Volume Title: Economic Research: Retrospect and Prospect, Volume 4, Public Expenditures and Taxation

Volume Author/Editor: Carl S. Shoup

Volume Publisher: NBER

Volume ISBN: 0-87014-253-4

Volume URL: http://www.nber.org/books/shou72-1

Publication Date: 1972

Chapter Title: Discussion of "Quantitative Research in Taxation and Government Expenditure"

Chapter Author: Various

Chapter URL: http://www.nber.org/chapters/c4286

Chapter pages in book: (p. 61 - 74)

## DISCUSSION

*Includes comments by Walter Heller, Chairman of the Board of Directors of the National Bureau of Economic Research and professor at the University of Minnesota, who was moderator of this session; and by James Buchanan, of the Virginia Polytechnic Institute, and Richard Musgrave, of Harvard University, who acted as program discussants; also, a reply to the discussants by Carl Shoup, of Columbia University. The recorded oral presentations were edited by, or with the cooperation of, the speakers. Remarks made during the open discussion period are not included.*

**Introductory Remarks by Walter Heller**

I want to add my welcome to the fourth colloquium of the National Bureau's Fiftieth Anniversary series. I doubt that there's been such a gathering of the public finance clan since the occasion of the late Harold Grove's sixty-fifth birthday party. It is particularly appropriate that this colloquium should be dedicated to Harold's memory. Not only did he play an important role in the Bureau, but he devoted much of his professional lifetime to the stated purpose of these colloquia, namely, "to consider future research needs in light of present and anticipated policy problems." And he spawned no less than sixty-one Ph.D.'s during his career to help him with that quest!

By way of brief tribute to Harold's contributions and also as a reminder of how the public finance agenda has changed in the past thirty or forty years—and yet remains in many respects the same—I thought it might be interesting to quote just a few passages from his 1933–34 series of tax articles in the *New Republic,* which are classics. I divide these quotes into three categories.

The first was *love's labor lost:* "It now seems very clear," said he in 1934, "that we have reached the point where we can no longer afford the leaks and special privileges in our tax system which have been so abundantly demonstrated to exist. To mention only one of those leaks and privileges, what about the huge volume of tax-exempt interest? Second," he said in this connection, "those who fear the cumulative effect of federal and state income taxes should endorse the suggestion frequently made that the federal government give the taxpayer a credit against his federal tax for any income taxes that he pays the state." (I thought that the federal credit should have equal time with tax-sharing.) And then, "If the states wish to make the property tax more in accord with ability to pay, they can do so by making it apply to the 'net worth' of the taxpayer, including all assets, both tangible and intangible." A bold man was Harold.

The second category is *virtue rewarded.* In "Yachts Without Income" (what a great title!), he said the greatest single gap in the tax system was the unrestricted deductibility of capital losses, which permitted J. P. Morgan to have—you guessed it—yachts without income. Then he manfully put himself in the same class as J. P. Morgan by calling for an end to exemption of government salaries, including his own as a state university professor; the Congress obliged.

And then there is the third category, *the changing agenda.* He called for a tax on undistributed profits as well as a permanent excess profits tax. And he made the typical pre-Keynesian call for higher federal taxes to cut the deficit and sustain our money and credit. Now on the last point let me hasten to Harold Groves's defense with the following quote from an appeal by sixty-two members of the Johns Hopkins faculty to the U.S. Congress in 1932, calling for "the prompt adoption of a budget balanceable by vigorous retrenchment in the expenditures of all federal departments and by adequate emergency taxation."

* * *

**James Buchanan:** I suspect that the difficulties that Richard Musgrave and I face in discussing Carl Shoup's monograph were exceeded only by those which he faced in writing it. I don't know what Shoup's guidelines were, but I get the impression from reading the monograph that his heart wasn't really in it. We can perhaps appreciate why the National Bureau might have had an interest in a summary review of research as a part of its Fiftieth Anniversary. But in this age where we expose all hypocrisy, perhaps we should also acknowledge that research surveys are rarely of much value unless there is something other than a commemoration objective to be served. A more effective use of resources, I think, would have been to allow Carl Shoup to write a monograph on a topic of his own choosing. This would certainly have been more interesting to his discussants, and it might have served a commemorative purpose equally well.

But we are not here to dwell on what might have been. What we have before us is a monograph that purports to survey quantitative research in public finance over a half-century period. Now I like to think of a technically competent nonspecialist who might read this monograph.

