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Debunking the Myths of Computable General Equilibrium Models

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Abstract

Computable General Equilibrium (CGE) models are probably the most utilized tool globally for development planning and macro policy analysis. Despite this their history is not available in the literature, their theoretical grounding is never explained, and the mechanics of the models remain hidden under layers of rhetoric, myths and hand gestures at various theoretical structures. Therefore, this paper explains what CGE models are, how they work, and where they came from. It will be shown that CGE models have always been and are macroeconomic models, and not based on general equilibrium theory. In doing so, it will detail how Walras's general equilibrium is *not* equivalent to Arrow-Debreu's General Equilibrium, and that CGE models use *neither* of those frameworks. Furthermore, this paper gives the history of the model and identifies its (relatively) few key variables, in order to explain how model builders construct CGE models, and consciously *impose* causality, while choosing exogenous variables that *define* results. CGE models can be a very useful policy tool, but only by understanding that it is a static fixed output model, not built for dynamic analysis along the lines of Arrow-Debreu general equilibrium. With this paper in hand, users will be able to know which questions to ask model builders, to ensure more transparency and clarity in future CGE models.

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The aim of this paper is to directly address and debunk the rhetoric and myths that surround Computable General Equilibrium (CGE) models. The paper aims to explain that there is only one CGE model and then understand where this model comes from, what it does, how it works, and why there is so much confusion in the literature.

The following popular myths of CGE models are *debunked*: 1. CGE models are general equilibrium models. 2. CGE models were invented in 1960, but only developed after 1978. 3. CGE models are based on micro economics. 4. Applied General Equilibrium (AGE) models are a special case of CGE models (or vice versa). 5. Walras is represented by modern general equilibrium theory, so Walras is the base of CGE models. 6. Formal general equilibrium is computable. 7. There are different types of CGE models. 8. CGE models are dynamic.

It is *not* the objective of this paper to reject CGE models, nor to disenfranchise them from the policy analysis world. The aim of this paper is to do away with the rhetoric and confusion that persists in the general CGE literature, both between insiders, and for outsiders, in order to promote proper use and understanding of CGE models.

The impetus for writing this paper is the prevalence of misrepresented historical facts surrounding CGE models, and the increasingly complex models and manuals, which avoid clear exposition of the internal mechanics. This is an introduction to the model for newcomers to the arena, and a clarification of the models background and theoretical roots for experienced model builders.

To achieve this objective, it is necessary to understand the history of CGE modeling, how it evolved alongside other modeling practices, and has become a global tool for development and policy analysis. This history is presented in section 1, which covers the developments of the model and model builders, and in section 2, charting the institutional rise of CGE models, from a purely academic exercise, to being the World Bank and World Trade Organization's primary tool for analysis.

To cut through the General Equilibrium rhetoric in CGE models, one has to separate Walras's general equilibrium from Arrow-Debreu's, and in turn separate that from the CGE model, which is done in section 3. Admittedly this has some consequences as we find that ideas like Walras'ss Law or Walras'ss Auctioneer weren't Walras'ss at all, and we come to address the issue that Arrow-Debreu General Equilibrium is not computable.

Finally, the aim is to encourage CGE model builders to be more transparent about a lot of their choices within the model which define results, and determine causality, but are hidden in complex equations, or remain unpublished. Section 4 outlines how CGE models are built, how they work, and the relatively few key variables which model users need to be aware of, as they define the results, and often overshadow them.

CGE models dominate a large part of what is considered applied econometric analysis of issues surrounding economic development and domestic policy. They are the primary tool of analysis in international trade debates and government planning, from NAFTA to the Doha rounds, from Norway to Mozambique and is used to analyze tax reforms, welfare distribution, and more recently global warming, weed management, sports events and even the effects of an *intifada*[1]. As such, it is high time that an accessible guide to these models was produced, and the myths in which they surround themselves be debunked. This paper hopes to provide exactly that.

1. A history of Modeling

The standard history of CGE models would have us believe that they were developed in the "early 1970s, when Adelman and Robinson (1978) built a model for the South Korean

[1] On the last four topics, see Böhringer et al. (2006), Wittwer et al. (2005), Madden (2006) and de Boer and Missaglia (2006) respectively.

economy" (Bandara[2] 1991: 10). This model and all its subsequent reformulations then spawned many types of CGE models based on the Arrow-Debreu general equilibrium framework and microeconomic theory. Model builders have the task of constructing this complex, yet neutral, system of equations and mappings in a standard format and solving the general equilibrium problem under new policy regimes.

This story, as repeated in every single survey on CGE models[3], and quite a few models, is however both highly misleading and incorrect in every one of its implications.

The biggest issue with this literature is that "there exists no *overall* survey article because there are too many different CGE models to cover the complete field" (Thissen 1998: 2, his emphasis). This is exactly the problem. A survey is not needed, but a proper historical analysis of the models roots, in chronological order will reveal that there is only one CGE model, and it will address the historical inaccuracies and the theoretical claims.

To extract this history, I have used a three pronged approach: First, the latest published papers and surveys on CGE models will be investigated to see where they trace their epistemological roots to. Secondly, starting from the first attempts to build statistical models for a whole economy, I have identified 'academic relations'[4] up through time where links from author-to-author are not obvious through academic referencing. Thirdly, interviews and e-mail exchanges with modelers and historians have plugged holes, as have previous surveys of the literature.

[2] Variously referred to in writing as Dr. Jay Bandara, Dr. Jay Bandaralage or Dr. Jayatilleke S. Bandara.

[3] Bandara (1991) and Robinson (2003) which are probably the best surveys on the topic in general, as well as Shoven and Whalley (1984), Pereira and Shoven (1988), Decaluwé and Martens (1988), Robinson (1989, 1991), Bergman (1990) and Dixon and Parmeter (1996), all of which cover a mix of many models and history, as well as Thissen's (1998) more focused survey on all the models applied to just Egypt.

[4] I gratefully use Deardorff's (2007) 'family tree' analogy to describe how some economists are connected even if they do not reference each other. They may have worked together, studied together, supervised or studied with each other etc. Deardorff's list is not complete and his focus on trade economists is not perfect for this particular enquiry about CGE models, but it has been helpful in connecting the dots, and as a conceptual vehicle for thinking about connections.

1.1 Leontief

Bandara (1991) and Jorgenson (1984) both recognize that the beginnings of CGE (or 'empirically estimated economy-wide') modeling started with the work of Leontief in the 1930's when he was fresh out of Harvard and working for the Bureau of Labor Statistics (BLS). At the BLS Leontief attempted to set up a *Tableu* of input-output accounts for the US economy, much akin to Quesnay's attempt in 18th century France (Kohli 2001, Quesnay 1758). Leontief created an accounting system that encompassed "all branches of industry, agriculture, and transportation [and] also the individual budgets of all private persons" (Leontief 1951: 11). Once this project was underway he realized that "in addition to the table, he needed a model" for the economy (Kohli 2001: 30).

As this was before Stone and Meade had composed the national accounting framework we today take for granted, Leontief set up his own structure for a 'national account' of the economy wherein all sectors produced goods or services that were consumed fully by another sector. Leontief's "*Inter-relation of prices, output, savings and investment*" (1937) contained a snippet of his US model with 44 sectors estimated for 1919 and 1929. Some of the sources for this work were later given in the appendices of "*The Structure of the American Economy, 1919–1939*" (Leontief 1951). These input-output models were the first 'complete'[5] statistical overviews of the flows and sources of funds in an economy, since 18th century France, as conceptualized by Leontief.

At the end of the Second World War the BLS were asked to forecast steel demand, in a non-war scenario. To do this, the BLS took Leontief's input-output estimates of the 1939 US economy[6] and introduced demand and behavioral assumptions between variables in the

[5] The original accounts did have an unaccounted portion of the circulation which amounted to 19.8% in the 1919 model, and 19.4% in the 1929 version. By the time of the 1947 estimation, which had many more sectors, this 'undistributed' figure had fallen to 3% (Kohli 2001).

[6] These estimates remain unpublished, but are referred to by Leontief himself in *The new Palgrave: A dictionary of Economics* (Eatwell et al. 1987) in the "input-output analysis" section. They are included in a 1947 pamphlet

framework. This input-output model was one of the few forecasts which suggested that steel demand would not fall as a result of the war ending, but would be strong – which turned out to be correct. Leontief (1951b) argued that this was one of the reasons why this form of analysis became important to the U.S. administration already then, and academics started adopting multi sector input-output models for economy wide analysis.

Hollis B. Chenery was Leontief's Ph.D. student at Harvard, and during the 1950's he (among other things) carried on working in the input-output framework which Leontief had formulated. The work included new behavioral functions and demand systems which could be used to simulate full economy responses to changes, much like the BLS project. An exemplar of this work would be "*Interindustry Economics*" (Chenery and Clark 1959) which explored the "interdependence between the productive factors of an economy" (1959: 2) and related them to development policies. A review at the time noted that "There are no standards for evaluating the differences between the input-output coefficients" (Henderson 1960: 339) and this issue of statistically testing coefficients (or '*parameters*' in modern jargon) persists today. What Chenery, and others at the time were working on, was then (independently) formalized into a consistent framework by Leif Johansen in Norway.

1.2 The unwitting father

Leif Johansen (1960) is widely credited as the originator of the CGE model, although various parts of the literature ignored his work for a stretch of time. Johansen's aim was to analyze the "*deviations* from uniformity in the growth process" (1960: 5, his emphasis), in stark contrast to the typical Arrow-Debreu general equilibrium or growth theory at the time, which talked of a balanced growth across sectors.

To do so, he constructed a 'fixed output stochastic model' around the data given by the definitions of the national accounting system. First he chose a 'base year' from where all the

entitled *Full Employment Patterns 1950* (BLS, 1947) inventoried at the California Department of Industrial Relations, and were partly included in a published article by Cornfield et al. (1947).

starting figures would come, with 1950 serving as his benchmark. The reason for the static nature of the base year was that more years of full data were generally not available, and even if time series were available, "the structure [of the economy] would have significantly changed over the period" (Johansen 1960: 60). So Johansen set out to create a data set for the economy at one specific point in time, which would have a matrix form of the national account. Therefore he used the Input-Output method. Once all the data had been entered into the matrix, the rows and columns did not all equal, so Johansen explicitly edited the data, particularly in the household sector to make the matrix balance[7].

After all the data for the base year was 'adjusted', he assigned production functions and demand functions to producers and consumers. He defined trade and industry labor shift elasticities, allowing labor and capital to move freely, but rewarded idiosyncratically. He then assumed the economy to be in full employment (for both capital and labor) as he argued that the "institutional conditions" for this were present in Norway's 1950 economy (Johansen 1960: 19). Investment and Exports were fixed exogenously while equilibrium was defined as $X_i = \Sigma x_{ij} + C + Z$ which he described as a "book keeping relationship" (Johansen 1960: 48) where investment and exports equaled savings and imports – as defined by the national accounts and the Keynesian circular flow of income. Johansen's model found equilibrium in the macro balancing equations, given by the national accounting framework. It had nothing to do with Arrow and Debreu's notion of general equilibrium, and in fact Johansen explicitly stated that "a development path satisfying the equations of our model is not necessarily an optimal one, even though the model is characterized by marginalistic assumption" (Johansen 1960: 172). There were no references to either Arrow or Debreu, or any of the mathematical pre-requisites for the neo-Walrasian[8] general equilibrium, and they were unnecessary for the

[7] See pages 65-66 in Johansen (1960) for explicit changes to make the household account 'add up'.

[8] I borrow this term from Costa (1998) to refer to the neo-classical (Arrow-Debreu) general equilibrium theory, which enjoys referring to Walras when justifying certain assumptions. The connection from Walras to Arrow-Debreu is raised in section 3.

Johansen model. Johansen never claimed a link with general equilibrium theory, so it is interesting how he became the father of the supposedly computable version.

The choices Johansen made in setting up the behavior (i.e. equations) between the different sectors and factors yielded a result, when the full matrix was solved, with new endogenous variables. Effectively, to solve the model he changed the exogenous variables to simulate a one time change in the economy, from 1950 into the experienced future (remember he is writing in the late 1950's). Then he checked the model results with empirically observed data to see if his endogenous elasticities and changes to endogenous variables were consistent with the real change in the economy since 1950. If the values did not correspond before performing a test into the unknown future the 'method' was "virtually tantamount to pure guesswork" (1960: 135). When he was satisfied that his parameters conformed to the real outcomes observed up through the 1950's, he used the model to make a forecast beyond 1960.

The model result is a highly impressive 86 x 46 matrix[9], where the results for the shifts in labor and capital drive the results of which sectors are flourishing. By the nature of the model however, one cannot get an endogenous result for changes in the growth rate of the economy ($\ddot{K}_B$ & $\ddot{K}_m$ in the model[10]). You could only boost demand in one (or more) sectors allowing their contribution to output to rise, at the expense of other sectors. To get a result for a changing growth rate, Johansen had to define "growth rates in the endogenous variables in terms of growth rates of exogenous variables" (1960: 53). So the model builder's choice of the size of change in the exogenous variables is the only thing that will result in the expansion of the modeled economy – i.e. economic growth. The model would then spread the growth out in the economy, according to the elasticities, but it could not feed back a

[9] Johansen was very proud of the matrix and the solution as calculated by the computer FREDERIK at the Norwegian Defence Departments Research Institute, "a Ferranti computer of the Mercury type" (p. 122)

[10] Pages 51-52 in particular show how Johansen (1960) would insert growth into the model, but could not get growth as an endogenous result.

growth prediction for the simulated year. A similar problem would have presented itself in the Balance of payments, where the proportion of exports from each sector, could not change in the model, as it is fixed. Johansen also noted that changes in technology would have to be imposed exogenously of the model, and its relationship to the 'growth' in the simulation was as good as guesswork (1960: 126)[11].

The model proved successful, despite what he termed unrealistic assumptions of suppliers operating in perfect competition and a marginal equality between wage rates and marginal values of output, both "realistically unknown" (Johansen 1960: 171). The results of the model were not optimal, nor in the spirit of general equilibrium (be it Walras or Arrow-Debreu), but it was a balanced macro model (not a *balanced growth* model), successfully solved with linear equations. Johansen justified the majority of his assumptions with empirical observation for the past decade, and 'tested' those coefficients which both he and Chenery could not explicitly derive, by using observed values. This is probably the best practice even today, as no objective test can really be performed.

Johansen wanted to estimate the elasticities of substitution between sectors labor and capital movements, to explain "relationships in Norwegian reality" (1960: 3), and in doing so he invented a method for economic analysis through a combination of the national accounts, macro economic balancing equations and input output analysis – that is what made this the first 'CGE' model.

1.3 The Pioneering Students

Chenery continued to work on linear and non-linear applications of the Leontief system (Chenery and Kretschmer 1956), and also on numerical solutions to the Arrow-Debreu general equilibrium system, which had become feasible by the early 1970s (Chenery and

[11] He made similar points for depreciation and the raw material producing sectors, and skipped across the fact that prices in the base year all equal 1 (p. 46) – a notion he got from his Ph.D. supervisor Ragnar Frisch, which allowed Johansen's model to calculate cross price elasticities for all sectors simply by simultaneously solving the matrix for the economy.

Uzawa 1958, Chenery and Raduchel 1971). In 1970 he joined the World Bank as an economic advisor to the President of the World Bank, co-authoring "*Redistribution with Growth*" (1974) which moved his focus away from CGE and onto inequality and growth for a while.

His Ph.D. students at Harvard continued to work on economy-wide models, with some taking the Neo-Walrasian General Equilibrium framework as their starting point (Raduchel 1971), while others stayed with the Johansen type approach using macro equations (Lance Taylor and Sherman Robinson).

The first paper that explicitly built "a model similar to the one developed in Leif Johansen's" book and used it on a new problem was Taylor and Black (1974: 37) who applied it to Chile. There is some conflict on dates of publications and date of authorship, and when the question of '*being first*' is at stake, this will inevitably be a delicate subject, which requires some extra detail.

