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The payoff of investment in CGE modelling

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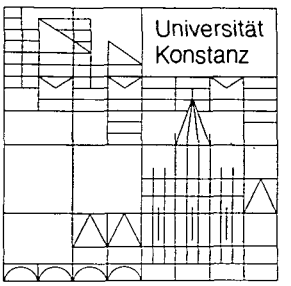

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The Payoff of Investment in CGE Modelling

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THE PAYOFF OF INVESTMENT IN CGE MODELLING

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Serie II - Nr. 156

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Abstract

This volume collects three independent contributions which discuss the role of computable general equilibrium (CGE) modelling in both research and teaching. The first paper by John Chipman suggests that statistical methods could be incorporated into CGE analysis. He argues that this would allow CGE modellers to make more systematic use of available data and subject their models to empirical testing. In the second paper, Friedrich Schneider discusses a number of weaknesses of CGE analysis "from an outsider's perspective". He emphasizes the role of the calibration method and suggests the use of experimental results as well as a richer set of assumptions with respect to government behavior. The third paper by Bernd Genser focuses on the possible role of CGE modelling in university education, arguing that courses devoted to this method may substantially improve the students' understanding of economic interdependencies.

Empirical Methods in Computable-General-Equilibrium Modelling*

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Introduction

My remarks have been stimulated by the very interesting opening address by John Whalley (1991). I will concentrate on methodological questions.

In Section 1 I discuss the defeatist proposition that the number of parameters in typical CGE models is so huge that estimation of them by traditional econometric methods is not possible given our lack of sufficiently long time series. I show that this proposition is based on an erroneous calculation of the degrees of freedom in a simultaneous-equations model, and that this can therefore not be used as a valid excuse to eschew econometric methods. In Section 2 I take up the simplest possible case of a calibration procedure, that used by Leontief in determining input-output coefficients, and ask whether there is a way to interpret the procedure in terms of statistical inference. I come to the conclusion that calibration can be given a valid statistical (Bayesian) interpretation provided one makes the *a priori* assumption that prior variances of the input-output coefficients are small. I go on to argue that under this interpretation there is no reason to limit oneself to observations from a single sample period (the "benchmark" year) when observations are available for more than one period.

*Introductory Statement at the Round Table: "The Payoff of Investment in CGE–Modelling," Workshop on *Issues in International Economics: Questions to and Answers from Computable General Equilibrium Analysis*, 9 July 1991, University of Konstanz.

In Section 3 I outline the history of the calibration procedure in CGE modelling, which evolved gradually before the term "calibration" came to be adopted as a description of the process, and has become ever more explicit as a method. I observe that a parallel development has been taking place in the macroeconomic literature, and that the macroeconomists and CGE modellers have apparently been unaware of their mutual activities. In the macroeconomic field there has also been a tension between calibration and conventional statistical inference, as well as a recent attempt to merge the two.

In Section 4 I challenge the point of view that existence of equilibrium is of no policy interest, particularly if the true state of the economy is one of growth and/or cyclical fluctuation. Finally, in Section 5 I offer some suggestions for improvements over existing procedures, and in particular stress the need for *post hoc* verification of policy prognoses based on CGE models.

1 Are traditional methods of statistical inference applicable?

Whalley (1991) has told us that since CGE models typically contain up to 10,000 parameters, one would need over 10,000 observations to estimate them statistically. If one has quarterly obervations, this means that one would have to have a time series of over $10,000/4 = 2,500$ years. If the last year is 1990, the first would have to be before 510 B.C.—the time of the Pharaohs.[1] And one could not assume that over that time period there was no structural change. Ergo, statistical estimation in CGE models is impossible. Let us consider this argument.

Suppose for simplicity one takes a Taylor approximation of the CGE model around the initial equilibrium point. Then the CGE model could be represented as a classical simultaneous-equations model of the form

$$Y\Gamma = X\mathrm{B} + U \tag{1.1}$$

where Y is an $n \times m$ matrix of n observations on m endogenous variables, X is an $n \times k$ matrix of n observations on k exogenous variables, Γ and B are $m \times m$ and $k \times m$ matrices of unknown parameters to be estimated, and U is a $n \times m$

[1]The last of the Egyptian kings was Psamtik III, who was defeated in 525 B.C. in the battle of Pelusium by Cambyses, King of the Persians.

