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COMPUTABLE GENERAL EQUILIBRIUM MODELS: RETROSPECTIVE AND PROSPECTIVE

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One interesting issue is why it should have taken so long before the general equilibrium models originating with Johansen caught on in empirical policy analysis. After alI, these models are more closely related to microeconomic theory than are input-output, linear programming, or macroeconometric models.

There were both theoretical and empirical reasons for this lag. The theory of general equilibrium in the 1950s and 1960s was concerned with establishing the existence and uniqueness of equilibrium. It was not clear at the time that equilibrium was stable. The Scarf counterexample provided a demonstration that, under quite plausible conditions, equilibrium might not be found through a tattonement process; and, under the circumstances, a search for an empirically implemented general equilibrium solution quite naturally appeared to be premature. Also, Samuelson proved that, under a certain set of restrictive conditions (the extent of whose restrictiveness was not clearly understood), one could formulate a linear programming problem whose solution prices would approximate the purely competitive market prices. Last, but not least, computational methods for solving linear programming problems and for estimating macroeconometric models were routinized and readily available.

At the same time, the issues for which such models seemed most appropriate were not the main issues salient for empirical planning. The main issues

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addressed by development planning in the 1950s and 1960s were how to allocate government investment and how to choose a target growth rate which might harmonize expected foreign capital inflows with foreign exchange requirements, on the one hand, and with savings-investment requirements on the other. The first issue appeared to be best answered by dynamic linear-programming models; the second, by national accounts-based econometric models. Links through relative prices to incentives that affect production and investment decisions and the make-or-buy decisions that determine a country's foreign trade posture were secondary to these choices. Decisions were centralized in planning offices, credit was uniformly undervalued, and foreign exchange was overvalued so that the role of incentives in allocating scarce resources was minimal. Quantitative controls and rationing of credit and of import licenses were the mechanisms used to implement government choices of investment and trade policies in a regime of chronic excess demand for import licenses and for credit. Whatever incentive planning took place, primarily in the countries of the Organization for Economic Cooperation and Development, took the form of an institutionalized interaction between groups of experts, on the one hand, and econometric forecasts on the other. It was only when economies became more complex and investment decisions became more decentralized that the role of relative prices became more important in implementing plans. The shift of concern toward understanding the distributional implications of alternative investment and trade strategies also contributed to an interest in relative prices as prices are major transmitters of the consequences of these strategy choices to household incomes.

The first-generation computable general equilibrium (CGE) models applied to developing countries were aimed toward the analysis of distributional

issues and were more complex than the second-generation models. They were dynamic and combined macroeconomic features with a core general equilibrium model. They were, also, less neoclassical than the second-generation models. The price paid was that their structure was less transparent.

When the first-generation CGE models were first formulated, two issues were raised: (1) would it be possible to solve these models and (2) would it be possible to understand what drives their results? Clearly, both issues have been laid to rest. There was general agreement among the participants at this Conference that solution problems are not an issue; indeed, one of the models presented was implemented on a microcomputer. As to the question of interpretability, the organization of this Conference provided a rather stringent test. The models were presented not by their originators but, rather, by a discussant who had had nothing to do with the implementation of the model. Not only were all of the discussants able to understand quite clearly the specific features of each model, they were also able to illustrate by means of simple analytic and/or graphic models what drove their results. Not one of the model implementers complained that the nature of his model had been misunderstood.

The second-generation models presented to this Conference are all comparative statics models. Investment and the price level are fixed, and neither money nor assets is part of the system. Some of the effects of tariff changes, such as induced increases in the domestic price level and induced changes in the rate and level of investment, are, therefore, impossible to analyze with these models.

The second-generation models have become specialized. There are three rather different types of models that have been developed for special

purposes: (1) tax models that are applied mostly in public finance relating to developed countries, (2) energy models that have been applied mostly to energy investment decisions for industrial countries, and (3) trade policy models that have been applied mostly to structural adjustment issues for developing countries. Each class of models has evolved rather distinctive characteristics.

The public finance models are generally static; they are the most nearly neoclassical in structure, and in them both capital and labor are fully mobile. They are used in comparative statics experiments relating to alternative tax structure to investigate issues of incidence, tax-burden shifting, and effects upon the structure of production.

The energy models, by contrast, are dynamic, generally fairly aggregative, and use fairly flexible production functions that are capable of exhibiting varying degrees of substitution and complementarity in factor use. They generally use intertemporally consistent dynamics and try to focus upon long-term, turnpike behavior. The major issue they address is energy-investment policy.

The third class of models, the trade structural-adjustment models, are the least neoclassical in that they incorporate various rigidities in the product and factor markets; they have simple dynamics of the "lurching equilibrium" type that is appropriate to the intermediate run; and they incorporate substitution possibilities between domestic production and imports. They vary in what adjustments are needed to bring about the ex post equality between savings and investment (known as macroclosure rules). The focus of these models is on the choice of trade strategy and the structural adjustments in domestic production that these changes imply. Thus, the second-generation models have become more sharply focused and more problem specific.