This model is the '*first*' despite other similar publications in 1974, as it was in academic circulation as early as 1972. Dervis (1975) refers to a mimeograph version from '72 in his sources from a paper on a similarly structured model. More than that, Black – who co-authored the paper – finalized his thesis (bachelors) on "a multi-sectoral study" of Chile in 1971 at Harvard (Black 1971), where Taylor had taught since 1970, and Chenery still had a presence[12]. This gives some reason to believe that the paper was done in draft form in 1971 after Black finished up. Taylor confirmed[13] that he was in Scandinavia in 1970 to discus the basic premise of the paper with Leif Johansen, and that it was submitted to the Journal of International Economics in 1971-72. More conclusively all the papers from the 1970s following on from Johansen method referred back to Taylor and Black (1974) as the model

[12] Based on the graduation date of Raduchel in 1971.
[13] In a private e-mail

they were using. In the same way, in the late 1970s papers on economy wide modeling (or CGE models as they were to be called) all referred back to Taylor and Lysy (1977) which in turn was based on the Taylor-Black 1974 paper.

Of course, CGE models were not named explicitly in 1974, but Taylor and Lysy published a paper on 'closure' issues of "computable general equilibrium models" in 1979 (Taylor and Lysy 1979[14]: 11). They attributed this model-name to studies by Adelman and Robinson (1977!) and Taylor and Lysy (1977). The Taylor and Lysy paper was officially 'processed' by the World Bank in 1977, but was presented as early as August 1975 to the Econometric Society at the Third World Congress (Robinson 1976). Adelman and Robinson's book however, was only published in 1978, and the only reason why the reference could have been 1977 (banning a mis-print) is because Taylor and Lysy had access to the manuscript in draft form in 1977. Adelman and Robinson "took our inspiration from the early work on price endogenous planning models by L. Johansen" (1978: 3). They noted that a number of Ph.D. dissertations had been the result of this book[15] "and other projects (Lysy and Taylor 1977)… using the same basic type of model" (Adelman & Robinson 1978: 3). So Taylor and Lysy (1977) predated the Adelman and Robinson book, according to Adelman and Robinson's *Income distribution Policy in Developing Countries* (1978), but Taylor-Black (1974) pre-dates them both.

Adelman-Robinson and Taylor-Lysy were in fact parallel projects under the supervision and with the support of Chenery who was the director of the Development Research Center (DRC), at the World Bank at the time. Both models, and the Taylor-Lysy book had been written up by 1977 for a April conference in Bellagio. The Taylor-Lysy model was delayed for publication by a year, due to a difference of opinion between the modelers and Alan

[14] This paper was received by the *Journal of Development Economics* in July 1978

[15] These Ph.D. dissertations were those of Dervis, Ahmed, Zoonor and de Melo. The first two are referenced in Robinson (1976) and the first three were in fact Ph.D. students at Princeton when Robinson taught there. De Melo, was a 1975 Ph.D. graduate from Johns Hopkins working with Bela Balassa, who knew Taylor, Adelman and Chenery previously.

Walters (later a major Thatcher advisor) over the use of different saving propensities for wage and non-wage incomes. Chenery finally over-ruled Walters' opinions and pushed the book through in 1978.

That being said the numerous dissertations which were from the mid 1970s pre-dates both of these 1977 publications, and they all refer to Taylor-Black (1974) as their original model source. However, to confuse matters, Adelman and Robinson claimed that "The earliest version of *our* model was formulated and *solved* in 1972" (1978: 3, my emphasis), in order to make them the 'first' again.

Adelman proposed the Korea project sometime in 1971-72 to Chenery (which became the 1978 book), based on her input-output modeling work and experience with Chenery (Adelman and Chenery 1966[16]). More so, she had good data on Korea through a Ph.D. student Larry Westphal, who had graduated in 1969, and spent a year working as an advisor to the Economic Planning Board of South Korea 1969-1970[17]. They co-authored a 1971 paper (Adelman and Westphal 1971) which provided the empirical basis for the project pitch. Robinson was independently working on economy wide Linear Programming models, and joined the project later. If the Adelman-Robinson model was formulated and solved in 1972 it could not have been solved independently from Taylor-Black, which was already in circulation by then, and no evidence exists of an independent 1972 model, only the above statement in the opening paragraph of their 1978 book.

The supporting references and apparent use of these solutions, as provided in Adelman-Robinson (1978) are referenced to the Ph.D. Dissertations of Dervis (1973) and Ahmed (1974), both of whom refer to Taylor and Black (1974) as their primary source – and not to any CGE (or similar) models by Robinson or Adelman. Taylor and Black (*published* 1974)

[16] Reprinted in Chenery (1971)
[17] Dr. Westphal went on to work at the World Bank as a manager of research on industrial development for the next eleven years.

was circulating at Princeton where Ahmed, Dervis and Robinson were studying and working in 1972. So it seems clear that the '*first*' formulation of the Adelman and Robinson model is found in Taylor and Black (1974), which formed the base for Taylor and Lysy (1977). So while the two 1977 papers were the first named 'CGE' models, the original post-Johansen model was the Taylor and Black (1974) paper, which the rest of the literature then built on.

1.4 Not Quite General – Taylor and Black (1974)

Taylor and Black (1974) were critiquing the concept of Effective Rates of Protection, and the use of partial equilibrium models. They argued that this approach could not capture the widespread effects that a tariff change could have on anything from exchange rates and wage demands through world prices. So they proposed a "local, but feasible, method of calculating resource pulls which takes both general equilibrium effects and all relevant data into consideration… [which would] analyze the effects of exogenous parameter changes on a market or set of markets allegedly in equilibrium" (1974: 38) – effectively the same method Johansen used.

While Leontief had to build his own model of how the economy hung together, Taylor and Black (like Johansen) used a simplified version of the national income accounts. The model set up data for 35 sectors, through a number of equations, which could be re-written into a social accounting matrix if so needed.

The model had domestic demand and the Balance of Payments (BoP) which was assumed to be in deficit. Part of that BoP contributed to tax revenue (through tariff earnings) and part of it contributed to the investment of the economy as capital inflows. These equations together, provided the basic macro economic balancing equation:

$$(I - S) + (G - T) + (X - M) = 0$$

Following that, a number of behavioral and functional forms had to be specified to explain how labor, demand, prices (in wage terms), etc would react to a change in an exogenous variable. It was a typical modeling exercise, where some exogenous shock is thrown at a system (e.g. change the tariff rate) and the number of equations equals the number of endogenous variables in the model. They solved the linear equations (inverted the matrix), and as the endogenous variables are solved according to the new exogenous variable, they found the new equilibrium, defined by the macro "pre-specified balance" of leakages and injections equaling zero (Taylor and Black 1974: 47).

This model was not a general equilibrium model in the sense of Arrow and Debreu's simultaneous market clearing conditions. Nor was it a general equilibrium model in the sense of Walras system of continuous equilibrium, and there were no references to any of these authors or their work in Taylor and Black (1974)[18]. In fact, it could include disequilibrium by definition (as there was a fixed $X < M$ in this model). The model had a set number of macro equalities, which provided the matrix of equations with a benchmark towards which to 'adjust'. That is what Johansen's model, and Taylor and Black's CGE model did. The decisions about those behavioral equations of agents, the functional forms for production, choice of exogenous (and thus endogenous) variables (one must adjust to the other), was where theory, empirics and guesswork came in.

Johansen (1960) had built a 1950 model and tested it with observed variables for elasticities and results empirically observed from throughout the 1950's. Taylor and Black relied on literature searches for elasticity approximations where "all elasticities were adjusted" (1974: 56). Time and space, as well as the fact that it was a current base year, only allowed Taylor

[18] In the interest of completeness, Taylor and Black does cite Saito (1971), who was trying to use the Arrow-Debreu system to study "interdependency of prices and outputs" (Saito 1971: 11). The framework used has nothing to do with Johansen, nor Taylor and Black, except that they both used input output matrices, and thus both had a set number of balancing equations, which did not necessarily correspond.
The reference to general equilibrium is specific to a U.S. interpretation, following Jones (1965), where 'general equilibrium' could be anything with more than two markets clearing in a model.

and Black to test their elasticities against the current years' base year data – a practice which has come to dominate elasticity testing, or 'calibration' in the jargon.

1.5 Robinson Disagrees

In 1976 Robinson wrote a paper on long term models (all the CGE literature so far, were self confessedly short run), and he stated that long run models should consider distribution and demographic changes, to such an extent that.

> Any model which attempts to address distributional questions should endogenously determine both wages and prices. Wage and price endogenous general equilibrium models (WPE models) are currently available and can be implemented.
> (Robinson 1976: 123)

These WPE models, Robinson argued, were represented by Taylor and Lysy (1977), Adelman and Robinson (1975) as well as the work of two of Robinsons Ph.D. Students: Ahmed (1974) and Dervis (1975). These models were necessarily based on the work of Taylor and Black, but had two distinguishing features. First, the focus of the model was distribution, not the explicit search for the adjustment of certain macro or micro variables. This focus is related to the World Bank which was explicitly focusing on poverty and distribution at the time, and Robinson affiliation there. These 'WPE' papers referred back to the earlier CGE models for their basic structure, indicating that it was not problematic to specify prices and wages as endogenous in the CGE model, and then choose suitable exogenous variables to set them off with (e.g. quantity and labor supply). In effect, a WPE model, was simply a CGE model where particular variables were endogenous, rather than exogenous. This choice of what is inside and outside the model is called 'closure' in the jargon.

A second, more important point to note, is that a 'WPE closure' did not include the Johansen assumption of prices being reflected as wages (to get the cross elasticities easily).

These two now became separate variables, but the ability to work with such a change was not a theoretical invention as much as an extension based on data availability and computing power, both of which had increased exponentially since Johansen's 1960 model. In particular Robinson's references to Taylor, Blitzer and Clark (1975) as basic models, went some way to solidify this very simple relationship between CGE models and 'WPE models', the former of which now had access to more computing power, and could then do WPE closures as a technical choices within the existing model structure.

As late as 2005 Deverajan and Robinson made the claim, that the "earliest CGE models of developing countries" were in Adelman & Robinson (1978) and Taylor & Lysy 1979 (Devarajan and Robinson 2002b: 421). That claim could only be substantiated, if we consciously forget the CGE work undertaken in the early and mid 1970s, or re-classify Chile, Brazil, Turkey and Bangladesh as developed nations[19]. So the myth that the CGE literature leapfrogged from Johansen 1960 to 1978 is untrue, and similarly, all these models claim the same intellectual heritage from Johansen (1960) and Taylor and Black (1974), thus not being separate or alternative models.

1.6 AGE Matters

Much of the confusion around CGE models, has happened because a second model type, called Applied General Equilibrium (AGE) models were created independently, then co-existed, developed and ultimately, I will argue, merged with the CGE models, at least at the rhetorical level. This confusion is what leads to Vellupillai's paradoxical notion of "applied CGE modeling" (2006: 361).

In the modern literature, there is no recognition that CGE and AGE models differ now, or ever did. Bergman (a prolific CGE writer) considered it a difference of merely labeling:

[19] These countries were the focus of the CGE models of Taylor and Black (1974), Taylor and Lysy (1977), Dervis (1973) and Ahmed (1974) respectively.

> Sometimes this class of numerical economic models [CGE] is called Applied General Equilibrium (AGE) models. However...the *label* "computable" seems more appropriate than the *label* "applied". (Bergman and Henrekson 2003: 1, my emphasis)

Devarajan and Robinson treat "CGE and AGE as synonymous" (2002b: 1) and similarly in training material for CGE models at Monash, Horridge (with Powell) introduced a "small and simple CGE model" (2001: 1) where "the theory underlying these equations is typical of a static AGE model" (2001: 5). When he later referred to that statement, in the same course material, he explained that "the same theory is used in every other CGE model" (Horridge with Powell 2001: 21). So the notion that AGE and CGE models are similar, is introduced to students, and exemplified in the technical literature, yet never explained, even in teaching material. As it turns out, AGE models, are a different ballgame from the CGE models of Taylor, Robinson and Johansen. More than that, the AGE literature pre-dates the CGE literature, but there is no evidence that either group found the other groups work relevant for what they were trying to accomplish. There are no cross-references between the AGE literature 1967-1975, and the CGE literature 1960-1977, and this is an important part of the story, because while CGE models were macro models, AGE models, were explicitly general equilibrium models, of the Arrow-Debreu specification.

1.6.1 AGE and Beauty…

"Scarf's research agenda of making the elegant theoretical general equilibrium models fully operational, implementable with actual data" was the first attempt at making the Arrow-Debreu model empirically useful for policy work. (Kehoe et al. 2005: 1). His aim was to provide "a general method for the explicit numerical solution of the neoclassical model" (Scarf with Hansen 1973: 1).

In order to do so, Scarf used Arrow-Debreu general equilibrium theory, to build an algorithm to find the numerical solution to an applied Arrow-Debreu model.

> Brouwer's Fixed Point theorem states that a continuous mapping of a simplex into itself has at least one fixed point. This paper describes a numerical algorithm for approximating in a sense to be explained below, a fixed point of such a mapping
> (Scarf 1967a: 1326).

Introduced with the above paragraph, Scarf presented the first algorithm for solving the Arrow-Debreu market clearing price vector (Scarf 1967a, 1967b and Scarf with Hansen 1973). His approach was based on the two fundamental theories of welfare economics, and the existence proof as well as the extended Arrow-Debreu system[20]. His reference lists read like a "who's who" of general equilibrium theory, and its mathematical properties, featuring Brouwer, Kakutani, Von Neumann, Debreu, Arrow and Uzawa. Conspicuous by their absence in the references, are any (and all) of the CGE model builders from the 1960s and '70s.

Scarfs method was an algorithm (based on simplical subdivisions) that would narrow a 'net' around the possible solution to the quantified general equilibrium problem. With enough iteration, the net was tightened sufficiently to choose a cut off point, giving a price vector that could clear the market. For example, in a very clear exposition of his method Scarf ran the "algorithm to solve an equilibrium price vector for [a] 6 man economy, [and] after 2,200 iterations a result was chosen" (Scarf 1982: 1052)

Scarf never built an AGE model, but hinted that "these novel numerical techniques might be useful in assessing consequences for the economy of a change in the economic environment" (Kehoe et al. 2005, citing Scarf 1967b). His students elaborated the Scarf algorithm into a tool box, where the price vector could be solved for any changes in policies (or exogenous shocks), giving the equilibrium 'adjustments' needed for the prices. This method was first

[20] See McKenzie (1959) and Debreu (1954) for Scarf's key theoretical background.

used by Shoven and Whalley (1972 and 1973), but was used up through the 1970s by Scarf's students and students' students[21].

Scarf's simplex method was quite appealing to the economic theorists at the time, who wanted constructive proofs for the existence of equilibrium (even if he didn't quite get there). Taylor[22] noted that as a solution algorithm, it ignored 2nd derivatives and curvature information, thus being much less effective than Newton methods for the highly convex model specifications which AGE modelers at the time were creating.

These models had a valid claim of being Arrow-Debreu general equilibrium models; however the issue of their strict computability has been disproved, as noted in section 3.4.

These models, when published, were all presented with the individual agents first, building up to the price vector and general equilibrium system, and early versions all included existence proofs. However, by the early 1980s an institutional shift took AGE (and CGE) partly out of academia, and completely into the policy arena, which meant that theory started to yield to empirical necessity. That being said, there is no doubt that the AGE models were a Arrow-Debreu micro model, completely separate in both content and approach from the CGE macro-balancing models. This was simple enough *until* the early 1980s when AGE model builders started to subtly reference CGE models.

1.6.2 Demand goes up… Theory goes down

In the late '70s and early '80s, economy wide modeling made a second inroad into the policy world, this time through the doors of the World Bank and International Monetary Fund. Chenery had by then expanded the Development Research Centre, "transforming a small

[21] A list is provided in Kehoe et al. (2005: 5): Ph.D. Students: Terje Hansen, Timothy Kehoe, Rolf Mantel, Michael Todd, Ludo van der Heyden and John Whalley, and the 'Yale core': Andrew Feltstein, Ana Matirena-Mantel, Marcus Miller, Donald Richter, Jaime Serra-Puche, John Shoven and John Spencer.

[22] In a private e-mail

group of economists into the leading center for research in economic development" (World Bank 1994: [on-line]).

