COMMENTS ON PAPERS BY JORDI GALÍ AND BY STEFANIA ALBANESI, V.V. CHARI, AND LAWRENCE J. CHRISTIANO

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The theme of these comments is that we need to remain vigilant against the possibility that "standard" modeling conventions in macroeconomics, originally introduced as experimental or tentative, start to be used unquestioningly, despite serious drawbacks. It is in a sense unfair to focus such comments on the papers presented in this session, since the conventions to be criticized are not at all special to these papers, but there must be some point at which we step back and consider where the conventions in our literature are headed, and a quinquennial World Congress seems as appropriate an occasion as any for doing this.

I will consider the papers by Galí and by Albanesi, Chari and Christiano (henceforth ACC) that appear in this volume, and also the empirical paper (Galí and Gertler, 1999) that forms the foundation for much of the paper in this volume that Jordi Galí presented.

1. Optimal Monetary and Fiscal Policy

Both papers discuss optimal monetary policy in the context of general equilibrium models. General equilibrium models, with their own internally generated, explicit measures of welfare, allow us to avoid postulating an *ad hoc* objective function for the policy authority, instead evaluating policy directly in terms of its welfare implications for private agents. Galí explicitly, and probably ACC also, see this as an improvement over approaches that use models with a less complete behavioral interpretation. Such models must either postulate plausible measure of policy effectiveness directly, or else present multidimensional measures of policy performance — variances and means of a variety of important variables, usually — and leave the reader to draw conclusions about ranking the results.

But these papers only confirm that reliable conclusions about rankings of policies from general equilibrium macro models are, for the time being at least, completely unavailable. ACC focus most of their attention on two welfare effects of inflation — the decline in demand it generates for "cash goods", under their particular assumptions about timing in discrete time cash-in-advance

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style models, and the increase in output toward the efficient level it can produce under sticky prices. Output tends to be below the efficient level in their models because of the existence of monopolistic competition. Even within their framework, their conclusions are more fragile and dubious than a casual reading of the paper might suggest.

Galí and related literature he cites, on the other hand, assume both these effects of monetary policy away at the start. The assumption that the costs of squeezing down real balances are small is justified by no more than an appeal to intuition. The assumption that there are no gains from pushing output above its non-inflationary level, despite the same monopolistic competition assumption as in the ACC model, requires postulating that the government knows and sets exactly the efficient level of employment subsidy, financing it by lump sum taxes. The costs that are left are only those due to inefficient, but mean-zero, fluctuations around the efficient level of output.

That there is no overlap in the effects of monetary policy considered in these two papers, since they are representative of much other work in the recent literature, suggests that we have no professional consensus on how to set about this task. Furthermore, a good case can be made that each is considering potentially important effects, and thus also that each is ignoring potentially important effects.

It is common to dismiss as unimportant "shoe-leather costs" imposed by inducing people to economize on real balances, so in doing so Galí is far from alone. If we think of these as the costs imposed on us when inflation induces us to carry less cash in our wallets, it certainly seems justified to treat them as small. From this point of view, the fact that cash-in-advance and limited-participation models are capable of implying these costs are significant is a flaw in those models. ACC do not provide much discussion of why they think we should take these costs to be quantitatively important. They also appear to be an artifact of the models' reliance on discrete time, and the timing ambiguities that introduces. As ACC point out, they are using the Svensson, rather than the Lucas and Stokey, timing assumptions. But these differences in timing assumptions are difficult to evaluate for plausibility, since time is in fact not discrete.

The way the timing assumptions operate to create different conclusions is as follows. If M_t/P_t (where the time subscript indicates the date a variable is known and/or chosen) enters a transactions cost term or the utility function, then surprise monetary expansion, with flexible prices, is neutral, though there are still usually real effects from anticipated monetary expansion. Expansion in M is accompanied by an expansion in P that offsets its effects on transactions balances. The simple

version of the natural rate hypothesis is thus stood on its head. This result was present in Lucas and Stokey (1983) and has been underlined since, for example in a paper of mine (1994). If instead it is M_{t-1}/P_t that provides utility or transactions services, then surprise monetary expansions have real effects, but they are purely contractionary in a flexible price model. This is because, with M_{t-1} fixed, any inflationary effects of expansion contract the availability of transactions services. It is this type of contractionary effect on which ACC rely in generating their results.

But the time unit over which people are stuck with cash, and hence subject to surprise inflation taxes on them, is short on average. It is also endogenous. Thus an increase in the price level can be offset by slightly more frequent trips to the bank, so that the effects on spending of the shrinkage in real balances are slight. Cash in advance models, being in their simple form tied to a unit delay between putting aside cash and spending it, cannot account for these possibilities. If one has to choose between M_t/P_t and M_{t-1}/P_t on these grounds, I think the former makes more sense.

