

# COMMENTARY

Automatic stabilizers are a very old idea. Indeed, they are a very old, very Keynesian, idea. At the same time, they fit well with the current mistrust of discretionary policy and the focus on policy rules. Yet in the last ten years, they have not been discussed much by academics. For all of these reasons, it is indeed a good time to revisit the issues. Are automatic stabilizers an old and good idea? Or an old and bad one? Should we use them more? Less? Differently? That is why the paper by Darrel Cohen and Glenn Follette is so useful. It should be seen as a start: the paper raises more questions than it answers. It is full of interesting bits and pieces, although its most lasting contribution will surely be the most careful construction to date of the elasticity of taxes and transfers to output.

I shall use the wide-ranging structure of the paper as an excuse to also make a number of wide-ranging points.

## 1. SHOULD WE EXPECT AUTOMATIC STABILIZERS TO WORK, THAT IS, TO STABILIZE?

This question is clearly a special case of a more general question: Does fiscal policy, defined here as the intertemporal reallocation of taxes, matter?

The standard discussion typically starts from the proposition of Ricardian equivalence, the proposition that the

timing of taxes does not matter because, given spending, taxes will have to be paid sooner or later. Some of us stop here. Most of us go on to list a number of reasons why Ricardian equivalence is likely to fail:

- **Death:** Current taxpayers will not be there to pay when taxes are adjusted in the future.
- **Myopia:** The adjustment of taxes may be too far in the future to even think about.
- **Credit constraints:** If some people cannot borrow against future income, then changing taxes today will lead them to change consumption today.
- **Insurance:** To the extent that taxes are proportional, rather than lump-sum, they will reduce the uncertainty associated with labor income and affect consumption.

Which of these factors is most relevant? The answer is likely to depend on the fiscal experiment being conducted or examined.

Take, for example, the debate over the effects of Social Security on saving and capital accumulation, or the discussion of the effects of the increase in deficits in the 1980s or of the large fiscal consolidation in the 1990s. In that case, we are dealing with long-lasting changes in the path of taxes. Factors 1 and 2, death and myopia, are almost surely central to the issue. But the answer is likely to be different for automatic stabilizers. Recessions rarely last more than a year or two: lower taxes during a recession are likely to be offset by higher taxes only a

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few years down the line. Thus, factors 3 and 4 are likely to be the most important ones. How many households find themselves credit-constrained is likely to be the critical factor.

This has an important implication. What we learn about the effects of fiscal policy in one case (the effect of Social Security reform or of fiscal consolidations on consumption) may not be sufficient to give us the information we need to understand the other case (the effect of automatic stabilizers on consumption). Given the recent progress both in developing models of consumption that allow for credit constraints and precautionary saving, and in fitting them to panel data, we can probably make progress here, at little extra cost. This may help us to predict what will happen to automatic stabilizers as changes in financial markets continue to modify the nature of credit constraints faced by consumers.

## 2. DO AUTOMATIC STABILIZERS STABILIZE?

This is again part of a more general question: Does fiscal policy, again defined here as an intertemporal reallocation of taxes, affect output? Let me start with the more general question, and then return to automatic stabilizers.

The macroeconomic models we have say yes. And the FRB/US model is no exception. But there is a clear sense in which they largely assume the answer. Nearly always, they rule out Ricardian equivalence in their specification of the consumption function. (In the FRB/US model, future labor income net of taxes is assumed to be discounted at a high rate, independent of the nature of the income being discounted—

the tax part or the labor income part. This high discount rate, higher than the rate paid by the government on government bonds, implies an effect of an intertemporal reallocation of taxes on consumption.) And nominal rigidities imply that shifts in aggregate demand translate into shifts in output. I happen to believe these two assumptions (the failure of Ricardian equivalence and the effects of aggregate demand on output). However, to somebody who is skeptical of either or both, showing the results of a model that takes them as given is hardly proof. One wants to see evidence not based on these assumptions. I will briefly discuss two such pieces of evidence.

I discuss the first mostly for fun, and a bit for provocation. An implication of Ricardian equivalence is that, other things equal, exogenous shifts in public saving should be reflected one-for-one in shifts in private saving. This suggests looking at the joint evolution of the two. Because shifts in public saving are not exogenous, and because other things are not equal, this can only be suggestive, but it is still worth doing. The relationship between U.S. private and public saving since 1970 is shown in Chart 1; the relationship between U.S. personal and public saving is shown in Chart 2. One is struck at how good the inverse relationship is, especially in recent years: as fiscal deficits have vanished, so has personal saving. The coefficients in simple regressions of private or personal saving on public saving since 1980 are -0.68 and -0.82, respectively. Can one look at these charts and still not believe in Ricardian equivalence? I think so. I believe both evolutions are caused by higher growth—actual growth, which leads to an automatic improvement in the budget, and expected growth, which leads to an increase in wealth and a decrease in saving. But I would feel better if I had done the algebra and convinced myself that this explanation can indeed account for the data.

