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Public Finance Theory – Then and Now

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When I began studying public finance (in 1961 with E. Cary Brown), Richard Musgrave’s *The Theory of Public Finance* had just been published (Musgrave, 1959). This book, with its general equilibrium structure, set the base for my public finance research in the 1960’s (talk about standing on the shoulders of giants). The use of duality as a theoretical tool and the ideas of second-best analysis were very much in the air in the mid 60’s. And combining these two elements played a key role in my research at the time. Indeed, it was the formulation of optimal taxation in a dual setting that led to the extension of optimal taxation from an (effectively) one-person economy of Ramsey, Samuelson, and Boiteux to the many person formulation that Jim Mirrlees and I developed in the mid 60’s. Also newly available as a tool was the OLG model of Samuelson (the Allais version being essentially unknown) at a time of great interest in the effects of the public debt and rising interest in social security (Aaron, 1966). The coming together of tools and topics can be a great opportunity for researchers. One sign of progress is when a topic being explored turns into a readily usable tool.

As I work on public finance now (both social security and taxation), general equilibrium is still the foundation for much of theoretical analysis, with the addition of wide use of the OLG version of GE. The ideas of behavioral economics are very much in the air. I am excited about the prospects for behavioral economics – the potentials for increased understanding of decisionmaking and for the development of tractable insightful behavioral models that can be readily used for analyzing public finance questions. The theoretical tool that has recently come into wide use is large-scale simulation (CGE and OLG models). While large-scale simulation seems likely to be important for the future of public

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1 I am grateful to Jim Poterba for comments on an earlier draft and to Tom Davidoff for research assistance.

2 Our paper on optimal taxation was presented in pretty much its final form at European and North American Econometric Society meetings in 1967, although publication was delayed by a combination of editorial delays and the difficulty of long-distance collaboration in those pre-email days.
finance, I suspect we are not at a point of the coming together of the tool of large-scale simulations and the topic of behavioral economics in a way that will bear a lot of unusually good fruit (although one very good example is Laibson et al., 1998). I suspect that the biggest payoffs are not in this combination, but no one should rely on the instincts of someone who cut his teeth on earlier tools and topics – spotting the future is work for the young.\(^3\)

In addition, there is considerable work extending public finance beyond the settings of the Arrow–Debreu and competitive OLG models. This includes work based on monopolistic competition, incomplete markets, and search. I suspect that these approaches will be important in the long run, but seem like candidates for incremental advances right now, not breakthrough developments.\(^4\) In contrast, the use of large-scale simulations has exploded and the expansion of behavioral ideas in public finance may well. After discussing some aspects of behavioral economics, I will briefly discuss what I feel to be some limitations in large-scale simulations.\(^5\) To be concrete, I will discuss behavioral economics and simulations in the context of social security, although many of my comments would apply as well to other public finance issues with a large intertemporal dimension.

1. Behavioral economics

   Social security is frequently justified as a response to the (behavioral) tendency of many people to save too little for retirement. "Too little" is a controversial notion in some circles since with the usual rules for proofs in economics, one can not prove that people are not optimizing in their savings decisions. But then we can not prove that they are optimizing either. Some may feel that this is a sufficient justification for preserving the assumption of rational decisionmaking together with the use of social evaluations that respect revealed preferences. But there is a great deal of evidence that people do not successfully make rational decisions all the time (e.g., inconsistent responses to the same decision when framed in different vocabularies). That alone does not lead to a conclusion that many people save too little for retirement – such a judgement is indeed judgmental.

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\(^3\)This belief raises the question of why I am writing this piece.

\(^4\)This is just another way of saying that I do not see the shape a break-through analysis might have that would open up a significant new terrain for research advances.

\(^5\)I consider the political economy dimension of public finance evaluations to be extremely important. However, at this stage in the development of mathematical models of political outcomes, I find that mathematical modeling is in too early a stage to be genuinely helpful in public finance considerations. This is not meant to suggest that we learn nothing from the ongoing mathematical studies of equilibrium under different political decisionmaking models of public finance questions, just that I do not see large potential payoffs here in the near future. On the other hand, reading political scientists, like Arnold (1990) has seemed very fruitful to me.
and comes from looking at the distribution of wealth of people reaching retirement ages.

