Comments and Discussion

Martin Feldstein: The paper by Howrey and Hymans represents a serious effort to approach an important issue in a new way. The low saving rate of the United States in comparison to most other industrial nations is notorious. It is not surprising, therefore, that a growing number of economists and others who are concerned about this problem are asking whether we should follow the lead of the European countries in placing less emphasis on the taxation of investment income (relative to consumption and payrolls) and in devising special schemes to exempt a substantial fraction of personal interest income from the individual income tax. The Howrey-Hymans paper seeks to contribute to the analysis of that tax policy question by measuring the effect of the real net-of-tax interest rate on what the authors call personal cash saving.

The authors are certainly correct that theory alone cannot predict the effect on the saving rate of a change in the net-of-tax interest rate. Indeed, the ambiguity is even greater than they appear to realize when they refer to the countervailing income and substitution effects. Even if we consider a *compensated* change, such as increasing the payroll tax and reducing the rate of tax on interest income, theory cannot predict the sign of the personal saving response.[1] All that we know from microeconomic theory is that a compensated increase in the interest rate (that is, a compensated fall in the price of future consumption) causes an increase in the *quantity* of future consumption. But current saving is equivalent to *expenditure* on future consumption. The expenditure on future consumption only rises in response to a compensated fall in its price if the compensated demand

1. This paragraph and the next are discussed more fully in Martin Feldstein, "The Rate of Return, Taxation and Personal Savings," *Economic Journal*, vol. 88 (September 1978), pp. 482–87.

elasticity exceeds one. The substitution of a payroll tax for an interest income tax may therefore reduce current personal saving.

Despite this ambiguity about personal saving, traditional theory has an unambiguous implication about the effect on total national saving. A compensated tax change that raises the real net return on personal saving unambiguously reduces present personal consumption. If government consumption remains unchanged, total national consumption must fall. Thus total national saving (government, private, or both) must increase.

It is nevertheless interesting to consider empirically the magnitude of the saving effect of uncompensated changes in the real net yield. Unfortunately, such an analysis is difficult to do in a convincing way. The basic problem is that the expected real net yield available to individual savers is not observable and is hard to measure. What asset or combination of assets should one look at to measure the yield? Savings accounts? Series E savings bonds? Corporate bonds? Corporate equities? Mortgage interest rates? Consumer credit rates? The yields on these assets have behaved differently and there is no obvious choice among them. The problem is exacerbated because the mix of assets and liabilities differs among individuals according to their tax situation, wealth, and other circumstances.

And what about expected inflation? The Michigan Survey Research Center responses deal with very short-run inflation expectations, not the horizon of fifteen or twenty years needed for calculating real returns on the long-term bonds that the authors use to measure the interest rate appropriate to life-cycle saving decisions. The autoregressive extrapolations used by Michael Boskin may be better, but they clearly introduce a further source of noise.

If we limit attention to a bond interest rate, the real pretax yield has remained approximately constant. All the variations in the net yield therefore reflect changes in the effective tax rate. But what is the relevant effective tax rate for this aggregate equation? It is a weighted average of marginal tax rates, but with what weights? Certainly the weights are not income or ex post saving. This crucial variable is hard to define correctly and harder to measure in practice.

In summary, the key variable in the analysis—the real net yield—is subject to substantial measurement error. Even if this error is purely random, the traditional errors-in-variables analysis implies that its coefficient will be biased toward zero. But there is no reason to believe that this error is random; the systematic relation that would result if the error contains a trend or if the error is correlated with any of the other variables could bias the interest coefficient in any direction.

The problem is exacerbated (and the fruitful use of instrumental variable estimation precluded) by the small effective sample size: no more than twenty-three annual observations and as many as eleven explanatory variables in a single equation. With possibly little variation in the expected real net yield (or its certainty equivalent), substantial noise in its measurement, and a relatively small sample size, there is insufficient information in the data to provide useful parameter estimates. While I recognize the dangers, I am more sympathetic to Boskin's decision to use a longer sample period in which there was greater variation in the relevant variable, thereby both reducing the likely ratio of noise to signal and increasing the sample size.

Let me now turn to the principal innovation in the paper, the focus on personal cash saving. I think that specification of saving behavior in this way is basically a mistake. A reasonable theory of individual long-run decisionmaking should focus on a much broader concept of saving that more closely resembles the increase in the individual's net worth. The authors recognize this to some extent by including some proxies for a number of other forms of wealth accumulation among the explanatory variables, implicitly treating personal cash saving as conditional on the other forms of saving. Unfortunately, these other saving measures are not defined in a satisfactory way. Why is *gross* business saving included rather than net saving? Why are employer and personal contributions to social security used instead of "social security wealth" or some other measure of expected benefits? The government surplus is included even though it is an endogenous variable: a disturbance that increases consumption is likely to raise tax revenues and increase the government surplus, a correlation that may account for the negative sign on that variable in the saving equation. Moreover, the theoretical case for including the government surplus among the explanatory variables implies that the correct variable is the change in real government debt; obviously, recent deficits have been offset to a considerable extent by the effect of inflation on the real value of such debt.