By the early 1980s, CGE models were heavily ensconced as the approach of the World Bank for development analysis. Social Accounting Matrices (SAMs) were similarly a mainstay of Bank analysis, which had been adopted as a presentational device by the CGE modelers, and had come to the Bank with Graham Pyatt in the 1960s. Pyatt, a student of Richard Stone, had worked for Stone at the "*Cambridge Growth Project*" which developed the first SAM in 1962 (Stone and Brown 1962). Pyatt left Cambridge and "developed SAMs, mainly at the World Bank" (Vanoli 2001: 170), becoming together with Thorbecke, the leading proponents and developers of SAMs (see for example Pyatt and Torbecke 1976). A quick note should be added, that SAM's are simply a matrix representation of the national accounts, not a separate theoretical instrument.

The CGE model builders had been with the Bank for over a decade and were using SAMs to present and build precisely computable models when AGE model algorithms could only "be applied to [neo-]Walrasian models of a reasonable size". By 1984 that meant being able to calculate problems of a "modest size, say, involving 30 to 40 disaggregated sectors" to an *approximate* solution (Scarf and Shoven 1984: x). Note that Taylor-Black had started with 35 sectors, precisely computable in 5 minutes a decade earlier, and by the time AGE was catching up to this level of disaggregation, CGE models already had 26, 29, 44 and up to 265 sectors[23] precisely calculated even before the Scarf and Shoven (1984) publication[24].

At this point in time, as the Bank and IMF increased their use of these models, the AGE model builders *started* to take notice of the CGE literature. Scarf and Shoven, made what

[23] In Staelin (1976), Taylor et al. (1980), Adelman and Robinson (1978) and Bourgignoun et al. (1983) respectively.

[24] Even though Scarf and Shoven's book reflect a conference held in 1981, there is no evidence that the preface (which is quoted) was written before 1984, and even if it was, the Bourgignoun CGE model was first presented at the Econometric Society World Congress (Aix-en-Provence, 1980).

amounted to a sales-pitch in comparing how the two model types could track changes in the economy:

> It [an AGE model] shares this property with input-output analysis [i.e. CGE] but permits a more flexible treatment of the consumer side of the economy and is less rigid in the requirements placed on the productive side (Scarf and Shoven 1984, xi-xii)

Mansur and Whalley (1984) stated *for the first time* that the AGE model was also a derivative of Johansen's work, although they did not explain how this connection was made, or what it was, despite Johansen never having been referenced by any of the AGE literature till that date. Jorgenson later argued that the link between the two was present because both Johansen (1960) and AGE models were "estimating unknown parameters to stand in for the rate of change between K & L, and by using Linear Logarithmic production functions" (Jorgenson 1984: 140). However, where Johansen was looking to solve for those elasticities in a macro balancing model, AGE models calculated those elasticities as an intermediary step, in order to get the market clearing price vector. So the link is tangential at best, and seemed like attempts to compete with the CGE input-output model.

By the mid 1980s, AGE models stopped calculating the intermediary parameters with existence proofs and the general equilibrium model, and rather they were "solved through 'calibration' " (Jorgenson 1984: 140), meaning they tested parameters backwards using a SAM like structure, much like the Johansen and Taylor-Black method. The methodological approach that separated AGE from CGE started to disappear as empirical demands on models grew, and the CGE model starting point could be implemented much more easily than the bulky general equilibrium frame.

> Implementation of econometric models of producer behavior is very demanding in terms of data requirements. These models require the construction of consistent time series of interindustry transactions tables. By comparison, the non econometric approaches of

> Leontief and Johansen require only a single interindustry transactions table. (Jorgenson 1984: 141)

Mansur and Whalley (1984) had previously argued that the National income accounting definitions were not suitable for neo-Walrasian general equilibrium analysis. But by the mid 1980s, they had to concede that the modeling they were doing, still *called* AGE, had moved away from estimating the existence of general equilibrium, and had started using the CGE 'benchmarked' national accounting (i.e. SAM) data sets to build their models.

> [A] micro consistent equilibrium data set is constructed using *national accounts* and other sources for purpose of both making the comparison possible and provide a data base for model calibration (Mansur and Whalley 1984: 87, my emphasis).

In "*Frontiers in Applied General Equilibrium Modeling*" (Kehoe et al. 2005), there is a clear confusion of heritage as chapters by Arrow, and Devarajan and Robinson talk of CGE models (not AGE), while there is a quiet recognition that "Recent models don't check for existence [of equilibrium] either" (Kehoe, Srinivasan and Whalley 2005: 3), nor do they model the neo-Walrasian general equilibrium system. When the editors (All Scarf students) talk of AGE models, they refer to an 'earlier' period of time which had "heavy interaction with the general equilibrium theory of Arrow, Debreu, McKenzie and Scarf" (ibid: 4) as opposed to modern AGE models.

> The technique of AGE modeling – namely calibrating and benchmarking observed data on economies into and initial equilibrium data set and then doing counterfactual policy analysis – have spread. (Kehoe, Srinivasan and Whalley 2005: 1-2)

What the AGE model builders do not want to admit, but have effectively said here, is that their method was subsumed into a matrix form of the national accounts in the early 1980s. Once that happened, their model ceased to be based on the micro foundations found in

Arrow-Debreu, and were controlled by the macro balancing equations, which in turn controlled the national accounts and CGE models. The 'techniques' of modern AGE modeling as promoted by Kehoe, Srinivasan and Whalley, are those used by the BLS in 1945, Johansen in 1960 and Taylor and Black in 1974. It is not what Scarf did, and in fact all AGE models post 1985 have been CGE models in all but name.

The early 1980's are notable for another change in the World Bank, namely the ideological shift that occurred with the promotion of Anne Krueger in particular. The Bank wanted to take a much more neo-classically rigorous approach, but could not separate itself from the CGE models, that had originated from planners such as Hollis Chenery and Leif Johansen (a card carrying communist at that). It could be that the administration realized the potential of grafting the AGE language and theoretical background onto the CGE models, to give them some micro-economic camouflage, because that was exactly what happened.

The AGE model was subsumed into the CGE structure, to the extent that it brought its language and presentational approach only. The model being practiced post 1985 was purely a CGE model, in the Johansen, Taylor-Black tradition. After 1985 published papers on CGE (and AGE) models started with the behavioral equations between agents and institutions and their respective indifference curves and isoquants, rather than macro accounting, which had introduced earlier CGE models. These behavioral equations were usually of the simplest micro economic nature resembling the basics of general equilibrium theory. One should add that these had been a part of some of the World Banks distribution CGE models, but suddenly CGE models introduced a rational agent, *before* tying him to a macro balancing equation – this was the cosmetic change that AGE absorption brought. This of course does not make a micro economic Arrow-Debreu general equilibrium model, but the rhetoric and confusion was enough to plant the ultimately false myth that CGE models became micro models sometime in the 1980s (not that anyone could, or have ever, substantiated this claim).

1.7 Lurch anyone?

Robinson-Adelman (1978) is continuously pushed as being the first CGE models, as in this recent interview of Irma Adelman for *Feminist Economics*:

> My work with Sherman Robinson (1978) pioneered the use of computable general equilibrium models rather than single-equation regressions, in policy analysis of developing countries. (Adelman, interviewed by Grossbard-Shechtman and Gagnier. 2002: 110)

The nicest interpretation would be that the book published in 1978, kick-started the mainstreaming of CGE models in official development institutions. However these recurring claims to originality and specialization notwithstanding, Adelman and Robinson (1978) were by no means a first, in the core modeling sense.

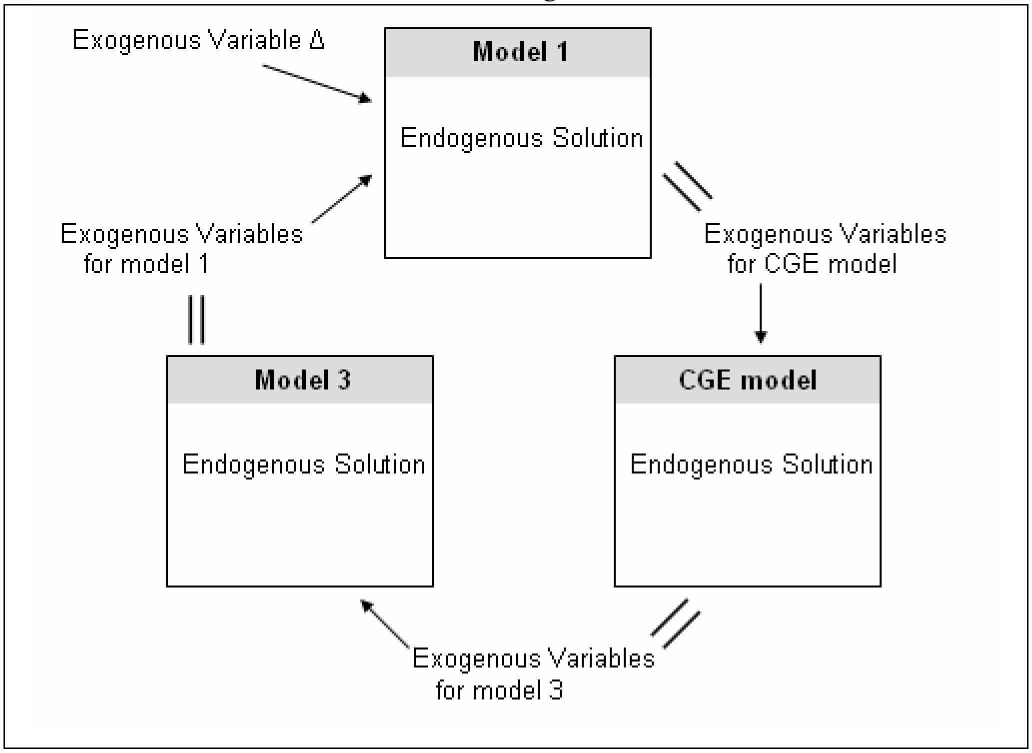
The 1978 model was innovative in one particular respect: It was set up as three separate models (of which one was a CGE model), each solved one after the other. Of these models, they argued that "the pervasive importance for distribution of relative factor prices and product prices led us to formulate a wage- and price-endogenous CGE model" (Adelman and Robinson: 1978: 8). As already argued, these WPE models have quite a few forerunners, and here Adelman and Robinson themselves classify WPE as a particular subset of CGE models. So in fact, they are building a CGE model, which will be placed between the two other models.

This idea of having a 'chain of models' where one a set of exogenous variables would be endogenous further down the chain, was formulated in Robinson (1976) and described in Adelman and Robinson (1978). It was described in both publications as "neither full neoclassical equilibrium nor full intertemporal equilibrium" (Robinson 1976: 125)[25]. Rather

[25] In Adelman and Robinson (1978) these models are defined as "neither a full neoclassical general-equilibrium model nor a pure disequilibrium or partial equilibrium model" (1978: 9)

it is defined as a "lurching equilibrium" model (Adelman and Robinson: 1976: 9). Consider figure 1.

FIGURE 1: *'Lurching' CGE models*



Based on Adelman and Robinson 1976

The 'Dynamics' in this chain of models, consists of an initial exogenous shock to 'Model 1', and by solving a static or temporary equilibrium model, one gets an endogenous solution. The variables for this solution equal the exogenous variables required for the second link in the chain (here, the CGE model), and they are then fed into the CGE model, which again is solved as a static model. These solutions are equivalent to the variables needed for 'Model 3', which again is solved, providing the new variables for static model number 1, thus completing the circle.

'Dynamics' are reduced to solving one static model with another *ad infinitum*. This process is repeated until enough 'short runs' have been added together for the model-builder to call it a 'long run' model. Later innovation then added an exogenous time-trend to be added for each iteration, but the whole result depends on the initial change which sets off the cycle. This was an innovation at the time, in the context of CGE models, and the method persists today in the major CGE modeling institutions, but there was nothing new about the CGE model which was based on the Johansen, Taylor-Black approach.

1.7.1 Rejecting the second coming

Bourgignon et al. (1980) in their CGE model, briefly surveyed Adelman and Robinson (1978), and considered it a typical equilibrium model, with some quirks.

> Their [Adelmand and Robinson] model is essentially a sequence of temporary equilibria mainly defined by a fixed structure of sectoral capital stocks and some exogenous parameters (Bourgignoun et al. 1980: 22-3).

Adelman and Robinson listed 5 criteria that defined and distinguished their model from other CGE models (1978: 3). These boiled down to: 1. No Johansen-Frisch assumption (meaning they were building a WPE model). 2. Choosing particular behavioral equations. 3. Adding more endogenous (and exogenous) variables. 4. Particular closure decisions and 5. dynamics. All of these were within the scope of the standard CGE model except the dynamics. However, seeing as dynamics are in fact just solving one model into the next, of which the CGE components are the same as previous models, there was no fundamental different model class in Adelman and Robinson (1978). What Ezaki (2006) identifies as a 'third' type of CGE models (beyond CGE and AGE), is in fact nothing but the same CGE model as before, but placed in a chain between other models.

Adelman and Robinson were the first to link CGE models in a chain, and this idea has become very influential since. Another thing they also introduced, was their cross referencing to both their own intellectual heritage (CGE), and that of Scarf (AGE) although they chose not to apply the AGE methodology at any point (1978: 10). Finally, they took the rhetorical strategy of AGE papers, and began their book with describing the choices of agents (for model 1) leading up to the 'equilibrium' solution, which in fact was already given by the macro balancing equations in the CGE specification. This tradition continues today, as model builders hide the macro equations at the end, and pretend that rational micro behavior has led to a 'general equilibrium' outcome.

1.8 The Developments that followed

The period since 1986 has been relatively theoretically uneventful for the CGE model, although new applications have been found. The number of model builders and models grew, through focused classes and institutional demand, but the models themselves have only changed in so far as they have been attached to new model chains (*a lá* Robinson and Adelman).

> Computable general equilibrium (CGE) models have become the standard tool for the analysis of the economy-wide impacts of greenhouse gas abatement policies on resource allocation and the associated applications for incomes of economic agents… The main reason for this is that the general equilibrium framework represents price-dependent market interactions as well as the origination and spending of income for various economic agents based on rigorous microeconomic theory. (Böhringer et al. 2006: 407)

The notion that microeconomic theory still runs the CGE model (as they did in AGE models) is the popular version taught at the few institutions specializing in this type of model building. However CGE models are *used* because they solve cross sectors systems serving as the "aggregate top-down macroeconomic models" of these chains (Schäfer and Jacoby 2006: 171). Not because of their supposed theoretical rigor. The foundations of these models are not microeconomics (or AGE models), but the typical CGE model structure, and the accounting consistency of a SAM.

The World Bank's most recent addition to CGE literature was the Poverty Reduction Strategy Papers (PRSP), where the CGE model solved the relative prices and wages in a chain (Devarajan et al. 2002). The model ran from household data (and rational decisions) to the CGE model. The solutions from the CGE model then fed into 2 different models (one long term, one short term) and finally this result fed into the Financial Programming model. These together form the theoretical 'core' of PRSP packages. This still depends on the traditional macro balancing CGE model.

Strictly on CGE model development, the changes that have occurred since 1986 are best summed up by comparing Robinson's (1986) e*ssential* components of CGE models, and Essama-Nssah's (2006) World Bank lecture notes of the key components of an 'AGE' model. The latter was used at a recent internal CGE training course at the Bank, aimed at "any interested World Bank/IMF staff, in particular, economists and analysts dealing with the issues of poverty and distributional impact of economic policies" (World Bank 2006).

