Introduction

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THE Conference on Models of Income Determination reflected an attempt to bring together economists and statisticians interested in national income theory and measurement with those interested in using national income and related data for constructing either "complete" models of income determination or individual "structural" relations which can be used in such models. The papers in this volume, prepared for that conference, will well repay the reader's effort. They are of high quality, present substantive new results as well as methodological insights, and deal with some of the more basic problems of economic analysis in general and of income models in particular. Even a hasty perusal of the volume will indicate the progress made in the state of these arts in recent years. However, it will also indicate how much more remains to be done before we have reasonably satisfactory models for forecasting, policy-making, or better understanding of economic behavior. As a result, prior to consideration of the individual papers it may be useful to comment briefly on a fundamental difficulty of model construction.

In constructing a model of income determination, there are an impressive number of possible ways of combining different forms of each of a number of structural equations if the number of equations and the number of forms fitted or tested for each equation are at all large. Thus, for a system of thirty equations and ten forms tested per equation, the theoretical number of combinations is 10^{30}. The number of time series observations available for distinguishing among these combinations is painfully limited. Consequently, it is extremely difficult if not impossible to choose among a glittering array of estimates of even the most basic parameters of the system, such as the (short-term or long-term) marginal propensity to consume out of income, to say nothing of the more esoteric parameters.

An obvious approach to the solution of this problem, which has been experimented with to a very limited extent, is to make use of cross-section data for households and business firms (whether they are obtained by special surveys or other means) to derive as many as possible of the parameters in the model. However, this approach has probably led to less reliable parameters in general than even the time series data, for reasons which should by now be well known,

including in particular the difficulty of inferring intertemporal behavior from interpersonal data and of separating long-run from short-run income and other effects.

A more satisfactory solution to the problem would seem to require the collection and use of continuous cross-section data, where the response of economic behavior to changes in relevant variables can be traced either as these variables change normally over time or in response to special stimuli. Thus, continuous cross-section or panel data for households should be able to provide reasonably reliable estimates of the marginal propensity to consume out of different types of income or out of wealth. Such data for business firms—which already exist—might provide more reliable estimates of the key parameters in the relationships for inventory and perhaps also for plant and equipment demand. To give one other type of example, specially designed surveys could measure the relation of hours of work or length of the workday or workweek to productivity (the importance of which for growth models is indicated in Edward F. Denison's *The Sources of Economic Growth in the United States*).[1] It would probably not be a very fruitful exercise to try to determine this effect in the usual framework of a production function embedded in a complete economic model all of whose parameters were derived from time series data.

One further comment on model construction may be in order. Virtually everyone will agree that the model used should depend on the subject of the analysis and that it is highly unlikely (at least in our time) that the same model will explain the national income in a period, the price of wheat, the quantity of steel produced and consumed, interest rates, stock prices, and the level of assets of various financial intermediaries. However, as some of the subsequent papers and comments will indicate, there does not seem to be the same agreement on the likelihood that different models of income determination may be useful for different objectives, e.g., for forecasting versus more ambitious purposes, or for short-run versus longer-run forecasting. These papers and comments show a substantial divergence of opinion about the relative superiority of the large-scale models for short-run forecasting of the national income—a method increasingly being used by econometricians—versus smaller models which concentrate on variables and relationships of primary cyclical importance. It may be useful therefore to point out that, regardless of

[1] Committee for Economic Development, 1962.

the merits of these different views, if secondary variables are introduced into the analysis, the models may have to be made considerably more complicated than even the large-scale models now customarily used.

Thus, if an attempt is made to integrate financial variables into a model of income determination in order to analyze the interrelation of the financial and real variables, I suspect that the usual introduction of an interest rate (or sometimes short-term and long-term rates separately) would be inadequate without expanding the model by introducing also the cost of equity financing, since these two costs may frequently move in opposite directions. Short-run forecasting can conceivably be done best on the basis of a simple model consisting of a few key relationships which may or may not require financial variables, but once an attempt is made to assess the effect of financial variables of a somewhat lower order of importance it is essential that all interrelated "secondary" variables also be included.