And I should like to ask what impressions this nonspecialist would get about public finance and its development over the period from reading this monograph. In this connection I submit that the survey does provide a misleading and distorted picture of what has been and is happening in public finance. We have witnessed the transformation of what was a dull, unimaginative, extremely limited, and almost irrelevant subdiscipline into one of the most exciting areas in political economy. Yet this monograph fails to convey more than a trace of the revolution that has taken place. A nonspecialist reader would get the impression that public finance economists have continued to work with the same old problems, the only change being the employment of more and more sophisticated tools. Furthermore, he would have to conclude that, while some progress seems to have been made around the edges, nothing much has been accomplished by way of concrete results. And this nonspecialist reader of the survey would then surely not place a very high value on research prospects.

Now, what is the trouble? Is the over-all impression so misleading because Shoup has chosen to concentrate his attention on quantitative public finance research, which he juxtaposes against qualitative or theoretical research? It seems to me that this offers us only a small part of the answer. What has really happened in public finance is that our paradigms have been modified. We simply do not look at the subject matter of this subdiscipline in the same way that we did thirty years ago. And let us ask the question, what was public finance like thirty years ago? There were, of course, both positive and normative parts or elements of it. When I was a graduate student positive public finance consisted in analysis, almost exclusively nonquantitative, of the shifting and incidence of taxes. We tried to explain the effect on private economic behavior of individuals and firms that was exerted by the levy of taxes of different kinds and different magnitudes. And, as Shoup's monograph properly indicates, we're still trying to do this, but without having made much progress. In those days, normative public finance embodied the so-called "principles of taxation," a value-laden and extremely naive discussion about how taxes should be imposed. I think it is fair to say that public finance in those days was not political economy at all. The positive aspects were simply applied Marshallian price theory. Public finance did not embody a study of the public economy.

Today, by contrast, public finance is public economics. It is the economics of government, the study of the allocation of resources through public or collective decision processes. It is this incorporation of public choice that has made the dramatic difference in public finance. In one sense you could say that we put the word "public" back in the name. In another sense we could say that English-language public finance has made up a half-century lag that existed before World War II between its own level of sophistication and that of the Europeans. The contribution of Wicksell, Lindahl, and the Italians has now become a central part of the modern public finance tradition. And this change has come about through both normative and positive approaches. Such men as Musgrave, Bowen, and Samuelson showed us how welfare conditions for public sector allocations could be derived from individual evaluations. Alongside this, such men as Kenneth Arrow, Duncan Black, and Anthony Downs begin to show us the positive results of attempts to combine individual evaluations into collective outcome. Surprising as that may seem to us here today, public finance economists have come to look on governments as being made up of ordinary men only within the past two decades. Only within those two decades has a serious analysis of the actual workings of the bureaucratic process been commenced by such people as Gordon Tullock, Anthony Downs, Roland McKean, William Niskanen, and others. I think we have indeed come a long, long way; so far, in fact, that the study of public economy now includes as only one part, and a relatively small part at that, the traditional problems of tax shifting and incidence. It therefore seems to me little wonder that Shoup's summary survey should be somewhat misleading in its concentration on what has now come to be a small part of a much larger and more important subject. Most of you who know my own approach to public finance could have predicted this to be my general response. I have summarized it here because there are, or so it seems to me, quite important implications for public finance research. I think we have no more than scratched the surface of investigation into just how governmental processes do, in fact, work. I think there is a veritable mine to be exploited here, and we can point to only a handful of empirical contributions. Let me mention just one or two. How are budgets actually made? Surely the Davis-Dempster-Wildavsky results cannot be ignored when people start asking that question. How do regulatory commissions actually work? The research efforts of George Stigler and his cohorts must remove some of the naiveté from some of the people who ask that question. We need hard-headed empirical research into the workings of every constitutional or legislative assembly, court, bureau, agency, commission, and committee of every level of government in the land.

The research that I'm calling for is empirical and quantitative, but it does not require complex or complicated techniques, and it will scarcely excite the young economist who has a set of fancy tools and no ready-made places to apply them. The research tools needed are the simple motivational assumptions that the economist carries around with him along with a hard-headed critical understanding of the way that actual and political collective-decision institutions function. We have too long neglected institutional considerations. I would personally like to see far more institutional analysis reintroduced into public finance and into economics generally. And I was a little impressed by Shoup's remarks where he called for research on the workings of the tobacco industry and the multiproduct corporations. But neither in his remarks nor in his monograph did he call for research into the way the government works and the way bureaus work. I see Roy Blough here, and I am frankly a little embarrassed when I think back to my reactions to his book *The Federal Taxing Process*. I remember that, before I saw the light, my initial reaction to that book was negative, but that is the type of research that we need.