matrix of random errors with zero means $\mathcal{E}u_{tj} = 0$ and covariances $\mathcal{E}u_{si}u_{tj} = r_{st}\sigma_{ij}$ where $R = [r_{st}]$ is a matrix of serial correlations and $\Sigma = [\sigma_{ij}] = \mathcal{E}u_{ti}u_{tj}$ is the simultaneous covariance matrix. It is known from standard simultaneous-equations theory that the parameter matrix $[\Gamma', \mathrm{B}']$ is identifiable if each of its $m \times (m+k)$ rows is subject to $m-1$ homogeneous linear restrictions and if $n > k$. Let us look at the so-called "reduced form" of (1.1). This may be written as

$$Y = X\Pi + V \tag{1.2}$$

where $\Pi = \mathrm{B}\Gamma^{-1}$ and V is a matrix of random error terms v_{tj} with zero means and covariances $\mathcal{E}v_{si}v_{tj} = r_{st}\omega_{ij}$, where $\Omega = [\omega_{st}]$ is related to Σ by $\Gamma^{-1\prime}\Sigma\Gamma^{-1}$. Denoting

$$Y = [y^1, \ldots, y^m], \quad \Pi = [\pi^1, \ldots, \pi^m]$$

where

$$y^j = \begin{bmatrix} y_{1j} \\ y_{2j} \\ \vdots \\ y_{nj} \end{bmatrix} \quad \text{and} \quad \pi^j = \begin{bmatrix} \pi_{1j} \\ \pi_{2j} \\ \vdots \\ \pi_{kj} \end{bmatrix}$$

for $j = 1, 2, \ldots, m$, we may write the reduced form (1.2) as a set of m equations

$$y^j = X\pi^j + v^j \quad (j = 1, 2, \ldots, m). \tag{1.3}$$

Now stacking these on top of one another we get

$$\begin{bmatrix} y^1 \\ y^2 \\ \vdots \\ y^m \end{bmatrix} = \begin{bmatrix} X & 0 & \ldots & 0 \\ 0 & X & \ldots & 0 \\ & & \ddots & \\ 0 & 0 & \ldots & X \end{bmatrix} \begin{bmatrix} \pi^1 \\ \pi^2 \\ \vdots \\ \pi^m \end{bmatrix} + \begin{bmatrix} v^1 \\ v^2 \\ \vdots \\ v^m \end{bmatrix}, \tag{1.4}$$

or in more compact notation

$$\mathrm{col}\ Y = (I \otimes X)\mathrm{col}\ \Pi + \mathrm{col}\ V.$$

This shows that the correct number of observations is not the number of time periods, n, but this number times the number of endogenous variables m. The number of parameters is km. Thus, the correct measure of the degrees of freedom is not

$$n - km$$

but rather

$$nm - km = (n - k)m.$$

If for example we take $k = m = 100$ then we have $m^2 = 10,000$ parameters to be estimated, and the required number of years of quarterly observations is not $10,000/4 = 2,500$ but rather $100/4 = 25$. Therefore we need not go back as far as 510 B.C. but only as far as 1966 A.D.

This assumes that there are no *a priori* linear restrictions imposed on the π_{ij}s. However, there are many examples in which such linear restrictions are indeed imposed.[2] Take the case, for example, of a system of consumer demand functions when the structural form is that of the linear expenditure system. If there are demands for m commodities as functions of m prices and income, then from homogeneity there are in general m independent parameters for each equation, or m^2 parameters altogether. But in terms of the linear expenditure system the demand functions have the form

$$(1.5) \qquad q_{ti} = \frac{Y_t \alpha_i}{p_{ti}} + \frac{1}{p_{ti}} \sum_{j=1}^{m} p_{tj} \gamma_j (\alpha_i - \delta_{ij}) + e_{ti}, \quad (i = 1, 2, \ldots, m)$$

where q_{ti} is the quantity of commodity i demanded at time t, p_{tj} is the price of commodity j at time t, and Y_t is disposable income at time t (δ_{ij} denotes the Kronecker delta). It will be noted that there are m α_i's and m γ_i's in (1.5) hence a total of $2m$ rather than m^2 parameters to be estimated. The number of degrees of freedom is then

$$nm - km = nm - 2m = (n - 2)m,$$

hence for the sample size to exceed the number of parameters to be estimated we need have only more than two quarters. For the estimates to be approximately normally distributed we need $n - 2 \geq 30$ hence $32/4 = 8$ years of quarterly observations. One need only go back to 1983—hardly the times of the Pharaohs!

[2]Such linear restrictions on the columns of Π will result from so-called overidentifying restrictions on the structural parameters, i.e., restrictions in excess of the $m - 1$ homogeneous linear restrictions on the corresponding rows of $[\Gamma', B']$ needed for identifiability of the parameters in that row.

2 A statistical interpretation of calibration

The practice, though not the terminology, of calibration goes back at least to Leontief's input-output models (cf. Leontief, 1951). For each industry, j, the ratio in a particular time period of the input from industry i to the output of industry j is used as an estimate of the corresponding input-output coefficient a_{ij}. Since for each input-output coefficient there is one observation and one parameter to be estimated, there are zero degrees of freedom. Leontief assumed the input-output coefficients to be fixed.

