Rowing along the Computable General Equilibrium Modelling Mainstream

Maurizio Grassini
University of Florence
Italy

1.Introduction

Computable General Equilibrium (CGE) modelling has become "mainstream" economics. Mainstreams, however, are prone to form meanders, and sometimes a meander is cut off and dries up. This article explains why such could become the case with CGE modelling.

Keynesian economics has a clear fountainhead in the writings of Keynes, as does input-output analysis in the seminal contributions of Leontief. Walras is widely considered the father of general equilibrium theory and is recognized in research which refers to it. Other economic subjects do not have such a clear original set of documents as source of stimuli for further research. Neoclassical theory and Computable General Equilibrium (as well as Applied General Equilibrium – AGE) are good examples of theories and practices coming from a variety of sources. It is possible to refer to this kind of theory only when a 'synthesis' becomes available; it takes the form of a comprehensive declaration of paradigms collected in one or a few books and articles.

In other words, this kind of subject looks like a river where each branch has its own spring; the branches flow together to create the main river; finally, it is claimed that the history of the subject is summarized in the mainstream. Its description assures orthodoxy to whomever wishes to easily row in the right direction of the stream.

An authorized history of the Computable General Equilibrium is available in a number of contributions by John B. Shoven and John Whalley who have been actively working in the field for decades. A recent contribution by Peter Dixon and B.R. Parmenter in the *Handbook of Computational Economics* (1996) edited by Amman, Kendrick and Rust may be considered an updated description of CGE modelling together with its history. Considering the Shoven and Whalley's lifelong project experience and that the Dixon and Parmenter's paper has been written for an handbook, although the sample is small, the historical references gleaned among their contributions seems to cover very well the common recognition of the main sources of CGE.

These are the formalization of the general equilibrium structure by Kenneth Arrow, Gerard Debreu, Frank Hahn, and others in the 1950s, the first CGE model built by Johansen in 1960, the algorithm written by Scarf in1967 for computing solutions to numerically specified general equilibrium models. Theorists gave the foundations of general equilibrium theory, practical economists tried to look at the real economy using these theoretical foundations, and mathematicians provided tools to ease the required computations. The diffusion of computing resources supported the dissemination of this kind of modelling. These are the springs which fed the three main branches which significantly determined the CGE river flow.

As in any river basin, it is possible to detect a large number of branches; they are not all the same; some give large contributions to the river flow, others are no more than seasonal streams. Here, these small contributions are intentionally ignored and the reader is invited not to use them to avoid the main questions which will be raised.

2. A definition of CGE

Dixon and Parmenter (1996) produce the following definition of CGE models grouping their '*distinguishing characteristic*' as follows:

> *i) They include explicit specification of the behavior of several economic actors (i.e. they are general). Typically they represent households as utility maximizers and firms as profit maximizers or cost minimizers. Through the use of such optimizing assumptions they emphasize the role of commodity and factor prices in influencing consumption and production decisions by households and firms. They may also include optimizing specifications to describe the behavior of governments, trade unions, capital creators, importers and exporters.*
>
> *ii) They describe how demand and supply decisions made by different economic actors determine the prices of at the least some commodities and factors. For each commodity and factor they include equations ensuring that prices adjust so that demands added across all actors do not exceed total supplies. That is, they employ market equilibrium assumptions.*
>
> *iii) They produce numerical results (i.e. they are computable). The coefficients and parameters in their equations are evaluated by reference to a numerical database. The central core of the database of a CGE model is usually a set of input-output accounts showing for a given year the flows of commodities and factors between industries, households, governments, importers and exporters. The input-output data are normally supplemented by numerical estimates of various elasticity parameters. These may include substitution elasticities between different inputs to production processes, estimates of price and income elasticities of demand by households for different commodities, and foreign elasticities of demand for exported products.*

This definition largely matches the CGE models which are encountered in related studies.

The definition of 'maximizing economic actors' clearly evokes the structure of a theoretical general equilibrium model where the number of economic agents is specified. When we move from the theoretical to the computable model, the number of economic agents is determined by the available statistical information about a given economy. In other words, the implicit one-to-one representation of the economy taken for granted in the world of abstract concepts is absent. Thus, a CGE model builder should be aware that the usually available quantitative description of the economy is not a suitable basis for applying the tools offered by the microeconomic theory.

While some macroeconometric models may refer to a single good economy, CGE models are fed with a detailed description of the economy; but the detail is halfway between micro and macro variables. The level of detail may be labelled as 'meso'; that is, the level which policy makers are interested in. Therefore, CGE models fall short of the theoretical representation of the economy suitable for a general equilibrium model, but match the requirements of the policy maker.

This limit seems well perceived in the second point of the above definition: CGE models, it is said, *determine the prices of,* ***at the least****, some commodities and factors*' which means that these models are less than General. However, they may still focus on important issues.

The third point explains what 'computable' means: CGE models *produce numerical results*. Thus, any estimated econometric model may be labelled computable. Furthermore, a CGE model is characterized by a database which 'usually' contains input-output account(s) and a set of 'normally' supplemented numerical estimates of various parameters. The role of such a database will be discussed later on.

3. How to make a general equilibrium model computable.

3.1 Social accounts and economic theory

To move from a theoretical to a computable model, measurements of the model variables must be available. How to bridge theoretical and observable economic variables is a well known problem which tormented many economists long before the beginning of the systematic production of national accounts and related by-product statistics. When commenting on "Abstract Models and Reality", Haavelmo (1944) stressed the distinction between "observable", "true" and "theoretical variables"; he wrote:

> *In pure theory we introduce variables ...... which, by construction, satisfy certain conditions of inner consistency of a theoretical model. These theoretical variables are usually given names that indicate with what actual, "true", measurements we hope the theoretical variables might be identified. But the theoretical variables are not defined as identical with some "true" variables........To impose some functional relationship upon the variables means going much further. We may express the difference by saying that the "true" variables .... represent our ideal as to accurate measurements of reality "as it is in fact", while the variables defined in a theory are the true measurements that we should make if reality were actually in accordance with our theoretical model.*

and he concluded the discussion with the following advice:

> *.....one should study very carefully the actual series considered and the conditions under which they were produced, before identifying them with the variables of a particular theoretical model.*

Forty years later, in the article where the word 'calibration' was defined for economists, Mansur and Whalley (1984) wrote:

> *General equilibrium analysis is perhaps the most widely used theoretical*

framework for economywide microeconomic analysis, but is only explicitly recognized in the construction of current national income accounts in the aggregate income-expenditure identity, not in any of the subaggregate detail in the accounts..... The detailed information presented in most national accounts, although clearly of enormous value to economists, nonetheless is largely a by-product of the process of assembly of macro aggregates and typically does not aim at consistency in various areas of detail that general equilibrium analysis requires.

