Working Draft of SGM Advisory
3/24/05

1. Model Structure

- General

► The SGM model is presented as a computable general equilibrium (CGE) model. The model developers claim that "the SGM estimates a complete albeit condensed set of economic accounts." (Brenkert *et al.*, 2004, Appendix D, page 48). Indeed, all usual economic agents and markets are presented in the model: producers, consumers, government, and international trade. Investment decisions (which are based on expected profitability) play an important role in the model structure. At least three options about the expectations are presented: myopic foresight (decision makers assume that prices will remain at their current levels indefinitely into the future), expectations built upon past experiences, and perfect foresight. However, a closer inspection of the model shows that almost every component of the SGM requires a substantial improvement and further development.

► Given that the model has evolved in an ad hoc manner and that some features were developed as many as fifteen years ago (before the advent of several conceptual and operational advances), there is a need to re-examine the overall model structure. The overall structure of SGM should be re-examined with an eye toward the following:

- Decide whether to reformulate it into a more conventional CGE framework
- Improve the consistency between components in terms of levels of aggregation
- Improve compatibility in the linkages among the model components
- Streamline the overall solution algorithm
- Improve the clarity of model documentation
- Make the model more accessible to other users

► The model goes to great lengths to provide detail in certain areas, particularly in energy sectors, yet seems to neglect potentially important assumptions in other sectors affecting energy and capital demand. I worry that this detail gives a false sense of completeness with respect to the rest of the model.

► Prefatory Remarks

Fact-finding was generally hampered by the confusing presentation of the model's behavioral equations in the documentation. The root of the problem is the fact that SGM's authors have not specified its overall structure in the standard complementarity format of general equilibrium, which is the state of the technical art.

I would therefore encourage the SGM's authors to streamline their communication of the model's complex details in ways that enable the reader to quickly get a grasp of SGM's overall structure. What I would like to see is a clear, consistent and transparent presentation of:

1. The firms' cost functions and the associated dual activity levels
2. The representative consumer's expenditure function and the associated dual income level
3. The market clearance equations for goods and associated dual commodity price levels
4. The market clearance equations for primary factors and associated dual factor price levels
5. The income balance (consumption = expenditure) for the representative agent
6. The specifications of additional exogenous or price-endogenous sources of final commodity demands and supplies not within the utility function (e.g., investment, imports, exports, government activity)
7. The specifications of additional exogenous or price-endogenous sources of factor endowments (e.g., supply curves for natural resource "fixed factors" or for labor)

In my experience, these are the building blocks which ought to be at the core of any CGE model.

What worries me about the presentation of SGM's behavioral equations in the documentation is the continual references to *primal* (i.e., quantity) variables. Yet the problem of solving for general equilibrium is one of finding the equilibrium *price* vector that simultaneously supports market clearance, zero profit and income balance. I therefore find it quite difficult to relate the description of SGM's structure and operation to the numbers which it generates.

The points touched on above are symptoms of a general problem of lack of transparency. As things stand, the SGM documentation seems to be inadequate in its ability to clearly and concisely enable the reader to come to grips with the model's structure and operation. Because of this, I very much appreciate the SGM authors' willingness to make their source code available to the review panel. Being able to see the code has been illuminating and has laid to rest a number of questions I have had about the model structure and solution method. Still, this is only a partial remedy. SGM is written in a programming language few panel members might be able to adequately comprehend. This is not the model authors' problem but our own—I for instance readily admit to having forgotten most of the FORTRAN I learned as an engineer in college!

I will take this opportunity to say that if SGM was coded in GAMS (which represents the standard for CGE modeling), I know that both Sergey Paltsev and myself could easily discern how exactly the model was specified and was being made to solve. This would have tremendously expedited the process of review. This is not to say that the same level of transparency could not be achieved in either the SGM's current modeling language, or for that matter the model documentation. The key problem is that SGM is neither implemented computationally, nor documented, in a way that (i) transparently sets up and solves the optimization problems of firms and households, (ii) links the resulting demand functions through the general equilibrium conditions that underlie the circular flow of the economy to yield a closed-form expression for the aggregate excess demand correspondence, which is then (iii) solved using numerical techniques. Rather, the logical structure of SGM appears to tightly combine elements of the solution procedure with the specification of the behavioral equations of the economy's agents, so much so that it is exceedingly difficult to tell one from the other. This leaves the panel with the onerous task of discerning whether the SGM is operating according to accepted tenets of general equilibrium theory and computational modeling practice.