For some people, too little saving takes the form of little or no savings, so it is easy to model their lack of savings responses to modest changes in social security. For others, there is some savings and equilibrium analysis needs a model of the response of whatever savings there is to changes in social security. Indeed it is not just those with too little savings who may be inadequately modeled by the standard rational model (and some of those with little savings may fit the rational model). We are in the early stages of developing behavioral savings models (e.g., Laibson, 1997) and incorporating in such models retirement decisions and social security (e.g., Diamond and Koszegi, 1999). I think it is difficult to judge what models will prove to have long-run value. The difficulty comes from several sources. One is heterogeneity in savings behavior in the public, suggesting that there will not be a single model that becomes universally applicable by merely varying parameters. Currently we have too little experience with models to form good judgments of long-run importance.

A further difficulty is inherent in the behavioral framework itself. The rational model is premised on the assumption that it is the same preferences that are underlying all decisions. In this framework, empirical evidence on any decision is important evidence on many other decisions. Insofar as behavioral economics reflects the context-dependence of decisions, models and parameters (estimated or calibrated) may well not carry over from one setting and one question to another setting and the same or a different question. The practice with rational modeling of carrying parameters from one setting to another should not move simply to behavioral models. The impact on current savings and labor supply of changing legislation about future social security benefits may vary enormously in the population (in the sense of the kind of model that makes sense), varying with age and with experience, as well as with underlying differences in attitudes toward the future, such as discount rates. The difficulty in deciding what is worth working on can be a trap (as people explore lots of models that prove not to interest other researchers) or an opportunity (as one might strike one that has wide impact in such a wide-open setting).

A second aspect of social security in most countries is that benefits are not available as a lump sum (at least not fully available). Usually the benefits come as an annuity (single or joint-life in some form). But some countries (e.g., Chile) also preserve a phased-withdrawal option. Some analysts might defend a mandate to annuitize as a protection against adverse selection, which otherwise results in pricing that excludes some people from the advantages of annuitization. I feel that this defense is inadequate (and a defense based on behavior is needed), just as

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6 For example, see Carroll (2000), on the savings of the very wealthy.
7 Indeed, we already have the contrast between naive and sophisticated versions of the model with quasi-hyperbolic discounting.
adverse selection is not an adequate explanation for the extremely small amount of voluntary annuitization that does occur.

Some people (many people?) would like to trade some of their potential estate for more consumption if alive, if they understood the tradeoff. An extreme version of this is people with little or no (or lexicographic) interest in a bequest (relative to own consumption). While we think of annuities as a single contract covering payments for the rest of one’s life, this is not a necessary aspect of trading consumption if dead for consumption if alive. Indeed a complete market Arrow–Debreu setting would allow such trade separately for any time period and any state of nature. For example, consider a bank that offers an alternative CD with an interest rate that is somewhat larger if you are alive when the CD matures, and no payment if you have died. As long as the administrative cost of checking that you are alive is less than the expected yield from the death of purchasers of such CDs, this would be a dominant asset for a CD purchaser without interest in a bequest.

Indeed, we could think of many investment opportunities (e.g., mutual funds) organized like CREF annuities, with the accounts of those who die during the investment period allocated to those who survive. Yet such contingent short-period investments do not exist. Adverse selection is not a reason for this since the subset of people without bequest motives would find this a dominant asset even if not priced actuarially fairly, as long as there is any surplus payment. So, I think we need to relate the absence of such market-organized trading opportunities to behavioral responses – the limited understanding of insurance and the inadequate thinking that makes it difficult for suppliers to sell such ideas to the public. This leads us to think about social security, as commonly is done, as providing the only annuities in town.

As the US moves to considering individual accounts that might include voluntary annuitization, the question of annuitization choice becomes very important for evaluating such a policy. Indeed the bequeathability of individual accounts is frequently presented as one of their important advantages (ignoring the ability to use social security benefits for monthly payments for life insurance to serve the same purpose). I worry about the quality of annuitization decisions that would come in a relatively unfettered environment and think that behavioral bases of this intertemporal choice are of great importance, along with the decisions about

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8For more discussion of annuitization, see Brown (2001), and Davidoff et al. (2001).
9This is the underlying framework in Yaari (1965).
10For a discussion of CREF annuities, see Valdés-Prieto (1998).
11Current use of annuities raises issues of the quality of judgements. There is the heavy reliance on nominal annuities, suggesting that money illusion is rampant in this arena. There is the popularity of annuities with minimal number of guaranteed payments, a choice that converts what could be a deterministic bequest into a stochastic one, a choice that seems hard to justify. Behavioral explanation of the popularity of such choices (in terms of gains and losses instead of outcomes) seem plausible, but raises issues about normative evaluation.
saving for retirement when working. Also important is decisionmaking within the family in contemplation of one member of a couple outliving the other.