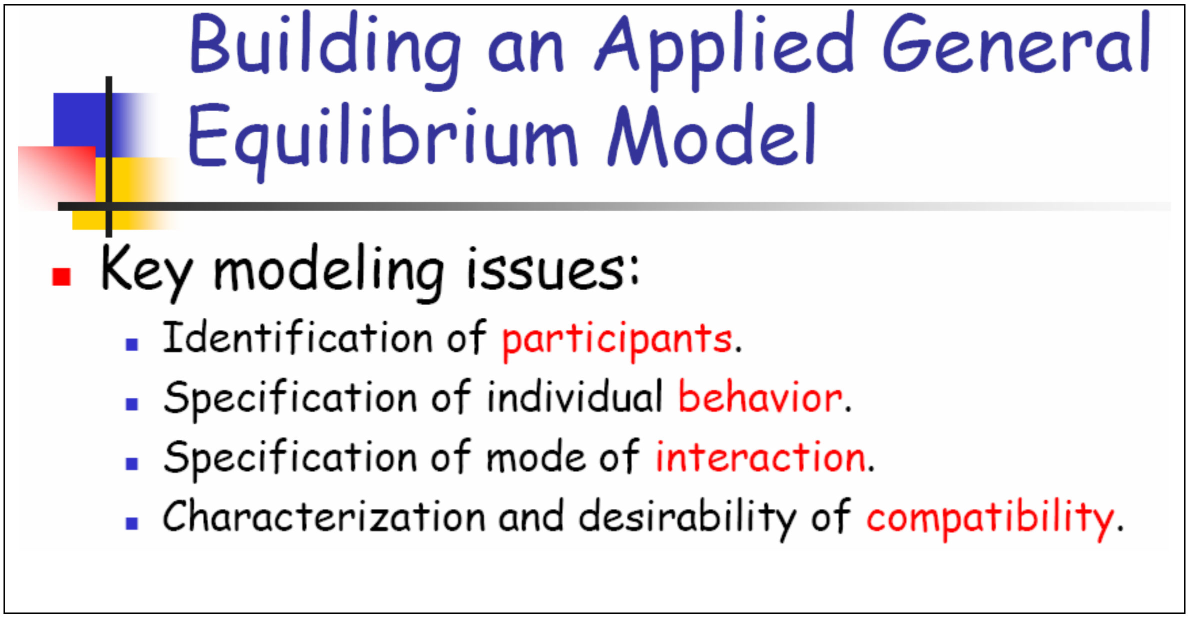
FIGURE 2: *Robinson 1986 CGE Core*

Robinson (1986, pp. 33–4), the essential components of the neo-classical approach to CGE modelling can be described as follows:

1. specification of the representative agents whose behaviour is to be analysed,
2. identification of their behavioural rules and conditions under which they operate (for example, profit maximization behaviour of producers and utility maximization behaviour of consumers),
3. specification of the signals which are used by the agents for their decisions (for example, prices are important signals in a neo-classical CGE model), and
4. identification of the 'rules of the game' (for example, assuming perfect-competition, a CGE model allows each agent to act as a price taker).

(Bandara 1991: 9, citing Robinson 1986)

FIGURE 3: *World Bank 2006 CGE core*



(Essama-Nssah 2006: 6)

Barring the change in color scheme and font, the two lists of key components are effectively equivalent, and illustrate exactly what happened in 20 years. 'AGE' and 'CGE' are now substitutes in model rhetoric, despite the fact that no AGE models (in the micro oriented tradition of Scarf) have been built since the mid 1990s, and none have been in evidence in explicit policy work since the early 1980s. The teaching of CGE models has moved from academia to a few specialized institutions of which the World Bank is one, and the active work of the *model builders* and these institutions, has ensconced CGE models in high profile policy debates, such as poverty reduction analysis.

2. The Current CGE Community

CGE models went from being a one-man project in 1960 to dominating development planning and trade analysis, and it has done so through its network of high profile promoters, and an increasing appeal to rhetoric over substance in its theoretical and historical foundations, as the opportunity cost of asking these questions in classes has sky-rocketed, and the instructors and model builders publish the details they want to.

Any consistent study of CGE models will, as the above has shown, be able to separate CGE from AGE, and thus input-output -and social accounting matrices from Arrow-Debreu general equilibrium.

While it is in the interest of model builders to promote their results, institutions and training courses, there is no incentive for them to offer a clear picture of who the CGE community is, how they work and most critically, their own history as well as that of the CGE model, and its inner workings. Only through an understanding of who does CGE modeling, why it has become such a high-profile model, and what a $5,000 USD week-long training course can teach its few participants, can we start to see that they are still teaching macro-balancing models, and have always done so, only with a rhetorical hand wave at Arrow and Debreu.

2.1 High Profile Policy makers

As noted in the history, Robinson, Chenery, Taylor and Scarf's influence on their employers and students started the CGE and AGE drive. However it was the events of the 1980s which drove the point home as successive vice presidents at the World Bank became involved in CGE models, while national governments adopted the approach for forming policy advice.

The whole story of who's who in the CGE policy world would be long and tedious, so the aim here is to give a flavor of the insular quality of the 'inside' community who have both influenced policy and taught a new generation of model builders.

A good selection of model builders were found at the 1981 conference on Applied General Equilibrium analysis in San Diego, which highlighted the growing importance of CGE models over AGE models in practice, if not in rhetoric. A host of familiar people and institutions, including Peter B. Dixon, Andrew Feltenstein (IMF), Dale W. Jorgenson, Ahsan Mahsur (IMF), Sherman Robinson (WB), Herbert E. Scarf, Jaime Serra-Puche, John B. Shoven, Laura Tyson and John Whalley (Scarf and Shoven 1984). A similar conference in 2002 on the "*Frontiers in AGE modeling*" had more or less the same attendance, with new institutional representation: Timothy Kehoe, Shantayan Devarajan (WB), Sherman Robinson (IFPRI), Anne Krueger (IMF), Jaime Serra Puche, Dale Jorgenson, François Bourguignon, T.N. Srinivasan, John Whalley, L. Abrego (IMF), John Shoven and Herbert Scarf (Cowles Foundation 2002).

As for the people, and institutions, they are a relatively close knit group despite their international flavor. The World Bank has been very active in this modeling type, and Srinivasan, Bell, de Melo and Robinson all started their modeling careers with Chenery at the World Bank's DRC[26], while a host of others were the students of Scarf and their collaborators[27]. Anne Krueger became the Vice President for Economic Research at the World Bank[28] during the CGE modeling heyday and oversaw the grafting on of AGE rhetoric. Towards the end of Chenery's time at the DRC, Robinson became the Division Chief of Development Strategy, but his tenure was outlasted by both Srinivasan and de Melo.

Towards the end of the 1980s when the NAFTA trade agreement came up for debate in the U.S. congress, CGE models were the mainstay of arguments from all sides, best exemplified

[26] The Development Research Center employed Srinivasan 1977-present (consultant, then special adviser); Bell: 1974-85 (as staff economist, then senior economist); de Melo 1980-93 (Economist [Research dept.], then Division Chief, Trade Policy Division) and Robinson 1977-83 (Economist, Senior Economist and Division Chief).

[27] Whalley, Yeung, Bell and Srinivasan all of whom it should be noted were 'academically related' to Scarf or co-wrote with his 'academic relations' at the World Bank in the 1980s: Srinivasan graduated from Yale in 1962, and was re-hired as faculty in 1980. He has been an advisor with the World Banks Development Research Center (DRC) since 1977 and co-edited an AGE book in memory of Scarf in 2005 with Kehoe and Whalley (Kehoe et. al., 2003), Yeung co-wrote his first journal publication with Whalley on AGE models (Whalley and Yeung, 1984) and Bell published with Srinivasan during his time at the World Bank DRC 1974-85 (Bell and Srinivasan 1982), in a book co-edited by Taylor – who is arguably 'related' to a completely different branch of the CGE family tree.

[28] Krueger later went to Stanford for a decade before she joined the IMF as the Deputy Managing Director.

by the Mexican delegations senior negotiator (and treaty signatory), Jamie Serra-Puche, a member of the CGE community. Laura Tyson taught at Princeton with Robinson in the seventies, and in the 90s she held the two top economics positions in the American Administration, as negotiations to further open international trade continued[29]. It is the placements and connections between these people and other CGE model builders which pushed the model into becoming the mainstream method that it is today. The people who eventually got the top positions have further promoted the model and expanded (and founded) a number of institutions working on CGE projects.

2.2 High Profile Institutions

Bourguignon became the chief economist of the World Bank in 2003, and now presides over the largest CGE modeling team internally. Externally the Bank was the first 'consortium' member of the Global Trade and Analysis Project (GTAP), the biggest trade database built around a SAM CGE model.

GTAP itself was started by Dr. Tom Hertel at Perdue University (GTAP 2007), but was based around the work of Alan Powell and Peter B. Dixon who were involved with the Australian CGE modeling databases (ORANI and IMPACT) at the Center of Policy Studies (CoPS) at Monash University[30]. These two institutions, Monash and GTAP, have been closely linked ever since, circulating staff and management between themselves, and the Bank.

Robinson was the Director of the Trade and Macroeconomics Division at International Food Policy Research Institute (IFPRI) from 1993-2003. They have since become great promoters of the CGE method, and the institution is closely linked with the World Bank, through cross publication and staff 'trades'.

[29] 1993-95 Chairman of the President's Council of Economic Advisors,
1995-96 Director of the National Economic Council.

[30] For a good history of the Australian experience, see Dixon (2006), or for models of the Australian Economy see Powell and Snape (1993).

There is an explicit pattern of an inside community, which has held firm since the early 1980s. Part of the reason why the model history has been so clouded, is arguably that everyone has wanted to claim some form of originality and expertise in what became a very prestigious field. Further, learning about CGE models has become centralized with the institutions that are run by this community, and this is a very lucrative business.

2.3 An Oligopoly of Three

The World Bank, the Center of Policy Studies (CoPS) at Monash and IFPRI constitute the 'big three' who train and hire the majority of CGE model builders today. We all know that two things result from a collusive oligopoly: High prices and low supply. In that spirit, GTAP and IMPACT (Both CGE modeling projects with the World Bank and CoPS respectively) went from being open source and free, to charging money for the software and database updates while limiting spaces at expensive annual teaching workshops.

The institutions see it differently, namely that when the price was zero, demand was infinity and:

> Researchers did not take the Data Base seriously. This was the first of many failed experiments from which the GTAP staff learned valuable lessons. The version 2 GTAP Data Base carried a healthy price! (GTAP 2007 [on-line])

So to assert that they were serious, the cost of attending a one week CGE workshop, is generally above the average academic budget, that is assuming one gets a place at these heavily over-subscribed seminars (Monash has 30 places, while GTAP has 32 places annually). None-the-less academics usually form a third of the attendance[31]. The General Economic Modeling Network (GEMN) is an organization aimed at providing training in

[31] For example at the GTAP 14th Annual Short Course (2006), there were 13 academics, 9 Government employees, 4 UN representatives, 1 World Bank, 1 IMF and 4 'other' representatives (GTAP 2007d)

basic and advanced CGE models, and is made up of 'second generation' CGE model builders.

FIGURE 5: *Cost of one week course at different institutions:*

All USD	GTAP	GEMN	IFPRI†	MONASH*
Anyone	$ 3,495	$ 2,760	$ 2,500	
Academics				$ 2,825
Non-Academic				$ 2,575
Australian Academic				$ 3,150
Australian Non-academic				$ 3,740

Source: GTAP 2007b, GEMN 2007, IFPRI 2007, CoPS 2007.
† No IFPRI classes on CGE upcoming, so reference price to upcoming 7 day investment course.
*Converted Australian dollar to USD at 1:0.83 and rounded.

All the courses are taught using one of the specialized CGE computing packages, which have been developed explicitly to construct and solve models. These again come with a price tag, although GEMN and Monash provide two month licenses for the software with the course. GEMPACK is made and sold by Monash, while a package called GAMS is a more general mathematical package used for modeling as well. GTAP does not have a CGE modeling tool, but sells access to their database and its updates. So once the course is over, and two months pass, each participant needs to invest in one (or two) of the following:

FIGURE 6: *Software package prices:*

All $ USD	GEM PACK*		GAMS	GAMS solver	
	New	*Update*		*Min.*	*Max.*
Single Academic	$ 2,660	N/A	$ 640	$ 320	$ 1,920
Multiple Academic	$ 4,000	$ 2,570			
Non- Academic			$ 3,200	$ 1,200	$ 9,000
Government	$ 5,950	$ 4,814			
Non-Academic & Non-Government	$ 6,480	$ 5,270			

Source: CoPS 2007b, GAMS Development Corporation 2007 and 2007b.
*Converted Australian dollar to USD at 1:0.83 and rounded.

FIGURE 7: *GTAP Database Prices:*

GTAP: *All $ USD*	Standard Price		Lower-Mid Y**		Low Y**	
	New	*Update*	*New*	*Update*	*New*	*Update*
Single Academic	$ 4,600	$ 2,300	$ 2,760	$ 1,740	$ 1,840	$ 1,160
Multiple Academic	$ 1,700	$ 850	$1,020	$ 510	$ 640	$ 340
Non- Academic	$ 900	$ 450	$ 540	$ 270	$ 360	$ 180

Source: GTAP 2007c.
**Discounted prices for clients from countries in these income categories; World Bank classifications.

The shortage of course availability and their high prices, coupled with rising demand over the last two decades has made the CGE world a multi million dollar business. Monash claim that at least 400 separate clients use GEMPACK (CoPS 2007c), and assuming those are split evenly between academic, government and other institutions, the revenues from annual updates exceed $1,500,000 on this one piece of software alone. This financial motive, I would argue (despite the non-academic flavor), is a further reason why CGE model structure and information cannot be easily obtained, and allusions to originality or theoretical robustness are so widespread, as institutions fight to maintain their market share.

2.4 What does $5,000 teach an academic?

The minimum expense for a course and software would run to $5,000 for a single academic, so one would assume that these intensive courses would cover all the ground. Despite this, prior knowledge of General Equilibrium *theory* is generally not a pre-requisite, and "previous hands-on experience in solving GE models is not required… [although] a burning desire to learn how to do CGE modeling" would be useful (CoPS 2007 [on-line]). As long as the desire is to *do* CGE modeling, and not learn what CGE models *are* or what general equilibrium *is*, the courses are in fact very good[32].

The courses focus on teaching participants how to use software to set up, modify, and ultimately solve CGE 'problem sets'. 'Students' are first taught that CGE models are micro based models of some sort. The World Bank course, introduces "CGE-Micro-Simulation" models where you create a "household model of expenditure, or income generation to allow a rich analysis of poverty and inequality" (Essama-Nssah 2006b: 10). Then the synonymous nature of AGE and CGE models is introduced, as if it held true in the 1970s and still did today. Examples like the ones in *figure* 3, section 1.8.1, where 'AGE' models are being built

[32] This survey is based on the Monash CGE Training Package (CoPS 2001), The World Bank internal course materials (World Bank 2006), and the GEMN starter course outline (GEMN 2007b)

at the World Bank. Or the earlier example from 1.6 where Horridge and Powell (2001) material from Monash opens on page 1 with CGE models, and by page 5 is building AGE models, is the norm, not the exception.

This micro base idea is not developed in any form of detail, and the material quickly progresses to SAM's at the World Bank (as in King 1985) or the programming language itself at Monash (Horridge 2001). The computer software takes up a majority of class time, where "all the sessions include personal work in the computer lab" (GEMN 2007 [on-line]). Consistent for all these courses is the need for students to grasp the programming language in relation to the models functions (Horridge with Powell 2001).

By this point, most courses are deep in programming languages and the SAM, as the myths of CGE model 'roots' are implicitly assumed, and never developed. The material then introduces the choices which have to be made in regards to all the functional relationships between sectors, agents, goods, prices etc. in suitable terms, for example where:

> The same functional form applies in all cases: derived from the Constant-Elasticity-of-Substitution (CES) production function, which is very widely used in CGE modeling (Horridge with Powell 2001: 11)

One consistent streak across the courses is the inherent use of simple micro related equations to describe a majority of relationships between different parts of the economy. This does not make a macro-balancing-model approach general equilibrium theory, but that point is never made.

The focus on *doing* rather than *understanding* is hammered home at the World Bank, where a model to analyze poverty and distribution, makes the "assumption of full employment of all primary factors of production" (Essama-Nssah 2006: 19) because the "*Full employment* assumption means real GDP can be made exogenous" (ibid: 28, her emphasis). Here the focus on getting the 'right' exogenous variables means that the model specification being taught will have no unemployment. On closer inspection, the assumptions of behavior and competition mean that factors of production receive all the value they produce, in one wage labor market. Individual agents are the producers thanks to perfect competition (and not some macro sector or monopoly institution). So the model framework set out to analyze *poverty* and *inequality*, assumes (due to its behavioral and closure rules) that everyone working earns the same wage, and everyone is working. So the basic reasons for people living in poverty and inequality, which is obviously unemployment and unequal pay, is effectively modeled away in the equations which take precedence over relevance[33]. Finally, of course, the 'micro' model is solved in a SAM using the macro balancing equations, in the same manner that Johansen approached his model, making it a very macro model.