In a paper that ACC cite, Nicolini (1998) gives a more extensive discussion of the motivation for this modeling style. He argues that the unit time interval is misleading here, that in a realistic model with heterogeneous agents that make randomly timed visits to the bank, there will be at any given time some with large balances. He prefers the M_{t-1}/P_t timing (but really, as a long run modeling strategy, models with heterogeneous agents) because it seems to him a stand-in for missing model realism.

My own view is that Nicolini's argument does not go far enough, because it remains too much focused on cash and transactions technology. The economy is laced with incomplete, nominally denominated contracts, both formal and informal. Surprise inflation has real effects on the terms of these contracts, and a lot of surprise inflation is likely to make people move away from simple nominal contracts. We do in fact see the nature of some contracts, e.g. labor contracts, shift with the level and variability of inflation. Since nominal contracts reemerge when inflation stabilizes, they apparently are cost-effective in some sense. We have little understanding of why these contracts take the form they do, or of the costs of distorting existing contracts via inflation, or of inducing people, via surprise inflation, to contract differently. These costs may not be very important, but they may also be large. We have no quantitative handle on this issue, and none of the existing approaches to general equilibrium modeling confront the issue.

Nicolini's arguments do not, then, suggest that cash-in-advance or limited participation models are quantitatively reliable for evaluating the effects of monetary policy. They do, by recognizing the

importance of heterogeneity and redistributive effects of inflation, point toward a broader critique of all current approaches to using general equilibrium models for monetary policy evaluation.

Galí's approach justifies its measure of welfare effects by a now-widespread idea, originally due to Rotemberg and Woodford. Under the monopolistic competition assumption, and with the Calvo story about the reason for price stickiness, inflation creates a distribution of prices, with firms that have not for a long time been hit by the exogenous random variable that allows price changes likely to have prices far from the desired level. This leads to firms to have dispersed output levels, which is inefficient. But the Calvo story, while a clever modeling idea, has little claim to be grounded in empirical fact. In particular, the costs of inflation it implies rest on firms having no power to affect the timing of their price changes. While for some purposes this may be an acceptable simplification, for evaluating welfare this restriction on firm behavior becomes central. If firms can decide to change prices when their prices get far enough out of line, then the distribution of prices, and thereby this source of inefficiency, may be quite insensitive to inflation.

The New Keynesian literature of course ends up with a criterion that is a weighted average of variance in inflation and output. Usually, as in Galí's paper, we see tables that show the underlying variance numbers as well as the weighted average that defines welfare. When this practice is followed, we can see how robust are the rankings of policies to changes in the weights. Attempts to translate differences in variances into utility-equivalent differences in mean consumption, though, depend on our taking seriously the derivation of the utility function, and should be treated as speculation.

There is no good argument that the non-inflationary level of output would be socially optimal in the absence of random disturbances. One might think that so long as we assume that policy-makers can make commitments, so that the possibility of short-run employment gains from surprise inflation does not lead to an inflation bias, it would not make much difference to conclusions if the noninflationary level of output were not optimal. But this is not true. The effects on welfare of variance in inflation and output are second-order in the scale of the randomness in the model. In a nonlinear equilibrium model like those underlying the Galí paper model, randomness affects the mean as well as the variance of the variables in the model. The effects on means are also second-order in the scale of the randomness. Unless the effects of changing the means are small, as they are in the neighborhood of the optimal equilibrium, policies cannot be correctly ranked

without considering their second-order effects on means. In other words, the log-linearized models standard in the literature cannot produce correct rankings of policies. [1]

The defects in the New Keynesian story about welfare costs of inflation and in the ACC story are both cases of a relatively simple, ad hoc, rigidity having been suggested as a starting point for modeling non-neutrality, then propagating in the literature because of its convenience as a modeling technique. There is no harm in this process itself. But we need to remain aware that there are many potential ways to generate price stickiness and non-neutrality. Similar qualitative aggregate observations may be accounted for by mechanisms with contradictory implications for welfare evaluation of monetary policy. We therefore need to remain modest in our claims for conclusions about welfare effects of monetary policy. Indeed it might be best to limit our claims to assertions about the effects of policies on the behavior of aggregate variables. For such claims we have firmer checks against historical data.

