

Book Review

Fiscal Policy: Lessons From Economic Research, Alan Auerbach, ed. MIT Press, 1997

Alan Auerbach's volume *Fiscal Policy* is a superb overview of the state of public economics in the 1990s. There are eight essays discussing various sub-fields written by leading academic experts. For the most part, the articles are written at a policy maker's level, but graduate students and specialists will also find the works highly interesting.

The book makes clear at the outset that the papers are not meant to be dispassionate surveys of the literature. Instead it asks leading scholars in each area to evaluate the existing literature and give their opinions and commentary. This approach makes the discussants' comments integral to the book since they present counter opinions to the authors'. That said, most of the pieces do a good job laying out the existing literature despite the disclaimer.

The book is laid out two articles in each of four general areas: institutions (articles on federalism and on budget rules), expenditure policy (welfare spending and health), personal savings (privatizing Social Security and savings tax incentives), and corporate taxation (investment and multinationals).

Since the book overviews much of the field of public economics it includes too much material to review each of the papers individually. Instead, I do three things. First, I discuss what the subjects chosen for the book say about the field of public economics. Second, I summarize what I consider to be a major theme running throughout the papers in the volume, namely the ongoing attempt to distinguish between impacts of government policy which are real and permanent from those which are accounting changes or temporary shifts. Finally, I look at three of the chapters in particular to emphasize what they suggest we know about various subjects and where research can go from here.

The transformation of public finance

Generally the centerpiece of public economics has been the study of how marginal tax rates affect economic behavior, particularly in response to the tax bills of the 1980s. In the last decade, though, there has been a dramatic shift in emphasis in the field toward non-tax topics such as welfare, health, and social security or to the evaluation of particular tax incentives like savings programs or investment subsidies rather. Although there could have been a chapter in *Fiscal Policy* on the effect of marginal rates on reported income--a literature which is still very active, the volume is correct to portray the main focus of public economics as drifting away from traditional tax considerations.¹

I think there are two reasons for this. The first is simply that there has been more policy "action" on issues of spending, unfunded mandates, and so on, in the 1990s than on tax rates. Just as the interaction of inflation with the tax code became a major issue in the 1970s with the rise of inflation, this move away from the study of marginal tax rates should be expected. A less obvious issue, though, and one I believe to be no less important is that public economics is an overwhelmingly empirically oriented field and the 1990s have witnessed major explosions in the availability of micro data sets which can be

¹ For critical surveys of the existing literature see Slemrod (1996) or Goolsbee (1998a).

used for research, reducing the relative payoff to the traditional research program of “old school” public finance which used tax return data to look for the effect of tax rates on behavior.

The chapters in the volume make this clear. In almost every field presented the important breakthroughs have come directly from the use of data much more detailed than one can find in a tax return including welfare, savings, health, investment, and multinationals. *Fiscal Policy* does a great job showing how rich the results can be when good data is available.

Contemporary Public Economics and the Slemrod Critique

One prevailing theme of the volume, surprising in its ubiquity, is the ongoing attempt in almost every field of public economics to distinguish permanent, real responses to government policy from temporary shifts and accounting changes. I call this the Slemrod critique due to his consistent raising of this issue in the context of evaluating tax policy (Slemrod, 1992; 1994; 1995).

The Slemrod critique is obviously central to the discussion by James Hines of international investment where the literature has found dramatic accounting and financing responses to tax rates but usually much less response of real behavior. Likewise for the chapter by Doug Bernheim on personal savings incentives where the literature centers on determining whether savings incentives generate new savings or simply lead individuals to transfer assets from other accounts, the chapter by Hillary Hoynes on welfare spending where the labor supply impact of welfare policies must deal with the issue of unreported work, or the chapter by James Poterba evaluating the evidence of whether budget rules have real effects on spending or simply accounting chicanery.

The Slemrod critique, however, is no less important for the other issues. The chapter by Larry Kotlikoff discussing the privatization of social security emphasizes that we must be clear about what circumstances will lead privatization to have effects on savings rates, etc... as opposed to just reclassifying assets as private or public with little effect on the real economy. The chapter on investment by Kevin Hassett and Glenn Hubbard addresses whether the investment generated by subsidies is infra-marginal or if subsidies lead to short-run asset price effects and rent transfers to capital suppliers. Even the discussion of health policy by David Cutler has an element of addressing the Slemrod critique when it examines whether marginal health spending has any effect on health outcomes.

Short-run and accounting changes have not received nearly the attention that they should have in traditional public economics theory or empirical work. The sheer number of tax changes that have taken place in the last twenty years ought to make us concerned about the bias created by assuming steady states and neglecting the relative importance of these effects and this book is an excellent place to get an overview of their importance.