The CGE model has not changed from being a macro balancing model, set up in a matrix form of the national account. Despite this, the myth that CGE models are somehow micro models persist even in the most expensive educational programs, as the macro nature is hidden in equations. There is no doubt that second and third generation *model builders* firmly believe that CGE models are General Equilibrium models, and that General Equilibrium means Walras and Arrow-Debreu. They believe this, because they were taught that 'of course' there is a Walrasian relation, and you are not doing macro balancing.

2.5 Walrasian models

Robinson (2003) argues (repeatedly) that there is a "Walrasian paradigm underlying the CGE model" (2001: 9, 19)[34]. Bandara (1991) does not give an opinion on this, but he quotes Shoven and Whalley's ambition of "converting the Walrasian general equilibrium structure (formalized in the 1950s by Kenneth Arrow, Gerard Debreu and others) from an

---

[33] This type of model assumptions has been noted previously for the World Banks PRSP models (Fine and Hailu 2002), but not for the PAMS models which is what we are looking at here. Granted this is 'only' the teaching material, but the critique of the PRSP models refer to the published guidelines on model construction.

[34] There are 12 references in the text explicitly linking Walras with CGE models ranging from CGE being "theoretically grounded in Walrasian general equilibrium theory" (p.1) to the repeated notion that CGE models should properly be referred to as the "Walrasian CGE model" (p. 5, 9, 10, 19).

abstract representation of the economy into realistic models of actual economies" (Shoven and Whalley 1984: 1007, in Bandara 1991: 5). By doing so Bandara separates himself from the notion that CGE is explicitly Walrasian, and places it within the "neoclassical general equilibrium approach" (1991: 12).

Following the historical discussion, we can agree with Bandara that Shoven and Whalley set out to build Arrow-Debreu type models, but we must remind ourselves (and Bandara), that these were AGE model, and not the CGE models of the post 1985 world, which he is actually surveying. A popular term today is the separation of 'Walrasian' and 'non-Walrasian' CGE models, which is simply long hand for pre-1985 AGE models (supposedly Walrasian) and everything since then classified as 'non-Walrasian'.

Walras's name is a very valuable *rhetorical* tool, especially in the CGE literature, but more than that, he is frequently misrepresented and used as an excuse for lack of sound theoretical reasoning. "We do what economists have always done" (Taylor and Black, 1974: 39) is an often heard refrain in CGE models, and as such we need to address Walras in some more detail.

3. General Equilibrium and CGE models

The idea that "Computable General Equilibrium modeling is an attempt to use general equilibrium theory as an operational tool" is a widely distributed myth in the CGE community (Bergman and Henrekson 2003: 1). CGE is commonly associated with Walras and the Arrow-Debreu general equilibrium, especially by those who where taught modeling after the mid 1980s (i.e. with no first-hand knowledge of the models development). This section is an attempt to logically prove that this association is wrong, and further illustrate that Walras does not carry over into the early AGE models, and thus not the neo-Walrasian general equilibrium models either. Furthermore it will be shown that pre 1985 AGE models and CGE models are solved in completely different ways, negating *any* claim of transferring Arrow-Debreu from AGE to CGE models.

A word of warning should be inserted here, as this section when referring to Walras will dive into some controversial waters, which is unavoidable. Even his translator, William Jaffé, changed his own interpretation of Walras's work from the time of his translation in 1954 to his publication of two articles in the early eighties[35], and there has been a long and problematic history of reading Walras ever since the 1920's when:

> The few economists of that period, who were conversant with Walras's theory, were so strongly conditioned by the then prevailing "stationary-equilibrium approach" as to read even Walras (or Pareto's) theory as a stationary-equilibrium theory. Such erroneous belief was further strengthened by the fact that, since the `20s, direct reading of the Elements was almost completely abandoned (even by academic economists), being replaced by the easier reading of the simplified model put forward by Cassel. (Donzelli 1989: 45-47, cited in De Vroey 2002: 411)

The same could be said today, as the few readers of Walras tend to read that which they think corresponds with the modern notion of general equilibrium theory, which is not the same as that of the 1920's. The English translation of Walras's book was only available in 1954, and the tendency today is to read some simplified Arrow-Debreu, and:

> Like most celebrated classics, the *Eléments* is more often cited than read.
>
> (Jaffé in Walras 1926 [1954]: 5)

Because of this, much misrepresentation takes place in general equilibrium theory, however well intended and I will try to untangle some of these issues to show the difference from Walras to Arrow-Debreu to AGE models through CGE models.

[35] For an overview of his original views see Jaffé (1967), or for the later view see Jaffé (1980, 1981)

3.1 All the things Walras didn't say

The translated version of *Pure Economics* was the definitive edition (fifth version, with notes on previous edits), and there is widespread debate on whether this was a good choice for reference, as the two latest editions were finalized in the late 1890's, and the revisions made were small in quantity but big on confusions. While Walker (1999) amply illustrates the deteriorating health and output of Walras in these later years, there is grave disagreement on which parts, and which versions, of "*Pure Economics*" should be read to understand what Walras was trying to say. There is agreement across the board however, that while Walras may have meant well in trying to revise parts of the book at this stage, it "does not mean, of course, that the last 1900 version of his general equilibrium model is perfectly coherent; quite the contrary" (Bridel 1996: 233), unlike the earlier versions, or the earlier chapters[36] which were coherent, and remained untouched throughout the editions.

Some items which are taken for granted in today's framework, were innovations of the 1950's and 60's and were never part of Walras's model, nor his writings. They have simply become 'Walrasian' for the very strong authority which his name invokes in economics.

3.1.1 Uzawa's Auctioneer

> Let it be accepted that Walras failed to provide an explicit complete account of the institutional hypothesis underlying his mathematical models. The standard view is that this lacunae had been filled up by the auctioneer hypothesis (De Vroey 1999: 426)

While this is the generally accepted view, we can't neglect to note that "the auctioneer hypothesis is not explicitly made by Walras" (De Vroey 1999: 427). In fact, as will be shown, the auctioneer is not necessary for Walras's general equilibrium except in some of those later editions' late chapters which were not coherent. One could frankly rename 'Walras's Auctioneer' to 'Uzawa's Auctioneer' as his modern form is only formulated in the 1950s as Uzawa chose to:

---

[36] 'lessons' in Walras

> Interpret the competitive exchange economy as a game which R individuals and a fictitious player, say a Secretary of Market, play according to the following rules : (i) Secretary of Market announces a price vector. (Uzawa 1960: 184)

There is no need for the auctioneer in the original "*Pure Economics*". He is a logical extension to a problem caused in later versions of the last chapters. Calling it Walras's Auctioneer is akin to giving Henry Ford credit for the automatic gear change, which may be a logical extension of his original design, but it was hardly his idea…

3.1.2 Dorfman-Samuelson-and-Solow's Law

'Walras's Law' was the subject of some controversy in the 1940's-1960's, with some authors claiming that it was in fact superfluous in models (Klappholz and Mishan 1962). Lange (1942) named and created Walras's Law "because Walras was the first to recognize its fundamental importance in the formulation of the mathematical theory of prices" (Lange 1942, quoted in Mishan 1963: 62).

But whereas we today think of Walras's Law in terms of 'when all markets less one clears, then the final market must also clear', this was not Lange's reading. He defined Walras's law as: Money supply plus goods supply is identical to money demand plus goods demand[37], based on the notion that Walras had "proved the theorem that if demand equals supply for n – 1 commodities, it does so also for the *n*th commodity" (Lange 1942: 51).

Dorfman, Samuelson and Solow (1958) used the latter quote to reinterpret Walras's Law, and showed that over a given period, the value of all goods bought is identical to the value of all factor payments. Mishan (1963) shows that while Walras's proof does supports both the interpretations, it does so on the pre-requisite that all other markets have to clear individually *before* the final equality holds, and it would only do so for a long run equilibrium. What we

[37] In symbols $M_D + AD \equiv M_S + AS$ or rather: $AD - AS \equiv M_D - M_S$

today refer to as 'Walras Law' is in fact an abstraction away from Walras's goods market clearing system, to one of linear programming identities as promoted in 1958, and could reasonably be called 'Dorfman, Samuelson and Solow's Law' – this is admittedly less catchy.

3.2 Tâtonne*what*?

Turning to general Equilibrium theory, there is one particular difference between Walras and the neo-Walrasian in the way that *tâtonnement* works and the equilibrium is 'reached'. AGE models rely explicitly on the existence proof of Arrow-Debreu general equilibrium, and the way in which those markets 'work' in the neo-Walrasian system. Therefore, if the Walrasian and neo-Walrasian are fundamentally different in the way equilibrium is achieved, Walras himself has no role in AGE models, except maybe as the academic grandfather.

I will refer to Walras's own writings in assessing his use of *tâtonnement* and the notions of equilibrium, as even Jaffé had issues with Walras's definition. Jaffé argued against it following his contemporary (1940-55) understanding within the economics establishment of what *tâtonnement* should be. Jaffé only finalized his translation in 1954, and referred to Stigler's (1941) book on production and distribution theories, for an alternative translation of the term and its use[38].

Jaffé also refers to "a discussion of Walras theory of *tâtonnement*" in Goodwin (1951) (Jaffé in Walras 1926 [1954]: 520). Goodwin however is already starting to re-adjust Walras's own ideas to fit within a context of 'mechanical economics', where the movement towards equilibrium could be likened to:

[38] Stigler in turn openly admitted his possible errors in interpreting Walras, as he had not studied this particular author as closely as others "On the side of intensity, all known and available works of each economist have been consulted with two exceptions. The first exception is Walras, and is due to Professor Jaffé's impending though as yet unavailable varioum translation of the *Eléments*." (Stigler 1941: 10)

> Modern automatic control devices (servomechanics) [that] work by error control, e.g. guided missiles, automatic pilots, anti-aircraft fire control, thermostatic heat controls etc. (Goodwin 1951: 3)

Goodwin, worked along the lines of his contemporaries (also in 1940-1955) and took Walras's concepts and made them "pass from trial and error solution (which does not necessarily actually take place), to a dynamical process which does happen"[39] (Goodwin 1951: 6).

The notion of the market economy with a price vector announced prior to finding the equilibrium does *not* – as opposed to popular myth has it - correspond to the Walrasian economy. It is a modern invention, created through selective readings of Walras required to build the existence proof for the Arrow-Debreu framework, which AGE models eventually came to build on. As such I contend that AGE models are not Walrasian, but Neo-Walrasian, and the Neo-Walrasian and Walrasian models are very different.

3.3 Walras, the whole of Walras, and nothing but Walras

Walras, in his first model[40], where he introduces *tâtonnement*, later extrapolates from the simple case to the m goods case, in order to explain the full implications of his theory. Using that logic, if the behavior of the simple exchange case is understood, the theoretical notions applied in the 'general' market can also be understood. This opens Walras's 12th lesson on "The general formula of the mathematical solution of the problem of exchange of several commodities for one another. The law of the establishment of commodity prices" (Walras 1926 [1954]: 164)[41].

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[39] Rewrote 'passing' to 'pass' to keep flow of sentence.

[40] which remains intact throughout all the books , except for some small textual additions which were made in the second edition, and they are pointed out in the quotes.

[41]All the references in this section are to Walras 1926 'edition définitive' of the *Elements of Pure Economics*, as translated by Jaffé, and all emphasis in quotes are Walras's own.

> In the case of the exchange of any number of commodities for one another, as in the case of exchange of two commodities for each other, the individual effective demand equations are mathematically determined by the condition of maximum satisfaction of wants (1926 [1954]: 164).

So by extension, as no change in behavior for the market or agents is noted from the simple (2 good) to the m-goods case, we can progress to the two good scenario:

> Value in exchange, when left to itself, arises spontaneously in the market as a result of competition. As buyers, traders make their *demands* by *outbidding* each other. As sellers, traders make their *offers* by *underbidding* each other. The coming together of buyers and sellers then results in giving commodities certain values in exchange, sometimes rising, sometimes falling, sometimes stationary" (1926 [1954]: 83, his emphasis)
>
> The markets which are best organized from the competitive standpoint are those in which purchase and sales are made by auction, through the instrumentality of stockbrokers, commercial brokers or criers acting as agents who centralize transactions in such a way that the terms of every exchange are openly announced and an opportunity is given to sellers to lower their prices, and to buyers to raise their bids (1926 [1954]: 84).

Notice that the price came from the coming together of buyer and seller – i.e. exchange – and the terms of each trade (the price and quantity) are only announced *post facto* to the rest of the market. It is the explicit act of buying and selling, not the actions of an auctioneer nor the hypothetical musings of traders, which is setting prices.

Walras proceeds to give an explicit example of a "well-organized market" (1926 [1954]: 84) like the stock exchanges of London or Paris, where the somewhat misunderstood neo-Walrasian notion of 'no trading outside equilibrium' can be traced to. In the market for '3 percent *Rentes*'[42] trading at 60 Francs he sets a situation where:

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[42] A form of bond yielding 3% return

> Each broker, on either the buying or selling side, finds another broker with an exactly and equivalent counter-proposal to buy. Exchange takes place. The rate of 60 francs is maintained. The market is in a *stationary state* or in *equilibrium* (1926 [1954]: 85, his emphasis) [43].

Now suppose that

> The brokers with orders to buy can no longer find brokers with orders to sell. This is clear indication that the quantity of three per cents demanded at 60 francs is greater than the quantity offered at that price. Theoretically trading should come to a halt. (1926 [1954]: 85)

Notice that he makes a quantity argument, not a price or equilibrium argument. It makes sense that if there is no-one willing to sell at 60, (i.e. sold out at 60) there can be no more buying taking place at 60 francs! As such, Walras immediately continues.

> Brokers who have orders to buy at 60 francs 05 centimes *or who have orders to buy at higher prices* make bids at 60 francs 05 centimes. They raise the market price. (1926 [1954]: 85, his emphasis)

The bids at higher prices, after all the trading has exhausted the supply at 60, brings new sellers 'forward', and 'withdraws' buyers from the market due to the new price.

> [If] equality between effective offer and effective demand is restored, the *rise in price* ceases. Otherwise, the price continues to go up from 60 francs 05 centimes, to 60 francs 10 centimes, and from 60 francs 10 centimes to 60 francs 15 centimes until offer equals demand. A new stationary state is thus found at a higher price (1926 [1954]: 85, his emphasis)

[43] The two first sentences do not appear in the first edition of *Pure Economics*, but in all the following (Translators notes, p. 566), and could be seen as a later emphasis for a point he thought obvious in the first edition.

Notice that there is incessant trading at every price level in the well functioning market, buyers push prices higher according to what they deem to be the price at which they want to buy, and then they explicitly buy!

After those transactions other buyers may step in, as there is no *Rentes* left for sale in the market. These new buyers have a higher price threshold, and will place bids at a higher price until there are no more sellers trading at that price, and if no-one wishes to buy or sell any more (he has an equal and opposite system for the sellers), then the price remains static until preferences change.

The important points to note here are: *Tâtonnement* in Walras own vocabulary is the process by which an equilibrium price is 'groped' for, and it is done by bidding and explicit exchange throughout the process, while the market adjusts towards the new (and newer) equilibrium price. That adjustment takes place through trading at p_1, p_2, p_3 …p_n all the way until a stationary state is found where no-one wants to sell or buy at lower or higher prices, p^*. The point is that the commodities (or *rentes*) are traded completely at each p_i, all the way to p^*, if a p^* even exists – which is not necessary for the market.

It is the "*effective offer*" (1926 [1954]: 84, his emphasis) or explicit bidding and trading which drives the price changes towards a quantity equilibrium, and as such both have to take place outside equilibrium, as they are the process which lead to the equilibrium outcome in the market. The notion that once the price is in equilibrium, no trade can take place outside it, is correct in as much as 'no trade can take place outside the equilibrium price, as no-one wishes to sell or buy at that price' – a purely quantitative argument. There may however be bids at higher prices or offers at lower prices, which when executed will continue the price movements through new equilibria.