Then there is the issue of tax policy. What would be the appropriate policy implication if the authors' conclusion that the uncompensated interest elasticity of saving is zero were accepted? Contrary to the final paragraph of the paper, if the uncompensated supply elasticities of saving and work effort were both zero, a substantial welfare gain would result from reducing the tax on interest and increasing the tax on wage income. The intuitive reasons for this statement, which I have proven formally elsewhere,[2] are that welfare gains depend on *compensated* supply elasticities, and the relevant price elasticity for intertemporal distortion is the *quantity* elasticity (future consumption) rather than the smaller *expenditure* elasticity (saving).

More generally, what policy implication follows if one believes that a higher saving rate would be desirable but accepts the view implied by this paper that our methods of statistical measurement are not powerful enough to assess the effect of the interest rate on the basis of the experience of the recent decades? Howrey and Hymans state that the tax on interest income should be lowered only if there were a "reliably measured" and "important" effect on behavior. Why? Because a compensated reduction in the tax can be predicted to increase national saving. The worst that can happen is that the increase may be small. There seems nothing to lose and everything to gain by trying.

Some participants at this meeting will object on the grounds that any move away from taxing all income at the same rate is somehow unfair. I reject this point of view for two reasons. First, I believe the current tax laws are unfair to those who cannot benefit from the many special rules that allow some forms of saving to go untaxed (accrued gains, pension contributions, IRAs, Keoghs, homeowner reinvestment rollovers, and so forth). Second, and more fundamentally, I believe that a fair tax system allocates the tax burden on the basis of consumption rather than income. As is well known, a progressive tax on consumption is equivalent to an income tax that exempts all investment income.[3]

Finally, it is interesting to ask why other countries like France have tax policies that are much more favorable to capital accumulation in general and to saving by low- and middle-income families in particular. It is certainly not that they are less egalitarian or more committed to private enterprise capitalism. Perhaps they know something that we do not. Perhaps the answer lies in the differences in our historical experience and intellectual tradition: the current European tax policies may reflect an

2. See Martin Feldstein, "The Welfare Cost of Capital Income Taxation," *Journal of Political Economy*, vol. 86 (April 1978, pt. 2), pp. S29–S51.

3. Exceptions occur when there are differences in progressivity that reflect timing differences.

earlier desire to rebuild capital stock after the war while tax policies in the United States are conditioned by the vestiges of a Keynesian fear of oversaving that was never very influential on the Continent but that still, remarkably, influences economists in the United States and Britain.

John B. Shoven: This is an important paper on a topic that has received increasing attention and deserves more. The authors, as well as those whose previous work is examined in this article, are to be congratulated for their work on such a key issue. In all growth and macro models two important variables are the average and marginal propensities to save. In all evaluations of the general efficiency of the economy, the consumption-saving margin—that is, the intertemporal allocation of consumption—is second in importance only to the labor-leisure choice. In political discussions regarding the competitiveness of the U.S. economy in the world market, the analysis of saving behavior is often looked upon as the major problem. And, most relevant to the paper, the debate on how heavily capital income should be taxed depends crucially on the elasticity of substitution between present and future consumption, which in turn is a function of the uncompensated elasticity of saving with respect to the real after-tax rate of return estimated here. The authors should, however, explicitly recognize that it is the substitution elasticity that is the variable of final interest when considering efficiency, and that a *zero* elasticity with respect to the real rate of return implies a *unity* rather than a zero substitution elasticity. One final word regarding the efficiency consequences of all this: it is important to bear in mind that this is a "second-best" problem. Eliminating the taxation of saving certainly will improve efficiency by itself. However, the lost revenue must be made up in some manner, presumably one imposing inefficiencies upon the economy. The replacement tax must be considered in order to complete the analysis.

The paper begins with definitions of types of saving and their relative magnitudes. The first striking fact is that personal cash saving (the net accumulation of demand plus savings accounts, bonds, new equities, and so on) amounts to only a small fraction of gross private saving. Table 2 indicates that personal cash saving was only 14 percent of NIPA gross private saving in 1975 and only 20 percent of business cash saving. Personal cash saving accounts for approximately 35 percent of net private saving, and the average propensity to save in the form of personal cash saving is shown to be only 0.22 percent for the period 1951–74. Certainly the direct effect of this form of saving is relatively small in determining economic growth. Unless Howrey and Hymans impute business cash saving to consumers and unless it is total private saving that is interest-rate sensitive, their measure of interest elasticity may be misleading in terms of policy implications because it applies to such an unimportant component of the entire picture.