In practice, Leontief used values of inputs and values of outputs. As was pointed out by Klein (1953, pp. 205–6),[3] the form of production function for which these ratios of input values to output values are constant is the Cobb-Douglas, and the ratios are precisely the exponents of the inputs in the production function. It was suggested by Klein (1953, p. 193) that one could use geometric means of observed input shares over a sample period to estimate these exponents. Can one justify the practice of estimating these exponents from a single observation?

We may formulate the problem as follows. Let w_{tij} denote the share of the ith input in the cost of production of the jth commodity at time t. In a deterministic model, this should be exactly equal to the exponent β_{ij} of the ith input in the Cobb-Douglas production function for the jth commodity. Allowing for error, however, we could postulate that in a sample of size n,

$$w_{tij} = \beta_{ij}\varepsilon_{tij}, \quad (t = 1, 2, \ldots, n)$$

or

$$\log w_{tij} = \log \beta_{ij} + \log \varepsilon_{tij} \quad (t = 1, 2, \ldots, n),$$

where $\log \varepsilon_{tij}$ is assumed to have mean 0 and variance σ_{ij}^2. The least-squares estimate of $\log \beta_{ij}$ being the sample mean

$$\log \hat{\beta}_{ij} = \frac{1}{n} \sum_{t=1}^{n} \log w_{tij},$$

the corresponding estimate of β_{ij} is the geometric mean

$$\hat{\beta}_{ij} = \left(\prod_{t=1}^{n} w_{tij} \right)^{1/n}. \tag{2.1}$$

[3]See also Klein (1952–53) for a more detailed development, in particular one allowing for joint production.

The variance of the estimator $\log \hat{\beta}_{ij}$ is

$$\text{Var} \log \hat{\beta}_{ij} = \frac{\sigma_{ij}^2}{n}, \tag{2.2}$$

and since the best quadratic unbiased estimator of σ_{ij}^2 is

$$\frac{1}{n-1} \left\{ \sum_{t=1}^{n} \left(\log w_{tij} - \frac{1}{n} \sum_{t'=1}^{n} \log w_{t'ij} \right)^2 \right\},$$

our estimate of the variance of the estimator $\log \hat{\beta}_{ij}$ is

$$\frac{1}{n(n-1)} \left\{ \sum_{t=1}^{n} \left(\log w_{tij} - \frac{1}{n} \sum_{t'=1}^{n} \log_{t'ij} \right)^2 \right\}. \tag{2.3}$$

In a sample of size 1, the estimator (2.1) reduces to

$$\hat{\beta}_{ij} = w_{1ij},$$

which is the estimator used by Leontief. However, since (2.3) is now undefined, there is no way to assess its reliability. It may be inferred from some of Leontief's writings, however (cf., e.g., Leontief, 1954), that he makes the implicit assumption that the variance, σ_{ij}^2, is small, e.g.,

$$\sigma_{ij}^2 \leq \delta,$$

where δ is a small positive number. In that case from (2.2) we obtain for the variance of the logarithm of $\hat{\beta}_{ij}$ the estimate

$$\text{Var} \log \hat{\beta}_{ij} \leq \frac{\delta}{n},$$

so that for $n = 1$ this variance does not exceed δ.

An alternative way to formulate the problem is as follows. Let y_{tij} denote the contribution at time t of the input of commodity i to the cost of production of the output x_{tj} of commodity j (i.e., y_{tij}/x_{tj} corresponds to w_{tij} in the previous formulation), and let us assume a regression of the form

$$y_{tij} = x_{tj}\beta_{ij} + \varepsilon_{tij}, \quad \mathcal{E}\varepsilon_{tij} = 0, \quad \mathcal{E}\varepsilon_{tij}\varepsilon_{t'ij} = \delta_{tt'}\sigma_{tij}^2. \tag{2.4}$$

Ordinarily one assumes $\sigma_{tij}^2 = \sigma_{ij}^2$ for all t. The least-squares estimator from a sample of size n is

$$b_{ij} = \frac{\sum_{t=1}^{n} x_{tj} y_{tij}}{\sum_{t=1}^{n} x_{tj}^2} \tag{2.5}$$

which is unbiased with variance $\sigma_{ij}^2 / \sum_{t=1}^n x_{tj}^2$. The best quadratic unbiased estimator of σ_{ij}^2 is then

$$s_{ij}^2 = \frac{\sum_{t=1}^n (y_{tij} - x_{tj} b_{ij})^2}{n-1}, \tag{2.6}$$

hence the corresponding estimator of the variance of b_{ij} is

$$\text{Est Var } b_{ij} = \frac{1}{n-1} \frac{\sum_{t=1}^n (y_{tij} - x_{tj} b_{ij})^2}{\sum_{t=1}^n x_{tj}^2}.$$

If $n = 1$ the estimator (2.5) reduces to

$$b_{ij} = \frac{x_{1j} y_{1ij}}{x_{1j}^2} = \frac{y_{1ij}}{x_{1j}} \tag{2.7}$$

which of course is the estimate used by the "calibration" procedure. It is unbiased, with variance σ_{ij}^2 / x_{1j}^2, but there is now no way to estimate σ_{ij}^2 and thus to assess the significance of one's estimate.

