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Comments

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A Public Choice Explanation for the Budget Surplus

For two decades prior to 1990, large budget deficits and rapidly growing federal expenditures were the norm. Public choice economists explained this pattern with models in which special interest pressure for more spending, combined with rationally ignorant voters and shortsighted politicians, led to a situation in which deficit financing was more attractive than current taxes. In the 1990s, the budget situation changed dramatically. Expenditure growth slowed, and the budget began running an ongoing surplus for the first time in almost 50 years. The challenge for economists is to understand the recent turn of events.

A recent article in this journal by Alberto Alesina (Summer 2000, pp. 3–19) typifies the academic work that has been done to explain the recent budget surplus. Such work has often involved little more than factual accounting discussions of growing revenues and slower expenditure growth. For example, Alesina (p. 6) concludes: "The current budget surpluses have been the result of: 1) the exceptional performance of the American economy since 1991, a performance which has generated a surge of tax revenues; 2) low interest rates and 3) a large reduction in defense spending as a share of GDP."

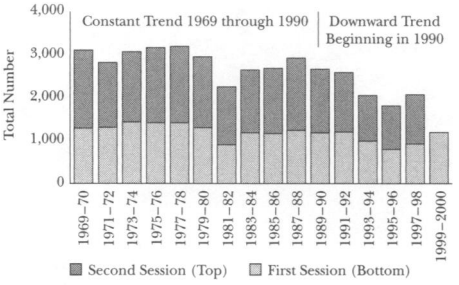
Nobody can disagree with the observation that the current surplus is *mathematically* the result of faster revenue growth combined with slower expenditure growth. One can always look historically at the data and see which categories of spending have fallen, or note that revenue has grown faster than usual, but such observations reveal nothing about the underlying process that generates government spending. *Never before in recent history has the federal government had trouble finding how to spend additional revenues, nor how to reallocate money when one program is less draining than usual on the budget.*

While it is true that the lower demands on national defense did free up some resources and that faster revenue growth has provided more revenue, neither of these events are uncommon historically speaking, and in the past politicians have easily found ways to reallocate these moneys. Why not this time?

More rapid revenue growth is explained by the unexpectedly robust economy, so the deeper puzzle is why expenditure growth slowed. The average annual real growth of federal outlays between 1970 and 1990 was 3.3 percent, but since 1991, the average annual real growth of federal outlays has fallen to 1.2 percent, or about one-third of its previous rate of increase. In fact, had expenditure growth maintained its pre-1991 rate, the federal budget would not have run a surplus to date. The key issue is why politicians have not found it attractive to spend the additional tax revenue (and the money freed up from other programs) elsewhere. Why has this slowdown in expenditure growth been politically optimal?

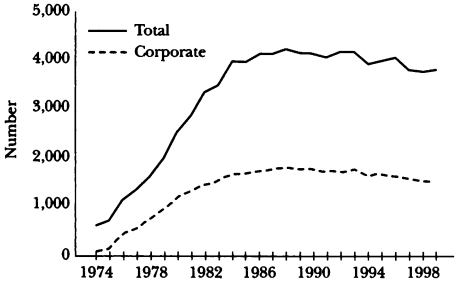
Previous literature on government expenditure growth has focused on how interest groups affect government spending (Holcombe, 1999; Husted and Kenny, 1997). The point of my comment is to suggest that the level of interest group activity may have fallen in the 1990s, which in turn is partially responsible for the decreased pressure on the expenditure side of the budget and thus for the budget surplus.

Figure 1
Measures Enacted by Congress



Note: For 1999–2000, only first session data available.
Source: Statistical Abstract of the United States.

Figure 2
Number of Federal PACs



Source: Federal Election Commission.

Interest group activity manifests itself throughout the political process. Political action committees (PACs), for example, spend millions of dollars each year lobbying, donating to political campaigns, and providing legislators with free trips, meals and recreation. In return, legislators draft new legislation, sponsor bills and vote for bills that may be favorable to the interest groups. Change in interest group activity will thus become visible in the measured output of legislation. Figure 1 shows the number of measures enacted by Congress (during both sessions) since 1970. Beginning around 1990, there has been a downward trend in the number of measures enacted by Congress. Thus, not only has expenditure growth slowed, but so also has the production of new legislation. This same trend is also visible in data on the number of measures introduced before Congress.

A more specific look at interest group activity can be obtained using data on registered federal PACs and PAC contributions. Figure 2 shows data on the total number of PACs and also shows individually the single largest subcategory, corporate PACs. After rapid growth throughout the 1970s and early 1980s, the total number of registered PACs remained fairly constant throughout the late 1980s and actually began decreasing in the 1990s. This same pattern is visible in PAC contributions to federal candidates over the same period. These data suggest that interest group activity was rapidly growing prior to the mid-1980s, but began slowing down and actually decreasing throughout the 1990s. Again, this reduction in interest group activity is consistent with less pressure on the expenditure side of the federal government budget during the past decade.