CHART 1  
U.S. Private and Public Saving, 1970-99

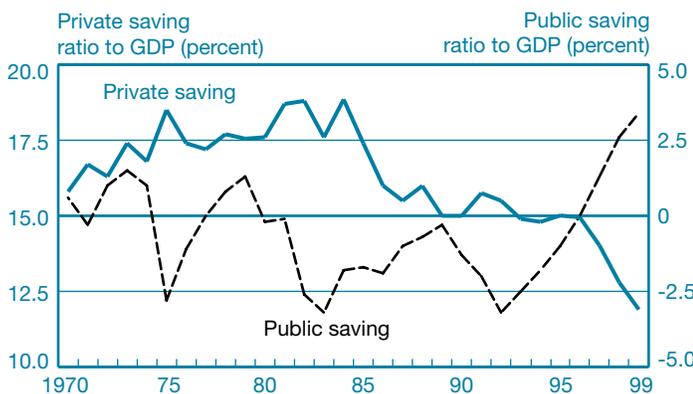
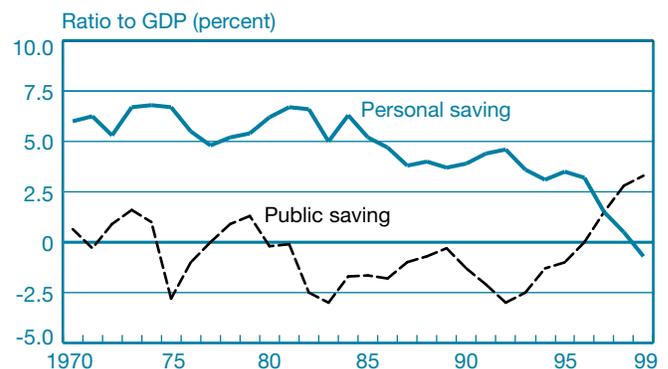


CHART 2  
U.S. Personal and Public Saving, 1970-99



The second piece of evidence I take more seriously, for the simple reason that it comes from my own research. In work with Roberto Perotti (1999), we have done for fiscal policy what had been done earlier by Ben Bernanke and others for monetary policy, that is, we have estimated the dynamic effects of exogenous changes in taxes on activity. This is actually easier to do for fiscal than for monetary policy. Given information about the tax structure (very much the same information used in the last part of the Cohen-Follette paper to construct the high-employment budget), one can easily decompose, for each quarter, the part of the change in taxes that is a response to activity and the part that is not. One can then trace the effects of the second part on output and on taxes themselves. The basic results, taken from Blanchard and Perotti (1999), are reproduced in Chart 3. Panel A shows that a change in taxes of, say, one dollar, has an effect on taxes that lasts for about six quarters, and an effect on output that builds up before going away. The multiplier is around 1. In general, our paper finds strong but not overwhelming evidence that fiscal policy indeed affects output, typically with a multiplier around 1 (as in the FRB/US simulations with a Taylor rule reported in the Cohen-Follette paper).

What about automatic stabilizers? As discussed in my first point, the fact that one type of fiscal policy has an effect on output does not imply that automatic stabilizers will have the same impact, or indeed any impact at all.

To learn more, one can think of exploiting either the change in automatic stabilizers over time, or over countries.

In the United States, as in most countries, the share of taxes and transfers in GDP has increased over time, suggesting an increase in automatic stabilizers. And indeed, the variance of

output has decreased over time as well. But given the number of other factors that have changed, this seems like a weak reed to rely on.