Distinguishing himself from the neo-Walrasians, Walras associates the process of *tâtonnement*, not as a process leading to an equilibrium outcome for the economy, but:

> Such is the continuous market, which is perpetually tending towards equilibrium without ever actually attaining it, because the market has no other way of approaching equilibrium except by groping[44], and, before the goal is reached, it has to renew its efforts and start all over again (Walras 1926 [1954]: 380)

P* is *not* something Walras thinks would be the outcome for the economy, and if it was specified at the outset, it could change as soon as no-one wished to trade at the P* any more. All the agents who wish to sell at lower prices will still try to sell, even if a price is shouted at the start – just like Walras's stock market.

3.3.1 Neo-Walrasian Innovation

The idea of the Walrasian Auctioneer, as propagated (and named) by the modern general equilibrium literature (post 1950), and the *tâtonnement* idea he oversees does not have their roots in the economy which Walras proposed to investigate. The notion of an omnipotent, omnipresent and benevolent utility collector, who declares a clearing price vector (no less) for the economy to be in equilibrium, is at once redundant and anachronistic in relation to the Walras economy. Buyers and sellers declare their own prices, to each other, and search each other out to execute these trades. It is this interaction between bidding, buying and selling of each good which leads to price changes. It is then a desirable feature if the market has a collator of information *after* these trades have been made.

The number of buyers and sellers correspond to what each person's reservation price is, and as one price is settled, new traders enter the market bidding higher prices, or offering at lower ones, in order to maximize what they perceive to be their own utility. The changing

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[44] '*Tâtonnement*' in the French edition.

number of traders, even for a fixed number of goods, establish what amounts to an almost continuous equilibrium of changing prices, much in line with the statement that:

> The market is like a lake agitated by the wind, where the water is incessantly seeking its level without ever reaching it (Walras 1926 [1954]: 380).

The 'Walrasian auctioneer' did not appear in the "*Elements of Pure Economics*", as he would have had no role to play there, as the process of *tâtonnement* leads prices through multiple equilibria which individually clear each local price market.

One could speculate that this interpretation, of how Walras's markets functioned, was either a result of a mistranslation (before Jaffé's work) or an explicit choice by the interpreters. One of the reasons for its inclusion as 'Walrasian' could simply be that the original source material was inaccessible to students of general equilibrium at the time. It is telling that most of the work on the Walrasian auctioneer, comes from the English speaking schools of economics thought, while *The elements of Pure Economics* was not available in English until 1954, and there were different versions, both in content and context circulating in French.

If it was not for this 'mistranslation', the existence proof of general equilibrium would not hold. As such there is a clear dividing line between what Walras envisioned the economy to be, and what Arrow and Debreu wrote out as general equilibrium theory.

3.3.2 Why the Auctioneer?

A final digression would be the question, what purpose did the Walrasian auctioneer serve beyond the mathematical existence proof of general equilibrium?.

The auctioneer features as omnipotent, omnipresent and benevolent, as he wishes to maximize everyone's utility. In this manner he could be likened to the Smithian position that there is an invisible hand which guides society to its optimal outcome. It has been argued that the invisible hand is an allegory to the workings of a deity, which in Christian theology is defined as omnipotent, omnipresent and benevolent. And that the black box mechanism which lead to the social optimum, was in fact the deity's master plan (Denis 2002). Whether Arrow and Debreu had such providential ideas in mind is doubtful, but the similarity is striking.

In a more social context of the early 1950s and the cold war, both in relation to political and economic ideology, it is perhaps fitting that arguments for capitalism should be based on 'western enlightenment' figures like Smith and Walras[45]. The auctioneer could then be seen as the mathematical market maker, somehow imbedded in the virtues of capitalism, based on the 'hard' science of econometrics which was emerging as the dominant form of discourse during this period.

The point, no matter which interpretation is chosen, is that the Walrasian auctioneer and his modern notion of *tâtonnement* cannot be traced back to Walras.

The neo-Walrasian market differs fundamentally from Walras's market. Walras's equilibrium is the result of decentralized traders making real exchanges at changing prices, while prices are announced for each trade *post facto*, in the best case scenario. The neo-Walrasian, and AGE general equilibrium, is a centralized system, with a price setter – the auctioneer – who determines prices *a priori* to any trades, and at the given price all trade is exhausted. As a result of this neo-Walrasian interpretation of Walras's *tâtonnement*, modern general equilibrium has a mathematical proof of existence, as well as a myriad of other qualities, which Walras has none of. Walrasian and neo-Walrasian general equilibrium, are only related in name, not in content.

[45] But the auctioneer could also be interpreted as the ultimate social planner, which was less popular in the Anglo Saxon world than continental Europe.

3.4 AGE is not, and was never, CGE

I have argued that Walras is not neo-Walrasian, and in section 1 that AGE models are not CGE models. It must however be recognized that AGE models were for all practical purposes completely neo-Walrasian models up until the early 1980s. So in order to pre-empt an argument which links neo-Walrasian general equilibrium theory to CGE models through some 'absorption' or 'implicit transfer' (neither of which occurred) from AGE models, one has to show that AGE models are computed differently from CGE models, and establish whether both model types are solvable numerically.

CGE models, being based on macro balancing equations, and an equal number of equations and unknowns, are solvable as simultaneous equations, where exogenous variables are changed outside the model, to give the endogenous results. This process is uncontroversial.

AGE models, being based on Arrow-Debreu general equilibrium theory works in a different manner completely. The model first establishes the existence of equilibrium through the standard Arrow-Debreu exposition, and then inputs data into all the various sectors, and then apply Scarf's algorithm (Scarf 1967a, 1967b and Scarf with Hansen 1973) to solve for a price vector that would clear all markets instantly. This algorithm would narrow down the possible relative prices through a simplex method, which kept reducing the size of the 'net' within which possible solutions were found. AGE modelers then consciously choose a cut-off, and set an approximate solution as the net never closed on a unique point through the iteration process.

This was not due to a lack of computing power, but as Velupillai's (2006) proof of the non-computability of AGE[46] models shows that.

[46] Velupillai refers to AGE models as CGE models, as he is a recent student of CGE models. However his references and sources are all based on the Work of Scarf, and none to the CGE literature. He stated categorically that "The economic foundations of CGE models lie in Uzawa's equivalence theorem; the mathematical foundations are underpinned by topological fixed point theorems" (i.e Brouwer, Kakutani etc.)

> From a very elementary (classical) recursion theoretic standpoint it is easy to show the absence of a computable (and constructive) content (Velupillai 2006: 366).

He effectively proves how the AGE models, can *not* be precisely solved numerically[47], and this puts it in direct opposition to the very solvable CGE models. As such, models can either be incomputable general equilibrium models *or* computable macro equilibrium models. Arrow-Debreu and AGE models belong to the former, while CGE models are the latter.

4. How CGE models work

This section explains how CGE models are built, in a clear and concise fashion. The technical jargon is covered, explaining terms such as '*benchmarking*', '*calibration*', '*closure*' and '*balanced closure*' and the function of the various processes is outlined to allow model users, or interested readers, to pick up a CGE model and know what they are being sold, and what they are not being told.

CGE models, despite their mathematical complexity, can be boiled down to a few key operations and technical terms. When these are laid out, the functions and solutions of CGE models become clear, and it emphasizes the scope for theoretical issues in the framework. Asking these (or any) questions of CGE models is rarely done, because a large part of the assumptions and theory chosen in the CGE model is never published. Journals tend to publish creative ways of extending existing functional forms, allowing authors to flex their formulaic muscle, rather than explain the magnitudes which determine results. As such it is hoped that a wider knowledge of the model's workings will push model builders to be more open about the critical elements of their work, especially those fixed *a priori* to the actual

(2006: 361). All of which is true of Scarf and his immediate AGE models, but not CGE models at any point in time.

[47] Velupillai's overall aim is to encourage mathematical economists and AGE model builders to use non-topological fixed point theorems, which would be both constructive and computable in order to use the neo-Walrasian general equilibrium theory properly in policy analysis..

simulations. Otherwise readers will now have a guide for where and when to ask pointed questions.

CGE models are composed in stages, each of which will be outlined here, as large parts of model specification, data adjustment and parameter selection remains unexplained in published models.

4.1 Uncle SAM

The Social Accounting Matrix (SAM) is a way of representing the national accounting framework in a matrix form and is important for "CGE models, which are always based on a SAM framework" (Robinson 2003: 1). SAM's (and thus the national accounting framework) form the empirical base on which the CGE model runs. Thus understanding the SAM is the first agenda item at specialized CGE courses, although their composition and meaning is rarely discussed in papers.

A simple SAM is presented for illustration purposes in Figure 8. They are built to identify all monetary flows from sources to recipients, within a disaggregated national account. The SAM is read from column to row, so each entry in the matrix comes from its column heading, going to the row heading[48]. Finally columns and rows are added up, to ensure accounting consistency, and each column is added up to equal each corresponding row.

FIGURE 8: *A typical SAM*

	Firm	Household	Gov't	RoW	Net Invest	Total (received)
Firm		C	G_F	$(X\text{-}M)_K$	I	$C + G_F + (X\text{-}M)_K + I$
Household	W		G_H	$(X\text{-}M)_H$		$W + G_H + (X\text{-}M)_H$
Gov't	T_F	T_H				$T_F + T_H$
RoW	$(X\text{-}M)_K$	$(X\text{-}M)_C$				$(M\text{-}X)_K + (X\text{-}M)_C$
Net invest		S_H	S_G			$S_H + S_G$
Total (spent)	$W + T_F + (X\text{-}M)_K$	$C + T_H + (X\text{-}M)_C + S_H$	$G_F + G_H + S_G$	$(X\text{-}M)_H + (X\text{-}M)_K$	I	

48 For example, Consumption (C) comes *from* households and is paid *to* firms.

FIGURE 9: *Abbreviations*

Capitals:	Subscripts:
Taxes;	Firms;
Wages;	Household;
Imports;	Government;
Exports;	Household Goods
Savings;	K: Capital Goods;
Investment;	
Consumption;	
Government Transfers;	

The SAM, performs two functions. First, it can be easily extended to include other flows in the economy, simply by adding more columns and rows, once the standard national account (SNA) flows have been set up. Often rows for 'capital' and 'labor' are included, and the economy can be disaggregated into any number of sectors. Each extra disaggregated source of funds must have an equal and opposite recipient. So the SAM simplifies the design of the economy being modeled.

Secondly, the SAM, by virtue of being a representation of the national accounting system, fixes the Keynesian macro balancing equations in the model, and immediately any model built in a SAM becomes a CGE model, of the modern variety, completely separate from AGE and Arrow-Debreu models. The national account settles Leontief's problem of a model for the economy, but The SAM introduces its own problems as the "national income data depend on a particular model of the economy (at least in a broad way) and omit very large areas" (Arrow 2005: 21), which is where '*benchmarking*' comes in.

4.1.1 'Benchmarking' – Empirics vs. Theory

Using a SAM includes the institutional structure assumed in the national accounts into any CGE model. This means that variables and agents are not treated with monetary source-recipient flows in mind, but are rather grouped together in different categories according to the UN SNA Guidelines. For example, the national accounts usually imputes the value of household investment or home-owner 'rental' income and treats some public sector institutional investment as direct income flows[49] - whereas the SAM is trying to show just the explicit flows of money. Thus the data has to be untangled from its inherent SNA definitions to become money flow variables. Model builders, however, never report these empirical adjustments, and rather than disaggregate the whole SAM, and break down all the money

[49] Public hospitals and universities in the US for example

flows, the tendency is to focus on one aspect of the macro balancing equation depending on the question at hand. That means building models around either the investment-savings cycles *or* international trade debates *or* government spending decisions. Typically one or two parts of the macro balance is disaggregated into *n* sub-accounts, and the last one is left as a simple aggregate account, towards which the model must still adjust.

A theoretical SAM always balances, but empirically estimated SAM's never do in the first collation. This is due to the problem of converting national accounting data into money flows and the introduction of non-SNA data, compounded by issues of inconsistent national accounting data (which is prevalent for many developing nations, while developed nations tend to include a SAM version of the national account, generally precise[50] to within 1% of GDP). This was noted as early as 1984 by Mansur and Whalley, and numerous techniques have been devised to 'adjust' SAMs, as "inconsistent data estimated with error, [is] a common experience in many countries" (Robinson, Cattaneo and El-Said 2001: 1)

SAM adjustments are rarely covered in the literature, as it showcase how a large part of building a CGE model, includes 'filling in the blanks' or 'adjusting' (read: changing) data entries.

Robinson et al. (2001) suggests an improved method for 'adjusting' an unbalanced SAM in order to get all the rows and columns to equal, and gives the example of a SAM created for Mozambique's economy in 1995, where the process of gathering the data, creating the SAM and 'adjusting' it, is thoroughly covered by Arndt et al (1997)[51].

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[50] Imprecision is caused by omissions in the data-sets, but I use the word 'precise' rather than accurate, as the process from raw micro-data to national accounting data is not a streamlined system globally, and even if there is a 1% discrepancy in the account itself, this is more than often the result of careful double-entry book-keeping and classifications, rather than accurate descriptions of economic activity, or theoretical reasoning. That being said, if data is consistently recorded the same way, there should be no issue comparing year to year, but the 1% precision is often a better indicator of the accountant, as opposed to the quality of the data.

[51] In Robinson et al (2001) the reference is in fact to a 1994 SAM for Mozambique, as created by Arndt et al 1997b, but this is somewhat misleading as IFPRI had already published Arndt et al. 1997b as a working paper

What we find, on inspecting the changes made to the Mozambique's 1995 SAM[52] to achieve balance is an adjustment of $295m USD[53] which meant that $227m USD was added to the SAM *net*, just to balance the rows and columns. For 1995 this adjustment is equivalent to 11.65% of GDP! More disconcerting is perhaps the fact that agricultural producers (which according to FAO (1995) employed 85% of the labor force in 1994) were given a $58m USD pay rise in the SAM, meaning that 10% of agricultural income (equivalent to 5% of GDP) in the SAM was created, out of thin air. In other words, for a country where 38% of the population lived for less than $1 in the period 1994-2004 (UNICEF 2008), this SAM 'adjustment' added $4.40 to each persons income in the agricultural sector[54] – more than any of the later trade and tax models using this SAM could hope to achieve.

Robinson et al. (2001) as well as Robinson and El-Said (2000) suggests a method by which these changes might be reduced, by changing the mathematics behind the adjustment choices. Robinson et al. (2001) is successful to the extent that they reduce the potential change of the SAM to a *reported* $84m USD. However, as they try to fix the GDP as close as possible to the officially released GDP figure[55], the SAM has to fix a quite a few of its variables. The result is that total Agricultural expenditures rises by $3.98m USD net, creating 30 cents of extra expenses per capita to the agricultural sector, due to the SAM adjustment. However, the reported deviations are from the *balanced* SAM, which is somewhat misleading if one wishes to compare this new method over the old one. Doing so,

(referenced here as Arndt et al [1997])which is the only available copy and which included Robinson as an added author. While both papers created a SAM for 1994 *and* 1995, the one used in Robinson et al 2001 is in fact the 1995 SAM, not 1994 as referenced in the paper.

[52] The SAM is reproduced in appendix I, for the unbalanced, the official balanced, and the suggested improved version, although some corrections have been made, as Robinson et al. (2001) had a number of mistakes in the SAM presentation, for example denoting the currency values in millions, as opposed to hundreds of billions, as well as missing adjustment figures – these have corrected in the appendix.

[53] Exchange rate used is the Bank of International Settlements (BIS 2005) average exchange rate for 1995 at 8819.75 Mozambique meticais per $1 USD,

[54] Both figures based on 85% of the population being in agriculture

[55] They still miss the original GDP figure, although only by $250,000 USD, or by 0.01%.

it becomes clear that the alternative method still needs to adjust the SAM by $275m USD, and they still add $149m USD *net*. This is an improvement over the previous adjustment, but of a much more modest proportion. And in fact, agricultural production still receives the same $58m USD extra.

This is not a special case, but a typical illustration that building a SAM for any CGE model includes a host of discretionary choices which have to be made, *a priori* to doing any simulation, in order to balance the SAM. The final idiosyncratic SAM creates what is called a '*benchmarked economy*' at a '*base year*', meaning that all the data in the SAM is 'adjusted' to conform to the macro balancing equations of one particular year (1995 for the above model). How these changes are performed is based on the model builders own decisions, and model users should ask what adjustments were made to the SAM, and what their magnitude is. Otherwise you may find agricultural income quadrupling in the results, not because of the CGE model and the policy change, but because the SAM was '*benchmarked*'.