Later in the 1940s, the production of national income accounts flourished all over the word (mainly in the more developed market economies); under the guidance of the manual entitled System of National Account, published by the United Nations in 1953 and updated in 1968 and 1993, national accounting has been progressively implemented.
The production of national accounts is not simply a matter of diligent data collection. The statitics have to respond to theoretical requirements and, in a way, the system of income-production accounts may be considered a set of equations of a theoretically founded economic model[1]. It is obvious that there is an enormous amount of statistical information about the economic activity, but *"No amount of searching in primary records...in the books of a firm or individual, will enable us to detect the income that has been made. To ascertain income it is necessary to set up a theory from which income is derived as a concept by postulation and then associate this concept with a certain set of primary facts"* (Stone, 1951, pag. 9). On the other hand, *"statistical information is always collected with some theory in mind and the concepts adopted in the process of collecting the statistical material determine the range of models, for which this information can be used in a meaningful way"* (Rainer, Richter, 1989, pag. 235). Given this view, since the System of National Accounts 1993 aims at showing *"the economic behaviour of the economy's participants, their interrelationships and the results and their economic activity"*, one may question which economic theory is behind national accounting. Undoubtedly, the macro economic variables of the Keynesian model have inspired the national accounts statisticians and this is also well preserved in the input-output accounts. An economic theory may guide the social account statistician only if the theory states a sufficiently clear relationship between economic variables and observed facts. For example, whereas the firm's accounting books are considered to be the basic economic statistics the economic theory must be well suited to the firm's economic environment.

3.2 The social accounts of the CGE's economic environment

In the line connecting 'x' defined as 'consumption' in an abstract model and the consumption registered by an housewife in a diary given her by a national statistical bureau for primary data collection, the economist usually focusses on statistics located somewhere in the middle of that line. This is because the economist cannot ask the housewife what she really means by consumption expenditure, but he has to use for his purposes the data made available by official statistical institutions. In general, the official statistics yearbooks are the place where

[1] Stone (1951) gives an example in which, in the construction of social accounting, four variables - income, consumption, saving and asset formation - are related by two independent relationships. Almon (1997) shows that the Standard National Accounts, the accounting system used in the United States, involves some 150 items connected by 40 identities; these may be used as the cornerstone for the so called *identity-centered* modeling.

economists ‘observe’ the extant economy.

As mentioned above, in the definition of a CGE, Dixon and Parmenter reveal that a CGE modeler is aware that the model falls short of being general; however, he refers to a micro economic representation of the economy and does his best to match the observed economy with his point of view. While macro economists have clearly influenced the structure of economic national accounts all over the world, micro economic general equilibrium economists have had a very modest influence on designing the collection of economic data. This fact was underlined by Mansur and Whalley (1984) as mentioned above; in order to bridge the gap between the ‘theoretical variables’ and those available, they suggested a reorganization of the available economic statistical data within the ‘spirit’ of the general equilibrium theory. Showen and Whalley (1984) wrote:

> *In practice, benchmark equilibria are constructed from national accounts and other government data sources. In general, the information will be inconsistent (e.g., payments to labor from firms will not equal labor income received by households), and a number of adjustments are required to the basic data to ensure that equilibrium condition hold. Some data are taken as correct and others are adjusted to be consistent in the process of generating a benchmark data set.*

The treatment of profits is a good example of the suggested *adjustment* of the economic statistics. The neoclassical paradigm implies that at the equilibrium firms realize zero profits. In national accounts, profits are not zero; this is not due to the fact that the observation of the economy is done out of the equilibrium. On average, profits are strictly positive and this is good for all of us. This fact does not shock a CGE modeler who looks at the economy through data bases specifically manipulated to match his needs. ‘*In fact, the assumption of an ‘observable’ equilibrium leads directly to the construction of a data set that fulfills the equilibrium conditions for some form of general equilibrium model’* (Showen, Whalley, 1984). Although the ‘*detailed information presented in most national accounts.*. [have] *enormous value to economists’* (Showen , Whalley, 1984), some adjustments **‘*are desired*’.** Then, what happens to the profits? They are simply removed by renaming them as compensation for capital[2]. The rational for such a manipulation is that profits have a destination. They are distributed to a variety of incomes so that the flow will lose its original character. This rational may be applied to each item (of the primary distribution) of value added; once the distribution of valued added to the institutions is completed and their disposable income is defined, neither profits nor any other component of the value added are found in the ‘use of income’ accounts. But profits are well rooted in the primary distribution of value added and are well appreciated as such by the entrepreneur; furthermore, profits may be seen as a buffer variable between costs and revenues which may play the role of a strategic instrument in the hand of the entrepreneur. As they are the difference between costs and revenues, profits are strictly related to prices which in turn have a strange location in the CGE models.

[2] Showen and Whalley (1984) are very explicit on this point. Since one equilibrium condition is that *‘Nonpositive profits are made in all industries’*, then ‘*This typically involves treating the residual profit return to equity as a contractual cost, as is implicit in most input-output transaction tables’*. Indeed, this treatment is not at all ‘implicit’.

In other words, the theoretical foundations of the CGE are not adequate to represent the real world; hence, the available representation of the world has to be modified. The CGE modeler does not reject the model; he rejects the data giving rise to the peculiar profession of the CGE data maker.

4. Getting on with a Computable General Equilibrium model

As it is well known to any model builder, the birth of a 'computable' model does not take place in a single step. The model builder may have a rather good knowledge of the model's cornerstones; they can allow him to quickly reach a rough version of the model. Subsequently, the model will be inevitably implemented. Firstly, this rough version calls for a refinement. Secondly, the model builder's experience will suggest where to introduce improvements; these largely concern the performance of the model and in particular those distinctive features for tackling particular simulation experiments. A model builder is aware that the refinement and the implementation of a model is an endless process.

A good example of model building experience is offered by The Michigan Model of World Production and Trade. The modelling framework was originally developed by Deardorff and Stern at University of Michigan in the mid 1970s. This model is still used and implemented; the presentation[3] emphasizes that the structure of the model has been extended to include features of the New Trade Theory (imperfect competition, increasing returns to scale, and product differentiation) and many other features to deal with actual and preferential trading arrangements such as the North American Free Trade Agreements, the effects on employment due to the Tokyo Round of Multilateral Trade Liberalization, and many other problems shown in a number of papers listed in the presentation mentioned above. This set of papers are part of the description of the model extensively described in two books by Deardorff and Stern (1986, 1990).

In a recent contribution, Brown, Deardorff and Stern (2001) investigate "*the options that two nations have in prospective trade negotiations at the multilateral and regional level*" by means of the "Michigan Model" that they "*have used for more than 25 years to analyze changes in multilateral and regional trade policies*". To analyze the multilateral trade liberalization provisions of the Uruguay Round agreements, they use a 20-country/18-sector version of their CGE model. This type of model, as any other model of this kind, requires an immense amount of data. Databases like "The GTAP-5 Database" provided in Dimaranan and McDougall (2002) at Purdue University address this need; the Authors are clearly aware that this practice has to be considered largely common to CGE model builders.