In addition to the issue of positive modeling, there is a major issue of forming normative judgments if one assumes that behavioral economics captures the right model. If people evaluate many or most decisions under uncertainty in terms of gambles in isolation, as loss aversion strongly suggests (Kahneman and Tversky, 1979; Tversky and Kahneman, 1992), should this basis of choice be the basis for normative evaluations? Note that even if decisionmakers are consistent in the loss-aversion functions across choices, they are not consistent with a single overall utility optimization in terms of outcomes. It is easy to do normative analysis in terms of an attributed utility function. What is an open arena is how to do normative analysis that reflects both reactions to choice, as well as to outcomes, and that incorporates differences across people that show up in different responses to choice. That is, people show concern about outcomes and people show concerns about choices that are not adequately captured in descriptions solely of outcomes. How should both of these reactions be incorporated in normative analysis?

While I have discussed only social security, the nature of intertemporal choice is relevant for many aspects of taxation. While Saez (2001) has advanced our understanding of the role of taxation of capital income by relaxing preference assumptions that are mathematically convenient but empirically wrong, I suspect that further advances await analysts who bring behavioral perspectives to this issue. Estate taxation is another prime candidate for a behavioral perspective. So too is the intertemporal movement of the realization of income, both capital gains and labor income.

I believe that good analysis of social security policy requires consideration of behavior issues. And I believe behavioral economists have an excellent chance of making great strides that are important for public finance. But, will the important advances in theoretical behavioral economics and its applications to public finance come through large-scale simulations or old-fashioned theoretical modeling?

2. Large-scale simulations

For analyses of policy that reflect the great diversity of individual situations and the great complexity of economic links, there is no substitute for large-scale simulations. But there are limitations in the use of such models and shortcomings in many of their current uses. The virtue of simulations lies in the quantitative answers one gets. But the quality of the quantitative answers depends critically on the quality of the underlying assumptions. Of course this is also true of theoretical analyses that yield qualitative answers and insight into causal structures. The difference between the two is that the focus of much theoretical analysis on economic insight links fairly well with the effort to evaluate the critical underlying issue of the robustness (and so relevance) of the insight. With simulations,
exploration of robustness is done by sensitivity analyses as one varies modeling and parameters. But I find it hard to evaluate robustness as a reader of simulations of others (as opposed to the couple that I have done myself). And when the basic assumptions seem questionable, then sensitivity analyses presented may be of little or no help.

For example, there is much use of multi-period additive preferences with the same utility function in each period. Casual observation suggests to me strongly that preferences are not additive and that utility functions vary significantly with age. Possibly more important is the underlying behavioral issue of whether people do consistent intertemporal optimization. If the underpinning of the importance of social security for the economy is precisely that many people do not save enough, any simulation based on lifetime optimization is inherently irrelevant – although we have seen many of them. Of course, simulations can incorporate behavioral models. But the dilemma is which ones to use. We may come to a time when we have such a large stable of completed simulations, many of them based on different underlying presumptions on the nature of intertemporal choice that we can get a good understanding of how policies that affect intertemporal outcomes work. But such a time does not seem imminent. Moreover, the exploration of the terrain of alternative large models is likely to be strongly (and positively) influenced by the understandings gleaned from simpler models meant to illuminate single issues and economic effects, rather than to explore the full implications of a policy. So these types of analyses need to go on in parallel. Indeed, the term “toy model” which some use with derision is to me a badge of pride – the contribution of a successful toy model is likely to be larger than a publication whose value depends on its mastering mathematical or computational complexity. But that depends on the term successful, which is a slippery one, since it is based on predictions of future research developments that ex post show that some analysis had influence. And there are successful publications of all sorts – indeed the collective research enterprise flourishes because we do not all do the same style theoretical research.

OK, I admit it, this is a glorification of the type of work I like doing. But we would not get so much done if we did not do that.

References


Public finance is the study of finance of government entities. It revolves around the role of government income and expenditure in the economy. Financial administration includes preparation, passing, and implementation of government budget and various government policies. It also studies the policy impact on the social-economic environment, inter-governmental relationships, foreign relationships, etc.
Finance Theory. Related terms: Financial Markets. Finance theory teaches that the value of an equity share is determined by its fundamental value: the expected discounted value of its future yield (or dividends). Consider a simple environment for testing this hypothesis: N=(9 or 12) subjects are each given an endowment of cash and shares; at the end of each of 15 trading periods a dividend is drawn from the four alternatives (0, $0.08, $0.28, $0.60), each with a probability of one-quarter of yielding an expected dividend of. Then, we will discuss theoretically the possible channels through which trade costs in goods markets can affect portfolio decisions. View chapter Purchase book. Read full chapter.