Suppose, however, that in (2.4) one replaces the assumption $\sigma_{tij}^2 = \sigma_{ij}^2$ by the assumption

$$\sigma_{tij} \leq \rho x_{tj} \quad (\rho > 0) \tag{2.8}$$

where ρ is assumed to be known. If $\rho = 0.1$, say, this means that one "knows" that the standard deviation of the error term in (2.4) is not more than 10% of the value of output. Under this assumption the variance of b_{ij} is at most ρ^2 which is assumed known. This may be seen as follows. If we replace (2.8) by an equality, then the best linear unbiased estimator of β_{ij} is readily seen to be

$$\tilde{\beta}_{ij} = \frac{\sum_{t=1}^n \frac{y_{tij}}{x_{tj}}}{n} \tag{2.9}$$

whose variance is ρ^2 / n. Using this approach one would estimate the input-output coefficients (in value terms) as the arithmetic means rather than the geometric means of the observed input-output ratios. In the case $n = 1$ the estimator (2.9) reduces to the same formula as (2.7). Reverting to the inequality (2.8), we may conclude that

$$\text{Var } \frac{y_{1ij}}{x_{1j}} \leq \rho^2.$$

It may be quite justifiable to make the assumption (2.8). If so, this provides a justification or at least a rationalization for the "calibration" process in terms of statistical theory.

3 Historical development of the calibration concept

In the published CGEM literature, the term "calibration" apparently first appeared in Whalley (1982), but only in the title of a section (p. 350). The section described the method used to adjust the parameters of the model to the empirical data, a method which had already been explained in considerable detail in Brown & Whalley (1980). Both the terminology and the general formulation of the methodology became much more explicit in St-Hilaire & Whalley (1983) which contains the following succinct summary:

> The motivation for data assembly is the current widely used practice of calibrating "empirical" general equilibrium models so as to exactly reproduce a base year data observation as an equilibrium model solution. The procedure enables empirically based models to evaluate counterfactual equilibria in a way which corresponds to comparative static analysis in theoretical literature. Under this approach parameters for underlying demand and production functions which characterize the model are determined directly from the model equilibrium conditions. The model is inititally solved for the equation parameters from the assumed equilibrium observation. The parameters are then used to solve the model in the opposite direction for a counterfactual equilibrium typically involving a proposed policy change. Policy evaluation then proceeds by comparing the counterfactual and historical (or benchmark) equilibria. The origins of this approach can be found in Harberger (1962) ...

Thus, Harberger (1962) may be regarded as the father of the calibration procedure in CGE models. The grandfather, however, as already suggested, was Leontief (1951). And as Jorgenson (1984) has stressed, the method was also pioneered by Johansen (1960).

The calibration procedure was described and applied in Piggott & Whalley (1985), and a very systematic discussion, constituting what appears to be the most explicit account to date, was presented in Mansur & Whalley (1984). These

methods have been applied and extended by Kimbell & Harrison (1984) and Harrison (1986). Jorgenson (1984) has shown how econometric methods can be used in place of calibration.

Parallel developments have taken place in macroeconomics, but apparently in complete isolation from those in the CGEM field. Kydland & Prescott (1982) introduced a method of "calibration" for a macroeconomic model (though it is and is decribed as a general-equilibrium model, in the dynamic sense); Altuğ (1989) showed how econometric methods could be used to estimate this same model, with different results. And recently, Gregory & Smith (1990) have studied the Kydland-Prescott calibration procedure, considered as a method of estimation, and compared it with the generalized method-of-moments estimation procedure introduced by Hansen (1982).

It is to be hoped that in the future there will be greater communication between "calibrationists" and econometricians, as well as between macroeconomists and CGE modellers, so that empirical methods may evolve that are both practicable and soundly based on principles of statistical inference.

4 The question of existence of equilibrium

The most startling statement made by Whalley (1991) is that "existence of equilibrium is of no policy interest." How can one explain or justify the great efforts undertaken to compute a general equilibrium if no such equilibrium exists?

My colleague Hans Weinberger of the University of Minnesota Mathematics Department once remarked to me that he could not understand economists' obsession with equilibrium. In physics, a theory is modelled in dynamic terms to begin with, leading to a system of total or partial differential equations. One possible property of such a system is that it has a singular point, or "equilibrium"; but this is a very special case. Many other types of solutions are possible, such as limit cycles; and no equilibrium need exist. Neverthless the system is perfectly determinate.