What could be expected as the effects of such research even if it should be undertaken and completed along precisely the lines that I suggested? I think that we must hold fast to the view that research results ultimately affect men's attitudes toward policy. If men come to understand more about how bureaucracy actually works, perhaps they will tend to assign to bureaucracy only those tasks that it can perform with reasonable effectiveness. Even more importantly, once a better institutional understanding is accomplished, the emphasis can then be placed on institutional innovations which are designed to foster increasing bureaucratic efficiency.

It seems to me that modern economists are prone to take either one of two routes in their research. The larger group undertakes research in any subject provided only that the data are such as to allow them to apply their econometric techniques. Karl Brunner calls them "econometrarians." This group tends to be far more interested in applications of technique than in any substantive results that might emerge. The smaller group will undertake research in the manipulation of models whose assumptions sometimes remove them too far from the world of reality. Of the two, I suppose it is clear that my prejudice is inclined toward the latter group, but I feel a little guilty sometimes because I feel strongly that the most productive research lies somewhere between these two poles. We need empirically based and empirically informed research into the theory of both actual and potential institutional structures. Let me give you a simple example. We do not need elaborate econometrics to explain why hospital costs are soaring under medicare. We do need a rudimentary understanding of the institutional structure of the hospital, and understanding of the decision processes that are involved, and of the reward-punishment structure confronted by those persons who must make the relevant decisions. It's only when we have such an understanding that the institutional innovations that are essential to resolve this cost-increase phenomenon could be expected to be forthcoming.

Of course, each of us would have his own favorite agenda for research, and it is not to be expected that mine would parallel that of Carl Shoup. Nonetheless, I submit that the agenda that I have just alluded to does offer a far more exciting horizon to the young scholar and to the research entrepreneur in the foundation than the somewhat tired topics in traditional public finance that Shoup has perhaps overemphasized. Each man should, of course, be encouraged to do his own thing, and I don't object to further research along the lines which Shoup has called for. My complaint is really not that he has led us down a false path—not at all. It is rather that he has failed to suggest that there are more interesting alternative routes to follow in what we may legitimately label public finance research.

**Walter Heller:** *Thank you very much, Jim. You did exactly what a discussant should do—injected some controversy, some substance, some positive suggestions, offended young economists, offended old public finance men, and even threw a few crumbs to us old institutionalists.*

**Richard Musgrave:** I am happy to follow the example of our other speakers in beginning with a few words about Harold Groves. Shortly before he died, I gave a lecture at Wisconsin which he attended. I took the opportunity then to say why I have admired him so much. It was his positive and courageous approach to the solution of public policy problems—the kind of attitude which expressed his midwestern progressive faith that ultimately things can be done reasonably. It was the same spirit that I admire so much in my teacher, Alvin Hansen, and I think there is not enough left of it today. I told Harold that I thought that he and Hansen symbolized what, as someone with a European background, I had come to think of admiringly as an American intellectual. Of late, too many American intellectuals have become European intellectuals, which, I fear, has been a dubious gain.

Now let me say a word about what should be included in public finance. Carl noted in his introduction that he did leave out fiscal policy, and that he considered certain aspects of cost-benefit analysis as marginal. It is, of course, true that the macroeconomics of fiscal policy and their methodology fit into a different course than the microeconomics of, say, tax incidence. But we may be going too far toward ruling out all macro aspects. When I was a graduate student, probably a decade before James Buchanan, I was taught that the only function of taxes was to be a deflationary factor in the economy, and at that time the public finance problem was seen in purely macro terms. Since then the macro aspects have come to be practically eliminated. But as Shoup points out, we need a general equilibrium approach to many of these problems, and this brings the macro aspects back into the fold.

But the very scope of the public finance field makes it increasingly difficult to hold all its facets together. Why should a student who wants to study cost-benefit analysis be interested equally in corporation tax depreciation or in the stabilization aspects of fiscal policy? This is something which all of us who teach the field encounter increasingly. One might well say that the only unique aspect of the economics of public finance—that is, genuine public finance—is the theory of social goods, normative and positive. This is the essence of public finance as a separate discipline, and all the rest is the application of various tools of economic analysis to one or another aspect of fiscal operations. But even with this narrow definition, the economics of public finance, once an obscure corner of the science, is rapidly coming to be a central subject of economic theory, as it is now being taught.

