Discussion of Seidman-Lewis and Auerbach-Hasset

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In a world of low inflation and low nominal interest rates the counter-cyclical ability of monetary policy operating in safe short-term markets may be hampered by the zero bound on nominal interest rates. From the monetary point of view this situation creates some discomfort: We could accept somewhat higher trend inflation than would otherwise be the case, in order to stay further away from the zero bound. We could operate in other markets, which raises issues as to our ability to influence rates and raises the specter of credit allocation by the monetary authorities. Or we could accept somewhat longer and deeper recessions. None of these are particularly palatable. In my thoughts on this matter it has always seemed obvious that we should think about ways of resurrecting counter-cyclical fiscal policy. The Seidman and Lewis paper makes a specific proposal for such a policy; Auerbach and Hasset discuss the conceptual issues at stake in designing such a policy regime. I begin with Seidman and Lewis.

They propose the enactment of a one-sided Taylor rule for fiscal policy. The federal government would make transfer payments to all households geared by the shortfall of real GDP below potential. The authors show that, given the assumption that the MPC from such transfers is equal to other forms of income, and using the Fair econometric model, such a policy would mitigate recessions—even if monetary policy were also responding to the downturn by following a Taylor rule.

The authors partly deal with the issue of the difficulty of computing potential, and the somewhat related issue of responding to supply shocks, by showing that a modified version of their policy rule, limiting the fiscal response if inflation is above a target level, will still help to moderate recessions.

There isn't a great deal to say in the framework of the paper about the specific computed effects of the proposal. Of course, not everyone would accept the Fair model, but no doubt any other reasonably sensible model would yield similar results. Clearly, the discussion should be framed to bring out other considerations.

My comments fall into three areas: 1. Bread and butter practical obstacles to such a proposal. 2. Some comments on the history of anti-recessionary fiscal policy. 3. Thoughts, which the consumption and the monetary policy literature raise to me about the uncertainty of the efficacy of this type of proposal; which will lead into the Auerbach and Hasset paper.

1. Practical obstacles:

The proposal rests upon timely receipt of information (within no more than a quarter or so) about the state of the economy and the timely payment of transfers. As to the information about the economy, the <u>precise</u> state is never really known all that well. I am a bit uneasy about tasking BEA to come up with a quarterly GDP number that will be directly tied to federal expenditures, even if we give them six months to do it. Some federal expenditures and many private contracts are directly tied to the exact value of the CPI, and that dependence has probably hampered the usefulness of that series—its official history is frozen, and desirable technical changes can get snagged. Could a similar situation develop for GDP if the Seidman-Lewis proposal were enacted?

As to timely payment, the proposal assumes that the Treasury can readily find everybody it needs to send the checks to. That's obviously not the case; for an occasional event like last year's rebate returned checks are a problem that can be dealt with leisurely. If these checks are to be mailed every quarter during a recession problems will cascade (of course, there's the added problem of sending physical checks—I believe last year's rebates went out in three stages precisely because of the difficulty the Treasury had in producing such a vast volume of checks).

These problems can be dealt with. The transfer menu could be, say, a step function tied to ranges of GDP shortfall, rather than continuous. Instead of checks, perhaps the payments can be made as reduced withholding (though that misses the unemployed). Of course, the practical details do not affect the core of the proposal, but it would take some time, effort, and money, to iron them out.

2. History.

I'd like to make a few comments about the history of counter-cyclical fiscal policy and what it suggests to me about the whether there has been a gap that this type of automatic policy response may fill. The way I see the record, the US actually has tended to enact significant tax cuts during just about all the downturns of the last generation:

1970—the 1968 tax surcharge (which had been reduced to 5% in 1969) expired in June.

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[1] The views expressed here are those of the author and do not necessarily reflect the position of the Federal

1973-75—the famed $50 rebate, mailed in May 1975

1981-82—tax cut passed in July 1981 (the cyclical peak!), first 5% stage effective Oct 1981, second 10% stage effective July 1982.

1990—the exception. A tax increase was enacted during the recession. Possibly, though, the administrative decision to reduce withholding in early 1992 (a year after a very shallow trough) can be thought of as a countercyclical tax cut.