Howrey and Hymans also discuss the inclusion of the accumulation of consumer durables, net mortgage repayment, and imputations in FF saving and (with the exception of consumer durables) in NIPA saving. They argue, correctly I think, that these forms of saving are not what persons concerned with capital formation have in mind. However, in a complete portfolio model of consumer behavior, the stocks and accumulations of these items will affect loanable-funds saving.

A major portion of this paper is devoted to a reexamination of three alternative approaches to the examination of saving behavior.

First, and most controversially I suppose, they look at the aggregate consumption-function approach associated with Michael Boskin. The technique is to add the real after-tax rate of return and inflation as explanatory variables to a relatively simple aggregate consumption function. Saving behavior is inferred implicitly. The central Boskin result is that the uncompensated elasticity is +0.4, which is derived from the examination of eight different specifications and econometric approaches to the basic equation reported in this paper.

Howrey and Hymans also challenge the robustness of Boskin's results. Each permutation of the data set or econometrics they make (including the deletion of 1934, the depression, substituting U_{-2} for U, and most importantly, using actual interest rates rather than expected rates) reduces the saving elasticity and frequently reduces it so that it is no longer statistically significantly different from zero.

I have several comments that apply both to Boskin's work and to this paper by Howrey and Hymans.

To begin, these are extremely simple aggregate equations. What interest rate should be used with them? Savers presumably look at an entire set of interest rates of different maturity and risk classes, borrowing as well as lending rates, expected future interest rates, and so forth, in allocating consumption over time. Choosing one interest rate is both difficult and simplistic. A paper by Backus and Purvis,[1] outlines an approach to estimate disaggregated portfolio holdings as a function of an entire array of rates of return. This approach seems more appropriate to the question at hand.

The strongest result in the Howrey-Hymans paper is that when actual long-run interest rates are used in the regressions in place of Boskin's expected interest rates, the sign of the saving elasticity becomes negative and significant. Should actual or expected interest rates be used? The answer depends on the planning horizon of the savers. If the household is saving for an acquisition to take place twenty years later, the actual rate offered on twenty-year bonds (preferably pure discount bonds) is appropriate. However, if the household is waiting for a period shorter than the maturity of the bond (say, the saving period is three years, using twenty-year bonds), then it would be correct to use *both* the current rate and the expected rate at the time of liquidation.

An entire literature exists on how demographics and life cycle considerations can largely account for aggregate saving. These issues are ignored here. In lengthy time-series analysis such as this, ignoring demographics seems untenable. Considerations of life cycle would also imply that savers (and dissavers) look at the entire spectrum of future expected short-run rates, perhaps derived from the existing term structure, in determining optimal behavior.

The time-series approach may not be the way to determine the real story here. Gleaning the effect of the real rate of return on saving seems nearly hopeless, particularly when business cycle effects are modeled by the unemployment rate alone. This may be why the inclusion or exclusion of the depression years is shown to be crucial to the results.

It should be noted that Boskin has done a considerable amount of work on this question since his article referred to above was completed, and he has produced some instrumental variable regressions that imply even higher saving elasticities than I have mentioned above. In his comment he describes this additional work and its implications and relevance for the current debate.

On the basis of the Howrey and Hymans paper, one must say that the weight of the evidence supporting the position that the real after-tax rate

1. David Backus and Douglas Purvis, "An Integrated Model of Household Flow-of-Funds Allocations," Cowles Foundation discussion paper 493, Cowles Foundation-Stanford Research Institute, flow-of-funds project (CF, July 1978).

of return to savers is not a prime determinant of saving is increased. However, because of the qualifications I have mentioned about both studies, I would still find a wide range of values (on both sides of zero) plausible for this key elasticity.

I have some briefer comments on the other approaches to estimating saving behavior in the paper.

The authors find a more robust positive saving elasticity in the equation representing the Houthakker-Taylor approach. However, the inclusion of inflation and the uncertainty regarding future inflation do weaken the rate-of-return variable to the point that it is statistically insignificant. They also observe that the propensity to save from different forms of income is substantially different, supporting to some extent the Cambridge theory, which states that the functional distribution of income is the primary determinant of saving.

