Comment on LIQUIDITY EFFECTS AND TRANSACTIONS TECHNOLOGIES, *by Finn E. Kydland*

This paper gave me a chance to think about where we stand in the literature on liquidity effects. A priori, I would have found it hard to come up with an interesting question whose answer would be affected significantly by the presence or not of liquidity effects of the type considered in this and in related papers. Such effects seem likely to be short lived and small and perhaps even exaggerated in models in which cash-in-advance constraints are the reason people hold money. The main case, it seems to me, for which this mechanism may play a reasonably important role is in the context of short-term forecasting—the type of thing Federal Reserve Banks do. [For a recent effort, see Altig, Carlstrom, and Lansing (1995).]

One could ask, Is the liquidity effect a deviation relative to established theory? Many economists believe it has been empirically well established that the nominal interest rate falls immediately after a monetary injection. If the effects are mainly short run, primarily affecting nominal variables, this question, to be of interest, almost has to be followed by another question: Does this mechanism account for volatility of real aggregates of any quantitative significance? If the answer to this second question is "not much," then there is little reason to be concerned about leaving liquidity effects unaccounted for in the models we use to address most questions.

As a way of getting a sense of how this literature has progressed, I found Fuerst's (1993) paper to be helpful. He provides an overview of the model types that have been used. As he points out, a challenge has been to resolve the original anomaly without introducing new ones, in the model's consumption behavior, for example. On the other hand, one can ask whether one really has to be too concerned about such anomalies. Suppose it turns out to be difficult to obtain real effects from this transmission mechanism: in other words, other impulses dominate. It is hard to see how it could be otherwise. Now let us say, as discussed by Fuerst, that liquidity effects have the implication that some aggregates move in anomalous directions. Since no model seems to imply large movements of that type, the fact that other impulses dominate just means that the monetary impulse modifies slightly what otherwise would have occurred in the model, but it is not a big deal quantitatively.

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The approach taken so far in the literature has been: Study the implications of a variety of permutations of who needs cash for transactions, who receives the injection of liquidity, and who, therefore, uses it. Perhaps one has gone too far in that direction. These efforts seem like attempts to fit the observations with not enough effort going into searching for empirical regularities or measurements that can indicate whether one is on the right track or not. The paper by Christiano, Eichenbaum, and Evans (1994) at the monetary conference at the Federal Reserve Bank of Dallas last spring used data that looked promising in this respect in an attempt to quantify actual liquidity positions and their flows. Unfortunately, because of the focus on VARs, to me at least the findings became more clouded than illuminated. It appears essential to dig deeper into the data at this stage before proceeding much further into theory.

What might one look for? Can we learn about the flows of funds from the fact that the behavior of postwar U.S. monetary aggregates has been very different after the early 1980s than before (Kydland and Prescott 1990, charts 9–11)? If payment patterns have changed over time, for example, because of greater prevalence of credit cards and electronic means of payment, have there been noticeable changes in the timing and magnitudes of the flows? Could one compare with other countries in which the speed of improvement in the means of payment has not been as rapid? There ought to be data that can yield statistics to tell us whether these models are going in the right direction and whether the flows of assets implied by the models correspond well enough with the data. Theory dictates what to look for.

Now I'd like to address some issues that are more specific to this paper, focusing on those that may have a bearing on the question, How sensitive are the findings from a typical liquidity-effect model to freeing up the ability to move funds between savings and cash after the monetary shock? That is really the question of this paper and is a well-posed and interesting one. I think the authors make considerable progress in answering it. How easy or hard it is to move funds is characterized by transaction costs for households and financial intermediaries, in this case in the form of time cost, which I think makes good sense.

As I went through the model, a couple of things startled me. An example was the CES aggregator for the labor inputs between pairs of successive subperiods. At that point, the reader may wish he had the original paper by Christiano and Eichenbaum to see what story they use for that model feature. The H function [eq. (7)] specifies a within-period elasticity of substitution between a period's labor input before and after the monetary shock. In other words, the marginal product of hours in the second subperiod depends on the labor input in the first subperiod. It is not clear that this feature ties in well with measurement, and in order to see how much difference it makes, I would like to see an experiment in which the two labor inputs in a time period are made perfectly substitutable in production.