2001—tax cut passed in June; withholding changes in July and January, rebates mailed in the third quarter.

Of course, only the 1975 rebate was explicitly countercyclical by design. Nonetheless, the record surely suggests that hesitancy to cut taxes when the economy is weak is not a special problem in the US (the perceived weakness in the economy in 1981 and 2001, of course, probably helped spur the package of the supposedly structural long-term tax cuts in those years). The authors do emphasize that the 1975 rebate was not paid until May, which, as it turns out, was two months after the NBER trough. They also are concerned about the failure to enact another consumer tax cut last fall. I'm not sure the latter will look like a major policy error. Given the size of the fiscal and monetary stimulus already in place at that moment, the mere fact that the economy appeared to be declining (as measured by GDP, of course, it was growing) does not strike me as a slam-dunk case for another round of consumer tax cuts.

In any event, there appears to be no reluctance to pass tax cuts during periods of economic weakness in the U.S. Perhaps the cuts are not passed or not effective until after the NBER trough. But why is the first derivative of output so important? Providing stimulus while the economy is operating under potential—as it certainly was in the spring of 1975, for instance—is what countercyclical policy should do (of course, if the 1975 cut had not happened, maybe the recovery after March would not have been sustained). If the Seidman-Lewis proposal recognizes the need for a recognition lag of a quarter or two to compute the transfer, it doesn't strike me that there would be a remarkable difference from the historic policy lag.

3. Uncertainty about the effects.

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The most important thing to note here is the inherent uncertainty regarding the exact amount consumption would rise upon receipt of these transfers. The authors hang a lot on there being a reasonably stable and predictable MPC from this income type. Now why would this be so? I guess it has to be some sort of liquidity effect. But why should that be stable over time? The other possibility, which I have mentioned occasionally, is that the evidence for these "large" effects of transitory fiscal policy may reflect the difficulty households have in sorting out "permanent" from "transitory" policy changes. Given the many, many, changes in fiscal policy this may be understandable. What seems to economists to be a permanent tax change—like the 1982 tax cut enacted in 1981—and should be forecastable and anticipated in spending, may be regarded more suspiciously by consumers. What seems to be temporary may be regarded as permanent. For instance, in the case of a tax rebate, households might say to themselves, sure, they say the rebate is only for this year, but they wouldn't dare not to pass another one next year (or people might honestly think there will be one). But what seems to be a large, stable effect of legislated counter-cyclical tax changes may not prove to be the case for an automatic countercyclical transfer. Suppose people regard a countercyclical transfer as a permanent part of the tax code. As that knowledge sinks in, I hypothesize there will be a permanent upward shift in consumer spending (there will be less need for precautionary saving; moreover, if we can get beyond Ricardian equivalence arguments there will be a higher level of permanent income), but the payment of the transfer should have little effect on spending. Of course, there will still be the liquidity effects upon payment, but I contend they are uncertain and possibly small (for instance, the impact of the 1975 rebate, presumably paid out to a lot of liquidity-constrained housheholds seems to have been remarkably small).

Turning back to the issue of household confusion about policy, let's take a look at the 2001 rebate. Legally, this was not a replay of 1975. Receipt of the 1975 rebate had no consequences for your 1975 tax bill. The 2001 rebate was actually an advance refund of a cut in taxes (the introduction of the new 10% bracket) made retroactive for the full year. You got your $600, but you had to add it back when you calculated your final settlement this spring, which you saw when you filled out your return . So the 2001 rebate was actually a permanent tax cut for the year, paid in a lump sum. It should have had a bigger impact than 1975. Did it? It doesn't look like it did, judging by the behavior of personal saving upon its payments. Of course, the interruption to spending in the immediate aftermath of 9/11 will probably forever cloud the interpretation of this episode.

Finally, a comment or two about the VAT proposal. This sounds good, but an on-off VAT will presumably work by changing the timing of consumer purchases, not consumption. We probably understand the determinants of consumption better than the determinants of consumer spending, just as we probably understand the determinants of the demand for capital better than the determinants of investment spending (I am segueing to the Auerbach-Hassett paper now). So the distinction between the VAT and the income tax is probably firmer in theory than it would be in practice.