When, in table 5, Howrey and Hymans estimate saving functions directly (for different definitions of saving) rather than consumption functions, they never obtain a significant coefficient on the real after-tax interest rate. Importantly, they use the actual Baa rate rather than the expected rate in this section. They also show that inflation reduces saving, whereas uncertainty about inflation increases it. Here, too, however, the coefficients are hovering near statistical insignificance.

The last section of the paper, in which functions are estimated for the small fraction of saving classified as personal cash saving, is a step backward in my opinion. One constantly must keep in mind how small the fraction of loanable-funds saving is that is referred to in interpreting the results of table 6. The authors examine the degree of rationality of savers with respect to business cash saving and government cash saving. Here, the single-equation approach is most offensive. The direction of causality implied by the equation (business cash saving "determining" personal cash saving) clashes with my belief that these variables are, at least to an extent, simultaneously determined. The single-equation approach severely distorts the results and does not provide meaningful tests of the hypotheses under consideration.

My final observation is that this paper does blunt the impression that empirical economists are finally coming closer to pinpointing the value of key variables in their models. That blunting may be valuable given the severe shortcomings of the studies undertaken thus far (including this one by Howrey and Hymans), but it should not discourage economists from continuing to tackle issues such as this, which the profession has avoided for so long.

Michael J. Boskin: I am pleased to have the opportunity to present comments on the Howrey-Hymans paper. Let me divide my comments into three parts: comments on their critique of my study; comments on their estimates of saving equations and on Denison's law; and discussions about the relationship of the interest elasticity of saving to the desirability of income or consumption taxation.

My original reaction to seeing eminent authors such as Howrey and Hymans devote so much attention to my early work on the consumption-saving choice was that I was flattered. Unfortunately, as I continued studying their paper, I noted that they did not cover in any detail the most important parts of my work. Therefore my first comment is they have totally ignored—to the extent of not even discussing—the most important results from my *Journal of Political Economy* paper or *any* of the results from my Treasury compendium paper with Lawrence Lau.[1] In each of these—the latter half of my *JPE* paper and the entire compendium paper—I estimated interest elasticities much larger than those contained in the equation Howrey and Hymans sought to reestimate. One of the major points of my *JPE* paper was that it was not reasonable to estimate consumption functions by single-equation methods; indeed, it was necessary to use an instrumental variables technique. In the second half of that paper I did so using as instruments principal components of a variety of exogenous variables from the major macroeconometric models. This resulted in a doubling of the estimated interest elasticity from around 0.2 to 0.4, with one estimate as high as 0.6. In my paper with Lau, we embedded the consumption-saving choice in a full model allowing also for a labor-leisure choice; this also resulted in precise estimates of an interest elasticity of saving on the order of 0.4. Once again, the instruments used were principal components of exogenous variables of macroeconomic models. This procedure not only accounts for cyclical fluctuations, but in principle distinguishes our saving (or consumption)

1. Michael J. Boskin, "Taxation, Saving, and the Rate of Interest," *Journal of Political Economy*, vol. 86 (April 1978, pt. 2), pp. S3–S27; Michael J. Boskin and Lawrence J. Lau, "Taxation and Aggregate Factor Supply: Preliminary Estimates," in Department of the Treasury, Office of Tax Analysis, *1978 Compendium of Tax Research* (GPO, 1978), pp. 1–15.

function from investment behavior. Hence, I am in the somewhat embarrassing position of critiquing a critique of my study that focuses on the issue of the interest elasticity of saving and uses my equations with the lowest estimated interest elasticity—equations that I personally, for economic and statistical reasons discussed in the papers mentioned above, do not claim to be my best results. In brief summary, the authors have been very selective in the part of my work they have chosen to critique, and under no circumstances would I consider their results and reestimations at all a satisfactory discussion of my previous work.

I do not even believe that the authors' interpretation of their reestimation of my equations casts serious doubt on the basic estimates. Any battery of reestimates of any time-series equation is likely to change the results. For example, in runs where the *t*-statistics are reduced to only marginal significance, Howrey and Hymans make much more of this than is reasonable. Reducing the *t*-statistic so that the estimated elasticity of approximately 0.2 is only marginally significant is not the same as demonstrating that it is remarkably small and economically insignificant. Indeed, most of the estimates confirm my previous results; for example, taking a Koyck lag results in estimates that are similar to my original equations. Dropping observations, lagging observations, changing the sample period, and so forth sometimes reduce the estimated coefficient to statistical insignificance (usually because the number of observations has decreased or because the variability of the right-hand variable is so reduced that a precise estimate of the coefficient could not be obtained). There are a variety of suggestions given as to why the authors have chosen to lag unemployment and so on, but again I must point out that the instrumental variables technique used in the second half of my *JPE* paper essentially accounts for the cyclical pattern of the economy, its growth, and the interaction of saving and investment. Hence I must conclude that even if the work they review was all I had presented, the Howrey-Hymans paper would not alter my conclusions very much. Indeed, their results reflect exactly what I would have expected would happen from a variety of transformations, dropping observations, changing sample periods, and the like. But again, more importantly, the selective nature of their critique ignores the most important sets of estimates which, coincidentally, are those with the largest estimated elasticities. This renders their critique somewhat less relevant than it might appear.