I shall now touch briefly on each of the papers to give some indication of their coverage, with a minimum of reference to the illuminating and often spirited comments made by the discussants of these papers or to the answers by the authors.

Lawrence R. Klein's paper is of particular interest since it describes, for the first time probably, the most comprehensive short-run model of the United States economy available and has already been used for forecasting. The paper presents and analyzes the utility of a large-scale quarterly model which consists of twenty-nine structural relations plus some accounting identities and tax and transfer payment relations, makes use of some *ex ante* as well as *ex post* variables, some financial as well as real variables, and is fitted to the period from 1948 through 1958, with all the parameters determined from time series data. The results seem promising, though it is too early to appraise this model adequately. However, a number of questions are raised by various discussants—including Edward F. Denison, Franco Modigliani, and Irwin Friend and Robert Jones—ranging from data problems to the rationale of signs and magnitudes of several regression coefficients, the degree of aggregation, the justification of some of the variables and equation forms employed, and the predictive accuracy obtained. The numerous issues and the different viewpoints are spelled out in the comments by these discussants and in the replies by Klein.

T. M. Brown's article represents a detailed description of the

annual moderate-size Canadian model (nine structural equations plus a large number of tax and transfer equations and accounting identities) on which he and his associates have been working for a number of years. The model is fitted to the periods 1926–41 and 1946–56. Again, the magnitudes of some of the regression coefficients seem suspect. Thus, the extremely high (0.94) long-run marginal propensity to consume services and perishables out of wage and salary disposable income does not seem plausible. Other questions are raised by Carl Christ, who compares the Brown and Klein models, and by William C. Hood, who points out that only half of the gross national product is covered by structural equations, the other half being exogenous. The information on goodness of fit and forecasts is not sufficient to appraise the results satisfactorily. However, many of the tentative results and the discussion of their possible policy implications are interesting, and a number of useful recommendations are made to improve the model. Thus, Brown suggests that comprehensive monthly economic data be collected from a stratified sample of households and firms to test for changes in structure from the past to the current period. This suggestion should probably be expanded, as noted earlier, to cover the determination of the historical "time series" structural coefficients themselves wherever possible.

While the first two papers are devoted to the presentation and discussion of specific complete economic models (in the United States and Canada), the four papers that follow are concerned with recent developments relating to individual major structural relationships. The paper by Jean Crockett starts with the thesis that we have been unable so far to measure accurately the effects of income or assets on consumption primarily as a result of three factors: inability, using either time series or cross-section data, to separate satisfactorily various "permanent" and "transitory" components of income which may have different effects on consumption; inability to differentiate among consumption (or saving) propensities of various groups in the population which, particularly in cross-section data, may greatly distort measurable asset effects; and failure to take account of the interaction between income and asset effects. Crockett next offers a careful examination of the biases involved in estimating income and asset effects, in which she notes the promise of continuous cross-section data for avoiding such biases. She then sets up a model containing six groups of families, classified on the basis of the relative magnitude of their saving propensities and the sign of their transitory

income, for each of which she derives a theoretical consumption function which is tested qualitatively against the 1950 BLS-Wharton data and the 1955 *Life* data. She concludes that the results indicate the desirability of grouping families in cross-section studies on the basis of both saving propensities and the relation of actual to normal income. Using time series data as an interim device, she estimates transitory income as the deviation from a time trend of actual income and relates consumption to actual income and transitory income; to actual income and a variable whose value depends jointly on liquid assets and transitory income; and to actual income, transitory income, and the deviation of actual from "normal" liquid assets. A tentative conclusion of this analysis is that the marginal propensity to consume with respect to normal income is a little higher than with respect to actual income, while the marginal propensity with respect to transitory income is distinctly lower, though well above zero. The discussant of Crockett's paper, Daniel B. Suits, discusses a different method, which he has utilized to achieve the same objectives.