So much for the general problem of staking out the field. Let me go on and say a word about methodology, because this is one of the most interesting aspects of the problem which Carl Shoup has discussed. Certainly, theorizing is not enough. There is a desperate need for empirical work. In most controversial issues of tax incidence and expenditure effects there can be a variety of a priori theoretical hypotheses which are not offhand unreasonable. For instance, you can convince yourself of a theory of the firm which says that the corporation tax is not shifted, or of one which says that it is shifted. The answer has to be found in empirical work. This need is well illustrated by the studies of tax burden distribution which Shoup discussed at some length and which I have been working on at various stages, following up the earlier work of Tarasov and Colm. The crux of these studies lies in the hypotheses made regarding the incidence of corporation and property taxes. These are the two strategic factors, and until we have good empirical evidence on them, the results are hypothetical. To get further, we must tackle this empirical job.

Moreover, as Carl points out, we must begin to deal with these problems in terms of a general equilibrium analysis. It is important, as he puts it, to consider them in terms of closed systems. This is surely right. If we ask what happens if this tax is imposed or if that tax is repealed, one must take into account the inflationary or deflationary effects. To disregard them would not be a very meaningful way of posing the question. So one should think of incidence in terms of differential effects. I disagree with Buchanan that incidence is a tired subject. To be sure, it is extremely interesting to think about how the political mechanism can be designed to lead to a more meaningful relationship between tax policy and expenditure policy. I quite agree with that. But the fact of the matter is that taxes are largely determined and set independent of expenditures, and that the benefit theory, beautiful though it is, is not easily applied. Taxes are not voluntary payments, nor need they stay put with the payee. Knowing what their incidence is thus becomes extremely important. It is especially important because our society must increasingly face the issue of income and welfare distribution, an issue which, in my judgment, is perhaps more serious than any other aspect of public policy.

Yet, the design of the distribution problem is made difficult by our lack of knowledge regarding differential incidence. The usual studies of tax burden distribution are subject to the criticism that they look at the absolute burden of particular taxes rather than at differential incidence. By assuming pretax income to be unaffected, one may compare a particular tax with a standard, such as a proportional income tax, and show the differential. This, however, is really cheating because it does not produce a solid general equilibrium analysis. As Prest pointed out, you cannot get around the difficulty by assuming that the distribution of income before tax is independent of the tax system. But to construct a general equilibrium model of the Walrasian type which includes not only all relative product and factor prices but also their relationship to the size distribution of income is an enormous, if not impossible, task.

Looking back over the decades, I am impressed with the lack of simplifying genius in the field of microeconomics, such as Wicksell and Keynes have produced in the field of macroeconomics. Ricardo did have a beautiful system which gave the key to incidence, but, as Schumpeter pointed out, its only difficulty was that it was all wrong. There has been Walras and his splendid system of $n - 1$ equations, a system which is right by definition but does not get us anywhere in solving practical incidence issues. What is needed is a breakthrough in terms of a manageable system of relative product and factor prices and their relative income distribution, a system which, one hopes, has fewer than 4,999 equations and can yield meaningful results.

This raises the problem of econometric models. These models involve a general equilibrium approach, but partial equilibrium aspects must be understood to construct them. The initial question to be considered is: How do the tax variables enter the consumption function? How do they enter the dividend function? How do they enter the investment function? It would be interesting to take the various models (Brookings, Wharton, Federal Reserve/MIT, D.R.I., etc.) and see just how the fiscal variables are inserted into these various expenditure functions, and what this implies regarding the effects of changes in fiscal parameters. Study of micro responses to fiscal variables thus comes first, because it is needed to determine how they enter into the model. Even though partial equilibrium analysis is not enough, it is needed as a first stepping stone toward a meaningful general equilibrium model.

Now I come to the most difficult and, in a way, the most important part of the problem; that is, the contribution to be expected from econometrics. I believe that this contribution is vital. Notwithstanding my primary interest in the normative theory of social goods, I know that governmental action must involve compulsion and is thus bound to induce responses in the private sector. The nature of these responses, therefore, must be known if government behavior is to be efficient. Theorizing alone is not enough and finding the answers stands and falls with the success of econometrics. The empirical science of public finance, we might as well admit, is largely an application of econometrics. The specialized aspects of public finance are only a minor part of the problem.