Next let me turn to Denison's law. It was pointed out to me by my colleague, Victor Fuchs, that the female-male wage ratio in both 1960 and 1970, holding other things constant, was approximately 0.6 and that if one looked at the Bible, in particular Leviticus, one would note that female slaves sold for 30 shekels of silver while male slaves sold for 50. I would attach no more structural interpretation to Denison's law, the alleged constancy of the gross private saving rate, and the inability of any economic policies to affect it than to the much longer apparent constancy of the female-male wage ratio. One of the two major points of my *JPE* paper was to point out how foolish it was to try to draw strong structural inferences about saving behavior from the apparent constancy of the gross private saving rate.

I do not see how anyone could disagree with this point. I am glad to see that Howrey and Hymans seem to agree with it, although it deserves more than their casual mention.

And what problems exist in the structural interpretation of Denison's law? First, neither the numerator nor the denominator, gross private saving or gross national product, measure the economically relevant concepts. Human capital is omitted from the analysis even though John Kendrick, Jacob Mincer, and others have indicated that much saving, especially early in life, is in the form of human capital. Also missing is the net saving of U.S. citizens overseas, which has increased substantially in recent years. Saving theory relates to net income and net saving, and again these vary markedly. Indeed, an interest in gross saving would only occur in the United States if we were strong believers in embodied technical change and cared about the rate of turnover of the capital stock. My own estimate suggests that the coefficient of variation of net saving is a large multiple of the coefficient of variation of gross saving for the postwar period. Further, this coefficient of variation would increase substantially if saving were adjusted to reflect replacement rather than historical cost depreciation. I take this to be a strong indictment of the simplest structural interpretation of Denison's law.

It is also worth noting that a constancy has never been noted in the private saving rate in any other country for a sustained period of time. Michael Edelstein has noted a substantial interest elasticity of saving in the United Kingdom, and Paul David has done so for the United States in the nineteenth century. Even if the view were taken that public and private consumption were perfect substitutes, so the share of total consumption, public and private, out of income was a constant share of wealth, as David and Scadding have argued, the fraction of wealth consumed would still be a function of the *net* rate of interest whereas income would be a flow from wealth at the gross rate of interest. As a consequence, policies that affected the ratio of the net to the gross rate of interest would affect the consumption-saving choice.

It is also worth noting some of the enormous changes that have occurred in the U.S. economy in the last few decades. The changing age structure of the population has been marked. The ratio of retirees to workers will go up 75 percent shortly after the turn of the century due to the combination of the post-World War II "baby boom" and the recent "baby bust." Since World War II, the life expectancy at age 60 has increased about a year and a half for men and three years for women, and the average retirement age has gone down substantially. For example, in 1948 one-half of men over the age of 65 were in the labor force. That number is now about one in five. This implies perhaps a 30 percent increase in the average retirement period. A large increase in the female labor force participation rate has occurred. The huge growth in the public sector includes a large rise in both average and marginal tax rates and an enormous growth in social insurance programs such as those for social security and unemployment, which may substitute for private saving. The increase in inflation in the last ten years affects saving decisions. There has been a sizable decrease in the average workweek, about 22 percent since 1929. This alone renders GNP suspect as an income measure. The saving rate out of "full income" has fallen substantially. And tremendous changes have occurred in typical family patterns. All these factors, if they had occurred alone, would have resulted in substantial changes in saving. The fact that they have balanced out each other is what leads to the apparent constancy in the gross private saving rate, and I see no reason to give a structural interpretation to that fact.

Efforts ought to be devoted to disentangling these effects rather than to giving strong structural interpretations to the reduced-form outcome. My own current research is specifically designed to disentangle such age and household effects from interest rate, income, and other effects on saving.