Using growth rates forecast for the period 1997-2010, provided by the World Banks's 1999 World Development indicators for various countries, the database was projected to approximate the picture of the world expected in 2005 if the Uruguay Round negotiations had not occurred. Accordingly, the impact of the Uruguay Round induced reduction in tariffs and

[3] the presentation is at www.Fordschool.umich.edu/rsie/model

non-tariff barriers has been analyzed in the course of 10-year implementation period. Once the computational scenarios have been shown in good detail, the Authors review the features of the model in order to help the reader to interpret the results. Then, they list a number of expected effects related to the computational scenarios, and warn that "*In the real world, all of these effects occur over time, some of them [more] quickly than others*" and continue:

> *Our model is however static, based upon a single set of equilibrium conditions rather than relationships that vary over time. Our results therefore refer to a time horizon that is somewhat uncertain, depending on the assumptions that have been made about which variables do and do not adjust to changing market conditions, and on the short- to long-run nature of these adjustments. Because our elasticities of supply and demand reflect relatively adjustments and because we assume that markets for both labor and capital clear within countries, our results are appropriate for relatively long time horizon of several years - perhaps two or three at a minimum.*
>
> *On the other hand, our model does not allow for the very long-run adjustments that could occur through capital accumulation, population growth, and technological change. Our results should therefore be thought of as being superimposed upon longer-run growth paths of the economies involved. To the extent that these growth paths themselves may be influenced by trade liberalization, therefore, our model does not capture that.*

This frank description of the limits of a model is not necessarily astonishing. A model builder is fully aware of the limits of his model; relevant economic questions stimulate him to improve it. However, it is worthwhile to notice that a) the model is static, b) it is based upon a single set of equilibrium conditions rather than relationships that vary over time, c) the results refer to a rather uncertain horizon, nevertheless d) the results are appropriate for relatively long time horizon which surprisingly may be approximately two or three years, and finally e), although the model is tailored for long time horizon, it does not account for those factors such as capital accumulation, population growth and technological change.

However, the Michigan Model of World Production and Trade was originally developed in the mid 1970s and after more than a quarter of century, it still shows naive limits. These limits are not at all new to macroeconomic modellers who replace them rather quickly by building and implementing macro and multisectoral models. The strange state of art of this CGE model requires further investigation to understand why it still suffers such serious limitations. A few questions are in order. Are they due to the indolence of the Authors? Are they due to the limit of the theoretical background? Is there any way out of such poor representation of the real world? Meanwhile, a further investigation about the properties of other competing CGE models would help to answer these questions properly.

Around the same time, Keuschnigg and Kohler (1999) used a CGE model to evaluate the impact of the European Union Eastern enlargement on Austria. Not constrained by the dimension of an economic journal article, the Authors produced a report with a detailed description of the required data for the construction of a 'micro-consistent data set' to feed the model, the calibration process, and described and stressed some properties of their CGE model. First of all, the Authors let it be known that the 'model is best thought of as consisting of a *macro part* which drives dynamic adjustment of the overall economy through time, and a *temporal part* which determines temporal equilibrium at any point in time and which focuses on sectoral aspects'[4]. In other words, Keuschnigg and Kohler largely follow a top-down approach where a 'dynamic' macro part drives the sectoral detail of the model.

Since the model is dynamic, '*General equilibrium* involves market clearing for all goods and factors, plus the fulfilment of an appropriate condition for the government budget *at each point in time*'.[5] At each point in time there are temporal equilibria which

> *are interconnected in two ways. First, sectoral capital stocks as well as the government debt and net foreign assets are inherited from the past. Similarly, the accumulation decisions of the present equilibrium will determine the initial conditions of the subsequent temporal equilibrium. Secondly, any temporal equilibrium is connected to the future through expectational variables. In our case, these are firm values, human capital, and the marginal propensity to consume which incorporates the expected profile of consumer prices. When solving for an adjustment path, we employ the assumption of perfect foresight. More specifically, the calculated sequence of temporal equilibria is characterized by two conditions a) The backward connection of successive equilibria turns out to corroborate ex post the expectations that underlie their forward connection, and b) the sequence leads to steady state where the relevant variables are stationary.*[6]

Hence, while Brown, Deardorff and Stern point out that the explanatory power of their model is limited by being static, Keuschnigg and Kohler emphasize the dynamic property of the model used in their study. Brown, Deardorff and Stern warn the reader that in their study the time horizon was somewhat uncertain, while Keuschnigg and Kohler talk about starting and ending equilibria and adjustment paths.

Dynamic model solution refers to a time variable; in fact, the solution is provided by an index t which reminds us of its location along the time axis where the time variable is measured. The time variable does not, however, necessarily refer to the calendar time. In an adjustment path, the index t may simply indicate that the variable follows its value at time $t-1$ and precedes its value at time $t+1$, where t may refer to an hour, to a month, to a year, to ten years, to a century or to any other fraction of time. This means that a dynamic model may be timeless with respect to the calendar time. In such a case, the dynamic model simply links two 'steady state' solutions along a timeless adjustment path; in other words, the model turns out to be useful only for comparative static exercises, as any standard static model[7].

---

[4] This statement is in the paragraph about 'A brief description of the simulation model' in Keuschnigg, Kohler (1999).

[5] This is the opening sentence in the paragraph 'Equilibrium in the short-run and in the long-run', Keuschnigg, Kohler (1999).

[6] Idem.

[7] Cautiously, Keuschnigg, Kohler (1999) measure the distance between two steady state solutions in terms of 'periods'. Monaco(1997) notices that CGE models 'tell us nothing about the time path to the new equilibrium. Dynamic AGEs [synonymous of CGE] might, but in practice relatively simple cost-of-adjustment functions are assumed, so the path and adjustment speeds are artifacts'.

Since they are operating at the same time, the two CGE models considered could represent the state of the art of a classic static computable general equilibrium model and one of the most interesting adventures in the field of the computable general equilibrium dynamic models. In order to evaluate the direction followed and the road covered from the static to the dynamic world, Blaug's comments (Blaug, 1992) may be useful.

Blaug proposes to distinguish General Equilibriun theory from the General Equilibrium model; the theory, deals with the existence of equilibrium, its stability and all the questions purely theoretical; the model, may be expressed as a set of simultaneous equations with a definite empirical content as a wider notion of an economic model. The CGE model comes from this General Equilibrium model's body. The CGE model has gained its own identity through the rich scientific contributions strictly dealing with its function (model selection, calibration, and simulation); however, it cannot exist independently of its theoretical image: the General Equilibrium theory. On this regard, Blaug cites Franklin Fisher who said (Fisher, 1987):

> *the very power and elegance of [general] equilibrium analysis often obscures the fact that it rests on a very uncertain foundation. We have no similarly elegant theory of what happens out of equilibrium, of how agents behave when their plans are frustrated. As a result we have no rigorous basis for believing that equilibrium can be achieved or maintained if disturbed.*

and Blaug observes:

> *This lacuna in GE theory produces the curious anomaly that perfect competition is possible only when a market is in equilibrium. It is impossible when a market is out of equilibrium for the simple reason that perfectly competitive producers are price-takers, not price-makers. But if no one can make the prices, how do prices ever change to produce convergence to equilibrium ? This problem is perhaps a minor blemish in an apparatus which has no role for money, for stock markets, for bankruptcies, or for true entrepreneurship.*

These considerations about the general equilibrium theory could be seen as a destructive criticism. On the other hand, any attempt to remove one or more blemishes is welcome and serves to reject this unfavourable judgement. Also Blaug offers a way out to his capital sentence by considering the general equilibrium theory as a field with no empirical content, so far from the real world that it could be labelled no more than a *framework* or *paradigm*. In this case, general equilibrium theory should no longer be judged through the general equilibrium practitioners: the CGE model builders. Indeed, it is hard to find any CGE modeler intentionally disconnected from the theorists' sphere, proud to be backed by the theoretical *framework* which supports the General Equilibrium theory. On the other hand, a CGE model depends explicitly on neoclassical general equilibrium theory, shaped on markets which operate to determine prices, and agents provided by analytically specified utility functions or production functions, who optimize their objective functions under proper constraints. Thinking that this is the authentic picture of the economy, the CGE model builder is pleased to have such *super* theoretical foundations. Many even think that this is the *unique* picture of the economy[8].

