Comments on papers by Jordi Galí and by Stefania Albanesi, V.V. Chari, and Lawrence J. Christiano

Christopher A. Sims
Princeton University
sims@princeton.edu

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Some things haven't changed

- Here we have freshwater US economists presenting a paper that elaborates a story in which policy makers, despite *seeming* to have improved policy over time and produced a better-performing economy, in fact will inevitably revert to producing the same bad results as before, unless their discretion, the root of their bad performance, is restrained by institutional reform.

- And a European economist, summarizing a literature with heavy representation from East Coast US economists, gives us a model that tells a story in which policy-makers have made great improvements, accounting for improved performance of the US economy, yet in which there is still room for further improvement. "Discretion" is in a certain sense a problem, but one that policy makers can avoid if they understand it, indeed one which they have already largely learned to avoid.

But some things have changed

The vocabulary, technical methods, and modeling conventions of these papers are quite close.

- Both use RE equilibrium models.

- Both assume monopolistic competition with a fixed fraction of firms "technologically" constrained to have sticky prices. (For Galí, it is a randomly selected set of firms, and firms never can change prices unless selected into the price-changing set. For ACC the sticky-price firms are stuck only for one, or perhaps one half, of a time period.)

- Both evaluate monetary policy according to its effect on agent utility in a general equilibrium model.

That the methods we use in macroeconomics are converging and losing their ideological charge represents definite progress.

Incredible assumptions

- 20 years ago I criticized large-scale macroeconometric models for thoughtlessly relying on identifying assumptions that had become conventional, and were thus rarely questioned or discussed seriously.
- In achieving a common technical language for macroeconomics, we need to be wary of developing a new set of conventional, yet actually incredible, modeling assumptions.
- Use of the assumption of atomistic, monopolistically competitive firms with exogenously fixed restrictions on the timing of price adjustments may be a case in point.

Monopolistically competitive stickiness

- While this type of model is sometimes claimed to be an improvement because it gives stickiness a "micro foundation", it does so in a peculiar sense. There is no empirical micro foundation for this assumption. A leading fact about the distribution of firm size in almost every industry is that it is extremely unequal, with a few firms nearly always supplying a substantial fraction of industry output. IO economists therefore emphasize models with strategic interaction.
- This type of model implies that a primary source of welfare costs of high inflation is that it leads to some firms, who are stuck with low prices, expanding output relative to the firms who have recently changed prices. This is inefficient. But it seems to me likely that in reality this source of inefficiency is quite limited. Firms stuck with obsolete low prices do not greatly expand output. They are more likely to run out of finished goods inventory, and to use the stockout as an occasion to reassess pricing.
- Nonetheless, this type of stickiness may be a reasonable working assumption for some purposes. It seems to stretch plausibility, though, to base serious welfare evaluation of policy on models where this is the, or even a, central source of costs to inflation.

Cash and credit goods

- This is a modeling convention used by ACC, not Galí.

- This gives "micro foundations" to money holding, but in a similarly peculiar sense. There are no actual cash or credit goods, and we have no idea how one would check this assumption against micro data.

- It is a metaphor, a possibly reasonable working assumption for some purposes. But again it stretches plausibility to base quantitative evaluation of the effects of different monetary policies on this metaphor.

GE modeling with no government budget constraint

- This is a shortcut that had become so conventional that most economists didn't even realize it was a shortcut until the recent FTPL literature.

- Both these papers are limited by sticking to this convention.

  Galí – In considering interest-rate pegging policies, the paper confronts an apparent indeterminacy from pure interest-rate pegging, and therefore considers a policy that responds to consumption levels in place of a pure interest rate peg.

  – But a pure interest rate peg is quite possible, without indeterminacy, if accompanied by an easily implemented supportive fiscal policy. It is likely that it would be a very good monetary policy in the context of this model.

  ACC – It is a well-established regularity that in models where the costs of low money balances are bounded, so a barter equilibrium exists, there tends to be indeterminacy, with a continuum of equilibria corresponding to various self-fulfilling expected inflation rates, when the monetary authority uses conventional policy rules (fixed M, or r that responds aggressively to inflation). These are exactly the conditions under which ACC find pairs of Markov equilibria. (ACC are "mindful of the possibility" of this type ∞-dimensional indeterminacy, but do not confront it.)