Let me now discuss the second half of the Howrey-Hymans paper in which the authors discuss their estimates of saving equations. They look at only a small fraction of saving. While this does account for a substantial fraction of the total variance, they essentially regress one component of saving on other components of saving, or the sum of other components of saving. This is the same as regressing the consumption of automobiles on the consumption of cigarettes, the consumption of food, and so forth. That is, it results in the usual kinds of specification bias and correlation between right-hand variables and the error terms in such estimated equations. Hence the estimated coefficients are biased, and I can give no statistical interpretation to their results. Ideally what ought to be done, and I think Howrey and Hymans would agree with me, is to disaggregate saving into its numerous components, include the rates of return of all types of saving in the economy and their covariances as well as a variety of other determinants of aggregate saving, and estimate a system of such equations. Unfortunately, this places extreme data demands on the researcher, demands which are well beyond current capabilities. That is why I focused on aggregate consumption functions in the first place.

The Howrey-Hymans interest-rate variable suffers from a major conceptual error. *They subtracted a one-year expected inflation rate from a long-term bond rate.* Obviously, an expected inflation rate over the time horizon of the bond is necessary. In my *JPE* paper, I contributed such estimates of long-term expected inflation rates. I also constructed estimates of the long-term expected return to capital from the Jorgenson-Christensen data on actual returns to capital. I used alternative measures of the long-run expected real net rate of return based on Moody's bond rates, high-grade municipal bond rates and the expected long-run return to capital. While the results differed slightly, each estimate of the long-run expected net-of-tax rate of return to saving produced a modest positive estimated interest elasticity of private saving. In view of the inconsistency in the generation of the Howrey-Hymans interest rate series and the likelihood that measurement error biases the estimated coefficients (in addition to the biases noted above), I do not believe much weight should be given to the equations they report with their own generated rates of return.

A variety of other issues relate to the interest elasticity of saving. To begin, it is simply not the case that a positive interest elasticity of saving implies that a consumption tax is preferable to an income tax and that a negative interest elasticity or a zero interest elasticity implies that an income tax is preferable. It could be that a consumption tax, or even an interest income subsidy, is desirable with a negative interest elasticity of saving; and a saving tax or a high interest tax—perhaps one even heavier than that at present—could be desirable with a positive interest elasticity of saving. As pointed out in papers by Joseph Stiglitz and me, by Martin Feldstein, and by A. B. Atkinson and Stiglitz,[2] for example, this choice depends upon the relative substitutability and complementarity with leisure of consumption early in working life and consumption during retirement. The full set of such compensated cross price elasticities must be known to reach a conclusion about the desirable, or efficient, degree of taxation of capital income. Lighter taxation, or subsidization, of capital income increases with the interest elasticity of saving only ceteris paribus.

Next, general equilibrium growth effects imply that the interest elasticity of saving in the overall economy is likely to be larger than that embedded in single-equation consumption estimates such as mine, or estimates of the elasticity of substitution in utility functions between consumption now and consumption in the future. The growth of the population and the likely growth of income due to technical change implies that, to obtain the total derivative of saving with respect to the interest rate, researchers would have to take account of the fact that a large fraction of total saving is being done by the young and it has to be compared with the dissaving being done by the elderly. Evidence of an enormous amount of dissaving done by young workers would be a strong indictment of a large estimated interest elasticity. Actually it appears that there is a substantial amount of saving done by young workers, although it is mostly in the form of investment in human capital.

I should also note that there are two issues in saving efficiency. The first is the "golden rule" rate in which the marginal product of capital will equal the rate of growth of the effective labor force, or the profit share in the economy will equal the net saving rate. If saving were below this golden rule rate, as Arthur Okun and others have mentioned, a variety of policy instruments could be used to deal with this: for example, by changes in government fiscal policy such as running a surplus, changing social security financing, and the like. There is still the issue of the mis-

2. Joseph E. Stiglitz and Michael J. Boskin, "Impact of Recent Developments in Public Finance Theory on Public Policy Decisions: Some Lessons from the New Public Finance," *American Economic Review*, vol. 67 (February 1977), pp. 295–301; Martin Feldstein, "The Welfare Cost of Capital Income Taxation," *Journal of Political Economy*, vol. 86 (April 1978, pt. 2), pp. S29–S51; A. B. Atkinson and J. E. Stiglitz, "The Design of Tax Structure: Direct versus Indirect Taxation," *Journal of Public Economics*, vol. 6 (July-August 1976), pp. 55–75.

allocation of consumption during individuals' lifetimes if their lifetime consumption-saving choices are distorted by heavy taxation of interest income. The second-best problem, as pointed out repeatedly by Feldstein, Atkinson, Stiglitz, Diamond, myself, and others, also needs to account for misallocations in the labor market. These misallocations are purely a function of the compensated elasticities, not the uncompensated ones, and even if the total interest elasticity were zero, the compensated elasticity might be positive; if the total elasticity were negative, the compensated forward-price elasticity of the demand for future consumption could still be substantially negative. In either of these cases a situation would result in which a consumption tax or a lighter taxation of interest than labor income might be desirable.