Robert Eisner's paper is another in a series of interesting studies by the author of the relative utility of a distributive lag accelerator versus past or current profits in explaining investment in plant and equipment. Eisner, using cross-section data, for each of the years 1955–58, to which he applies a wide variety of tests, concludes that the acceleration principle is highly useful in explaining plant and equipment expenditures, whereas any influence of past or current profits is "in large part, if not entirely, . . . a 'proxy' [effect]." The evidence he presents on this important proposition is impressive, though the discussant of his paper, Bert G. Hickman, presents a number of reasons for questioning the conclusiveness of Eisner's findings, which attribute an insignificant role to realized profits and internal funds in investment decisions. Eisner's finding is rather surprising, since there is no theoretical presumption that the large fraction of plant and equipment expenditures for cost-cutting and new products is affected by the acceleration principle, whereas they are presumed to be greatly influenced by the past and current profits environment and perhaps to a lesser extent by the availability of internal funds. The empirical evidence he adduces relies largely on a profits variable which is the ratio of profits before taxes to gross fixed assets, rather than the ratio of profits after taxes to net worth; and in the one set of regressions where the latter variable is employed,

profits become relatively more important. It would be interesting to test what would happen to the relative importance of change-in-sales (accelerator) and profits variables if, in addition to change-in-sales variables for a number of different years and the ratio of a single year's profits to net worth, profits to net worth for other years were introduced, to put the two types of variables on a more equal footing. It might also be useful to test change in (rather than the level of) profits versus change-in-sales variables.

Michael Lovell's paper on "Determinants of Inventory Investment" provides a highly thoughtful review of prior work, including that relating to the determination of equilibrium inventories, adjustment lags, and the usefulness of anticipated sales data versus suitable proxies based on *ex post* data. On this last point, he argues that the anticipations data give only marginal improvement over suitable proxies. In common with authors of several of the other papers, he notes the importance of continuous cross-section data (for individual firms) to answer the relevant questions for setting up a satisfactory structural equation in this area. The relative stress on unfilled orders and changes in unfilled orders as determinants of inventory investment in equations which Lovell presents is questioned by Ruth P. Mack, who states that ". . . unfilled orders and their rates of change explain too much and sales too little of inventory investment." In this connection, Mack points out that orders held by the machinery and transportation equipment industries alone "constitute on the average over 70 per cent of total outstanding orders and also dominate rates of change." It is interesting to note that a subsequent paper in this volume finds plant and equipment anticipations to be a more useful variable than unfilled orders in explaining inventory investment, suggesting that the former is a more powerful expectational variable.

The paper by Wilfred Lewis, Jr., on "The Federal Sector in National Income Models" covers a sector of the economy for which structural relationships obviously cannot be derived in the same manner as for the private sectors. However, Lewis does attempt to determine some of the interrelations between the federal sector (receipts and expenditures) and the rest of the economy "in the hope of increasing, if only slightly, the precision with which the government sector can be handled in long-term and short-term economic models." A wealth of useful results and hypotheses are presented on such matters as the long-range income elasticities of different types of

federal revenues (and of total revenues), long-range projections of federal spending, and the short-run behavior of the built-in stabilizers. One discussant, Bert G. Hickman, is dubious about the success of Lewis's interesting "shortfall method" of measuring the response of consumption to changes in income during economic contractions, which Lewis uses in arriving at his conclusion that the importance of the "direct" stabilizers has increased over the postwar period. The other discussant, Joseph A. Pechman, also questions the conclusion that the direct stabilizers have been materially strengthened.