Under the circumstances, I thought the adjustment costs were calibrated as nicely as can be done. These functions are quadratic; see page 1449, this issue. The curvature parameter of that pertaining to the financial intermediaries is set so as to match the standard deviation of a spread between interest rates on loans and deposits. Consequently, it is clear what is the basis for the calibrated value. The difficulty in quan-

tifying the corresponding parameter for the households is then remedied by trying out four different values as multiples of the transactions cost for the intermediaries. It was helpful that the authors did provide some numbers giving a feel for the sizes of these adjustment costs, say, in minutes per quarter. Otherwise, their numerical values are almost impossible to judge. My only suggestion is that the authors should make clearer how much "one unit" of money is when describing the magnitude of the marginal transactions cost.

My main problem with the presentation of findings is that it is all in the form of response functions rather than the covariance structure for the model variables. Perhaps one can make the case that it is justified in this paper when comparing four different scenarios of the same model economy. The purpose of getting intuition for what is going on is a valid one. I still think we want to see not only the presence of the liquidity effect in response functions, but also their implications for the cyclical statistics.

As an example of how incomplete the picture may be with only response functions, consider the model in Kydland (1991) designed to investigate the role for the business cycle of price shocks engineered by the Federal Reserve Bank. The idea, as in Lucas (1972), is that such shocks make it hard for private agents to observe the relative prices they need as a basis for their decisions. The model economy features aggregate technology shocks as well as the price shocks, but agents can only observe the sum of the two (interpreted as the nominal wage, say). Thus, a signal-extraction problem is implied. Of course, a price shock's response function indicates that it does have real effects, mainly because it has to be taken into account when forming the conditional forecast of the real shock. In an ongoing economy, however, where shocks of both types occur in every period, it turns out that the greater is the variance of the price shocks, the smaller is the variance in aggregate output. There are two effects in opposite directions: the price shock, whenever it occurs, affects output in the same direction as the shock. But, people react less to all shocks, including the real ones, the larger is the variance of the price shocks. It turns out, within the calibrated economy, that the second effect dominates. This finding is evident, not from response functions, but from cyclical statistics of model histories from computational experiments using the calibrated economy.

In the same vein, it would have been useful, in this paper, to have introduced technology shocks to see if the magnitudes of the contrasts across experiments with different transactions costs would hold up on the basis of standard deviations and correlations from model histories. One would still expect the same bottom line, I suppose, namely, that the magnitudes of the effects of the liquidity mechanism on real aggregates are sensitive to the cost of transfers between savings and cash. This is an interesting finding.

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Comment on LIQUIDITY EFFECTS AND TRANSACTIONS TECHNOLOGIES, *by Don Schlagenhauf and Jeffrey Wrase*

Because of the ability of the basic neoclassical business cycle to explain real business cycle fluctuations, an obvious extension to this framework is to introduce money so that monetary policy issues can be examined. A number of approaches have been employed in the literature to introduce money into a dynamic general equilibrium model. Lucas (1980) has used a cash-in-advance constraint. Since this technology has the implication that the costs of changing money balances are infinite within a period, Marshall (1992) has argued that a transaction-cost technology where agents sacrifice resources to effect transactions is a preferable approach. Kydland (1991), den Haan (1990), McCallum and Goodfriend (1987), and Lucas (1990) have introduced money into the general equilibrium framework through a shopping-time story. In this approach, agents sacrifice leisure in making transactions. Each of these approaches has the unattractive property that positive monetary impulses lead to declines in output if agents operate in an environment of complete information. This property is due to the fact that positive monetary injections result in large anticipated inflation effects in the models.

Recently, Lucas (1990), Fuerst (1992), and Christiano and Eichenbaum (1992a), following some earlier ideas by Rotemberg (1984), have used a model where certain segments of the economy make financial decisions before monetary injections are

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