5. Paradigms and functional forms

Dixon and Parmenter's CGE definition states that households are *utility maximizers* and firms are *profit maximizers or cost minimizers*. This definition may well be embedded in Lionel Robbins's definition (Robbins,1935) of *economics* as the science which studies human behaviour as a relationship between ends and scarce means which have alternative uses. We can simply assume that economic agents do their best according to this elementary fact of experience, so that the observed economic phenomena just reflect the outcomes of their behaviour. However, CGE modelers confine the human behaviour to the domain of the neoclassical theory and use behavioural equations derived from the optimization of well defined functional forms. Their strategy may be exemplified by paying attention to the widespread approach of modelling a system of demand equations in the CGE framework.

The (neoclassical) economist assumes that a consumer maximizes his utility function under his budget constraint. A sceptical observer may question if the utility function really exists. The answer is that it probably does not exist: it is a concept useful to the (neoclassical) economist, not to the consumer. In fact, the (neoclassical) economist is a (social) scientist and as such he builds or simply uses already available models to describe and predict the observed real world phenomena. The consumer behaviour theory based upon the maximizing postulate does not determine the consumer effective purchases on the market, but gives the economist operational hypotheses to figure out a quantitative representation of the consumer behaviour: the demand functions. In this framework, the optimization procedure results take the form of restrictions which are very useful in the (econometric) estimation of demand functions (see Phlips,1974).

The CGE modeler does not estimate functional forms; he simply calibrates them picking up parameter values from data banks which in turn are made up with information collected from the (economic) literature. But the calibration procedure and the optimization postulate force the CGE modeler to deduce, for example, the demand functions from the analytical form of the utility functions. The procedure is well known; once the first order conditions of the maximization of a *convenient* analytically specified utility function are obtained, the analytical structure of the demand functions is easily obtained. Afterwards, the demand function parameters are 'calibrated'. The CGE modeler likes such a demand system, being an orthodox fruit of the neoclassical theory. But, we must be also aware of the 'economic' properties of these demand functions and the impact of the chosen utility function on the model performance.

5.1. The economic properties of a demand system obtained from a 'known' utility function

---

[8] It is worthwhile to notice that Blaug's comment on General Equilibrium Theory in *The Methodology of Economics* does not throw any light on the progress made in the direction of a dynamic approach to a computable general equilibrium model. Brown, Deardorff and Stern surely represent the orthodox static CGE modelling; Keuschnigg and Kohler deal with a CGE model with dynamic flavour but their dynamic approach does not seem to add any realistic features to the static version.

From a textbook point of view, one can assume a utility function shaped as a Cobb-Douglas function or as the utility function implied by the Stone-Geary's linear expenditure system. The Cobb-Douglas utility function implies a set of demand curves with all own-price elasticities equal to -1.0, all cross-price elasticities equal to 0.0, and all income elasticities equal to 1.0.
The Stone-Geary's utility function leads to the linear expenditure system which implies that the specification of the income elasticities and one price elasticity is sufficient to determine all price elasticities. It is clear that there is insufficient room to seriously study the effects of price. Of course, such systems of demand function belong to a group which do not portray the real world; or, at least, out of the textbook environment, they do not deserve any attention.

Hence, the utility maximization process does not necessarily lead to useful demand systems. However, the neoclassical theory approach to the consumer behaviour (that is to say, the utility maximization postulate) allows us to derive interesting operational restrictions which can be profitably used in shaping a system of demand functions. In other words, the utility maximizing postulate may be matched through the indirect utility function approach which permits the exploitation of the consumer theory restrictions, and the imposition of those economic properties which the model builder thinks a demand system should have.

A good example of this approach is given in Almon (1979) who was looking for a system of demand functions for medium-long run projections in the framework of a multisectoral model for the United States economy. He put the question: What Should a Functional Form Offer? and then gave the following answer in ten points:

> *1. ......, a functional form should offer the possibility of expressing either substitution or complementarity between goods.*
> *2. It should permit some goods to have close substitutes and high price elasticities, while other goods, with no close substitutes have low elasticities.*
> *3. It should be homogeneous of degree zero in all prices and income, that is, doubling all prices and income should not affect consumption. Homogeneity is a necessary property for individual demand functions; the assumption that everyone's income changes in the same proportion makes it necessary for aggregate demand also.*
> *4. It should add up, that is, the amount spent in all goods plus the amount saved must equal income, or some predetermined fraction of income.*
> *5. It should be possible to use the assumption of Slutsky symmetry to reduce the number of parameters to be estimated. While this symmetry is by no means necessary for market demand functions, it is not implausible that it should hold closely enough to help us economize parameters.*
> *6. As income increases any asymptotic proportions of amounts consumed or of the budget shares should depend upon prices or at least this dependence should not be ruled out a priori.*
> *7. Marginal propensities to consume as income rises must be capable of being different for different goods. They should also depend upon prices in a way to be estimated.*
> *8. It should be easy to include effects of variables other than prices and income, such as stocks of durables, interest rates, lagged price and income, and time trends. The magnitude of these effects should be affected by prices.*
> *9. The parameters of the system should not be vastly numerous or difficult to estimate.*
> *10. Price changes alter the effect of income and non-income determinants of demand – such as stock of durables, interest rates, or time trends – in approximately equal*

proportions. Some forms concentrate all their attention on how prices affect marginal propensity to consume out of income; other forms just shift the consumption-income function (Engel curve) up or down without affecting the marginal propensity to consume out of income. Each has strange implications.

As it is well known, the direct utility function has a great intuitive appeal, but the indirect utility function is not without interest as it is endorsed by the above ten points; Almon respected the fundamental restrictions coming from the utility maximization and suggested and estimated functional forms (Almon, 1979, 1996) matching the above requirements. One could ask about the analytical form of the correspondent utility function. I think that this answer may be left to mathematicians playing with challenging integration exercises. It is widely thought that the knowledge of such utility functions does not give new light to the real working of the economy.