Both sets of authors deal in the papers at hand with policy evaluation in abstract models, at best calibrated roughly to data. In the background is related literature (Albanesi, Chari, and Christiano, 2000; Clarida, Galí, and Gertler, 2000; Sargent, 1999; Cogley and Sargent, 2001, e.g.) some of it by these same authors, which has examined the last half century or so of US monetary policy, evaluating how good it has been, whether it has been improving, and what forces have contributed to its evolution. While my comments at the conference included some discussion of this literature, particularly of the ACC paper (which is what V.V. Chari actually presented at the conference), here I will simply note that the apparent lack of consensus on the sources of monetary non-neutrality, the dependence of conclusions on assumptions about these sources, and the limited attention to statistical fit in this literature make its conclusions on these issues at best tentative. Despite a widespread belief among economists that the inflation of the 70's and the return to low inflation in the 80's and 90's reflect bad early monetary policy and better later policy, there is little solid empirical support for this view. In fact, there is considerable empirical support for precisely the opposite view: that the inflation and its end were not generated by monetary policy decisions, that monetary policy in fact has had roughly the same systematic component throughout the period.[2]

[1] This was pointed out in a simple example by Kim and Kim (1999), and there are now a number of individuals or groups preparing and using software that allows second-order expansion of equilibrium models (Collard and Juillard, 2000; Schmitt-Grohé and Uribe, 2001; Sims, 2000b)

[2] Hanson (2001); Leeper and Zha (2001); Sims (1999); Orphanides (2001).

2. Existence, Uniqueness, and Fiscal Policy

The first model in the ACC paper, with flexible prices, arrives at the conclusion that even a monetary authority that cannot make commitments will choose optimal policy, and that that policy is the Friedman rule — contract the stock of non-interest-bearing high-powered money at a rate equal to the real rate of interest. ACC observe that only the real allocation in this equilibrium is unique — there is a continuum of equilibria indexed by the initial price level. But this observation is given no further discussion.

As in most such models, the non-uniqueness of the initial price level implies the existence of sunspot equilibria, in which the price level is randomly non-unique at every date. There are two real variables that are not uniquely invariant across equilibria: The level of real balances and the level of real lump-sum taxation. Since in reality taxes are not non-distorting, especially if they must be imposed at high rates, it is clear that the policies ACC characterize as "optimal" in this model could not be recommended in practice as good policies. They would require a commitment to tax at unboundedly high rates if realized prices turn out to be very low.

The non-uniqueness ACC find here is essentially the same as that analyzed by Benhabib, Schmitt-Grohe, and Uribe (1998), and rests on the same somewhat strange type of specification of fiscal policy. Benhabib, Schmitt-Grohe, and Uribe postulate that fiscal policy makes the real primary surplus react positively to the real value of total outstanding government liabilities, including high powered money as well as interest-bearing debt. The ACC fiscal policy, which commits to taxing at a fixed ratio to the real value of the stock of outstanding money (here the only government liability), is a special case. We do not see historical examples of persistent taxation for the sole purpose of contracting the money supply, and current political budget rhetoric does not seem to leave room for this reason to tax. Why should a legislature feel an obligation to increase taxes, with no outstanding debt, when the price level has emerged as lower than expected?

It is more reasonable to suppose that the fiscal authorities make taxes respond, if at all, only to the real value of interest-bearing debt. Furthermore, if we are imagining that the monetary authority is capable of exactly measuring the real rate of return so as to contract M at that rate, we might imagine instead that the fiscal authority (another name for the monetary authority in this model) knows the level of real taxation that is required to contract M at the desired rate when real balances M/P are exactly at the satiation level. If they then formulate policy as a commitment to tax to produce exactly that real primary surplus, regardless of what happens to prices, and the

monetary authority commits to maintaining a zero nominal interest rate, then the ACC model will have a Friedman-rule equilibrium with a uniquely determined price level.

The Friedman rule is still a questionable policy, though. If the tax authority sets the primary surplus too low, the policy is unsustainable, and while the uncertainty about how the policy will become sustainable persists, high inflation is likely to result. The surplus could be set high enough to allow a margin of error — high enough to contract a volume of real balances well exceeding the level that satiates transactions demand at a rate above any value the real discount factor is likely to take on. But if we recognize a cost to high taxation, this policy choice is unattractive.

