Comments on Adão, Correia, and Teles:

"Short and Long Interest Rate Targets"

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1. Paper is about issues concerning short and long interest rates as monetary policy instruments. Major objective is to dispute conventional wisdom, with MW (2005) taken as representative. MW's argument is that use by a CB/MA of long term rate does not give additional scope for stimulus in face of ZLB trap (comment on McGough, Rudebusch, Williams in JME). Authors ACT state that "long and short nominal interest rates are independent mon pol instruments" and that "pegging of both [serves to] solve the classical problem of multiplicity that arises when only short rates are used ...."

I'm unsure about relationship. Denying results in MW analysis? If so, where does it go wrong? Are ACT using a richer model with diff results? Would like ACT to make disagreement with MW explicit to help reader to understand and evaluate.

[One place where the models differ is that MW assumes slow price adjustment of the Calvo type whereas ACT's main analysis assumes flexible prices. They also include section with prices set in advance, but specif is more rational than MW's.]

2. It seems surprising that there is no consideration of "determinacy," as in MW (2003) and most recent mon pol literature. ACT seem to object because it is only "local." What about linear models? Anyhow, it serves role of considering solns in which agents base expectations on variables that are not "fundamentals." I do not favor usual determinacy analysis but wonder how "sunspots" are being ruled out.

Model in Sect. 2 is a much richer version of the simple flex-price model in Cochrane (2007). I have argued with JC about that model so will discuss.

3. Issue is the role in NK analysis of std "determinacy," i.e., single stable RE solution. I consider this neither necessary nor sufficient for finding a particular soln to be plausible as equil (in sense of what the analysis is predicting as behavior of actual economy that model represents). My exchange with Cochrane (JME, 2009) relates to his NBER WP 13409. It argues that mainstream NK analysis (as in MW 2003 or CGG or CEE) is critically flawed: when Taylor principle *is* satisfied so determ obtains there is typically an explosive soln that cannot be ruled out by TC: explosive inflation leads to real money balances approaching zero.

4. My paper accepts this point, that determ is not sufficient to yield single plausible soln. But goes on to argue that JC's explosive soln can be ruled out by a form of informational feasibility— namely, E&H "LS learnability." I suggest that if a soln does not satisfy LSL then it is implausible. Idea is: for a RE soln to prevail agents must have quantitative knowledge of system's law of motion (for forecasting) obtained from past observations on data from this economy—it cannot be obtained by divine revelation. LSL obtains if system converges to RE soln in question. LSL process assumes: agents' expectations based on correctly specified VAR; using past

data; and an appropriate estimator; unlimited no of obs; on unchanging system. So if LSL does not obtain for a soln, it is not plausible. (Also, if LSL does not obtain, system departs from RE soln in question with prob 1.0.) So LSL is necessary for a soln to be plausible.

But I show that JC's explosive soln in NK models is not LSL and the usual soln is. Thus standard NK policy results are OK, when TP is satisfied, but not because of determinacy.

5. In his comment (JME 2009) Cochrane diagrees, arguing that the main policy parameter is not identifiable (by agents) in these models. But those agents are forecasting, not doing policy analysis, so this objection is not relevant. Also JC complains that policy shock is not observable, but I show that E&H (RES, 1998) results imply that this does not matter for LSL.

6. Determinacy is also not <u>necessary</u>. If there are (e.g.) two stable solns, but one is LSL and the other is not, then only the first is plausible. This occurs, e.g., with rules featuring strong short-rate responses to expected future inflation rates.

In sum, "determinacy" (SSS) is not what our analysis should be concerned with; LSL is more important. Does it always yield a single plausible soln? Not quite always, even in linear models. I'm working on "refinement" (but have been for many years).

7. ACT mention my 1981 JME paper and subsequent "large literature" which they consider inconclusive. In this context, a distinction should be made between today's usual concept of indeterminacy and "nominal indeterminacy," which occurs when every agent cares only about real variables (then model includes no nominal vars). I thought S&W (JPE 1975) was about the latter, with S&W (Em 1973) about multiple (real) solns. [Nominal interest rates and inflation are not in this context nominal.]

8. Sorry to have little to say about issues that ACT are concerned with—esp. whether MA has two indepen instruments. Inclination is: at level relevant for mon policy we should model MA as having one. MA can set the mon base and accept the one-period interest rate that results—or vice-versa. If include rates of various maturities we also include more behavioral eqns. The CB can control a linear combination but I do not see how it can control two rates.

9. What about "initial money stock"? This seems consistent with ACT neglect of std determinacy and also their expressed aversion to "timeless perspective" analysis. These issues are not settled.