► **Improve the Model-Structure Documentation** . The model documentation leaves a great deal to be desired. It is voluminous but often obscure. Many aspects of the appendix are entirely unreadable. The absence of coherent documentation makes it impossible for the SAB Panel to get a firm grip on the workings of the model and get a clear picture as to whether it is consistent. A top priority should be for PNNL to substantially improve its documentation, making it more clear and coherent.

One key problem from the documentation is that it is not fully clear how the various aspects of the model – production, household demand, trade -- are connected. The readers of the appendix should be able to see the all of the excess demand equations and count them up. From there the reader should be able to trace back the equations determining each of the elements on the supply and demand side of each of the excess demand equations. This is not possible from the current appendix. It should also be explicit what are the endogenous prices (or interest rates, etc.) that clear the excess demand equations. The number of endogenous prices should match the number of excess demand equations. One cannot discern this from the current documentation.

Another key problem with the documentation is that it is not clear which of the many off/on features of the model are off or on in the central case. When are prices in the "everything else" sector exogenous, and when are they endogenous? When do land prices play a role, and when do they not? Which production sectors use Leontief technology, and which use CES? What is the central assumption about price-expectations? Which of the various technological change parameters (related to labor, energy, etc.) are activated?

The nomenclature in the documentation could be substantially improved.

- <u>Household Behavior</u>

► Include the more standard representation of consumers by using utility functions.

► Given that the model is intended to evaluate GHG mitigation policy, it is imperative that it be able to evaluate tax incidence, e.g., in terms of progressivity and regressivity.

Recommendation: The model should incorporate an income distribution component either in static terms as in work by Rose and Oladosu (2002) and Oladosu and Rose (2005) or dynamic terms as in the work by Goulder et al. (1999) and Bovenberg et al. (2005). This work should overlap with the improved specification of household behavior in the model.

► In principle I favor introducing utility functions in the model, to allow for utility-based welfare assessments. However, I think it should be kept in mind that many policy makers will be most interested in impacts on GDP, present value of consumption, aggregate profits, etc. I suggest keeping the added complexity to a minimum – just enough to yield utility-based welfare calculations.

► A usual approach to finding the equilibrium in a CGE model is a determination of prices, quantities, and income levels by solving profit maximization and consumer utility maximization problems simultaneously. The SGM model has no utility maximization, and preferences and choices of consumers are not modeled explicitly. There is no consistency in the consumption, saving, and labor supply decisions. As a result, the SGM cannot be used for welfare analysis and the stated goals of the model such as "determining the least-cost way to meet any particular emissions constraint" and "providing a measure of the overall cost of meeting an emissions target" cannot be achieved with the current version of the model.

► If the selected preferences are assumed to be identical and homothetic, which is the easy case and a common assumption, within regions, then income distribution is irrelevant to modeled consumption choices. However, if intra-generational distributional outcomes are of potential interest, then income classes should be specified.

► Household energy demands are influenced by the energy-intensity of consumer durables. Increasing energy prices should induce substitution of more energy efficient durables for less energy intensive durables (e.g., hybrids for gas hogs) given existing technologies, and in the long run should induce energy saving technological change. Public policy to influence consumer choices is important in the carbon management context. The current model does not capture these effects directly, and would not allow direct modeling of the effects of policies targeting consumer durables. Here I would propose an assessment of the merits of a more comprehensive capital-theoretic approach that could capture technological change in consumer durables. There are significant challenges; are they worth the effort?

► Household energy demands will also be affected by climate change. Even if carbon emissions captured by the model have little impact on the course of climate change over the model's horizon, the impacts of projected climate change on household energy demands should be considered. For example, some preliminary work done as part of the EPA funded Consortium for Atlantic Regional Assessment indicates that climate change in the mid-Atlantic region could, even holding energy prices constant, result in a substantial net reduction in energy use. A model that includes only the welfare costs of energy price increases, as does this one, will therefore overstate the welfare costs and understate the reductions in the quantity of energy demanded.

► A fundamental weakness of the model is that the specification of the household sector does not permit formal welfare analysis. Implementation of a utility theoretic approach that permits the computation of formal welfare measures should be a first priority.