If these transactions costs considerations are the primary reason for worrying about inflation, then the best policy in practice is likely to be setting a low positive nominal rate. This, by fixing velocity uniquely, will, in combination with any primary surplus above some minimum level, guarantee a unique equilibrium. Such policies do require fiscal support. A steady flow of tax revenue is devoted to contracting the money stock. However, widespread existing conventions, in which central banks "own" some of the government's debt and can use the flow of interest income for policy purposes without legislative second-guessing, would support the necessary flow of fiscal resources.[3]

Specifying monetary policy not as a fixed nominal rate but instead as contraction of the nominal money stock at a fixed rate $g > \beta$ in this model is not compatible with commitment to a fixed real primary surplus. In the multiple equilibria that ACC find for this case, in all but one real balances grow at the rate g/β. This requires, with their specification of fiscal policy, that real primary surpluses grow without bound. With the primary surplus instead fixed in real terms, the shortfall has to be made up by borrowing, and the borrowing would have to produce real interest-bearing debt growth at the rate β^{-1}, which is incompatible with private transversality. The equilibrium in which prices and M both grow at rate g, so M/P remains constant, determines a unique nominal rate, thus a unique initial M/P, and thus a unique initial P. But in general this initial P will not be the value that matches initial $(B+M)/P$ to the discounted present value of primary surpluses, so there is no corresponding equilibrium. In other models, where transactions demand is highly interest-sensitive

[3]Actually, the situation in the US, where the central bank backs its high-powered money mainly with holdings of nominal domestic government debt, is not common. Where the central bank balance sheet includes a large component of real assets or of foreign currency denominated securities, the current conventions of central bank independence would not suffice to provide fiscal support for such a mildly deflationary policy. Explicit fiscal backing would be required. (Sims, 2000a)

and a barter equilibrium exists, the fixed M, fixed surplus combination is actually consistent with equilibrium — an indeterminate continuum of them, all of which involve inflationary dissipation of real balances and convergence to barter equilibrium.

The uniqueness issues I raise here in connection with the first of the three ACC models in the paper in this volume are more complicated to analyze in the sticky price and limited participation models, and ACC do not fully unravel them. They consider only recursive equilibria with certain natural candidate state vectors, which leaves open the question of whether there may be equilibria indexed by, e.g. lagged prices. Uniqueness issues are brought out, but in the same sense also incompletely analyzed, in the paper by the same authors presented at the conference (Albanesi, Chari, and Christiano, 2000). There they suggest that the non-uniqueness that can arise in a model that allows more elastic response of transactions technology to interest rates might explain postwar US monetary history. In that paper it would be even more important to recognize that jumps between equilibria may imply substantial jumps in the primary surplus with their assumptions about fiscal policy, and also the possibility that commitment to a real primary surplus can eliminate indeterminacy.

The Galí paper also ignores the fiscal aspect of monetary policy. This leads to its assertion that a fixed nominal interest rate implies indeterminacy of the price level if interpreted literally. This is not true if the fixed nominal interest rate is accompanied by a commitment to a real primary surplus. It would be simpler to describe the fixed interest rate policy this way. As the paper now describes it, it sounds difficult to implement, whereas in fact it would be easy.

In a model that recognized the revenue from seignorage, the fixed interest rate policy implemented as Galí describes it here (via a restricted variant on a Taylor rule, with fiscal policy implicitly making the primary surplus responsive to the real debt) would probably not deliver the same equilibrium as an nominal interest rate peg accompanied by a primary surplus peg. However, in the model as laid out here, the two versions of a fixed nominal rate policy probably do deliver equivalent equilibria. My own experiments suggest that, if a disturbance is introduced into the Phillips curve as Galí does later in the paper, an interest rate peg can deliver lower inflation variance (though still higher output variance) than the Taylor rule in this model. It would be a good idea, before taking the policy evaluations displayed here as the last word even for this type of model, to consider the effects of such shocks on the rankings of policy rules.

3. The New Phillips Curve

Simple New Keynesian models use a purely forward-looking "Phillips Curve" derived from Calvo-style price adjustment. These models imply that inflation "leads" the output gap, and Fuhrer and Moore (1995) in a careful two-equation analysis showed that in what was then their usual form these models are qualitatively at variance with the time series facts. In another paper, working with a formally similar inventory model, Fuhrer, Moore, and Schuh (1995) show that for these models likelihood-based multiple equation methods are much more reliable than inference based on instrumental variables.

The literature Galí cites on the newest formulation of the New Phillips curve (Galí and Gertler, 1999; Sbordone, 1998), retreats from the standard of care in empirical evaluation set by the Fuhrer-Moore paper, using instrumental variables methods alone for estimation and using informal measures of fit that can easily be misleading.