If through dynastic families or any other means, households took a much longer run view and, for example, maximized the sum of discounted utility a la Ramsey, all the problems under consideration would be transitory, and the economy would converge to a new steady state with the same real net rate of return.

Let me make one final statement about the Howrey-Hymans paper and one plea for more research. Howrey and Hymans do point out that my work on consumption functions—as all other work on consumption functions, with few exceptions—does not explicitly build a dynamic model of saving behavior. I concur with this observation, and I am working on this problem now. I only report that my original results did not do so because I was hoping to compare them with the traditional consumption function estimates. And there are few parameters of more interest in the economy than the interest elasticity of saving. This parameter affects our notions about the long-run efficacy of fiscal and monetary policy, the effect of inflation on the real economy, the incidence of various taxes, the desirability of consumption versus income taxes, and the social rate of discount or the social opportunity cost of public funds. Further work on this subject is desperately needed, and I look forward to adding Howrey and Hymans to the list of people who are working hard to improve our knowledge on the subject.

E. Philip Howrey and Saul H. Hymans: The discussants of our paper have raised several general questions that are best handled by a common response, after which we shall turn to some of the specific matters raised by individual discussants.

We acknowledge the need to consider more carefully yields on alternative types of saving assets. We also accept as valid those criticisms regarding possible simultaneity bias in our estimates. However, the concern of several discussants with potential simultaneity bias in our estimate of the effect of government saving (*GCS*) on personal saving is surely misplaced. It is argued that a disturbance that lowers saving (the dependent variable) is likely to increase tax revenues and hence the government surplus (an independent variable) and thus produce the estimated negative coefficient that relates *GCS* to personal saving in our equation. But the presence of personal tax payments (*TX*) in our equation effectively rules out this kind of spurious result. Simultaneity bias may be present, but if it is, it has a more subtle origin than is suggested by the discussants.

A resolution of the simultaneity issue as well as the question of which of several interest rates should be included in the analysis requires a richer data base than is currently available. The aggregate time-series approach is subject to severe limitations that cannot, in our opinion, be adequately overcome by increasing the data base to include observations for the period between the two world wars or by using quarterly data for the postwar period. We firmly believe that any chance of substantial refinement of the estimation of interest rate effects on saving awaits the ability to conduct the analysis as a panel study based on a time series of cross-sectional observations on household decisions. Within that context it would be possible to disaggregate according to wealth levels, to observe units that may react to different rates of return and that are subject to different marginal tax rates, and so on. At an aggregative level, there is little choice but to try to identify a best representative interest rate, as we did; the data are basically unable to distinguish independent effects of alternative interest rates at a high level of aggregation.

The specific issues raised by various discussants are much less compelling criticisms than the general issues just discussed. Michael Boskin's criticism of our work is based largely on a contention that we have failed to review all of his work on the estimation of the interest elasticity of saving. Virtually all Boskin's results are based on an interest rate processed according to some "magic" formula, which seems to have produced some anomalous results. For example, Boskin's expected real interest rate and expected rate of price inflation imply an expected nominal interest rate of −3.7 percent in 1934. We suggest that Boskin's results should not be taken seriously until the construction of his interest-rate series is explained and justified. It may be the uniquely correct measure of the rate of return to saving, but at this point it is merely the product of a black-box transformation.

John Shoven argues that our analysis is unimportant because personal cash saving is a small proportion of aggregate national saving. Surely this misses the point. Personal cash saving is small on average, but it accounts for a good deal of the variation in total saving, and that is what counts for stability of the aggregate source of funds for capital formation. Shoven also argues that the interest elasticity of saving is not the key parameter for the question of microeconomic welfare efficiency. That may be so, but it is the key parameter with respect to the availability of loanable funds that is the issue we attempted to study.

Martin Feldstein argues that the substitution of a payroll tax for a tax on interest income may reduce current personal saving, but that it cannot reduce total national saving. But suppose that our interest elasticity result is correct and that a tax substitution (a compensated tax change, in Feldstein's terms) leaves personal saving unchanged. By definition, a compensated tax change leaves current tax receipts unchanged so that government saving is also unchanged, and aggregate saving is constant as well. The only possibility for increased saving as a result of the tax substitution must therefore arise from its being accompanied by a change in the level or distribution of income. It would then be necessary to uncover and analyze the process by which the tax substitution would produce such a change in income and establish its effect on saving.