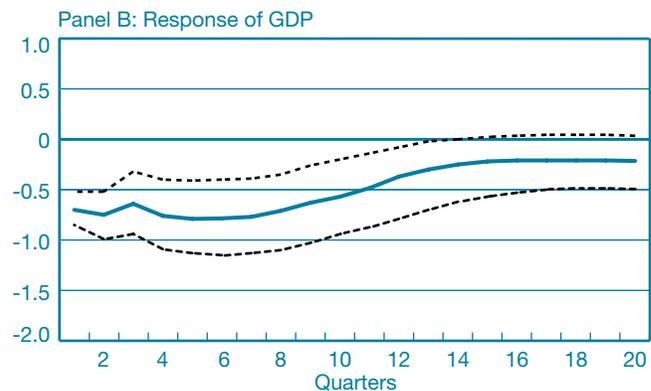
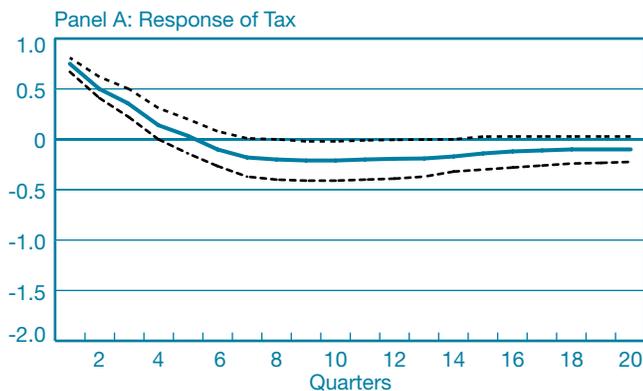
Some researchers have looked at the cross-sectional evidence, either across countries or across U.S. states. Two scatterplots of the variance of output growth versus the share of government spending in GDP, taken from Fatas and Mihov (1999), are shown in Chart 4. Panel A presents the evidence across OECD countries. Panel B presents the evidence across U.S. states. There indeed appears to be an inverse relationship in both cases. From the results in Fatas and Mihov, the relationship appears robust to a number of obvious controls. This offers suggestive evidence that automatic stabilizers indeed stabilize. (The estimated relationship implies, however, an effect of automatic stabilizers on the variance of output much larger than the numbers implied by the FRB/US model simulations presented in the Cohen-Follette paper. This makes the evidence a bit suspicious.)

### 3. IF WE ACCEPT THAT THE FRB/US MODEL IS A GOOD REPRESENTATION OF REALITY, WHAT DO WE LEARN?

First, we learn that the direct effect of activity on taxes and transfers is substantial. If GDP goes up by 1 percent, the increase in taxes and the decrease in transfers lead to an improvement in the budget—equivalently an increase in the net income of firms and consumers—of about 0.3 percent of GDP.

CHART 3

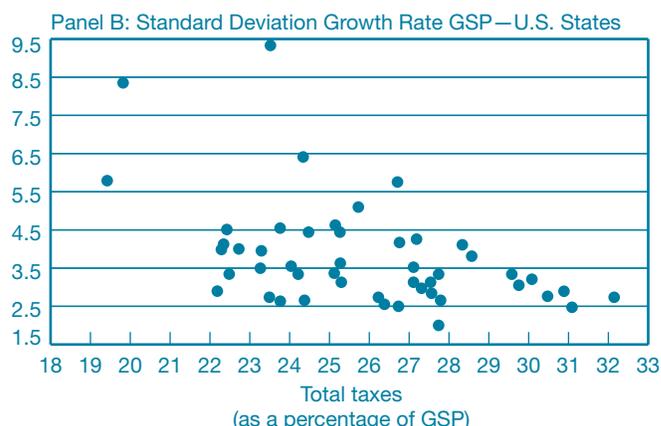
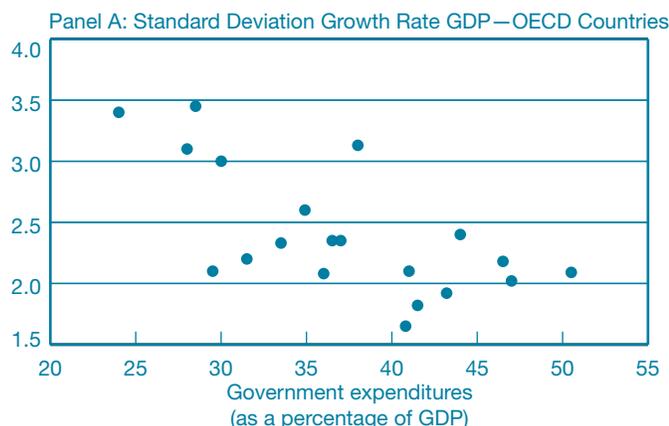
#### Responses of Tax and GDP, Deterministic Trend



Source: Blanchard and Perotti (1999).

CHART 4

### Volatility and Government Size



Source: Fatas and Mihov (1999).