To understand the problems associated with the approach in these papers to assessing fit, consider the New Phillips Curve model laid out the Galí-Gertler (GG) paper. It specifies the same Phillips Curve that appears in this volume's Galí paper:

$$\pi_t = \beta E_t \pi_{t+1} + \kappa x_t \,, \tag{1}$$

with x_t interpreted as being the log of the labor income share in non-farm business sector. This is, on the face of it, an equation that can be estimated by an OLS regression of π_{t+1} on π_t and x_t. Without explaining why, GG estimate it instead by instrumental variables, normalized as in (1), and omit π_t from the instrument set. [4]

Solving this equation forward in isolation, they obtain an implied relation between π_t and a discounted sum of expected future values of x,

$$\pi_t = \kappa \sum_{t=0}^{\infty} \beta^s E_t x_{t+s} \,. \tag{2}$$

Then they apparently[5] use an unrestricted bivariate VAR to form expected future values of x, substitute them into the right-hand side of (2), and call the result "fundamental inflation". The theory

[4]The normalization would not have mattered if they had kept π_t in the instrument set, as in that case instrumental variables estimates would have been identical to those from OLS.

[5]The nature of the forecasting rule used to form $E_t x_{t+s}$ is not completely clear from the paper. All that is stated explicitly is that it is based on data on π and x dated t and earlier.

predicts that fundamental inflation defined this way is exactly observed inflation. This is the intuition behind plotting the two, noting how closely they track each other, and counting what appears to the eye a close tracking as support for the theory.

But we are not here testing a theory in which an explanatory variable x is claimed to be important, while under an alternative view x is expected to be unrelated to π. When the gap is measured as the labor share, it is being measured as a variable that any reasonable macro theory would treat as highly endogenous, affected by all kinds of structural disturbances. It is not surprising that linear combinations of it with π should turn out to be highly correlated with π. The theory claims that fundamental inflation should be identical to actual inflation, and this is a simple linear restriction on the coefficients of the VAR. The theory should be tested directly as such a restriction. To understand this point, it may help to write out the unrestricted VAR as

$$\begin{bmatrix} \vec{\pi}_t \\ \vec{x}_t \end{bmatrix} = z_t = Az_{t-1} + \varepsilon_t \,, \tag{3}$$

where $\vec{\pi}_t$ and $\vec{x}_t$ are $k \times 1$ vectors containing current and lagged values of π and x. In this form, the system allows us to write

$$E_t z_{t+s} = A^s z_t \quad \text{and therefore, according to theory,} \tag{4}$$

$$\pi_t = e_1 z_t = e_{k+1} \kappa E_t \left[\sum_{s=0}^{\infty} \beta^s A^s z_t \right] = (I - \beta A)^{-1} z_t \,, \tag{5}$$

where e_i denotes the unit vector with a one in its i'th position. This implies

$$e_1 = e_{k+1} \kappa (I - \beta A)^{-1} \quad \text{or, equivalently} \tag{6}$$

$$e_1 \beta A = e_1 - \kappa e_{k+1} \,. \tag{7}$$

Note that this is simply the assertion that the first equation of the VAR should be exactly (1), normalized to have π_{t+1} on the left with a unit coefficient, and with the E_t operator dropped. The theory should be evaluated by examining, using likelihood-based measures, the validity of this straightforward linear restriction on A.[6]

[6]Comparing the time series behavior of the left and right hand sides of (2) is widespread in applications of what is known as the Campbell-Shiller methodology. Such time series plots can be useful in assessing a model, but they should not be treated as measures of fit. Many authors construct formal tests of (6) as a nonlinear restriction on A. As has recently been pointed out by Mercereau (2001), there is good reason to think that inference about $(I - A)^{-1}$ in these models, based on asymptotics, will be much less reliable than inference about A itself.

Even if all the inference were done more carefully, however, we should bear in mind that we are not interested directly in restrictions within a model incorporating a single gap measure. We are interested in comparing models. We are interested in how a full-fledged model works in providing forecasts and policy analysis when incorporating one or the other measure of the "gap", forward or backward-looking specifications, and so on. As yet the literature arguing for a Phillips Curve with labor share standing in for the gap has given us no evidence on these questions, even on the simple one of whether forecasts of a multivariate model are improved by a theoretically constrained addition of a labor share variable.

4. Conclusion

In emphasizing how tenuous and loosely linked to real data are the monetary policy evaluations in these papers, I do not intend to imply that this is uninteresting work. Precisely because we still understand so little in this area, work with stylized, unrealistic models can be interesting and worthwhile. Yet in central banks around the world there is increasing recognition of the value of being more explicit and quantitative about planned time paths of target variables like inflation, output and employment and about how current policy actions are expected to influence these time paths. Accomplishing this does not require models that explain why people hold money and why money is non-neutral, but it does require multivariate models grounded in data as well as in theory. While these two papers did not set out to provide or improve such models, they are representative of a great deal of academic macroeconomic research. It seems to me that work on improving models that contribute to current problems in implementing monetary policy deserves relatively more attention from academic macroeconomists.

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