Feldstein and Boskin both argue that our estimated interest-rate effect is likely to be biased toward zero because of measurement error in the rate of return entered in our equation. As is well known, the implication that measurement error biases coefficient estimates toward zero derives from the elementary situation in which a single independent variable is measured with error. We would not want to claim—even if Feldstein and Boskin believe it—that the only possible violation of the classical regression assumptions that pertains to our equation is measurement error in the rate of return. We doubt that it is possible to point to a particular direction of bias in our interest-rate coefficient with any degree of confidence. Indeed, we believe that our discussion of how the interest rate should affect saving according to what is included in different measures of saving is far more important and to the point than is the statement of a highly restrictive result concerning errors in variables.

Boskin and Feldstein argue more specifically that our interest rate series is conceptually incorrect because we have subtracted a short-run expected rate of inflation from a long-run nominal interest rate. Three points should be noted in this connection. First, a distributed lag involving past rates of inflation in place of the Survey Research Center expectations variable yields similar results in our saving equations. Second, observations on an expected long-run rate of price inflation are simply not available, except as may be measured by a weighted average of recent price changes. Third, we see no reason to believe that any mechanical procedure for deriving long-run expectations of inflation from past price changes is a better measure of inflationary expectations than the Survey measure we used. The Survey Research Center variable indicates what respondents think about inflation, and its interpretation can hardly be limited to the exact time frame of the survey question.

Finally, Feldstein—like Shoven—misses an important point by discussing a problem in which he is interested, rather than the problem that we addressed. We stated that a policymaker interested in increasing the funds available for capital formation would be unlikely to manipulate the tax rate on interest income unless the after-tax rate of return could be shown to have a substantial and reliably measured effect on saving. Our analysis casts serious doubt on the proposition that loanable-funds saving responds to the rate of return to saving, and that justifies our concluding paragraph. If Feldstein wishes to argue that other goals (such as increased welfare or economic efficiency) justify tax substitution, he is certainly free to do so. What we claim is that the argument for tax substitution cannot be justified by the proposition that it will change the supply of funds available for capital formation.

General Discussion

Several participants continued the discussion of the interest rate that was used by the authors and by earlier researchers. Arthur Okun questioned the use of the Baa bond rate because the great majority of savers do not hold such bonds. He was also doubtful of using financial assets with large liquidity premiums, such as money or time deposits, even though they are widely held and suggested that the rate individuals pay to borrow money was a much better indicator of their rate of time preference and was also a rate that applied to a great number of people. Frederic Mishkin noted that the Baa rate is sensitive to changes in the degree of uncertainty and conjectured that its success in the equations might reflect the response of saving to uncertainty. Thomas Juster and others remained puzzled by how Michael Boskin's interest rate variable was constructed and were troubled that the estimated response of saving to interest rates was apparently so sensitive to this construction. William Brainard noted that including both wealth and interest rates in Boskin's equation might confuse the relation between interest rates and saving because interest rates and wealth were themselves related. He also suggested examining the response of net savers and net dissavers separately to determine how much each group contributed to variations in total saving and to what the net saving of each group responded.

Christopher Sims did not believe the estimated equations could be used to infer the response of saving to a change in the taxation of saving. The historical dynamic relation of the after-tax real interest rate to saving is probably not reliable if used to predict the effect of permanent, policy-generated changes in after-tax real interest rates. Expectations are important in saving and investment decisions, and policy-generated changes would probably be expected to be more persistent than normal historical changes in interest rates. He suggested that international cross-section analysis might be more useful for identifying the response of saving to alternative tax treatments.

Participants discussed the other explanatory variables in the authors' preferred saving equations. Larry Dildine observed that the calculation of imputed incomes often utilizes interest rates, which might explain the large coefficient on this variable. He also noted the irony that the authors' income decomposition implied that saving would be increased by raising taxes in order to increase transfer payments. Mishkin reasoned that one would have to distinguish between changes in permanent and transitory components of the different income measures to derive long-run conclusions about the propensity to save out of different components of income. Juster suggested that the Denison's law results might reflect a rise in pessimism that correlates with a decline in corporate saving. He suggested that a direct measure of optimism be used to explore this possibility.

The panel also considered the paper's policy implications. Sims observed that whenever the high U.S. tax rates on capital income are discussed, economists can offer only vague theoretical discussions and uncertain empirical evidence regarding the effect of taxation on saving. The argument invariably turns to distributional effects of proposed tax changes, and Sims asked how much is known about that. Okun pointed out that, even if the evidence on the response of personal saving to interest rates were more robust, it would tell us little about whether investment should be increased. And if more national investment is desirable, there may be better ways to pursue it than by reducing taxes on personal saving. National investment and saving can be encouraged by altering the fiscal-monetary mix of policy or by changing fiscal instruments such as the investment credit or other business taxes. The distributional effects of these policies are less tendentious. And their effects on investment are more predictable.