In conclusion, I would agree that enactment of a proposal like that of Seidman and Lewis would probably help to insure against a very severe demand-driven recession in a low inflation environment. I'd use a grain of salt on their numerical estimates—as I think most of us, probably including the authors, would—and, deep down, I really don't think that reluctance to use activist expansionary fiscal policy in a recession is a major concern in the U.S., but bringing these issues into the debate is extremely helpful.

Auerbach-Hasset

As I mentioned, it's possible that the uncertainty agents have about the course of fiscal policy has a significant bearing on our estimates of the effect of policy changes. This is, of course, the Lucas critique brought to fiscal policy. Last year, somebody reminded me that in the original Carnegie-Rochester Econometric Policy paper Bob Lucas talked about fiscal policy before turning to money.

The Auerbach and Hasset paper takes the uncertainty issue seriously. Recently yet another in the long series of temporary ITCs was enacted. Like the temporary transfers and VAT rebates proposed in the Seidman-Lewis paper, a temporary ITC seems like a good countercyclical tool—the government is in effect offering a temporary price break on the purchase of capital equipment.

Auerbach and Hasset devote much of their paper going through the analytical issues of the effect of a temporary ITC. First of all, the distinction between a temporary and permanent ITC is muted slightly once adjustment costs are factored in. I would put it simply, if not necessarily precisely accurately, a sale doesn't spur spending if you can't make it to the store in time!

The more significant issues involve uncertainty, both about the fundamental business climate over the life of the investment and about the policy environment. The authors note that it's quite difficult to speculate about what the effects of fundamental and policy uncertainty are on the level and cyclical nature of investment; the results depend critically on the nature of the uncertainty, the nature of adjustment costs (and the irreversibility of investment), and the uncertainty about the determinants of policy changes. They appear to believe that the on-off nature of temporary ITCs in the US have probably been somewhat destabilizing and possibly worked to the long-run detriment of capital spending. While the aggregate evidence on this is thin I think I defer to their judgement. A more difficult issue is whether this increased investment volatility is destabilizing to the economy as a whole—after all, by definition, countercyclical policy is volatile in some absolute sense!

Auerbach and Hasset also discuss countercyclical changes in personal taxes, and compute the size of the automatic stabilizers currently in the federal tax system. They correctly note that you don't measure the size of an automatic stabilizer by simply looking at how taxes change when income moves—ultimately you really have to get to the spending impact. I would add that in the current environment, even the traditional computations just looking at taxes are a bit suspect. I've had conversations with other forecasters who note with satisfaction the support that has been given to disposable income by declining taxes this year, and how it seems to be larger than that explainable by the legislated tax cuts, suggesting that automatic stabilizers have been surprisingly strong. However, much of that weakness in taxes appears to be due to things like the decline in capital gains realizations, and weakness in other forms of income of that type, some of which does not show up the NIPA measures. So it's not necessarily the case that the stabilizers have been strong,--perhaps a more comprehensive income measure has been weaker than the NIPA concept.

Turning back to the spending effects, unlike Seidman and Lewis, Auerbach and Hasset contend that any effect on consumer spending from explicit countercyclical policy is problematic, since it works through changing transitory income. From my perspective, this is a glass that is half empty and half full. When I'm in my forecasting mode I fret about realized income and its effect on spending; when I'm talking to a group like this I'm more inclined to share Alan's and Kevin's skepticism. Accepting that there is a nonsignificant transitory income effect is not the same as acknowledging we have a good grasp on its size—especially as how it may vary over circumstances and time (this is the point I have been trying to raise about the wealth effect for a number of years). Alan and Kevin are looking at the issue of countercyclical policy from a normative viewpoint: do we need to know more to design optimal countercyclical policy? They argue we need to know a lot more, especially when we take policy uncertainty and its effect on economic agents into account. Seidman and Lewis are doing positive policy analysis: Can we come up with a policy rule that is likely to help smooth out the cycle, and is very unlikely to be destabilizing? They believe, and I can more or less agree with them, that their rule could help. However, if we look toward the future I think serious analysis of the sort Auerbach and Hasset advocate could yield even better policies.