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Comment

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This paper argues that countercyclical fiscal policy can smooth business cycles and in doing so alleviate liquidity constraints and thereby increase growth. The authors present empirical work that they interpret as saying that this opportunity is most pronounced in countries where financial development is low. I am sympathetic to the idea that time-varying liquidity constraints can be an important aggregate problem. But, for reasons explained in the following, I doubt that the evidence in this paper will convince skeptics that this is the case. I begin by reviewing some conceptual points and then spend most of my remarks discussing their empirical work.

1 Conceptual Issues

The theoretical foundations for the view that recessions are times when liquidity constraints become more binding are solid. Likewise, there is evidence such as that provided by Aghion, Angeletos, Banerjee, and Manova (2006) showing that liquidity constraints are more of a problem in countries where financial development is low. Additionally, there is a large literature suggesting that firm-level investment is sensitive to cash flow, cash on hand, and collateral values (even after controlling for investment opportunities). Thus many of the ingredients of the story in this paper are well-established. The new claim in this paper is that fiscal policy can be used to smooth business cycles and thus mitigate constraints and enhance growth. The authors go so far as to argue that “growth in the EMU countries could be fostered if the budget deficit became more counter-cyclical.” (p. 272, this volume)

There are several reasons to be skeptical about this policy advice. Although only mentioned in a footnote by the authors, perhaps the most

commonly cited argument in the other direction is the crowding out argument. It has been routinely alleged in the U.S. debate over fiscal policy (e.g., Rubin and Weisberg [2004]) that higher government borrowing raises interest rates and reduces private investment. While I agree with Engen and Hubbard (2004) that in the United States the actual evidence for this proposition is weak, it is possible that in countries with underdeveloped financial markets this mechanism could be operative.

More importantly, the presumption in the policy advice is that fiscal policy is nimble enough to be used to fine tune the business cycle. Auerbach (2002, 144) explores this possibility and concludes "there is little evidence that discretionary fiscal policy has played an important stabilization role during recent decades, both because of the potential weakness of its effects and because some of its effects [with respect to investment] have been poorly timed." Feldstein (2002, 151), in his published comment on Auerbach's paper, writes "although Auerbach's evidence is innovative and impressive, he recognizes that it confirms views that are now well-established and widely held in the profession. Even economists who did not consider themselves to be monetarists came to this conclusion on the basis of their own research." I concur with Feldstein that macroeconomists as a group are suspicious of fine-tuning arguments in general and using fiscal policy in particular.

Especially in this context, the argument for managing the problem using aggregate budget policy seems rather weak. If the root problem is fluctuating borrowing capacity, then instead of the blunt tool of extra spending, one can imagine many more direct government programs to address this concern.

Finally, the mechanism that the authors seek to uncover is necessarily subtle. As indicated in their figure 4.1, they imagine a scenario in which budget policy potentially affects trend growth. Yet the regressions that they run start by looking at the connection between deficit spending and the output *gap*. Given the filtering that is done to remove the trend, the regressions that are proposed are uninformative about the hypothesis that they seek to test. Moreover, it is not clear how panel data regressions of this sort could overcome this difficulty.

Regardless of what one thinks about the theoretical possibilities regarding countercyclical deficit spending in comparison to other policy options, the main contribution of this paper is empirical. Therefore I will spend the balance of my remarks reviewing the regression evidence in the paper.

2 Empirical Evidence

The authors adopt a two-step procedure for building their case. In the first stage, the increase in debt (relative to GDP) is regressed on the GDP gap. The coefficient on the gap is modeled as a time-varying parameter (that I will call the countercyclicality coefficients). The authors graph the countercyclicality coefficients from their three estimation measures for the United States in their figure 4.2. My figure 4C2.1 shows the underlying data that were used to derive their countercyclicality coefficients.

Without any detailed knowledge of American fiscal policy one can see that the period from 1993 to 2000 stands out. The authors' preferred auto-regressive (AR) procedure forces a smooth correlation but I am not convinced that doing so is an appropriate way to describe history. Rather I view the 1993–2000 period as an outlier and the periods before and after as having fairly similar fiscal regimes. One way to demonstrate this is to run a fixed-coefficient version of their first-stage regression from 1965 to 1992 and then simulate the subsequent years. The fitted values from this exercise are shown in figure 4C2.2. One can see that while this equation describes the 1993–2000 period badly, it fits the subsequent years quite well. Accordingly, I am skeptical of the authors' restriction