5.2. The choice of functional forms really matters

McKitric (1998) decided to revisit the debate about the appropriate methods to construct CGE models. In particular he put the question: calibration or econometric methods? Indeed, he did not get into the debate but focussed on the specific issue of functional form choice. He underlined the following three points on which he drew renewed attention:

[First] In the calibration method, some parameters are determined on the basis of a survey of empirical literature, some are chosen arbitrarily, and the remainder are set at values which force the model to replicate the data at a chosen benchmark year...... Second, the calibration procedure causes the quality of the model to be at least partly dependent on the quality of the data for an arbitrary chosen benchmark year....... Third, the calibration approach tends to limit the researcher to the use of 'first order' functional forms (those in the Constant Elasticity of Substitution (CES) class) all of which embody restrictive assumptions about the structure of the industries being modeled, by imposing a single non-negative substitution elasticity across all pairs of goods in the aggregator ....

McKitric defined the literature which emphasizes these points as the *'econometric critique of the CGE modeling'*. In truth though, this critique is inside the CGE modelling approach; it concerns the fact that as for any quantitative model, CGE model '*embodies three types of information: analytical, functional and numerical*', explained as follows:

The analytical structure is the background theoretical material which identifies the variables of interest and posits their casual relations. The functional structure is the mathematical representation of the analytical material, and consists of the algebraic equations which make up the actual model. The numerical structure consists of the signs and magnitudes of the coefficients in the equations which form the functional structure.

These three types of information are fundamental pillars of any model builder[9]. Due to the

[9] Here, VAR modelers are not included in the model builder profession, since they do not avail themselves of any economic theory knowledge. In fact, they clearly ignore the first information: *the background theoretical material which identifies the variables of interest and posits their causal relations*.

distinctive features of the CGE modeler's approach, the choice of the functional forms leads to different analytical 'behavioural functions' which, in general, figure out varied numerical results once they are part of a model. McKitric makes a generous working hypothesis; he decided to investigate if the neoclassical foundations supporting a CGE model are powerful enough to make the choice of the functional forms irrelevant with respect to the numerical results. In the CGE modelling approach, the 'solution' at the 'equilibrium' is the same for whatever functional form set chosen by the model builder. Out of the benchmark 'equilibrium', different functional forms inevitably produce different 'equilibria'. This means that CGE models built on the same data set, but with different functional forms, produce different results in policy simulation experiments; in other words, the '*choice of functional form appears to be influential in CGE model performance*' ....'*at both the industry-specific and macroeconomic levels, for large and small policy shocks'*. (McKitrick,1988, p. 565 and p.572).

These results are trivial; more than supporting the 'econometric critique' of CGE modelling, they are good to give this modelling approach the value of textbook exercises. McKitrick's paper is nonetheless an excellent contribution in the field of comparative modelling which should force CGE modelers to meditate on the quality of their work.

6. The choice of parameter values

The parameter values of a CGE model are obtained by means of a method which was 'officially' named by Mansur and Whalley (1984). They did not invent the practice; they simply named it which, to some extent, became associated with CGE modelling. First, they announced that a standard practice – the 'calibration' – had evolved among modelers and they claimed that it was much more useful than the 'stochastic estimation' later defined by Kydland and Prescott (1992) as 'system of equation approach'. They described the stochastic estimation approach in the form of a cursory review of the most popular estimation methods available at that time. Although the description of the methods is clear, their comments and suggestions reveal their lack of practice with the use of such estimation procedures. In fact, large macroeconomic models are never estimated by means of statistical methods which requires time series data inevitably shorter than what is required by the estimation procedure in order to preserve their statistical properties. Every macroeconomic model builder knows that estimation procedures relying upon asymptotic properties, whatever the chosen class (i.e., limited information, full information, instrumental variable and so on), are not practicable for large models, but the alternative is not necessarily the calibration method. At that time a large number of large macroeconomic models were estimated and were running. Observing that models based upon input-output tables involve '*dimensionalities which are quite outside those which econometrician are used to*', Mansur and Whalley (1984) maintain that '*estimation of all model parameters using a stochastic specification and time series data is usually ruled out as infeasible*'. This is true according to most estimation procedures widely described in econometric textbooks; anyway, Mansur and Whalley seem too confident about their criticism, presenting the calibration as a method with the primacy for parameter selection.

Their description of the calibration method is more clear and interesting in as much as it provides an authentic definition of the method. Before giving numerical value to the parameters of a CGE model, they said that a micro consistent equilibrium data set is constructed using national accounts data sources so as to provide a data base for model calibration. The manipulation of the national accounts is the holy sacrifice of the neoclassical divinity.

The calibration 'theory' nested into the neoclassical one leads to a procedure which is efficaciously described in Kehoe and Kehoe (1994) by means of simple numerical examples. Firstly, they calibrate a Cobb-Douglas production function assuming that the entrepreneurs minimize costs and earn zero profits; given the observed measure of primary inputs (labour and capital) and output and assuming that wages and interest rate are equal to one, the production elasticity of one primary input and the scale factor of the production function are computed. This example shows the case of parameters computed using variable observed values at the benchmark (together with a given functional form).

Secondly, Kehoe and Kehoe show that available information on parameters can easily be incorporated into the calibration procedure. This second example is addressed to the calibration practice where parameters and elasticities come from the 'economic literature'. For instance, it may be the case that we have information about, let us say, the numerical value of the elasticity of substitution in consumption. If we do not want to ignore such information, this numerical value can be embodied in a utility function which has it among its parameters. In general, since calibration involves only one year's data (or a single observation however made available), the benchmark data frequently do not identify a unique set of values of parameters in a given model. In this case, it is desirable to have at hand values of relevant elasticities to be used to identify a reliable set of parameters in each equation of the model.

Hence, the CGE modeler is used to regard the economic literature as the place where the economic life is in evidence; in fact, instead of looking at the economic data, CGE modeler likes to draw 'parameters' from the economic literature. Now, it is a common practice to draw parameters from data bases which in turn have been built selecting 'parameters' from the literature. While the CGE modeler stays very close to the neoclassical paradigms, this practice takes him very far from the primary source of economic data. The distance from the observed facts, favoured by the available data bases fed through selected economic literature together with the manipulated national accounts, means that the CGE modeler may not be aware of the economic content of the data used in model building.

The CGE critique to econometric modelling

In year 1984 Scarf and Shoven[10] edited a book containing a series of papers presented at the Conference on Applied general equilibrium analysis held in San Diego in August 1981. The selected papers dealt with a variety of topics. Some of them focussed on methodological issues, others tackled practical problems such as foreign trade, higher energy price effects, taxation impact and its effect on income distributions, and so on. Jorgenson's contribution ranks in the methodological group. Shoven described it in the Preface as "*an ambitious and sophisticated attempt to estimate and report on a large general equilibrium model..*". Jorgenson(1984) noted that "*the development of econometric methods for estimating the*

[10] see Scarf, Shoven (eds.)(1984).

unknown parameters describing technology and preferences in such [CGE] models has been neglected".