What type of econometric model should be used for public finance analysis? We appear to be at a point where some disillusionment is setting in among econometricians as to the usefulness of very large models. If so, it would be unfortunate for researchers in public finance to move toward their use. Moreover, the kinds of questions which these researchers must ask are much more difficult to answer by econometric models than the questions which the macroeconomists have asked. Given a large model with substantial lags and many exogenous variables, it does not appear too difficult to determine what will happen to GNP next year. Even though models may differ greatly in detail, they cannot very easily miss, especially if GNP keeps going up anyway. But the outcome is much less certain if one wishes to determine what happens if we change the corporation tax rate, or the property tax rate, or even the income tax rate, or if we adjust this or that item of government expenditure. If you ask these selective questions, the different models will give strikingly different answers. This is the case because tremendous weight is now placed on the structural nature of particular equations, the specific design of which determines the multiplicand generated by a particular policy measure. Since these particular equations—say, the investment equation—differ greatly in the different models, it is not surprising that we get very different results.

I am not saying this to discredit econometric work. I believe that this is where the future lies. But to improve matters, much more work has to be done on how the tax variables should be fitted into the basic functions. The recent debate over the nature of the investment function was most helpful, and Jorgenson—although he seems to have lost the now mythical battle at the Brookings conference—made a major contribution in trying to design a microeconomically meaningful investment function. This was an important step forward, but much more work will have to be done before the effects of selective policy changes can be generally predicted.

For purposes of incidence analysis, we must also take these macromodels and link them to the problem of income distribution. This surely is going to be a tremendously difficult task. Unfortunately, we cannot be satisfied any more (as was Ricardo) with studying the effects of various measures on factor shares. We are interested in their effects on the size distribution of income, presenting a whole array of problems which do not even appear in these models. Before the distributional dimension can be incorporated into the large models, I think that it would be well to deal with much smaller models in which one can experiment with these problems.

One of the great difficulties for econometrics in fiscal research is that, for so many problems, we have to use time-series analysis. Here we must recognize that, as I should well know, the fiscal variables (e.g., corporate tax rates) tend to be highly collinear with everything else. Tax receipts go up when the buoyancy of the economy goes up and go down when buoyancy goes down. And when both tax receipts and expenditures go up, you have an apparently insoluble problem of collinearity. As a consumer rather than producer of econometric tools, I think that we must develop more sensitive instruments to deal with collinearity if we are to get results in this field.

Now let me comment briefly on large versus small projects. Carl's emphasis has been on the need for large projects, but I would say, as did the chairman, let the flowers bloom. There is a place for everything. Important contributions have come from small-scale projects, as, for instance, Carl Shoup's work on the allocation of police effort in New York City, Dale Jorgenson's work, and many other cases. One can also think of large projects which proved extremely useful—for instance, Joe Pechman's income tax model. So, it seems to me that there is a place for everything. In research, as in other connections, it is good investment portfolio behavior to spread risks. When one can have a hundred small projects or one large project, one has to be pretty sure that the payoff on the large project is going to have a high probability of return. But surely there is room for both.

Finally, just a word with regard to what I think are some of the most important problems to work on. In reading the *New York Times* today, I learned that the administration may be thinking of an expanded revenue-sharing plan, not on a \$4 or \$5 billion but on a \$10 or \$15 billion scale. It seems to me that we are embarrassingly unprepared to deal with this problem. As I see it, the most important problem to be resolved at this point is to find some way of measuring not only the fiscal capacities but also the fiscal needs of the various jurisdictions. This is a difficult and messy issue, but I think it is one of the most important problems to be tackled.

Another issue which is most important from a current point of view concerns the incentive effects of the negative income tax. This is a field where, as Carl mentioned, some experimentation has taken place. While it appears that the experiment has not been very successful, this is certainly an area where work need be done. A third area of increasing interest, like it or not, is the possibility of substituting a value-added tax for the corporation income tax or other taxes. To do this, we need some greatly simplified macromodels containing the relationships most important for this purpose but cutting out excessive details. Some work of that sort would be most helpful, particularly if available by the time of Congressional consideration. The analysis of tax incidence, especially for the corporation tax and property tax, is something that has to be continued. I agree with my critics that, in order to get at the corporation tax problem, a more disaggregated model is needed and one which introduces some explicit assumptions about the pricing behavior of firms. The difficulty is that, to do a good job, detailed information is required on costs and prices, that products have to be defined over time, and so forth. Basic data work may have to be undertaken before much estimating can be done. As Buchanan suggests, the analysis of bureaucracy, of how government works, is also a challenging and important problem. Perhaps one of the best things in our field is that there is great variety, with different people interested in different things. The best bet, I believe, is that good researchers, left to their own interests, will find the most promising problems. Research foundations should respond to these interests, rather than guide them.