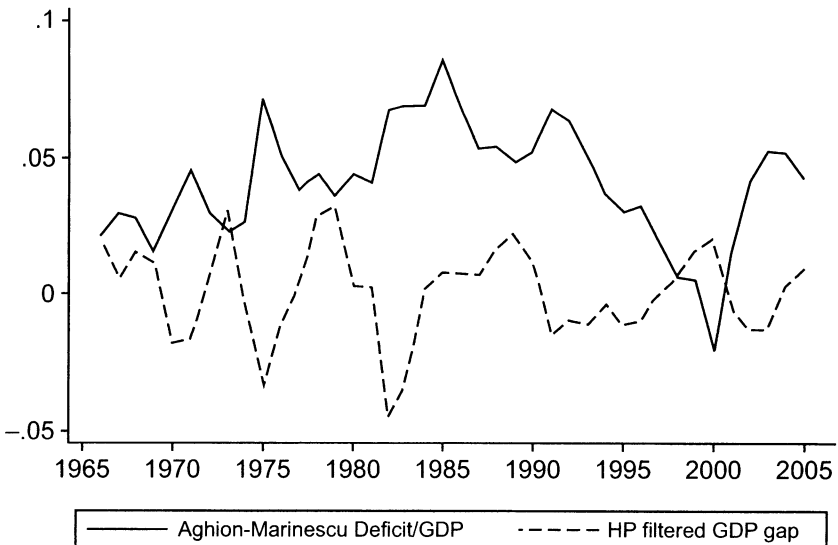


Figure 4C2.1
Budget Deficit and the GDP Gap for the United States

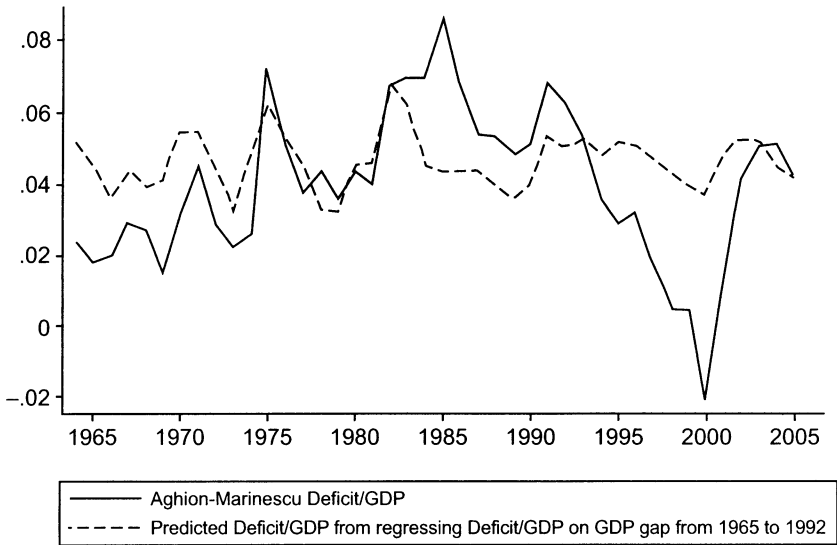


Figure 4C2.2
Predicted and Actual Budget Deficit for the United States

that forces the countercyclicality coefficients to smoothly vary over time.

The second step in the procedure relates per capita growth to the countercyclicality coefficients, (the lag of) the level of private credit relative to GDP, and the countercyclicality coefficients interacted with the lag of private credit to GDP, as well as a number of other controls. There are several questionable aspects of this regression. While regressions of this sort are common in the literature on long-run growth, they seem ill-suited to explaining short-term fluctuations. Surely the year-to-year fluctuations in the ratio of credit to GDP are endogenous to growth, and lagging the variable by one year hardly helps. Most of the other controls are also jointly determined with growth.

Another issue is that in the second-step regression the countercyclicality coefficients are transformed to account for the fact that they are generated regressors. While this is a reasonable econometric concern, in this application the effect is remarkably important. For instance, figure 4C2.3 (which matches figure 4.3 in the paper) creates the impression that the identification in the second step comes from sustained large differences between fiscal policy in different countries (in this case, the United

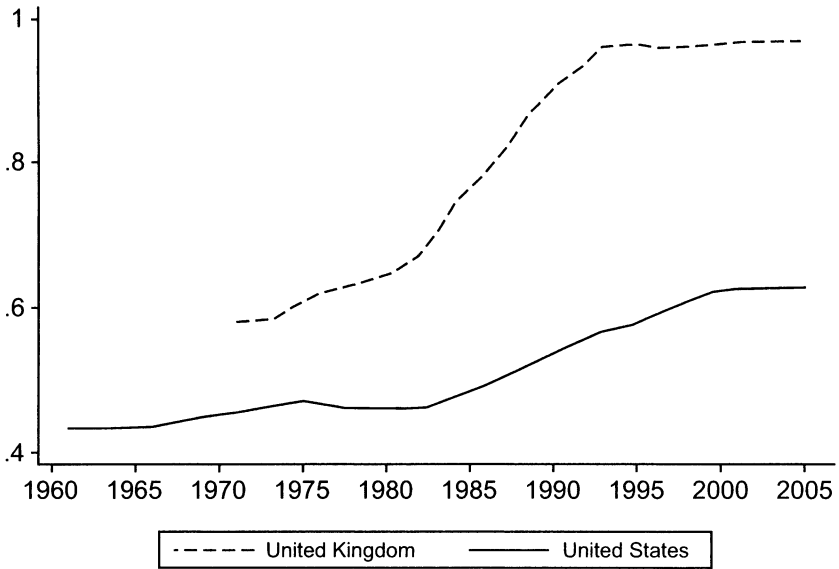


Figure 4C2.3
Aghion and Marinescu Counter-cyclicality Coefficients

States and United Kingdom). But the AR(1) estimation procedure in the first step will work best when there are many years of data. Thus, the sampling uncertainty is extremely high at the beginning and ends of the sample. Figure 4C2.4 shows the weights for the counter-cyclicality coefficients from figure 4C2.3; the same inverted-U pattern holds for all the other countries that are not shown in this graph. Consequently, the variables that are fed into the growth regression for the United States and United Kingdom are shown in figure 4C2.5.