There are many examples on could cite in economics as well. Arrow (1951) provided an example in which no competitive equilibrium exists. Scarf (1960) and Gale (1963) provided examples of situations in which the only competitive equilibria were dynamically unstable. Such examples are not necessarily unrealistic, as

argued in Chipman (1965). The problem with them is that they are incomplete; one needs to posit a dynamic process of adjustment in order to find out what the actual solution of the system would be—most likely a limit cycle. Goodwin (1951) showed that a simple macroeconomic model without technological change had one unstable equilibrium and a stable limit cycle, and that a model with technological change had a stable limit cycle but no equilibrium. Goodwin adjusted his models to data using a calibration method (pp. 15–16). In the 1960s business-cycle analysis fell out of fashion, and many economists were heard to say that the business cycle was obsolete, or dead; few people would say that today.

If the true state of the economy is an oscillatory one, one could get very different results in CGE modelling depending upon whether the "benchmark year" was chosen to be the peak or the trough of a cycle.

Another observation made by Whalley (1991) struck me as curious: that numerical calculations in economics were pioneered by Graham (1948). In fact, by theorizing in terms of numerical examples Graham was continuing a tradition begun by Ricardo and followed by Mill, Mangoldt, Marx, Taussig and many others. Certainly, Graham brought this process to the status of a high art, and his examples had a much greater air of realism than those of his predecessors. It was one of the singular accomplishments of one of Graham's students, McKenzie (1954), to demonstrate the existence of equilibrium in Graham's model, as well as to develop topological methods for computing efficient patterns of specialization in this model. Of course, McKenzie's method was nonconstructive, and Scarf's (1973) contribution was of major importance in providing a constructive method, i.e., providing a method of establishing existence of equilibrium under given assumptions by providing an algorithm for computing it. It is true than an author who reasons in terms of numerical examples is likely to keep potential real-world applications in the forefront, whereas there is always the danger that abstract analysis of economic models will take on a life of its own and eventually lose sight of their *raison d'être*. There is an opposite danger, too: those who come up with numbers, however obtained, are more likely to be believed than those who make less precise prognostications, if only because they easily intimidate those who are unacquainted with the fine points of their methods.

5 Concluding remarks

The scope and sweep of the CGE work carried out by Whalley and his colleagues are extremely impressive. To apply general-equilibrium models to real data for purposes of policy prognosis is one of the most important things economists can do; no doubt the most important. On this point I have no argument. It should also be possible to agree that it should be done right. To do it right requires the diverse skills and talents of a wide spectrum of theorists, econometricians, and applied workers. For this, I think the most important requirement is communication. The calibration methods used need to be further formalized so that they can be better understood by econometricians. Then it should be possible to progress towards methods that use to advantage the *a priori* assumptions implicit in the calibration procedures, and which are soundly based on principles of statistical and logical inference.

The use of extraneous estimators gleaned from literature searches, while perhaps better than guesswork, is quite questionable in many cases. More often than not, elasticity estimates found in the literature are based on partial-equilibrium and ill-specified models. At least, allowance should be made for error—in particular, possible bias—in these estimates.

Work on CGE modelling should not be allowed to ossify. Movement is needed towards dynamic models that allow for oscillations and growth; and better integration of calibration with state-of-the-art econometric methods should be an important objective.

Finally, there is the need for verification. A policy prognosis made before adoption of a policy is not of much use unless there is a way to verify its correctness after the policy has been adopted. Of course, this is very difficult to do, since other variables have changed. But the exercise is hard to justify unless verification is possible. Whalley (1991) has referred to projections made before the U.S.-Canada free-trade agreement that suggested a substantial welfare gain for Canada. Has anybody tried to verify the correctness of these projections after the fact? I am reminded of an experience I had when spending a summer with an advisory group in the Brazilian Ministry of Planning. One day, a member of the Brazilian staff made a presentation in which he set out projections of value added in the ten industrial subdivisions of the Brazilian economy for the next ten years. After the talk, I asked a naive question: could he please provide the corresponding data for

the *past* ten years? Of course not, he replied; such data are not available. He was presumably confident that ten years hence such data would still be unavailable, so there would never be a danger that his projections would be falsified. When the future is known better than the past, my suspicions are aroused.

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The Use of a Computable General Equilibrium Analysis: An Evaluation from an Outsider's Perspective*

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1 Introduction

Computable General Equilibrium Models have several advantages and maybe the biggest one is that they rely on the consistent and complete description of individual behavior. For individual behavior the standard neoclassical assumption is made, i.e. the maximization of utillty subject to constraints. All results of economic activities are derived from this behavioral assumption. The framework of the general equilibrium implies that markets are cleared through the price mechanism. Further advantages of computable general equilibrium models are that one is forced to construct the model very precisely and to specify clearly the assumptions as well as the model's structure. These models are theoretically demanding and partially allow a micro-foundation of macromodels, especially when studying macro-policy effects on individual behavior[1].