  – To rule out such multiplicity, when it is recognized at all, we invoke "backstop" fiscal commitments that cut off explosive inflation paths before they can start. But doing so in the ACC model would raise awkward issues: How can the government credibly make the backstop commitment but not monetary policy commitments?

Linearization

This comment applies to the Galí paper, not to ACC.

$$E[U(C(\varepsilon))] \doteq E[U(\bar{C}) + U'(\bar{C})dC + \tfrac{1}{2}U''(\bar{C})dC^2$$

$$\doteq \bar{U} + E[U' \cdot (\bar{C} + C'\varepsilon + \tfrac{1}{2}\mathbf{C''}\varepsilon^{\mathbf{2}}) + \tfrac{1}{2}U''\varepsilon^2]$$

- This setup is simpler than what we face in a dynamic RE model, but it allows us to display the point.
- If we ignore second order terms, then after taking expectations we have only $U(\bar{C})$.
- If we insert a first-order solution for $C(\varepsilon)$ into the actual U or a 2nd order approximation to U, we are ignoring the boldface term, which in general is the same size as the other second order terms.
- Computation is much easier with linearized DSGE's, and for many purposes — e.g. fitting them to data — linearization usually gives excellent results.
- But to justify use of a linearized solution in welfare evaluation, we must either assume C" small, which in nonlinear models is generally unattractive, or assume U' small, which in my view is usually almost as unattractive. The constraints in our model are being assumed not to be "very binding".
- Galí assumes a wage subsidy exactly sufficient to eliminate the monopoly distortion in his model that would otherwise lead to suboptimal levels of employment.
- One reason for this assumption is no doubt that without it second order terms in the solution to the model would affect the welfare rankings of policies.
- This assumption is awkward, and it is no longer really necessary. Collard and Juillard have a paper in which they compute higher order approximations to several discrete time dynamic stochastic economic models, and I have a program and a brief explanatory paper available on my web site that will compute a second-order approximation to the solution of an arbitrary DSGE.

Investment

- Both models are without any role for investment. This is a legitimate simplifying assumption at a very early stage of research, but seems unjustifiable when we get to the point of seriously evaluating monetary policy.

- In a model with investment and a realistic specification of how money contributes to reducing transactions costs, fluctuations in real rates generated by monetary policy are likely to be a more important source of welfare effects than they appear to be in these models with no real intertemporal allocation problem.

Fit

- Neither paper pays much attention to data and fit.

- In the Galí paper this reflects the survey nature of the paper, and the paper does discuss empirical work done in other papers.

- But the work discussed is primarily single equation estimation, using "instrumental variables" estimation methods that are not grounded in a complete model. It involves, for example, estimating "Phillips curves" that put on the right-hand side a "labor share" variable in place of the usual "gap" variables. That such a relation between endogenous variables has a high R^2, though, is not evidence in favor of one model over another except in the context of a complete multivariate specification for both models.

I understand that there is empirical work related to the ACC paper that I have not been able to read. Still, it is reflective of what is now our conventional style of professional interchange that the paper could reach the stage it has, and make the claims it does, without feeling the need to support its specification by a confrontation with the data.

Is monetary policy in fact now better?

- The New Keynesian literature surveyed by Galí that claims to find big changes in monetary policy between pre-1980 and post-1983 US monetary policy is not very convincing to me. It rests on identifying assumptions that are not very plausible in the pre-1980 period.

- Before 1980, policy-makers paid attention to money stocks. Ignoring this confounds policy shocks with money demand shocks. But taking account of it properly is likely to lead to a model with simultaneity — which shouldn't be, but apparently is, a barrier.

- On the other hand, the ACC story of multiple equilibria is also unconvincing to me. The qualitative mechanism they describe, in which increases in inflation become, beyond some point, less socially costly as the economy adapts to high inflation, is plausible in some contexts. It is probably important in understanding countries with a history of high inflation. It does not seem to me plausible that this mechanism had begun to be important in the US in the 60's and 70's.

- Also, models like the ACC model, in which monetary policy makers pay no attention to reputational considerations, are unrealistic. It sometimes seems that in fact they pay attention to nothing else

So my view is that policy makers are probably neither as much more enlightened now as the recent New Keynesian literature would have us think, nor as myopic and as needy of institutional reform as ACC's story would suggest. These questions are far from closed, and we should look forward to more good research on them.