► Consumption is not based on an integrable demand functions and cross-price elasticities are zero. This makes welfare analysis impossible. Such microeconomic consistency is less important if the goal is more accurate behavioral representation (e.g., econometrically estimated demand)—but that does not seem to be the case. As above, I would recommend comparison with other data sources, as well as switching to integrable demand functions.

- <u>Producer Behavior</u>

► Given the sectoral detail of the model and the important role of inter-fossil fuel and other types of substitution in mitigation strategies, it is imperative that the model accurately reflect key production relationships. <u>Recommendation</u>: Producer behavior should be modeled by nested CES production functions to allow differences in substitutional elasticities between pairs of inputs (this should pertain to KLEM aggregates, as it now does to sub-aggregates of energy).

► The CES is very restrictive. Move to a nested CES. Although coming up with the parameters for such models is always a challenge. I am in the process of updating my demand survey and should be able to provide some elasticities from that work in the coming months. (Dahl)

► Although the CES production function is far preferred to less flexible functional forms, such as the Leontief special case, it is still quite limiting. This is particularly true since it appears that elasticities are presumed to be constant (equal) across regions. The technology should be extended to a nested CES production function. Wherever possible, regional differences should be reflected.

► The use of non-nested CES production functions is significant restriction and degrades the quality of the results. Because input substitution is central to the costs and economic outcomes of emissions controls, I would far prefer an approach that allows analysis of substitution between key pairs of sets of inputs, rather than a structure that imposes the same elasticities on all.

► Climate change will affect the productivity and energy demands of economic sectors. These effects are ignored in the model. As with consumers, the absence of climate-economy feedbacks with distort the results. The obvious context in the model as specified in the model is agriculture forestry (the current sectoral specification does not lead to other equally obvious cases within the context of the existing model, but this is a limitation of the model). Agricultural and forest productivity, land use, and energy consumption are sure to be affected by climate change, with implications for the costs of emissions policies, and the benefits of sequestration policies. Including climate-feedbacks in these sectors should be a priority. Climate variables (soil moisture, temperature, CO2) can be (and have been) included as inputs in neoclassical production functions. Further spatial disaggregation of the regions will generally be appropriate for implementing this suggestions to capture significant intra-regional differences in climate change (e.g., southern state versus northern states and Canada in North America).

► The model lacks a theory of technological change. Inclusion of the process is admirable, but the specific procedures are not yet fully compelling. I find aspects consistent with the theory of induced innovation, but contrary to the theory of specialization. The current approach may be adequate, but I would recommend that the model documentation make a case for the treatment based on theory and evidence applicable to the problem.

► The assumption of identical substitution elasticities across regions is implausible for agriculture and forestry, given variations in the composition of output, climatic and other natural factors affecting production. I would expect there to econometric estimates that would allow differentiation.

► Profits in the SGM are defined as the difference between total revenue and the summation of all variable costs (costs exclude the only fixed input in the SGM – capital). One of the usual conditions in a CGE model is a zero profit condition, which comes from the fact that the economic theory tells us that a perfect competition in the long-run leads to zero economic profit. Increasing-returns-to-scale, decreasing-returns-to-scale, monopolistic competition, other types of imperfections which may result in positive profits in some markets are usually modeled explicitly. The SGM model does not implement the zero profit condition in its solution algorithm. It is not clear why in the SGM model the positive profits and increased investment into more profitable sectors of the economy do not result in increasing supply of these sectors and decreasing profits in the long-run as predicted by economic theory.

A very limiting feature of the SGM model is that all production functions are either Leonief or non-nested CES. The developers made a claim that the implementation of the nested CES production functions is one of the future goals. However, a closer examination of the SGM algebraic expressions and the model code raise some questions about such implementation. It should be noted that available software (for example, GAMS-MPSGE) allows modeling the nested structures with rich possibilities for substitution between inputs. Software like GAMS-MPSGE uses the fact that the Leontief production function (and Cobb-Douglas production function) is just a particular case of the CES production function. Therefore, switching from one to another does not require *any* additional programming. GAMS-MPSGE also allows to model almost any nesting structure of the production or utility functions. The choice of the modeling language (Fortran, C++) and the model structure makes it impossible to use this flexibility in the SGM model.

