by Evans and Honkapohja (2001).

be written as

$$\pi_t = \rho\pi_{t-1} + aE_t\pi_{t+1} - a\rho E_{t-1}\pi_t - a\varepsilon_t \tag{1}$$

where the only unobservable component is white noise. Then we can define $\xi_t = E_t\pi_{t+1}$ and formulate the model as

$$\begin{bmatrix} \pi_t \\ \xi_t \end{bmatrix} = \begin{bmatrix} a & 0 \\ 1 & 0 \end{bmatrix} \begin{bmatrix} E_t\pi_{t+1} \\ E_t\xi_{t+1} \end{bmatrix} + \begin{bmatrix} \rho & -a\rho \\ 0 & 0 \end{bmatrix} \begin{bmatrix} \pi_{t-1} \\ \xi_{t-1} \end{bmatrix} + \begin{bmatrix} -a\varepsilon_t \\ 0 \end{bmatrix} \tag{2}$$

If we use A and C to denote the two 2×2 matrices, does this system have the same learnability conditions as given for the system (10) and (11) in my paper? I have not been able to find any results in Evans and Honkapohja (E&H) (2001) that apply to this particular formulation,[2] but in E&H (1998, pp. 30-32) there is an applicable analysis, and it indicates that the presence of an unobserved white noise shock is irrelevant to learnability of the various RE solutions.[3] Specifically, the relevant mapping (from perceived to actual law of motion) does not involve parameters relating to the unobservable shock. This result evidently pertains to all models in the very broad class implied by the formulation in equations (10) and (11) of my paper. One way to understand this perhaps surprising result is as follows: Exogenous white noise shocks do not overturn learnability because they represent purely random influences that are overwhelmed asymptotically as the number of observations (on an unchanging system) increases. With shocks that are not white noise, the systematic component is taken care of by way of its effects on observable variables, as in equation (1). This is not strictly possible for moving-average shocks, rather than (11), but one can approximate moving-average shocks with autoregressive shocks of

[2] There are numerous examples in E&H (2001) in which unobservable white noise shocks are included and do not overturn results that obtain in their absence, but unobservable shocks that are autocorrelated are not considered. In the analysis of systems such as (10)(11) of my paper, there are no white-noise or other unobservables.

[3] In addition, George Evans has provided me with an explicit proof that, in the case at hand, the fundamentals solution is learnable and the alternative explosive solution is not learnable. His note is availabe on the web at___.

a larger-than-usual but finite order.[4]

The other major topic in Cochrane's criticism concerns his interpretation of the monetary policy rule as involving central-bank "hyperinflationary threats" or threats to "blow up the world" (Cochrane 2007, p. 4). In this regard I am unable to understand his position. In the model that we are discussing, monetary policy is specified as $R_t = \mu_0 + (1 + \mu_1)\pi_t + e_t$ in all circumstances. If "threats" were relevant there would be a threatened departure from this rule, to be invoked in certain specified situations. There are certainly good reasons to be interested in game-theoretic analyses involving alternative modes of central-bank behavior. But in the basic rational-expectations analysis of the model under discussion there is nothing of that type. What the central bank's rule promises is to make nominal interest rates higher than otherwise when inflation is above its target value. In the presence of some price-level stickiness—not included in our stripped-down models but included in the three-equation NK framework that both he and I would use if there is any essential difference in outcome—the higher nominal interest rate brings about a reduced level of real aggregate demand. The promise of the Taylor rule is to make demand low when inflation (and/or inflationary pressure) is high.

As a final matter, there are numerous references in Cochrane's discussion to the lack of identification of the policy parameter *a* (equivalent to $1/(1+\mu_1)$ in my paper). My conjecture is that learnability of the type under discussion does not require identification of this parameter. The way that this type of learnability analysis proceeds is by substituting forecasts from a vector-autoregression model (estimated on all past observations) for expectational variables in the model, and seeing whether it converges to a particular RE solution as time passes and data used in estimation of the forecasting regression increases. The relations estimated by agents in the

[4] This issue arises also with observable shocks.

model are therefore reduced-form, not structural, equations (from agents' perspective). So the non-identification of the structural parameter in question may not be crucial. This is, to repeat, a conjecture.

I have learned much from my attempt to come to grips with the arguments in Cochrane (2007) and his current comment. I continue to hold the beliefs expressed in McCallum (2009), namely, that Cochrane is right to argue that "determinacy" (in the sense of a single non-explosive RE solution) is not an appropriate guide for RE solution "selection," but that this point does not itself overturn the mainstream NK approach to monetary policy analysis.

References

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