4.1.2 Theoretical Implications

The '*benchmarking*' process gives modelers the *theoretical* equivalent of observing empirical macro balance implying that the economy is in macro equilibrium. Macro equilibrium should be self-correcting as we implicitly assume a theoretical steady state, and if the benchmark is assumed to be true – which it inevitably is – we now have a theoretical reason for shocking the system exogenously, and expecting an adjustment process. This of course is not the reality of the matter, but using the balanced SAM we can create that illusion.

Once the basic macro balance is benchmarked, you can add (or disaggregate into) any number of rows and columns with new variables, and as long as inflows equal outflows the assumed equilibrium holds. The next implicit assumption is that an equilibrium in a steady state should hold in future time periods, so we can perform external shocks and then empirically observe some (marginal) adjustment mechanism – which is the simulation method. Any debate on the empirical validity of such long run equilibrium is ignored, because the data 'speaks for itself' and "the economy under consideration is assumed to be in equilibrium" (Shoven and Whalley 1992: 103) no matter how long term or short term the model calculations will be.

4.2 Functional Forms

Once the benchmark is done, all the empirics of the CGE model have in fact been completed. The second stage is then to flesh out the relationships between the macro balancing equation elements (institutions), and agents if the model is disaggregated to that level. These are known as the '*functional forms*' and '*behavioral equations*' respectively.

If one considers the macro balancing equations as the first tier in the model, then the functional form is the second tier of equations. For every institution or variable under investigation some equation (linear or non-linear) is specified to account for the relationship from one to the other, and through to the balancing equations.

The usual suspects from neo-classical micro economics tend to dominate the relationships specified, with Keynesian multiplier relations cropping up on occasion in tier one. Cobb-Douglas functions are popular as they can give you uniquely determined parameter values through their calculation, similarly to how Johansen got his elasticity values. However, CES and LES functions are now more common, but they do *not* allow the model to determine elasticities endogenously, rather you need exogenous values for the parameters in order to make the functional forms work in the CGE model, as detailed in section 4.3.

Importantly, the share of economic activity that each sector contributes or uses is *predetermined* by these equations and it is limited because of the numerous convexity assumptions. More specifically the input shares of sectors will not change if the elasticities of substitution are all equal to one, and similarly the consumption shares will not change if demands are homothetic with unit price elasticities (again Cobb-Douglas). So a CGE model could not predict, nor deal with any major structural changes like China's recent boom in manufacturing, or India's booming service outsourcing sectors. Simply because those productive parts of the economy are given a set percentage of the nations output in the benchmark, that will not change. To make adjustments to this, one would have to post facto change these shares exogenously, but it cannot be incorporated endogenously.

The model as such can only deal with small changes within the given structure of the benchmark economy, but cannot react to changes in the structure of the economy, or predict those changes, without someone specifying how the economy will change over time *exogenously*, as well as *a priori*, no matter the functional form.

4.2.1 Behavioral Equations

Behavioral equations are the third tier in CGE models, and were not widely specified until after 1980. Their mathematical specification runs along the same lines as the functional forms, but addresses single agents rather than institutions.

These relationships usually crop up on page two in CGE papers today, as they are carbon-copies of the standard micro models, with isoquants and isocost lines tangentially touching budget constraints of a utility maximizing agent. As such they play an important rhetorical part in portraying the 'ground work' – or the first few pages – in a CGE model as resembling the standard neo-Walrasian general equilibrium model. However, these equations feed into the functional forms, which in turn equal under the macro balancing equation. The illusion of causality from beneath (rather than above) is achieved because the macro balancing equation that steer the model is buried between all the second and third tier equations somewhere after page 30. Again, if the equations chosen do not correspond to the CES forms, then the value of the parameters used, need to be specified exogenously.

Behavioral equations and functional forms are discussed extensively in CGE papers, and the normal considerations apply. Where CES or LES assumptions are made, perfect competition and constant returns to scale are usually found. Whether these are empirically relevant or simply clipped on to models, should be explained, as they are currently sidestepped.

4.2.2 Where micro-behavior matters

The explicit nature of the micro behavior, whilst subordinate to the macro equations, highlights a very important point, which is often missed in the results of CGE models. In order to solve the calculation from any exogenous shock, the CGE model speaks the language of micro demand and supply. More than that, the benefits and costs that results is generally the calculation of 'welfare'. The biggest example of this is the World Banks model of world trade, which is reviewed and critiqued in Taylor and von Arnim (2007).

The point is emphatically made when the conclusions note that the world will be *x* billion dollars better off, but those billions are improvements in consumer and producer surplus, due to the inclusion of the behavioral equations, and not incomes or profits. It is not the fact that a country or its population will earn more or less money, but that those little triangles which feature the deadweight loss in standard micro will shrink or grow.

Whenever a tariff or quota is imposed in the standard demand-supply diagram there is a deadweight loss. So liberalizing trade, automatically improves the micro world, and beneath all the World Banks equations (which is a 81 by 57 matrix with 4,500 odd solutions), this is the (obvious) result. Secondly, consider an 'improvement' in the case of a Sub-Saharan country. If a country is $1 million 'better off', and that comes at the removal of an import tariff, then what has happened is that more imports have been allowed into the country. In the model, this will be balanced by the SAM, so the increase in imports will find an equal and opposite response in either prices or quantity. There has been no empirical reason why the benchmarked economy should exhibit macro equilibrium (in fact it does not), and this extra inflow, may just add to balance of payment problems, despite being an 'improvement'. This cannot be captured in the model, which measures marginal micro adjustment, within a balanced SAM. So when a CGE model talks of benefits or gains, make sure to know if the reference is to income and costs, or utility maximization.

4.3 Closing time

The final internal theoretical choice and arguably the most important is to '*close*' the model. While the notion of closing a macro model dates back to Sen (1963) and his time at Cambridge, after Johansen's time there, it was addressed in Taylor-Lysy (1979) for CGE models, and is "the simple notion that the model should consist of an equal number of equations and endogenous variables" (Thissen 1998b: 7)

Given the solution based nature of the CGE model, every equation used in the SAM must necessarily have an exogenous and an endogenous variable. The exogenous is fixed *a priori*, and the endogenous will then change from its benchmark value, according to the functional forms prescribed, when the exogenous variable is shocked. This may appear to be standard macro modeling practice, but it is important because in the top tier, and throughout, the decision of exogeneity *sets* the direction of causality.

> 'Closure' is CGE jargon for assigning causality in a model. The practice often boils down to deciding which variables should be exogenous or endogenous (or which equations should be included or excluded) to make sure the model is 'closed' or has a solution, like a typical problem from high school algebra. (Taylor and von Arnim 2007: 29, footnote 37)

'*Closure*' must be prescribed for each endogenous variable introduced to the SAM as they each must have an exogenous counterpart. In the top tier of the model, decisions about whether savings adjust to investment, or investment adjust to savings, are crucial separators between neoclassical and Keynesian economics at the outset. The same applies to decisions of whether government funds adjust to tax receipts, or vice versa, and most controversially whether the exchange rate adjusts to hold current accounts constant, or an alternative behavior for the balance of payments exists.

These decisions have to be made at all levels of the model, and they are rarely, *if ever*, discussed or justified in model papers. This would not be an issue, if the closure direction was neutral towards the result – this however is not the case.

In one of the few papers that challenged closure assumptions, and only the top tier closures at that, Taylor and von Arnim (2007), '*close*' the World Bank global trade model the opposite way to the official model, with the reasoning that:

> It is unnecessary to claim that any closure assumption reflects the 'true' direction of causation. Applied economists are well aware that certain models and their assumptions fit certain situations, periods, and countries and might not add insight in other circumstances. For a model on a global scale and an issue as hotly debated as the Doha Round it simply seems prudent to look at the issues from different perspectives. Suppose empirical research could show (or have shown) that the exchange rate is a highly flexible international price that adjusts to guarantee balanced trade with 50 per cent probability. Should policy analysis not address the other 50 per cent as well?
> (Taylor and von Arnim 2007: 29-30).

The results from testing different closures indisputably showed that closure is *not* model independent. In fact results came out diametrically opposite for all country groups, with differences ranging from -1% to +100%.

Almost all models use only one closure method, and high priced courses and software restrict new model builders to a handful of options, despite the fact that there are as many closures to a model as there are variables to the power of two. As such a model with just the three balancing equations has eight distinct closures, all of which could yield different results for the model. Taylor and von Arnim's model includes 125 equations, and has a potential 15,625 closures, of which only a few were tested. The World Banks model in turn has some

50,000 equations, all of which are closed idiosyncratically to their model, with two and a half *billion* possible closures!

'*Closure*' punctuates the point that the CGE model is solvable or computable simply by counting equations and endogenous variables, and solving the simultaneous equations (by inverting a matrix). More than that, it prescribes causality based purely on the theoretical preference of the model builder, and there is no right or wrong standard one can apply. As such any model should justify *at least* why their top-tier closures are being used, explicitly explain the reasoning, and provide alternatives, and if they do not, ask for them.

4.3.1 'Balanced' or 'Neutral' Closures

Much of the CGE literature post 1990 has included what is referred to as 'balanced' or 'neutral' closures. This method does not address the issue raised above about neutralizing the closure so that choosing the exogenous variable is model independent, nor is it a closure in the normal sense.

After closing the model, three extra equations are created: The first of which defines the total absorption [A] of the economy, as the GDP at market prices plus imports less exports, expressed using Households [HH], Government [G] and Investment [I][56]:

$$A = HH_{cons\ (domestic\ +\ foreign)} + G_{cons} + I_{fixed\ +\ stock\ \Delta} \qquad (1)$$

Two extra equations are then added to solve the G and I for the equation by defining:

$$G/_{A} \times A = G_{cons} \qquad I/_{A} \times A = I_{fixed+stock} \qquad (2)$$

[56]For more information on use of balanced closures, see Bourgignoun (2002) and Davies (2004) or for their application see Löfgren et al. (2002) and Robilliard et al (2001).

The Government consumption and Investment shares of absorption in equations (2) are fixed to the base year proportion of absorption, and as the CGE model is shocked, A changes as the C+I+G+M-X variables change, allowing (2) to be solved, and then feeding the consumption shares back into (1). So this means that any change in absorption, is spread across each component of the macro balancing equation, in nominal terms, according to the pre-set proportions (G/A and I/A). Households are then balanced out with 'Walras (or Dorfman-Samuelson-and-Solow's) Law'[57].

The 'balanced closure' equations have to be closed themselves, which is done by setting the proportions exogenously, and getting A from the macro balancing equation, or exogenously. "This 'balanced closure' [method] aims at distributing the burden of policy changes equally among all final demand aggregates" (Wobst 2002: 8).

There is no theoretical rationale for distributing any shock equally between the components of demand, but this addition imposes the notion that all parts of demand will share equally in any macro-economic change. Basically it is a way to dodge macro questions, as the model builders assumes that any shock will not affect any particular aspect of demand, which would be the case if it was left to the functional forms and closures that the model builder had already set.

4.4 Parameters

Every functional form and behavioral equation has a number of parameters, all of which have to be quantified before the benchmarked and closed CGE model is used. If the CGE model is aimed at calculating cross market elasticities (like Johansen 1960), and uses only functions of a Cobb-Douglass nature then the models parameters can be estimated (based on

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[57] One could equally add a third equation for the household import/export share of absorption, but would then need a fourth equation to ensure that all three parts of A equals zero. Sometimes this method is followed not for the balanced closure but for the normal macro closure, and the variable which equals zero is often referred to as 'WALRAS'S in the programming code. (See Löfgren et al. (2002) p. 39 for an example).

the benchmark) in the model. However, the majority of models use other functional relationships, and generally attempt to estimate gains from trade, or changes to macro variables. When that is the case parameters have to be defined *a priori* to the model being used.

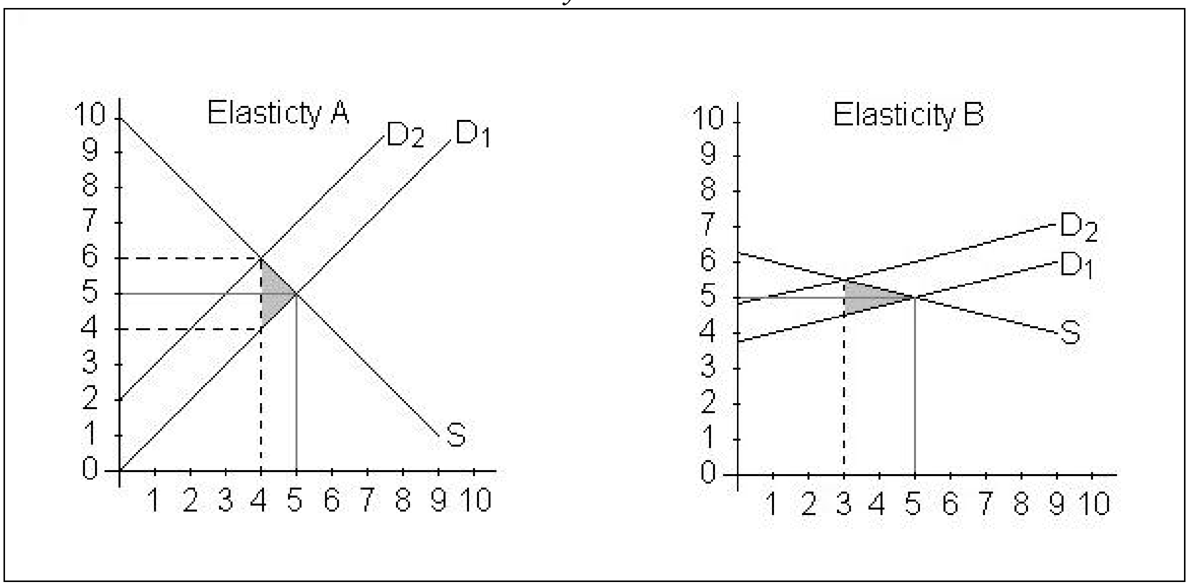
> Models are dense with parameters… Often no estimates exist of required parameters, so they are guessed; or multiple estimates exist that are contradictory. In the economic literature different estimation procedures, different data series, and different theoretical concepts are used, making it very difficult to use estimates drawn from the literature. (Kehoe, Srinivasan and Whalley 2005: 10)

Model builders *choose* the magnitude of the changes the model will predict, by setting parameters. Again parameter values are not generally specified in papers, which focus on the 'results'. A majority of parameters come from literature searches and best effort guesstimation of what a 'realistic' value should be, as each model builder chooses the parameters idiosyncratically, and can hide important elasticity values, without any issue.

The World Bank teaching material (Essama-Nssah 2006) highlights the importance of parameters, and says that to set them, you have to "assume a value for ω [the parameter] and compute… the share parameters [so they] add up to one". This will then give the size of all share parameters and determine the shifts in endogenous variables as the model is shocked. There is no mention of how to get ω's except by assumption, and these parameters are then fixed for the model's entire simulation run *a priori* just like input shares were fixed by the SAM.

These values are important for model results, and using the World Banks LINKAGE model as an example, there has been a tendency for the model builders to overstate trade elasticities by factors up to 3-5 in the past. The reasons behind this are multi-fold, but to point out the two biggest reasons: Higher elasticities will first of all ensure that the Marshall-Lerner condition holds for trade models. Secondly if you are using micro behavior (basic demand and supply) a higher elasticity of demand or supply gives better welfare improvements from smaller tariff cuts.

FIGURE 9: *Why Elasticities Matter*



Take the example in Figure 9, where a trade equilibrium (5, 5) is being distorted by an import tariff resulting in a deadweight loss of one for the economy. For elasticity A, the required tariff is 1, whereas B which is more elastic only needs a tariff of ½ to create a deadweight loss of one. So when the model runs a trade liberalization scenario, a higher elasticity will give better welfare gains with smaller tariff reductions. The double edged sword, which is then missed in the result, is that the higher elasticity country B, will after liberalization import more units than country A would, leading to a higher outflow of money. This CGE model misses this out completely in its tunnel vision on welfare gain, and if the closure has fixed the balance of payments, this effect would only materialize further down the chain of equations, and would not be obviously connected to the import parameter choice.