While Mansur and Whalley (1984) went on describing the 'calibration' method as one which gives numerical value to any parameter, as an experienced econometrician (he was a member of the team of the Brooking's Quarterly Models of the United States), Jorgenson clearly stressed that the detail of any CGE model was far from the micro economic atomistic world; furthermore, he noticed that the implementation of econometric models is very demanding in terms of data requirements as is well known to macro econometric modellers. Around the same time , Shoven and Whalley (1984) tried to explain why the calibration approach was so widely used; indeed, they underlined a) that the econometricians require unrealistically large number for observation, b) that the simultaneous estimation approach does not have shortcuts capable of fully incorporating all the equilibrium restrictions, and c) by means of a rather confused analysis of the benchmark data they asserted that it was not possible to have a sequence of equilibrium observations. So, the Jorgenson' econometric approach was rapidly put aside.

Although it is widely said that the calibration is the standard procedure for giving numerical value to the parameters of a CGE model, the so-called econometric critique of the computable general equilibrium modelling still pops up in many studies. Unfortunately this critique is multi-faceted and in many cases is based simply on the assumption that some alternative approaches to making the model computable are declared better than calibration. The critique may have some truth but it requires a better specification of the context.

In the 'system-of-equations models' defined in Kydland and Prescott (1992), who refer to Koopmans' Cowles Commission framework, institutional, technological and behavioural equations are given parameters using time series data. The selection of the parameters of the equations is done in order to give the system of equations (at least) the ability to mimic the used time series. In this modelling approach, it is said that behavioural equations are the response of groups of individuals or firms to a common economic environment. These responses may well be theoretically founded[11], but they are also designed on the available statistical information which refer to '*groups of individuals or firms'*, which, by the way, are those also used by any CGE modeler, who believes him/herself to be a microeconomic observer.

It is common to remember that macroeconometric models went into disarray during the 1970s. The oil shocks, which took place at that time, proved that a one-sector model was inadequate to describe and catch the main features of an economy. Many model builders learnt a lot from the failures of their models. Rethinking – thus stimulating scientific advancement – gave rise to interesting improvements in model design. At that time, some criticism was directed towards the 'foundations' of the 'system-of-equations models' approach. Kydland and Prescott (1992) add a peculiar critique; they say that '*[a]nother reason for the demise of this approach was the general recognition that the policy-invariant behavioral equations are*

[11] A good example of macroeconometric model with well defined theoretical foundation is given by Klein and Goldberger (1955); their model is a genuine translation into equations of the content of different chapters of the Keynes' General Theory.

inconsistent with the maximization postulate in dynamic settings', due to the advances in neoclassical theory that permitted the application of its paradigms.

So that, in contrast with the econometric critique to the CGE modelling, we have a case of CGE critique to the econometric approach, which is based upon the victory of paradigms. The peculiarity of this critique is not'scientific'. Indeed, Mansur and Whalley (1984) declare that they calibrate a model to an equilibrium point combining a data set with a literature search for key parameters, but they practice '*no test of model*'; they simply make sensitivity analysis: namely, they simply investigate how different parameter values generate different outcomes.

In this context, the intersection between the 'system-of-equations models' and the 'calibration' approach turns out to be an empty set. The CGE modeler is unaffected by any criticism, because he already lives in the (neoclassical) heaven and he is not required to make any effort to deserve it. Economists, not yet gifted with faith in the neoclassical theory, may be converted through education. They will examine the CGE literature and inevitably meet the functional form implied by the analytical forms obtained under the assumption of a 'revealed' agents' optimization processes. Many analytical forms used in CGE models derived from the agents' optimization processes clearly do not refer to the observed economy, and neither would an economist use them to describe it. Needless to say, this is not an econometric critique of computable general equilibrium modelling.

The *numeraire* and the observed prices

In the general equilibrium framework, there is a unit, named *numeraire,* used to express all the other unit values in the model. CGE models embody this measurement unit. The presence of a *numeraire* tells us that in CGE modelling only relative prices matter; unfortunately, relative prices are not observable. Furthermore, the meaning of the *numeraire* seems to be largely misunderstood.

The national products and income account in the benchmark data are usually produced in value terms and many economic data may be separated in terms of price and quantity components. As mentioned above, a General Equilibrium quantitative model is much less than General in the sense that the real world is not observed at the level of micro economic agents, and goods and services are not clearly defined[12]; in fact, economic statistics are collected at various levels of aggregation, and when the separation of the price and quantity components of the relative flows is possible, it is necessary to have to deal with indexes. The unclear argument suggested by Showen and Whalley is then surprising (1984) :'*A commonly used units convention ....is to choose units for both goods and factors so that they have a price of unity in the benchmark equilibrium*'. Kehoe and Kehoe (1994) are much more clear about this point; first, they are well aware that their model is going to be built on aggregates ('*apples and oranges have been aggregated into the primaries goods*'); second, they suggest that one should '*think of these variables as price indexes, which are naturally set equal to one in the base case*'.

[12] *clearly defined* means that no mix is allowed

Benchmark data (input-output tables, social account matrices, national accounts, etc.) are necessarily available in value terms; in fact, many variables in these data set have only nominal measure which – according to the double book-keeping principle – balance with all the other variables in the accounts; but some of them may be split into price and quantity components. Prices may be made available only for those variables which have a physically measurable component (tons, litres, dozens, hours, denumerable objects and so on); anyway, these variables are aggregates so that the appropriate measurement of their prices is done by means of indexes which are related to a base year. Hence, rather than to say that we adhere to a '*common used units convention*', it is convenient to make the benchmark year and the base year the same, so that at the base year all price indexes are equal to one[13].

This choice together with the homogeneity of degree zero in prices and income of the demand equations imply that in the calibration process price elasticities are necessarily drawn from the 'literature'. In other words, in the calibration process, observed prices are largely uneffective; the performance of a CGE model is largely independent from benchmark data as the price components are concerned.

Following the attention paid to prices in CGE modelling, it emerges that prices are not considered front line variables. Production, exports, imports are well described in the aggregates in sectoral detail; prices are often absent from the tables dedicated to simulation results. Prices are hidden in the welfare indexes which play an invading role when this detail should deserve central attention and visibility. Kehoe and Kehoe (1994) say about a '*typical practice*' which is '*to normalize prices so that a certain price index remains constant*'. It is surprising that the declared perfect elegance of the neoclassical theoretical background of the general equilibrium theory does not suggest anything better than a *typical practice* for modelling prices in CGE models.

An insight into price modelling in CGE models may be inferred from Hoffmann (2002). He says that '*economists normally view the field of imperfect competition in general equilibrium models as an open Pandora's box of theoretical problems*'; nevertheless, '*an increasing number of policy questions require that we incorporate imperfect competition*' in CGE models. In fact, considering that competition policy cannot be analysed in the traditional models with perfect competition, the implementation of these models turns out to be on the top of the CGE user agenda.

In an imperfect competition environment, firms are not price takers; their strategy is summarized in the choice of a mark-up. Indeed, CGE models with a mark-up on prices are numerous, but the implication of this amendment to the general equilibrium framework is not, in general, clearly considered. The CGE benchmark data set contains a modified national account system where profits have been removed in honour of the General Equilibrium Paradigms. Hoffmann (2002) rightly underlines the relevance of imperfect competition which implies that profits have to be put back into the benchmark data set. The inclusion of profits in

---

[13] Needless to say that the choice of the base year is conceptually not equivalent '*to assume that quantity units for all composite commodities are chosen so that their initial purchasers' prices are unity*' (Dixon and Parmenter, 1996).

the value added implies production function specifications which differ from those based on of the zero profits assumption. Of course, a specific benchmark data set may be designed for CGE model with imperfect competition. In other words, different theoretical specifications of a CGE model are tackled by building different benchmark data sets. What comes out from this practice is that the CGE modeler has disregard for the economic facts.