Second, and somewhat surprisingly, we learn that this large effect on net income leads to only a small reduction in the multiplier, and thus a small reduction in the variance of GDP. In Table 1 of the Cohen and Follette paper, for example, for a given real interest rate (looking just at the IS part of the FRB/US model), the multiplier decreases only from 1.35 (after four quarters) to 1.23. Should we be surprised? Yes and no.

- The part that is surprising is how small the multiplier is—even when the interest rate is kept constant, that is, when we are just looking at the IS relationship. A multiplier of 1.35 corresponds to a marginal propensity to spend out of changes in net income (consumption plus investment) of  $0.35/1.35 = 0.26$ . I am not sure that I understand why it is so low. True, in the FRB/US model, only 10 percent of the consumers simply consume their income. The others are more forward-looking. Given that the simulation shows that income goes up for quite some time (the multiplier effect is still above 1 after eight quarters), one would expect them to respond to the change in income more strongly than they appear to do. The same question applies to firms. It would be useful here for the authors to look a bit more into the entrails of the model and explain to us what is going on.
- The part that is not surprising is the small effect of automatic stabilizers on the multiplier. Given the small marginal propensity to spend, this is exactly what we would expect. Write the IS as:

$$Y = .26(Y - T) + X,$$

where  $X$  is autonomous spending. If  $T$  is fixed, the multiplier is 1.35. If  $T = .3Y$ , as Cohen and Follette document, then the

multiplier is 1.22, exactly as shown in Table 1. In other words, given the small marginal propensity to spend, the effect of automatic stabilizers on the multiplier has to be small as well.

#### 4. WHERE DO WE GO FROM HERE?

Based on the findings of the paper, should we use automatic stabilizers less, more, differently?

The distinction made in the paper between aggregate supply and aggregate demand shocks is nice, and I had not seen it before. The argument is not exactly right, however. The right one goes as follows. With respect to aggregate demand shocks, automatic stabilizers stabilize, and this is good. With respect to aggregate supply shocks, automatic stabilizers also stabilize, but this is not good: they do not allow for the adjustment of output that would be desirable in this case. Simple algebra will help here. Consider a textbook aggregate demand–aggregate supply model:

$$AD: y = c(-p + e_d)$$

$$AS: p = y + e_s.$$

All variables are in logs,  $y$  and  $p$  are output and the price level, and  $e_d$  and  $e_s$  are demand and supply shocks. A higher price level decreases the demand for goods (through the decrease in real money balances). Higher output leads to a higher price level. Stronger automatic stabilizers can be thought of as decreasing the coefficient  $c$  in the  $AD$  relationship.

Then, output is given by:

$$y = \frac{c}{1+c}(e_d - e_s).$$

If there were no nominal rigidities, output would instead be given by:

$$y^* = -e_s.$$

So that the output gap, the difference between actual output and equilibrium output, is given by:

$$y - y^* = \frac{1}{1+c}(c e_d + e_s).$$

This algebra yields two results. Higher automatic stabilizers (lower  $c$ ) stabilize output both with respect to demand shocks and with respect to supply shocks. But with respect to supply shocks, output should move and, in effect, automatic stabilizers prevent it from moving enough. Looking at the gap, we see that automatic stabilizers reduce the gap with respect to demand shocks, but increase it with respect to supply shocks.

This suggests that it may be worth thinking about automatic stabilizers that do well with respect to supply shocks. For example, a proportional tax on the price of oil is a useful automatic stabilizer in this context. It increases taxes in response to adverse supply shocks, in this case an increase in the price of oil. There are, however, many types of supply shocks other than oil prices. Can we think of automatic stabilizers that would work with respect to supply shocks in general?

Another question raised by the Cohen-Follette paper is an obvious one. If automatic stabilizers play a useful role, why should we be satisfied with the degree of stabilization implied by existing tax and transfer rules? Could we increase the degree of automatic stabilization without introducing other distortions? A few years back, John Taylor revisited the role of automatic investment tax credits in this context. It may be time to revisit this and other possible tax rules again.

## REFERENCES

*Blanchard, Olivier, and Roberto Perotti.* 1999. "An Empirical Characterization of the Dynamic Effects of Changes in Government Spending and Taxes on Output." NBER Working Paper no. 7269, July.

*Fatas, Antonio, and Ilian Mihov.* 1999. "Government Size and Automatic Stabilizers: International and Intranational Evidence." Centre for Economic Policy Research Discussion Paper no. 2259, October.

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