4.4.1 Calibration

'*Calibration*' is the method by which model builders try to 'test' their parameters, and most models will make mention of having performed this, although the results and any changes are rarely, if ever published. The method is similar to what Johansen did back in the 1960s, although the time period has shrunk from Johansen testing 10 years of data:

> This process solves the relevant equations for values of the parameters that are consistent with base year data (Essama-Nssah 2006: 31)

Using the closed benchmark economy, and the assumed parameters, the exogenous variables of the model are input to see if the endogenous solutions correspond to the benchmark data. If the margin of error is 'acceptable' then the parameters (and thus elasticities) are deemed sufficiently realistic for modeling purposes. The margin of error allowable is up to the model builder, and no solid standards exist.

One Alternative method for calibrating models, consists of setting the base year of the benchmark economy in the past, and then running the real changes in exogenous variables for the years to date to see if the endogenous variables still approach the historical observations. This practice was pioneered by Johansen, but is rarely used today[58].

> Calibration inevitably implies subjective judgment by the calibrator. How is this to be squared with econometric rigor? (Kehoe, Srinivasan and Whalley 2005: 9).

As most models perform calibrations, the above question is highly relevant. However, given that the calibration results themselves are highly dependent on the form of closure, and multiple closures are not tried, it is not clear that one particular calibration is better than

[58] Another alternative is to econometrically estimate the parameters. This method is what Bergman and Henrekson (2003) referred to as a Jorgenson's 'third' way of doing CGE models. What they should have said, is that this is a third method for performing calibration. On a related note, the paper where Bergman first published the idea of a third CGE model (Bergman 1990), was in a book he co-edited with Jorgenson.

another. Again, Henderson's point to Chenery in 1959 about the indeterminacy of parameters is still relevant[59]. Parameter choices should be made transparently, even if they are calibrated, as various permutations of 'correct' results could be found. Seeing that econometric rigor is not necessarily available, it needs to be replaced by modeling honesty.

One should note that the standard practice is not to adjust the parameter (elasticity) values if the base simulation misses 'too far' from the base year SAM. Rather than that, modelers tend to adjust the intercept parameter to make the parameter value of their choice affect the result to match the base year SAM, which means the size of the impact is still in the modeler's control[60]. What the size of these errors are, what effect they have, and whether the calibrated specification even makes sense, are all issues which should be raised in model papers, but never is. The range for mistakes, both conceptually and for results, is potentially enormous.

4.5 Running a simulation

After all the theoretical choices and numerical adjustments are made, the CGE model is finally going to be used. Although it would be fair to say that the majority of results are given by the above choices, rather than the simulations themselves. Using the CGE model boils down to taking the closed, calibrated benchmark model and changing exogenous variables according to what policies are being modeled, and solving for the endogenous results. Doing this a couple of times in a row (using the results as the next set of exogenous variables[61]), makes the results 'long term' in much of the literature.

Depending on the size of the model, and its level of disaggregation as chosen in the SAM, the chains of causality running through the economy (defined by the model builder), may be

[59] as noted in section 1.1

[60] For example, the modeler chooses an elasticity of substitution in a CES cost function and then adjust the relevant intercept parameter to generate the SAM's labor share exactly. The equation for the share then comes from Shephard's Lemma.

[61] as in the chain of models in figure 1, section 1.7

easy to discern in small models, or impossible to extract from the larger ones. In either case, the model will only re-distribute flows within the model, and repatriate dead weight losses, but not simulate economic growth unless the exogenous variables of the macro balancing equation are exogenously set to increase. If the macro balancing equation remains stationary, the economy must adjust all its prices to fit the macro equilibrium, no matter the micro behavior.

4.5.1 Static Dynamics

A last myth to be dealt with is the notion that CGE models are dynamic, or that their solution mechanism is a dynamic process, when in fact it rests on the number of equations and endogenous variables being equal so the *simultaneous* solution can be calculated.

The models which are 'dynamic' are the ones which have one CGE model feed its endogenous variables into the exogenous variables of another model, and back again. This is the lurching equilibrium discussed in 2.7, hopping from one to the other instantly. The assumption that the economy is in equilibrium is inherent in the SAM, and there are no adjustment mechanisms towards an empirically estimated or theoretical equilibrium. As such, the dynamics are actually static solutions, recalculated over and over. For any long term model, it has to be remembered that the original benchmark values and parameters will persist to govern the economy for any modeled future, unless exogenous (to the model) variables are introduced by the model builder. CGE models are not dynamic.

For example the World Bank trade model (LINKAGE) is a 'lurching' CGE model, but it also includes a sectoral productivity level which rises over time (read: each static iteration), exogenously from anything in the model of course.

5. Conclusions

This paper is admittedly long, but in order to arrive at an understanding of how CGE models work, it is necessary to have some working knowledge of how AGE and CGE models differ, how they changed, and ultimately what their theoretical foundations were. This information was not available in the extensive CGE literature, perhaps due to the nature of the CGE community, who have a niche market to protect, and markets shares to compete over through appearances of being the best or the first at their model. This paper is not meant as a critique of either the CGE model, or the CGE community, rather it is an attempt to relieve the literature from unnecessary rhetoric and myths when the model structure itself is of a solid macroeconomic nature, and:

> Because this work is necessarily subjective in design and execution, the credibility of the model builder is key (Kehoe, Srinivasan and Whalley 2005: 11)

This is an attempt to restore credibility with modelers, and to equip outsiders with the tools and knowledge to demand transparency, if this is not forthcoming from the inside.

In this paper it has been argued that CGE models are *not* and have *never been* general equilibrium models in the Arrow-Debreu or Walras tradition (myth 1). The historical review started in the 1930's and covered models up through the 1970s discarding the leapfrog from Johansen (1960) to 1978 (myth 2). It was illustrated, in both the history and technical review, that CGE models are macro balancing models, and *not* modern micro economic models (myth 3)[62].

In the history it was argued that AGE models were explicitly neo-Walrasian general equilibrium models, from 1967 until the mid 1980s and were *not* a special case of CGE models (or vice versa). After the mid 1980s, AGE models in the policy world stopped working on the Arrow-Debreu framework, for practical reasons. Instead AGE models assumed a SAM of the national accounts, balanced with macro equations, substituting

[62] Although they do incorporate a lot of pre-game-theory micro (especially duality) in their detailed third and second tier functional forms.

existence theories for benchmarking and calibration. As such, post 1985, AGE models in the policy world were CGE models (myth 4).

The only manner in which Walras could be at the core of CGE models would be through some transfer from AGE models as they were absorbed in 1985. However, AGE models relied on the existence proof and auctioneer of neo-Walrasian general equilibrium, which were not part of the Walrasian system. So Arrow-Debreu did not represent the Walrasian theory, and CGE models do not represent Arrow-Debreu, despite many allusions to this in the literature (myth 5). Further, Arrow and Debreu's general equilibrium did form the core of AGE models pre 1985, but it was proven (by Velupillai 2006) that the AGE algorithms for solving general equilibrium are *not* explicitly computable, whereas CGE models are precisely computable by inverting a matrix and solving simultaneous equations, not seeking for a price vector (myth 6). The naming of CGE models is a reference to Jones (1965) defining general equilibrium as any situation in which two markets are analyzed, and should not be interpreted as a reference to Arrow-Debreu, in contrast with AGE models, which *did* try to apply general equilibrium theory.

Despite many arguments about different types of CGE models, there is only the current CGE framework as outlined in section four, and which history was followed in section one. The 'others' are CGE models with different internal choices regarding their closure, parameter setting and so forth[63], or pre-1985 AGE models (Myth 7). Further it was shown that 'dynamics' in CGE models is a number of static equilibria solved one after the other, and *not* a dynamic process nor result of any kind, meaning they are no more dynamic than the standard Solow growth model (myth 8).

[63] WPE models for example are simply CGE models with Price and Wages set endogenously, while Ezaki 2006, and Bergman and Henrekson 2003 both identified three distinct types of models, of which CGE and AGE versions were two. Ezaki's third type of CGE model was chain-models as illustrated in1.7.1 where CGE models are part of a chain, which is not a separate type of model of course. Bergman and Henrekson argued that econometrically estimating parameters was a third type of CGE model, but this is simply a choice within the overall model framework, and is not a separate model type either.

5.1 The future

There is only one type of CGE models being built today, and in order to build transparent and credible CGE models, I suggest that its key assumption need to be addressed in model publications, and not hidden away:

The CGE model is set out in a SAM, and benchmarked to allow rows and columns to equal. This process should be explained for models, on the basis that each benchmark is idiosyncratic. Further, the benchmark is automatically assumed to reflect a stable equilibrium, and thus provides the theoretical *raison d'etre* for shocking exogenous variables and assuming an adjustment mechanism. All results are based on these values which become the standard economy, and as such their adjustment should be disclosed.

Functional forms and behavioral *equations* are explained to such a degree in model papers that they crowd out other information. More crucially, explanations of 'what' is being measured, be it utility, trade, income etc. should be made explicitly when results are discussed.

Closure defines the direction of causality for all CGE models, but can be critiqued and should be tested with various theoretical and causal relationships, rather than just applying one closure per model. The type of closure applied needs to be clearly stated, and there can be a theoretical debate on the relevance of closures for different models. Applying a 'balanced closure' set of equations should be juxtaposed with a non-balanced system, to illustrate any differences, and a justification for spreading all effects of policy equally across factors of demand should be presented.

Parameter choice drives the magnitude of changes when the economy self-adjusts after a shock. They are set *a priori* to running the model, and calibration, to test the realism of the parameters, may not re-produce results with a different closure. As such, the numerical value of parameters should be specified and explicitly sourced, and justified.

Finally, when the exogenous variables are changed, claims of dynamics and long term modeling should be restrained unless changes to the structure of the model have been modeled. If exogenous variables increase at a given rate year on year, or elasticities are modeled to change, these are external to the CGE model itself, and should be explained separately.

These are the key parts of any CGE model, and it is hoped that with this knowledge in hand, more people will be able to use and interpret CGE model results. Further, this is arguably a very fruitful modeling exercise, but the literature to date has focused overly much on rhetoric and allusions of theoretical foundations, and too little on the actual workings of the model and the choices of the CGE model builder. Hopefully, this paper provides a possibility to break with the convention of filling papers with extraneous equations and guarantees of micro economic roots, and re-focus modeling publications to explain and debate the essential choices made within the model. Again, if such a change is not instigated from within, it is hoped that it is now clear what needs to be asked.

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APPENDIX 1

SAM 1 **Perturbed unbalanced** 1995 Macro SAM for Mozambique (hundreds of billions of meticais)

Receipts	Expenditures 1	2	3	4	5	6	7	8	9	10	11	12	Totals
1. Agricultural Activity			20.00				30.49						50.49
2. Non-Agricultural Activity			12.46	195.00			2.14						209.60
3. Agricultural Commodities	1.58	13.00					20.12		0.00		0.09	8.58	43.37
4. Non-agricultural commodities	7.24	96.00					86.72	16.78	0.00	32.00	35.00	24.13	297.87
5. Factors	47.01	108.74											155.75
6. Enterprises					62.86								62.86
7. Households					91.63	60.00		1.33				3.46	156.42
8. Recurrent Gov't Expenditure			0.94	9.88	1.26	2.41	2.48		5.55				*22.52*
9. Indirect Tax	-0.19	-0.14	0.24	5.64									5.55
10. Gov't Investment												22.94	22.94
11. Private Investment						1.49	12.00	4.43		-11.00		24.79	31.71
12. Rest of World			5.01	78.89									83.90
Totals	*55.64*	217.60	38.65	289.41	155.75	63.90	*153.95*	*22.54*	5.55	21.00	35.09	83.90	1142.98

Source: Robinson, Cattaneo and El-Said (2001), Table 4, page 14, italic totals differ from the source by plus/minus 0.01 as they have been rounded with two decimal points

APPENDIX 1

SAM 2 **Balanced Official** 1995 Macro SAM for Mozambique (hundreds of billions of meticais)

Receipts	Expenditures 1	2	3	4	5	6	7	8	9	10	11	12	Totals
1. Agricultural Activity			25.14 (+5.14)*				30.49						55.63
2. Non-Agricultural Activity			12.46	206.28 (+11.28)			2.14						220.88
3. Agricultural Commodities	1.58	13.42 (+0.42)					20.12		0.00		0.09	8.58	43.79
4. Non-agricultural commodities	7.24	98.86 (+2.86)					86.72	16.78	0.00	33.94 (+1.94)	33.03 (-1.97)*	24.13	*300.70*
5. Factors	47.01	108.74											155.75
6. Enterprises					62.86								62.86
7. Households					91.63	58.96 (-1.04)*		1.33				3.46	155.38
8. Recurrent Gov't Expenditure			0.94	9.88	1.26	2.41	2.48		5.55				*22.52*
9. Indirect Tax	-0.19	-0.14	0.24	5.64									5.55
10. Gov't Investment												22.94	22.94
11. Private Investment						1.49	13.42 (+1.42)	4.43		-11.00		24.79	*33.13*
12. Rest of World			5.01	78.89									83.90
Totals	55.64	220.88	43.79	300.69	155.75	62.86	*155.37*	*22.54*	5.55	22.94	33.12	83.90	1163.03

Source: Robinson, Cattaneo and El-Said (2001), Table 3, page 13, italics as in SAM 1, brackets indicate change from SAM 1, while asterix notes correction of Robinson et al. (2001)

APPENDIX 1

SAM 3 Suggested **Improved Methodology** in Robinson et al (2001) (hundreds of billions of meticais)

	Expenditures												
Receipts	1	2	3	4	5	6	7	8	9	10	11	12	Totals
1. Agricultural Activity			23.36 (+3.36)				32.26 (+1.77)						55.62
2. Non-Agricultural Activity			13.40 (+0.94)	202.98 (+7.98)			1.68 (-0.46)						218.06
3. Agricultural Commodities	1.58	13.14 (+0.14)					19.96 (-0.16)		0.00		0.09	8.60 (+0.02)	43.37
4. Non-agricultural commodities	7.24	96.30 (+0.30)					85.57 (-1.15)	16.64 (-0.14)	0.00	33.93 (+1.93)	33.18 (-1.82)	24.11 (-0.02)	296.97
5. Factors	47.00 (-0.01)	108.76 (+0.02)											155.76
6. Enterprises					62.86								62.86
7. Households					91.61 (-0.02)	58.95 (-1.05)		1.35 (+0.02)				3.30 (-0.16)	155.21
8. Recurrent Gov't Expenditure			1.01 (+0.07)	9.82 (-0.06)	1.28 (+0.02)	2.39 (-0.02)	2.50 (+0.02)		5.54 (-0.01)				*22.54*
9. Indirect Tax	-0.19	-0.14	0.26 (+0.02)	5.62 (-0.02)									5.55
10. Gov't Investment											0.11 (+0.11)	22.82 (-0.12)	22.93
11. Private Investment						1.52 (+0.03)	13.24 (+1.24)	4.55 (+0.12)		-11.00		25.07 (+0.28)	33.38
12. Rest of World			5.35 0.34	78.55 -0.34									83.90
Totals	55.63	218.06	*43.72*	296.63	155.75	62.86	155.21	*22.54*	*5.54*	22.93	33.38	83.90	1156.15

APPENDIX 1

Source: Robinson, Cattaneo and El-Said (2001), Table 7, page 17, italics, brackets and asterixes as in SAM 1 and 2, but change is from unbalanced SAM

SAM 4 How to **Calculate GDP** in the SAM (hundreds of billions of meticais)

Receipts	Expenditures 1	2	3	4	5	6	7	8	9	10	11	12	Totals
1. Agricultural Activity							HH Cons'n						
2. Non-Agricultural Activity													
3. Agricultural Commodities							Private cons'n of mkt. goods	Gov't cons'n	-Export subsidy	Gov't invest't	Private invest't	Exports	
4. Non-agricultural commodities													
5. Factors	Value Added at Factor Cost												
6. Enterprises													
7. Households (HH)													
8. Recurrent Gov't Expenditure			Consumption Tax										
9. Indirect Tax	Output Tax		Import Tariff										
10. Gov't Investment													
11. Private Investment													
12. Rest of World			-Imports										
Totals													

APPENDIX 1

Note: Add up all light gray with black font, less export subsidies to get GDP from factor cost method, and add up all dark grey with white font, less imports to get GDP from expenditures.