The last three papers in this volume are concerned with different kinds of problems than those associated with the derivation of the usual type of "complete" economic models or individual structural relationships. In their paper on "Short-Run Forecasting Models Incorporating Anticipatory Data," Irwin Friend and Robert Jones tentatively conclude, both on a priori and empirical grounds, that *for short-run forecasting* a simple small-scale model is likely to do at least as well as the more complex large-scale models. They find that the specific quarterly, semiannual, and annual models utilized, containing four structural equations and one identity, seem to give both good fits for 1951–60 and relatively good "forecasts" for 1961–62, and that none of the coefficients seems unreasonable. The semiannual and apparently also the annual models give somewhat better results than the quarterly models, even though they entail forecasts for substantially longer periods ahead. This improvement in results may reflect the averaging out of erratic short-term changes in the data (including errors of observation). Of the business and consumer anticipations series tested in these time series models, only plant and equipment anticipations clearly and consistently add to predictive ability. Both discussants of this paper, F. Thomas Juster and Lawrence R. Klein, express reservations about the promise of small-scale models for short-run forecasting, with the former also questioning the form in which the anticipations data are introduced into the models.

Albert Ando's paper is an imaginative attempt to spell out the implications and statistically to derive the parameters of several growth models. Some of the assumptions include a linear consumption function homogeneous in labor income and consumer net worth, a Cobb-Douglas production function for each type of good, a constant rate of neutral technological change characterizing each production function, perfect competition in all markets, and labor which

is homogeneous and increasing at a constant rate over time. The last model, which receives most empirical attention, distinguishes two types of goods, consumption and investment, and introduces a government which purchases goods and imposes taxes on income of individuals, and a banking system which issues money in exchange for individuals' indebtedness to it. Ando notes that his models explain constancies in the relative shares of income, the rate of return on capital, the saving-income ratio, etc., without making these assumptions explicitly, and that they lead to empirical estimates of several key parameters which are not too unreasonable. However, Ando also points out the unsatisfactory nature of some of his results, including the substantial understatement of the required rate of return on capital (which he tentatively attributes, at least in part, to the absence of uncertainty from his models) and the evidence of an appreciable historical decline in the coefficient of capital in the production function for investment goods (which if taken at face value would invalidate the assumption of a Cobb-Douglas production function with neutral technological change). These models, as Ando points out, are not intended to reflect factors which cause the economy to deviate significantly from the smooth path of equilibrium growth defined by them, i.e., the models essentially are set up to explain full-employment periods. In his discussion, Ralph W. Pfouts observes that two of the three variables used to explain output—technological change and employment (capital is the third)—are determined as functions of time and that this seems unsatisfactory, at least for employment. He also questions the "passive" role assigned to investment in the models, which, he states, contain no "realistic" explanation of the determinants of investment, since the demand for investment is determined by marginal productivity, saving is determined by the consumption function, and the model adjusts so that saving equals investment.

The final paper, by Zvi Griliches, is largely a critical analysis of price data from the viewpoints of general reliability and economic relevance to the basic problems to which they are applied. Griliches discusses a suggestion he had made earlier on how to measure quality changes through cross-section price specification regressions; e.g., prices of automobiles (in logarithmic form) are related annually, for 1950–61, to such specifications as brake horsepower, shipping weight, and over-all length, showing the impact on price of a unit change in a particular specification, holding other specifications constant.

Griliches then uses this information to obtain "quality-adjusted" price indexes. The first discussant, George Jaszi, states that the conventional approach would give the same results in principle as Griliches' method and concludes ". . . it is a mistaken idea that we can get at a better measure of quality by a study of consumer evaluations of the various features inherent in a good than by a comparison of costs." The second discussant, Edward F. Denison, emphasizes that the real disagreement between Griliches and the national income statisticians (including Denison) relates to the determination of the commodity or service to be priced and points out that "switching the criterion for the 'commodity' to be priced from what the consumer actually buys (hospital care, surgeon's time, drugs, etc.) to what he 'really' wants is a dangerous and inconclusive game for the statistician to play." In reply, Griliches states that he considers this regression approach as more clean-cut operationally than the conventional method but more generally "as only a first step toward the construction of constant utility or productivity level price indexes." He disagrees with the "Denison-Gilbert-Jaszi position" in that he does not consider it feasible or of economic interest to construct "a value-free set of price and output indexes, independent of a welfare framework or of production or utility function considerations." The basic differences of opinion involved may or may not revive one of the older arguments in the national income literature.