**Walter Heller:** *Thank you, Dick, for a fascinating discussion. You did just what a second discussant should do, namely, disagree with both the speaker and the first discussant. The agenda of unfinished business gets longer and longer. I should also note that there were some very good practical inferences that can be drawn from what you said, namely, that Wilbur Mills and John Byrnes can wait until they have the results of your analysis before they change their opposition to revenue sharing. I imagine that will be rather reassuring to them. Of course, the real question is, will the Congress wait, will it heed?*

*Now before we turn to defenders of econometrics and the 5,000-equation models, we should give our initial speaker a chance to comment on some of the comments that have been made. Then also, Jim, if you want to say something in response to what Dick has said I think we might do that before opening the meeting to discussion from the floor.*

**Reply by Carl Shoup:** I'll try to be brief, although Professor Buchanan's remarks were all so quotable I hardly know where to begin. But at any rate, it does seem to me it's a great pity that Jim wasn't born forty years earlier, and also Gordon Tullock and his other coworkers, so that by now there would have been a big body of empirical research on their special interests for me to survey when I wrote my paper. But since fate decreed otherwise, there wasn't really very much that could be said on that score.

As to what should be covered in my paper, that, of course, is partly a matter of choice, partly a matter of assigning relative importance. I'm inclined to agree with Dick Musgrave, if I heard him correctly, that shifting and incidence are not really tired topics. Perhaps Jim is tired of them—I can understand that. We all get in that mood occasionally. But there is still more to be said in these fields and I'm a little disappointed that Buchanan himself wasn't excited about extending the field of analysis into government services, subsidies, and so on. What it seems to come down to is that there's still plenty of work for all of us to do. I can't associate myself with Buchanan's value scale with respect to the topics for research in public finance, but there's no reason why he should have to associate himself with mine.

As to the comments that Professor Musgrave made, I find myself pretty much in agreement with him, except it is a provocative thought that we haven't had any genius in this field of microeconomics that might be compared with Ricardo, Wicksell, and Keynes in macroeconomics. I suppose Dick would be willing to put Cournot in that category. To be sure, that was a long time ago. Cournot gave us the framework, we just haven't moved very far from his base—that's the trouble. But perhaps this field is not one that calls for genius—maybe it just calls for a lot of hard work from all of us. The task is, as I suggested, even bigger than we yet appreciate. It is to find out how firms work and what they do.

Not knowing much about it I can speak clearly and say that I'm in general sympathy with the idea of small models—how small, of course, is another matter. They should presumably be made small in a way that allows them to grow. I agree that the ultimate goal is the use of large models, sooner or later. But we risk a great deal by attempting to put too much into one large project—the large model. That was not what I meant when speaking of the large amounts of time and effort now needed; what I want to suggest, instead, is that for the really detailed work of finding out what goes on in the business firms of the world (micro work), we need far more time and more millions of dollars than we yet appreciate. Until we do appreciate that we will remain pretty much where we are.

As to current topics of pressing interest, I'm inclined to think that value-added taxation is going to be discussed, but its chief interest for us is not as a substitute for corporate income tax but as part of a general reform of the federal tax structure, along with a couple of other tax measures, notably a net worth tax and a progressive-rate spendings tax. The value-added tax, if it is needed at all, is needed as a chief instrument for counter-cycle policy, for which I think income tax has shown itself pretty well ineffective, I'm sorry to say. That's quite a change in my point of view from four or five years ago. The value-added tax is certainly a lively subject for discussion, but we should think of it in a much broader framework. Thank you.

**James Buchanan:** *Thank you, Carl. I want to comment by saying I withdraw the word "tired."*

**Closing Remarks by Walter Heller**

One thing that we can all see in the vast unfinished business of research in taxation and public expenditure is that Justice Felix Frankfurter was speaking the truth when he prefaced his remarks in a complex income tax case many years ago by quoting Edmund Burke, to the effect that "taxation, like love, is not founded wholly on reason." That certainly is still true today. By the way, in his dissent in that particular tax case, he went on to say, "This is a plain, unvarnished, luscious bonanza of a windfall." We can stand a little more of that kind of plain speaking in Washington even now.