They are used in public finance (for example to investigate the welfare and incidence effects of changes of the tax structure), in international trade (for example to study the effects of tariffs and quantity restrictions within GATT-negotiations),

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[1]For a broader and more general description of this type of models compare Scarf and Shoven (1984), Mansur and Whalley (1984), Waelbrock (1986, 1990) and Shoven and Whalley (1984).

in the development area (for example to study the basic effect of certain development policies), in the environmental area (for example to study the effects of carbon taxation), and general equilibrium models of regions can be constructed (to investigate macro-policy events in certain regions)[2]. In principle, the numerical specification of such models helps to gain additional insights into a number of poilcy issues.

Let me stress that I am not an expert in this field and in this type of model construction. My main research field is public choice and (positive) public finance, and I use mostly applied econometrics when analysing the effects of policy issues. Therefore I want to raise four points to be considered, which I think illustrate the problems with CGE models[3]. They are:

- the assumptions and specifications of general equilibrium models;
- the basic model philosophy and "parameter estimation";
- the missing parts and limited use of CGE models; and
- the institutional and the public choice aspects.

All four problems will be concisely presented in the following four sections, and finally in section 6 a summary is given and a preliminary conclusion is drawn.

2 Assumptions, construction and specification of CGE-Models

What I quite often miss when I come across papers using this type of model is that the basic assumptions are not made explicit. But the model philosophy or the "basic beliefs" (for example that free trade is in all cases seen as preferable) should be clearly documented, so that all other researchers who have not constructed the model are able to understand the basic philosophy and structure. Sometimes I have the suspicion that the assumption and basic model philosophy is not so clearly expressed, because one might have the feeling that the results such a model

[2]Compare, e.g. the contributions by Keuschnigg (1991), Mettelsieven (1991), Manne (1991), Conrad and Schroeder (1991), Alfsen (1991) and Rowse (1991).

[3]For a more detailed and basic criticism compare Schneider (1990).

produces seem to be predetermined by the constructor who believes such a model philosophy.

Another point is that quite often the results of CGE-models can not be easily reproduced. I think this is very critical and a basic point, because quite often it is not clear what happens, especially in complex CGE-models. So it is advisable that the model constructor provides some basic simulation runs, in which one can see the basic interdependencies responsible for the results the model used.

3 Basic model philosophy and "parameter estimation"

As indicated in the introduction, the basic principle in the CGE-models is the principle of an instantaneous market clearing process; i.e. markets are cleared through equilibrium prices, which determine supply and demand in *all* markets. In order to use CGE-models for simulation purposes, they have to be "filled" with parameter values. However, it is quite often not possible to estimate these parameters simultaneously, because of the lack of data and of the underlying basic model structure. The parameters are usually estimated with the (so-called) calibration method, to reproduce a micro-consistent data set given numerical values for crucial elasticities of substitution relations. Let me clearly emphasize that the calibration method is nothing else than just assuming certain values for crucial parameters, quite often without explaining why specific values are chosen and other ones are not! Basically, the values are determined in such a way that the model produces "plausible" (whatever that means) results, which may not be justified economically. I am convinced that this is one of the weakest points of the CGE-models, and researchers should undertake great efforts to overcome the calibration method.

I propose another idea which might help to gain more reliable parameter values. Is it not possible to construct a micro-consistent data set with the help of experiments? Not only in the US (just to name the two most prominent researchers like Charles Plott and Vernon Smith) a number of experiments have been undertaken which could be of help for finding reliable values for elasticities, which are crucial in the CGE-models. I think researchers of the CGE-models should have a very close look at all the experimental studies and come into contact with these

researchers in order to make use of their knowledge to improve parameter estimation in CGE-models. In my opinion, the calibration method should be abandoned for estimating "parameter values" because it is a questionable method. As long as we can not get rid of the calibration method, stability and robustness tests for the assumed parameter and elasticity values are necessary to avoid an economic interpretation of incidental numerical results.

4 Missing parts or limited use

In the CGE-models the basic unterlying assumption is utility maximization subject to constraints. Sometimes an attempt is made to use these models for analysing the behavior of bureaucracies and governments, which might not have such simple goals all of the time. Therefore, I see some difficulties if a government has another goal or different conflicting goals and wants to use such models for a policy analysis. Then these models can only be used under the assumption that all agents use the basic utility maximization concept subject to constraints. Also, money and financial markets are so far not integrated in the CGE-models. But these markets are of crucial importance in a GE framework and a strong attempt should be made to extend CGE models to include these two markets.