The evidence is certainly suggestive, although by no means conclusive, that recent reductions in the size and growth of interest group activity have led to a substantial reduction in the pressure on expenditure growth and in turn have contributed to the recent budget surplus. An intriguing task for public choice scholars is to see if they can further document and explain this trend. Do other measures of interest group activity support the hypothesis that interest group pressure declined in the 1990s? If interest group activity has slowed down, what are the possible reasons? Are there other consequences of this decrease beyond the slower growth of expenditures? Public choice scholars offered a plausible explanation for the rapid growth in government expenditures in the 1970s and 1980s, rooted in the dynamics of interest groups in the political process. Now they need to offer public choice explanations for the recent changes in the budget situation.

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References

→ **Holcombe, Randall G.** 1999. "Veterans Interests and the Transition to Government Growth: 1870–1915." *Public Choice.* June, 99:3-4, pp. 311–26.

→ **Husted, Thomas A. and Lawrence W. Kenny.** 1997. "The Effect of the Expansion of the Voting

Franchise on the Size of Government." *Journal of Political Economy*. February, 105:1, pp. 54–82.

Response from Alberto Alesina

Professor Sobel points out a reduction in PAC activity that leads him to conclude, in a rather provocative way, that Congress does not know how to spend revenues.

In order to argue that the alleged reduction in PACs' activities is the primary cause of the surplus, one should look carefully at the behavior of public expenditures, something that this note does not do. As I wrote in my original piece, the reduction of government outlays as a fraction of GDP (the most appropriate measure) is totally due to the reduction of the share of spending on defense. As pointed out in Table 1 (p. 7) of my paper, discretionary domestic spending as a share of GDP was unchanged between 1988 and 1998, and entitlements actually increased 1 percentage point of GDP. When Professor Sobel writes that expenditure growth slowed in the 1990s, he should be more clear about in which sector.

I am not exactly sure where the disagreement is. Professor Sobel seems to suggest that as an economist, I do not have enough sensitivity about the political process. Anyone who has read my piece would be surprised by this view. Bringing in additional data on PAC activity (with the important caveats discussed in the previous paragraph) is an interesting point. Incidentally, he does not tell us why PAC activity went down.

Finally, the tone of his first paragraph wants to cast "public choice economists" against the others, in this case myself, who seem to be interested in statements that are mathematically correct but uninteresting. I have never understood these labels and the view of "public choice economists" (assuming that I knew how to identify them) versus the others seems a bit obsolete.

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Revolutions in Economic Thought

In the Winter 2001 issue, Mark Blaug (pp. 145–164) argued in a spirited and—in my opinion—important article for a recognition of the desirability and necessity of studying the history of economic thought. I am in complete agreement with his arguments, which stress the importance of such studies not only for a better understanding of earlier writers, but also because of the frequent value in current theoretical discussions and disputes.

But a good intention can be endangered when one gets carried away too far by one's arguments. I think this danger is present when Blaug criticizes traditional accounts of the scientific significance of certain "revolutions," like the "marginal revolution," because there were important forerunners long before Jevons, Menger and Walras such as, for instance, Cournot, Dupuit, Thünen and Gossen, who had already very clear ideas about marginal analysis. The neglect of these earlier writers might cause a neglect of some possible additional viewpoints, but a main consequence is—in Blaug's view—that one gets a wrong impression of a "revolution" around 1870 caused by three well-known "fathers" of the marginal school. Looking at the forerunners, he writes (p. 159), "destroys the usual history of economic thought textbook account of the Marginal Revolution as a curiously isolated event in Manchester, Vienna and Lausanne." In the same vein, one could of course "destroy" also other revolutions: for example, the "monopolistic competition revolution" of Chamberlin and Robinson, who—among others—had a forerunner in Alfred Marshall; or the "Keynesian Revolution," where one can find—again among many others, and Kalecki, in particular—an astonishingly good account of the multiplier in a paper by a forgotten Danish economist N.A.L. Johannsen ("Geschäftliche Depressionen"), published as early as 1913.

The problem, of course, is that practically *every* fundamental idea is not fully born at a certain moment. There are always forerunners. To catch the attention of a broader section of the scientific community and to cause a major paradigmatic turnabout requires several factors. There must already be some forerunners so that the basis for a deeper understanding is "in the air." The time must be suitable for a wide preparedness to take up a new theory. Last but not least, to cause a "revolution," the idea must be presented in a suitable form and with wide ramifications that makes it suddenly visible to a wide circle of the scientific community.

Looked at in this sense, the above-mentioned "revolutions" were indeed revolutions, and their well-known authors can be properly called the originators of these revolutions. This may be sad and unfair to the forerunners (poor Gossen suffered badly from being neglected), and the history of economic thought can and should con-