Hoffmann(2002) revisits the problem of choosing the *numeraire*. He considers previously published contributions on the importance of the choice of *numeraire* underlining its influence on the measurement of welfare gains. Indeed, he refers to Ginsburgh (1994) who claimed that there might be '*more welfare gain from changing the numeraire than eliminating imperfections in the applied general equilibrium model*'. But the problem is that the equilibrium solution only gives relative prices which are not observable, and he dares '*argue that choosing the numeraire freely is economically meaningless*'.

Provisional Conclusions

Rowing along the computable general equilibrium mainstream, it is easy to detect the presence of other streams. The most important are the theoretical debate stream, the econometric critique stream, the economic quantitative relevance stream and the standard stream (namely, the CGE mainstream).

The theoretical mainstream critique runs parallel to the general equilibrium pillars; old (Kaldor), recent (Bowles,Gintis) and permanent (Blaug) criticism is uneffective. The criticism is directly or indirectly rooted in the 'falsification principle', whereas general equilibrium economists indulge in the worship of paradigms. Needless to say, this kind of criticism is uneffective. The general equilibrium economists do not care about it.

The econometric critique is addressed to the poor use of a scarce resource: the available statistical data such as time series data of the national product and income accounts. But, CGE modellers are used to build their model from manipulated data which contains statistical data prepared to match the CGE requirements. In other words, a CGE modeller asks for a data set with data (and parameters) which fit the model, not vice versa.

The economic quantitative relevance stream is strictly related to the previous one. Its criticism focusses on the process of giving numerical value to the parameters in the model. This is the area of conflict between the 'calibration' and the 'system-of-equation' approaches (Kydland, Prescott, 1993). Shoven and Whalley (1992,p105) offer a good support of the quantitative economic critique:

> *Typically, calibration involves only one year's data or a single observation represented as an average over a number of years. Because of the reliance on a single observation, benchmark data typically does not identify a unique set of values for the parameters in any model. Particular values for the relevant elasticities are usually required, and are specified on the basis of other research. These serve, along with the equilibrium observation, to uniquely identify the other parameters of the model. This typically places major reliance on literature survey of elasticities; as many modelers have observed in discussing their own work, it is surprising how sparse (and sometimes contradictory) the literature is on some key elasticity values. And, although this procedure might sound*

straightforward, it is often exceedingly difficult because each study is different from every other.

and add an operative justification for using calibration method:

> *in some applied models many thousands of parameters are involved, and to estimate simultaneously all of the model parameters using time-series methods would require either unrealistically large numbers of observations or overly severe identifying restrictions ....Thus far, these problems have largely excluded complete economic estimation of general equilibrium systems in applied work.*

Shoven and Whalley do not seem to be aware that microeconomic studies rarely estimate models that can be applied to the aggregates of the CGE models. More precisely, the economic environments of the microeconomic quantitative analysis and that of CGE are essentially different, and it is not reasonable to apply such parameter estimates to the CGE model. On this point Hansen and Heckman (1996) are very crude:

> *Given the less-than-idyllic state of affairs, it seems foolish to look to micro data as the primary source for many macro parameters required to do simulation analysis. Many crucial economic parameters – for example, the effect of product inputs on industry supply – can only be determined by looking at relationships among aggregates. Like it or not, time series evidence remains essential in determining many fundamentally aggregative parameters.*

The difference between these points of view is evident, but no debate has taken place. A peculiar case where there is an absence of scientific communication is given by MkKitrick's article (1998) mentioned above. Its title refers to the econometric critique to CGE modelling, but it deals with a more substantial question proving that the CGE model performance is seriously influenced by the choice of the functional equations. The critique is a capital sentence for the CGE model as a policy simulation tool. Nevertheless, since then the *Economic Modelling* journal has published a number of articles on CGE modelling, where MkKitrick critique is basically ignored[14].

The standard stream (that is to say, the CGE modelling mainstreams) seems to live in isolation, ignoring both criticisms and the other contributions to multisectoral modelling. Shoven and Whalley (1992) prove to be very uninformed by saying :

> *Applied general equilibrium analysis is not without its own problems. ....On the other hand, there are no clearly superior alternative models available to policy makers who base their decisions on efficiency and distributional consequences of alternative policy changes.*

The objective of a CGE model is to mimic the 'observed' equilibrium by means of a modelling approach based upon neoclassical microeconomic theoretical foundation, making the model computable by drawing parameters from data set built by using information collected from the

[14] This fact makes the role of the referees unclear.

economic 'literature'. Furthermore, a CGE model is not submitted to any test. In a scientific environment it is common to adopt a modelling approach to understand the working of the real economy; of course, the ability to describe the observed economy requires a testing procedure. Contributions from this field are obviously unknown to Shoven and Whalley.

Dixon and Parmenter(1996) are much more aggressive in defending the CGE domain. First they state that '*Relative to CGE models, the economy-wide econometric models paid less attention to economic theory and more attention to time-series data*'. This statement is inaccurate; many economy-wide econometric models are built with a substantial theoretical basis though it is true that CGE models pay no attention to time-series data. Perhaps, their statement becomes clear when they say that in the 1970s '*applied economists recognized the power of optimizing assumptions*' so that '*CGE modelling is now an established field of applied economics*' and ' *graduate students all over the world are engaged in writing CGE theses*'.

Dixon and Parmenter(1996) fully show their imperialistic impulse by stating:

> *Is the field past its peak? Is it in danger of going stale? We don't think so. We think that CGE modelling will generate high-profile academic careers for many years to come. More importantly, it is likely to be increasingly influential in policy making and in business.*

So, it is possible to learn that the academic career may fully diverge from that of the economist who wants to learn, to be trained, to understand the working of the economy. On the other hand, thinking of the epistemological problems of economics, it is not common to find such an immoral appeal.

Some peculiarities of the CGE modelling approach detected from the literature may be summarized as follows.
- The CGE is based on the neoclassical paradigms. The optimization principle is applied to a short number of well defined analytical forms of utility, production and cost functions. The choice of these forms is up to the model builder. The model builder does not care about the 'economic' implications of such choices.
- The simulation done by using CGE models are basically used for comparative static analysis. The CGE models may be declared static or dynamic, but they are both focussed on steady state equilibrium; what happens outside the equilibrium is not explained.
- CGE models are timeless. The dynamic CGE models are based on dynamic optimization processes, but the outcome sequence which links two equilibria does not refer to the calendar time. In fact, the outcome sequence time index is rightly named 'period' by the dynamic CGE modelers. The 'period' is not much different from the 'iteration step' in the static CGE model.
- CGE models do not refer to the observed economy. They are based on data collected in special data bases which contain manipulated economic data. The manipulation aims to suit the observed economy to the neoclassical paradigms (for example, the zero profit assumption in the perfect competition environment implies the removal of profits from the national product and income accounts).
- CGE models may consider only relative prices which have a didactical and theoretical appeal but are not observable and hence not applicable (nor available in the 'special data base' for

CGE models).