5 Institutional and public choice aspects

In most CGE-models markets are the only institutions used, and market clearing is the allocation mechanism of the models. One might ask who is the addressee for these models besides the academic world? The government is usually treated as completely passive in these models. Coming from a public coice tradition this seems to be somewhat strange. Politicians should be treated as actors maximizing their utility subject to constraints who try to reach their selfish goals (i.e. to stay in office, to fulfill certain ideological policies, etc.). Quite often politicians do not look at social welfare but rather pursue their own selfish goals, and politicians are quite often also interested in the process to reach certain policy goals as the policy goals themselves. Therefore, one should consider whether it is not possible

to include in such models more realistic assumptions about government behavior[4]. Today it is quite often crucial to analyze different institutional aspects under which (representative or direct) democracies operate, and here I do not see how meaningful the use of the CGE-models can be. Again, they can be seen as one benchmark-solution especially when contrasted with models who produce results in a more positive way.

6 Summary and a preliminary conclusion

Let me clearly emphasize that my critizism of such models should be seen as a positive one, aimed of improving these models. As all economic models, the CGE-models have certain advantages and disadvantages. I think most of the above mentioned disadvantages can be overcome. Therefore, one conclusion is that one should further develop these models but one should be aware of their limited use. If this were done and if one might get "rid of the calibration method" for estimating parameter values and elasticities, the CGE-models could be seen as an interesting and very fruitful tool in various areas of economics, in particular in normative models of government behavior as well.

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Does it pay to invest in Computable General Equilibrium Modelling?

Some reflections on the Round Table discussion
of the SFB 178 Workshop

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Superiority of general equilibrium analysis over partial equilibrium analysis is widely recognized from a methodological viewpoint, but many economists will also agree that its mathematical complexity and its empirical emptiness reduce its importance for the analysis of actual policy issues. General equilibrium analysis, tought in university classes, thus has been widely regarded as a purely academic exercise for students, an ivory tower science, which cannot provide clear-cut answers to real policy problems.

But many of these reservations have to be revised in face of the progress in computer technology. Today, every personal computer, used as a text editor in a secretary's office, is capable of running algorithms that solve complex nonlinear maximization problems with nonlinear constraints, in particular economic general equilibrium models. Since computational constraints have been overcome to a large extent, CGE analysis has become a technique of economic analysis which allows to specify, to solve and to simulate general equilibrium models of considerable complexity.

The payoffs to investment in CGE analysis are evident for the economic researcher, who is offered a powerful analytical technique applicable to a large number of interesting economic problems. Examples include incidence and welfare analyses of country-specific tax systems, cost-benefit evaluations of public investment projects, welfare analyses of exhaustible resources and environmental phe-

nomena, multisector and multicountry development planning, trade liberalization, tax competition, etc.

Since computational constraints to the implementation of general equilibrium models of satisfactory complexity have been overcome, CGE models have become a toolkit of policy analysis not only among academics - as documented in the contributions to our workshop - but also in major research institutes. In these institutions, economists have to analyze and to recommend policy programmes based on relevant empirical figures and institutional constraints. Therefore, CGE models were introduced as a further method to serve this purpose. The World Bank and the International Monetary Fund are using CGE models to analyze development perspectives for less developed countries as well as trade policy consequences for industrialized nations. The OECD and EC research groups develop CGE models to study the problem of international agricultural policy and tax policy. Within the IIASA (International Institute of Applied Systems Analysis) research projects on global pollution and global resources are based on CGE models. And last but not least, national economic research institutes, (especially in the US, Canada, Australia, the Netherlands, etc.) engage in projects which use CGE models. I personally believe that the political debate on the agenda of German unification would have gained in economic rationality, if some CGE analyses had shed complementary light on the structural problems associated with this historically unique economic process.

Still, there are investment costs associated with the entrance to the club of CGE analysts. Getting familiar with computer application, data selection, model specification, simulation design and data analysis is time consuming and generates substantial opportunity costs in one's individual research strategy.

Certainly, entry costs for economists could be reduced considerably if the fundamentals of CGE analysis were included in the curriculum of university education. Up to now this has not been done and there is little chance to get in touch with CGE modelling as a graduate student in an economic department. With respect to a revision of the curriculum, one has to weigh the benefits from CGE qualification against the individual and social opportunity costs of teaching, studying and applying CGE models in order to answer the question whether it pays to invest in this analytical technique. Since CGE analysis has not been included in the academic syllabus, revealed preference indicates that the costs of teaching CGE outweigh the benefits.

On the other hand, there is no doubt that both the costs of and the benefits from CGE modelling have been subject to major changes. Computer facilities have improved dramatically and access to tested and elaborated Newton routines for general equilibrium models based on CES specification of utility and production functions allows a fast learning-by-doing approach for teachers and students. But getting familiar with the algorithm, its simulation capabilities and especially with the analysis of abounding numerical results requires a lot of practice on the PC. Consequently, generous PC access for every individual user and guided laboratory sessions are indispensable prerequisites for successful teaching.