It is interesting to speculate on the future directions in which CGE models might evolve. There are two major directions in which more work is urgently needed--dynamics and micro-macro links. In both, the major impediments are the state of theory rather than the availability of data or modeling capacity.

With respect to dynamics, the major unsolved issues are how expectations are formed (rational or adaptive, consistent with realizations in the next period or consistent extrapolations from the past period) and how the pace and direction of technical change are set. It is in these areas that theory is weak and there is a paucity of verified econometrics and empirical studies. The dynamic features of labor markets also require more study: what drives occupational mobility, labor-force participation, and investment in schooling is not well understood. In other aspects of dynamics, the state of theory and empirical work is a great deal more developed; investment theory, population growth, and migration are all sufficiently well-studied phenomena to permit simplified models of these aspects of long-term change to be implemented empirically. Despite current deficiencies and lacunae in conceptualization, it is important to move toward implementation of dynamic models as there are many instances in which short-term effects and long-term effects do not agree, even in direction.

Currently, the major policy issues facing developing countries are how to cope with critical foreign exchange shortages and what combinations of structural-adjustment and stabilization packages to implement. The analytic treatment of these issues requires a melding of CGE models with macroeconometric models. The first-generation models attempted to do so, admittedly without great sophistication. The Adelman-Robinson model, for example,

combined a rudimentary model of money holdings with a fairly elaborate model of the loanable funds market and of the formation of investment demands by firms under two alternative money market regimes (loose money and tight money), the choice among which was endogenous. The second-generation models have no money holdings and, except for the energy models, set the allocation of investment exogenously (if at all). It is now time to reintroduce a form of micro-macro melding.

There are several strands of theoretical literature that should be helpful in this endeavor: The literature on alternative factor and product market rigidities and their macroeconomic implications originating with Barro-Grossman, Benassy, and Malinvaud; the literature on the demand for money in a general equilibrium framework originating with Hahn, Dreze, and Grandmont, on the one hand, and with Clower and Leonhuftvud on the other; the literature on investment and portfolio choices by individuals and firms attributable to Tobin; the literature on international capital movements originating with McKinnon, and Dornbush and Fisher; and the literature on closure rules and developing country macroeconomics originating in Latin America and in Cambridge (England) whose important contributors are Taylor and Kaldor. Thus, there is a rich theoretical menu to draw upon; the primary difficulty is an embarrassment of riches that requires reconciling sometimes conflicting characterizations of reality. The time appears to be ripe for such an effort; indeed, the modeling could be used to sort out the differences that matter from those that do not.

Other interesting but not so urgent extensions of CGE models are endogenizing the political sector, in an effort to forecast which policy

choices are the most likely to be made at a particular point in time, and incorporating transactions costs and uncertainty.

The burgeoning literature on "rent seeking," which started with the work of Tullock and was extended by Krueger, Baghwati, and Srinivasan, has been taken in an interesting and fruitful direction by Brock and Magee. It would be interesting to expand their game-theory models to include more political actors (classes, industries, parties, bureaucracies, and the military) that interact with the decision-makers, both directly and indirectly, in political markets aimed toward rent seeking. The CGE model could be used to evaluate the potential benefits of the alternative policy choices and the threat points in the bargaining games of the participants. They can also be used to figure out the "production functions" of rent seeking although there is good reason to believe that the resources used in this process are relatively minor in developing countries in which there are no permanent lobbies. Judging by the experience of my father, who was an industrialist and importer in a developing country operating in a regime of import licenses, these costs consist mainly of a few cups of coffee in a cafe and a few dinner parties at home. All other direct costs are transfers.

Transactions costs offer a more flexible way of incorporating rigidities and sluggish adjustment than the fixed-price, fixed-wage models currently in use. These would assign explicit costs to shifting resources from one sector to another in adjusting to changes in incentives and would result in partial adjustment of prices and factor use in response to policy-induced change. Much of the Conference was devoted to a discussion of the smallness of the effects obtained through tariff variations, and the papers presented offered

various "fixes" to circumvent the problem. These fixes are all special cases of transactions costs.*

The introduction of uncertainty into CGE models would increase their realism. Most of economic life relates to decision making under uncertainty. This is especially true with respect to decisions of farmers, whose yields are inherently stochastic, and with respect to portfolio choices in both the financial and the real sphere. Among the real choices that could be viewed as portfolio choices in a world in which the prospective returns to alternative actions are uncertain are the make-or-buy decisions involved in international trade, the labor-allocation decisions involved in rural-urban migration and in quasi-subsistence farming, and the dynamic decisions involved in investment planning. As is well known from theory, the portrayal of these decisions as "certainty equivalent" decisions may involve serious misspecification as optimal choices under uncertainty are frequently qualitatively different from the choices without uncertainty.

We can, therefore, look forward to much interesting work with CGE models in the future.

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*The minimization of transactions costs is also being offered by Douglass North as a theory of long-term institutional change.