A detailed representation of the economy (mainly based on input-output tables and institutional accounts) is a necessary and important foundation to build macroeconomic models tailored for policy simulation and forecasting, and useful for policy making. Even the CGE modelers work on this foundation, but they produce nothing more than a giant representation of the practice to prepare textbook exercises.

References

Almon, C. (1979), "A System of Consumption Functions and Its Estimation for Belgium", *Southern Economic Journal*, 46 (1), 85-106.

Almon, C. (1991), “The INFORUM Approach to Interindustry Modeling”, *Economic Systems Research*, 3 (1), 1-7 .

Almon, C. (1995), “Identity-centered Modeling in the Accountant of SNA Based Model”, *INFORUM Working Paper*, Series 95 (?), University of Maryland. (http://inforumweb.umd.edu/Workpapr.html).

Almon, C. (1996), "A Perhaps Adequate Demand System", *INFORUM Working Paper*, Series 96 (7), University of Maryland.(http://inforumweb.umd.edu/Workpapr.html).

Amman, H.M., Kendrick, D.A., Rust, J. (eds.) (1966), Handbook of Computational Economics, Vol. I, Elsevier Science B.V.

Blaug, M. (1992), The Methodology of Economics, Cambridge University Press.

Bowles, S., Gintis, H. (2000) “Walrasian Economics in Restrospect”, *The Quarterly Journal of Economics*, vol. 115, no. 4, pp. 1411-1439.

Brown, D.K., Deardorff, A.V., Stern R.M. (2001) CGE Modeling and Analysis of Multilateral and Regional Nogotiating Options, Research Seminar in International Economics, School of Public Policy, The University of Michigan, Discussion Paper n. 468.

Deardorff, A.V., Stern, R.M. (1986) The Michigan Model of World Production and Trade: Theory and Applications, Cambridge, MIT Press.

Deardorff, A.V., Stern, R.M. (1990) Computational Analysis of Global Trading Arrangements, Ann Arbor, The University of Michigan Press

Dimaranan, B.V., McDougall, R.A. (2002), “Global Trade, Assistance and Production: The GTAP-5 Data Base”, Center for Global Trade Analysis, Purdue University. http://www.gtap.ag econ.purdue.edu/databases/v5/default.asp

Dixon, P.B., Parmenter, B.R. (1996), “Computable general equilibrium modelling for policy analysis and forecasting". in Amman, H.M., Kendrick, D.A., Rust, J. (eds.) (1966), Handbook of Computational Economics, Vol. I, Elsevier Science B.V.

Eatwell, J., Milgate, M., Newman, P. (eds.) (1987), The New Palgrave: A dictionary of Economics, London, Macmillan.

Fisher, F.L. (1987), Adjustment processes and stability, in Eatwell et al. (1987), pp. 26-9.

Ginsburgh, V. (1994) "In the Cournot-Walras general equilibrium model, there may be 'More to Gain' by changing the numeraire than by eliminating imperfections: A two.good economy example", in Marcenier, J.,Srinvasan, T.N. (eds.) Applied General Equilibrium Analysis and Economic Development, University of Michigan, Ann Arbor, pp 217-224.

Haavelmo, T. (1944), The Probability Approach in Econometrics, Supplement to *Econometrica*, Vol. 12, July 1944.

Hansen, L.P., Heckman, J.J. (1996),"The Empirical Foundation of Calibration", *The Journal of Economic Perspectives*, Winter, pp 87-104.

Hoffmann, A.N. (2002), "Imperfect competition in computable equilibrium models - a primer", *Economic Modelling*, pp. 119-139.

Jorgenson, D.W. (1984) "Econometric methods for applied general equilibrium analysis", in Scarf, H.E., Shoven, J.B.(eds.) (1984) Applied general equilibrium analysis, Cambridge University Press.

Kaldor, N. (1975) "What is wrong with economic theory", *The Quarterly Economic Journal*, vol. 84), no. 3, pp. 347-357

Kehoe, P.J., Kehoe, T.J. (1994), " A Primer on Static Applied General Equilibrium Models", Federal Reserve Bank of Minneapolis Quarterly Review, Spring 1994, vol. 18, no. 2

Keuschnigg, C., Kohler, W. (1999), "Eastern Enlargement to the EU: Economic Costs and Benefits for the EU Member States: the Case of Austria", Study prepared for the Commission of the European Communities DG Budget, Final Report, September.

Klein, L.R., Goldberger, A.S. (1955) An Econometric Model of the United States, 1929-52, North-Holland, Amsterdam

Mansur, A., Whalley, J. (1984), "Numerical specification of applied general equilibrium models: estimation, calibration, and data", in Scarf and Shoven (eds.) Applied general equilibrium analysis, Cambridge University Press.

McKitrick, R.R. (1998), "The econometric critique of computable general equilibrium modeling: the role of functional forms", *Economic Modelling*, 15, pp. 543-573.

Monaco, R.M. (1997), "A Brief Review of Alternative Approaches to Inter-sectoral Policy

Analysis and Forecasting",*INFORUM Working Paper*
(http://inforumweb.umd.edu/Workpapr.html).

Nelson, R.R. (1994), "What has been the Matter with Neoclassical Growth Theory?", in Silverberg, G., Soete, L. (eds.) (1994), The Economics of Growth and Technical Change, Edward Elgar.

Phlips, L. (1974) Applied Consumption Analysis, North-Holland Pub. Co., Amsterdam

Reiner, N., Richter, J. (1989), "The SNA Make-Use Framework as a Descriptive Basis for IO Analysis", in FranzA. Rainer,N. (eds.) Compilation of Input-output Data, Proceedings of the 2nd International Meeting on Problems of Compilation of Input-output Tables organized by the Austrian Statistical Society, Baden near Vienna, Austria, 13-19 March, 1988, Orac.

Robbins, L. (1935) An Essay on the Nature and Significance of Economic Science, MacMillan, 2nd ed., London

Scarf, H.E., Shoven, J.B.(eds.) (1984) Applied general equilibrium analysis, Cambridge University Press.

Scarf, H.E. (1984), "Preface", in Scarf, H.E., Shoven, J.B.(eds.) (1984) Applied general equilibrium analysis, Cambridge University Press.

Shoven, J.B., Whalley, J. (1984), "Applied General-Equilibrium Models of Taxation and International Trade: An Introduction and Survey", *Journal of Economic Literature*, vol XXII, pp. 1007-1051

Shoven, J.B., Whalley, J. (1992), Applying general equilibrium, Cambridge University Press.

Silverberg, G., Soete, L. (eds.) (1994), The Economics of Growth and Technical Change, Edward Elgar.

Stone, R. (1951), Role of Measurement in Economics: Newmarch Lectures, 1948-49, Cambridge University Press.

United Nations (1953), A System of National Accounts and Supporting Tables, Series F., n. 2, New York