Once the first-step estimation is accounted for, the differences between the fiscal policies of the United States and United Kingdom are much less pronounced than implied by figure 4C2.3. Judging from figure 4C2.5 we can see that the explanatory power of the counter-cyclicality coefficients will depend mostly on the income dynamics toward the middle of the sample. It seems likely to me that what the interaction coefficients in these regressions are telling us is that OECD countries with less developed financial systems grew faster over these years. Given the convergence within the European Union during this time, this would not be surprising. In any event, the connection to the cyclicity of budget policy is murky.

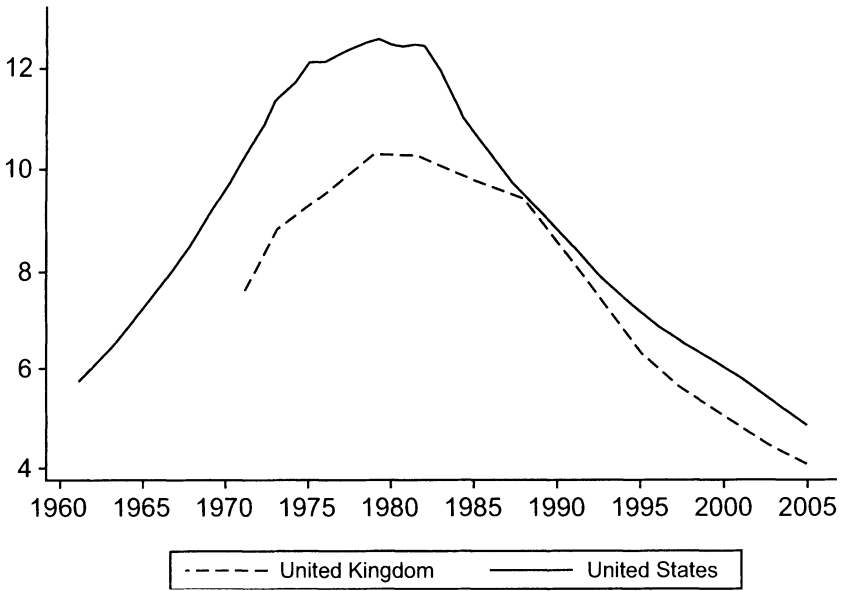


Figure 4C2.4
 Estimated Precisions of the Aghion-Marinescu Countercyclical Coefficients

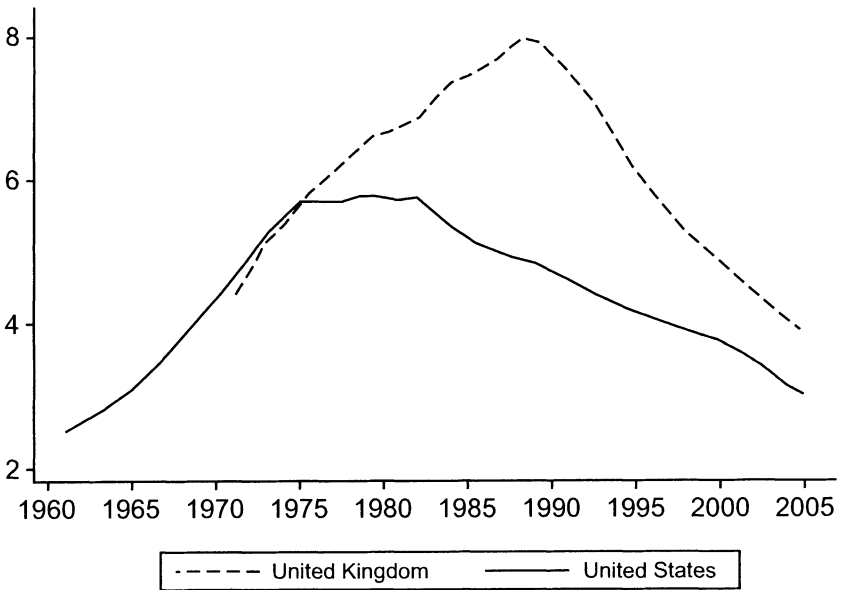


Figure 4C2.5
 Transferred Countercyclical Coefficients Used in Aghion-Marinescu Growth Regressions

3 Conclusion

The idea that time-varying liquidity constraints are macroeconomically relevant is plausible. The authors make the bold claim that these effects are so powerful that aggregate fiscal policy should be aimed at alleviating them. I am skeptical that this would be the best way to address this potential problem and do not think the empirical work in this paper will convince other skeptics.

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