But CGE simulation will only improve the student's economic understanding and contribute to his human capital, if it is based on a sound knowledge about the underlying theoretical framework. So students must have acquired a strong grasp of microeconomic theory, second-best economics, abstract modelling as well as calculus and optimization theory. Consuming CGE analyses as a black-box exercise with figures will not contribute at all to one's economic qualification. Since public economics and international economics have a long tradition in general equilibrium analysis,it seems to be most promising to incorporate CGE modelling into the curriculum of these two economic disciplines.

The primary benefit of introducing a CGE class in the graduate programme is, of course, a better understanding of economic interdependence. The traditional treatment of general equilibrium models using calculus and first order conditions suffers from two major weaknesses.

First, the variation of key variables can only be predicted in sign if severe restrictions are imposed on the model (in particular binary Robinson Crusoe economies with two commodities, two sectors, two factors).

Second, even if unambigous results are derived, they are only valid for marginal changes, in contrast to real reform policies which quite often cannot be regarded as "small changes" at all.

In contrast, the advantage of using a computable general equilibrium framework instead of a traditional marginal analysis is at least fourfold

(i) CGE models allow the user to deal with an arbitrary number of commodities, sectors, and agents and to evaluate selective distortions in second-best environments (e.g., distortionary commodity taxes, tariffs or trade restrictions).

(ii) Policy evaluations can be obtained for any arbitrary policy design and need not be restricted to marginal changes around an equilibrium.

(iii) Models with several consumers allow to study the personal incidence of policy measures and thus overcome the traditional restriction to a two-class functional incidence pattern.

(iv) Modelling endogenous factor supply allows to depart from simplifying assumptions and to get rid of non-distortionary (lump-sum) tax and subsidy measures as first-best policy devices, which are useless as a guideline for actual tax policy.

Working with CGE models thus increases the sensitivity for the economic importance of interdependencies and second-round effects, which tend to be suppressed by "ceteris paribus" clauses in traditional textbook analyses.

Besides these direct benefits of CGE practice, there are positive externalities associated with the compilation and the handling of empirical data and with data analysis required to interpret consistently the numerical results produced in simulation runs.

Evaluating the benefits and the costs of attending university courses in CGE analysis, there seems to be a net gain for those students, who plan to join an international institution where a sound knowledge in quantitative methods is expected. But besides the technical skills the pedagogical role of CGE experience must not be underestimated. With respect to the few but existing flexibilities of the academic syllabus for economists I would suggest that regular classes and laboratory sessions in CGE modelling should be offered within the optional part of our graduate programme. This will serve as a test to the pedagogical benefits from individual CGE experience and might lead to compulsory CGE classes in the end. To me, this seems to be a promising strategy and I hope to some of my colleagues as well, which would enable us to start a CGE programme for our students in Konstanz.

WORKSHOP ON

ISSUES IN INTERNATIONAL ECONOMICS: QUESTIONS TO AND ANSWERS FROM COMPUTABLE GENERAL EQUILIBRIUM ANALYSIS

JULY 8-9, 1991
UNIVERSITY OF KONSTANZ

MONDAY, JULY 8, 1991

10.00 Opening Address
Wolfgang Franz (Dean, University of Konstanz)
Bernd Genser (University of Konstanz)

"General Equilibrium Economics: Computation and Application"
John Whalley (University of Western Ontario)

11.00 Session 1: International Taxation

"Value-Added Tax Harmonization in the EC"
Wolfgang Wiegard/Hans Fehr/Christoph Rosenberg (University of Regensburg)

"Commodity Tax Reform under the Restricted Origin Principle"
Andreas Haufler (University of Konstanz)

14.30 Session 2: Factor Supply

"Simulation Models of Labour Supply"
Alan Duncan (Institute for Fiscal Studies, London)

"An Intertemporal General Equilibrium Model for Austria with Application to Foreign Trade"
Christian Keuschnigg (University of Bonn)/Wilhelm Kohler (University of Innsbruck)

17.00 Session 3: Environment

"CGE Modelling of International Public Goods: Air Quality and Global Warming"
John Piggott (University of New South Wales)

TUESDAY, JULY 9, 1991

09.00 Session 4: Imperfect Competition and Growth

"AGE Models in International Economics with Special Reference to Imperfect Competition and Increasing Returns to Scale"
Albert Schweinberger (University of Konstanz)

Growth and International Capital Movement: A Simulation Study
Karl-Josef Koch (University of Konstanz)
(presentation cancelled due to accident)

10.45 Round Table: "The Payoff of Investment in CGE - Modelling"

Chair: Bernd Genser

Introductory Statements:
John Chipman (University of Minnesota)
Friedrich Schneider (University of Linz)

Closing Address: Bernd Genser