Three Active Areas of Research

The chapter by Poterba which examines budget rules does a good job laying out the state, national, and international evidence on their effects on government spending. Certainly one of the most important trends in recent years has been the empirical examination of institutions. One might easily dismiss balanced budget amendments and the

like as simple gimmickry but the evidence on the states is somewhat compelling that tax limits have binding effects on their behavior following shocks. At the same time, Poterba is good at showing why research on institutions can be frustrating. The national evidence such as Auerbach (1994) shows that budget rules at the national level have clearly shifted the calculated deficits into future years not covered by the rules. Perhaps this comes from the differences in the institutions themselves.

This difference makes me somewhat skeptical of the international evidence Poterba discusses. Countries are just too heterogeneous to be confident of conclusions. Videgaray (1997), for example, shows how governments of even seemingly similar OPEC countries can have radically different reactions to oil price shocks. We need much more of an empirical base about the correlations between institutions and outcomes before we will be able to reach consensus on the effect of budget rules or of institutions more generally. This is an exciting area of further work, though, and one that policy makers care a lot about.

In the paper on savings incentives, Bernheim takes a distinctly behavioral approach to his discussion about whether savings incentives such as IRAs and 401Ks generate new savings or simply lead to reshifting of other assets. He observes that in the life-cycle model of savings, all that matters, essentially, is the rate of return to savings. Bernheim suggests that, in reality, the evidence has not been kind to the life cycle model and is much more consistent with the view that psychological factors drive the savings decision. The distinction could not be more important. One of the primary reasons many economists support the flat tax, for example, is to increase savings by reducing the tax penalty. In a behavioral world, however, by eliminating targeted accounts like 401k plans, the flat tax may do the opposite. The bad news about a behavioral model is that simple policies with clear economic incentives may have perverse effects on savings. The good news is that there may be a role for low cost measures like simple financial education to increase savings without any major efforts.

The debate over the effectiveness of 401k's and IRA's is detailed and the presentation of Bernheim and the discussion following by William Gale lay out the evidence on both sides credibly and I leave it to the reader to decide which is more convincing. The root of the problem is that people who save in 401k and IRA accounts have more savings than others but are high savers in every form. More sophisticated tests have followed. The claim of Gale and his coauthors is that the data indicate that people saving more in these targeted accounts appear to be borrowing the money out of their housing wealth. If true, this would be very damaging to the efficacy of targeted savings incentives but the jury is still out.

Taking a step back, though, anyone arguing that there are large effects of savings incentives should at least address the aggregate fact that in the 1980s, targeted accounts have exploded but the personal savings rate has not increased. It may be that there are other trends at work and that the cross-sectional evidence is better but the broad fact does not seem to indicate that these policies work dramatically. Likewise, the 1980s have demonstrated that people do not seem to be very Ricardian (personal savings didn't rise with government deficits) so any discussion of savings incentives should be put in the context of the impact on national savings, including their effect on *government* savings. Even if savings incentives generate 30 cents on the dollar of new savings, for instance, if

they cost the government 25 cents on the dollar of lost revenue, the net effect on national savings is much smaller than the effect on personal savings. We should keep this accounting in mind when examining the micro data.

The piece on taxes and investment by Hassett and Hubbard is an excellent description of the trends of recent investment research. It is at a somewhat higher level than the other pieces in the book and might be rather hard going for policy makers but for research in the field, it is an ideal. This large literature has, until recently, concluded that taxes have little effect on investment and the field has remained somewhat dormant. In the last five years, however, the subject has been reborn as an active area of research largely on the basis of two empirical findings.

The first is the importance of investment incentives in the cross-section. This was shown by Cummins, Hassett and Hubbard (1994) and in their subsequent papers. They argue that measurement error in the costs of capital has played a key role in the small estimated elasticities and show that looking at cross-sectional variation in the cost of capital within years of major tax changes there are clear effects on investment. Chirinko et al. (1996), use different methods and claim that and find a range of elasticities, mostly smaller, and claim that measurement error is not a problem but Goolsbee (1997) uses completely different data and confirms the Cummins, et al. result of substantial measurement error. Hassett and Hubbard's chapter is very clear on why their method gets around the standard problems.

The second important finding of the recent literature is the importance of investment incentives for the prices of capital, i.e. the short run effects on the asset markets the windfall rents. This view suggests that investment demand elasticities have been biased downward by the standard identification problem. Hassett and Hubbard claim that a world capital market prevents the existence of a price effect but the evidence appears in time series price data, cross-sectional price data, wage data, and R&D data and supplemental factors also point in the same direction (for an overview see the evidence in Goolsbee, 1998b or 1998c). A major effort of the next few years will be to incorporate these two facts into the investment literature and combine them (they are not contradictory). The effect of investment subsidies which are known to be temporary (all of them, in practice) seems a promising place to start.

Conclusion

The topics in *Fiscal Policy* cover a wide range in the field of public economics and I cannot do them all justice. Certainly anyone working in a field covered by the chapters in this book would be seriously negligent not to study it carefully. Perhaps the clearest thing to come from the effort is that the field is alive and well. Indeed, one can only hope that new data and new methods will make the upcoming years will prove to be as productive for the field as this volume shows the past decade has been.

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