The meaning of the "market" prices in the SGM is not clear. Usually, a CGE model is formulated either in consumer prices or producer prices. The SGM model adjusts its "market" prices (Brenkert *et al.*, 2004, Appendix A, page 16) for prices paid (Equation 9) and prices received (Equation 13). The difference between these prices comes from taxes. It is not clear why is it not possible to solve the model in terms of producers prices or consumer prices and why it is necessary to "mark-up" prices only after solving for "market" prices.

The base year data of the SGM is based on 1990 data. The model is rich in simulating capital investment and profit. However, the documentation does not report how the actual sectoral investment and profits correspond to the SGM simulations.

► I worry that the flat CES structure for production in each sector fails to take advantage of what we know about differing substitutability among capital, labor, materials and energy. My particular concern for SGM is how it affects industrial / commercial demand for energy and capital. I would suggest comparison with economic estimates (e.g., Jorgenson and associates), historical evidence, and / or other models

- Government Behavior

► As I understand it, the SGM assumes endogenous government spending based on fixed tax rates, transfers, and deficit / surplus. While arguably the availability of tax revenue affects government spending choices, the exact mechanism is unclear.
A more standard assumption is that transfers or tax rates adjust to keep real government spending exogenous; this would be my recommendation.
Because the SGM does not focus on consumption-based welfare measures, this assumption is less critical now. But it does affect the volume of output available for investment.

- Energy Sector Specification

► I like the scope of the energy representation and presume that given all the hype, they will want to add some hydrogen technologies.

► SGM 2003 lists the following sub-sectors for electricity: oil, gas, coal, nuclear, and hydro (Brenkert *et al.*, 2004, page 15). SGM 2004 introduced new technologies such as NGCC, NGCC with Carbon Capture and Storage, CGCC, CGCC with Carbon Capture and Storage, Pulverized Coal, Pulverized Coal with Carbon Capture and Storage, Light Water Nuclear, Biomass, Geothermal, Solar, Off-shore Wind, On-shore Wind, and Municipal Solid Waste (Edmonds *et al.*, 2004, page 15). These new technologies are appropriate and often used in the models of the similar type.

- International Trade Specification

► Fixed exchange rates seem pretty restrictive and trade changes from carbon policies would seem rather important. Thus moving to a better trade representation would seem a high priority item.

► The modeling of the international trade segment should be improved. There will be important changes in commodity trading, both exogenous and endogenous to greenhouse gas policies. The SGM model should borrow from existing CGE models with endogenous trade.

► Given the importance of informing many stakeholders on the advantage of fine-tuning policies, the model output should be presented at a finer degree of resolution. Recommendation: The model results should be presented at a higher degree of resolution. Tables of results should be provided to show impacts by sector and socioeconomic group.

► The current treatment of trade as exogenous is a serious restriction. The current treatment eliminates the model's degrees of freedom to identify new trade patterns that would reduce or amplify the costs of greenhouse gas policies. The treatment also ignores a trade as fundamental force for economic change that may well be more important in the next century than climate change or greenhouse gas policies; to the extent that the costs of greenhouse policies are contingent on trade patterns, the current model may give seriously biased results. The basics of trade modeling are well-developed. Including trade should be a high priority.

► International Trade specification is very weak in the SGM model in comparison to other models of the similar type. Markets are not open across regions and trade (and/or international price) is fixed. There is no trade in intermediate goods. Carbon-intensive production reallocation and carbon leakage cannot be addressed in the SGM. Exchange rates are fixed. A consistent solution for all regions simultaneously is questionable. All of the above raise serious doubts in the use of the model for the global analysis.

► I'm still not clear on how the model treats foreign trade. However, it seems important to allow for climate policies to affect the relative costs of traded goods within any country, based on their relative carbon content. It should also influence the terms of trade. To what extent does the current model capture this?

- Technological Change

► Given that key changes in energy technologies take place often and model results are highly sensitive to this consideration, it is important that the model be more inclusive and up to date in this regard. Recommendation: The model should incorporate a wider array of energy technologies, including a more explicit description of potential back-stops.

► The specifications for technological change are quite limited, and may lead to an upward bias in long run estimates of the costs of achieving emissions targets. The method for incorporating technological change does not allow for the discovery of whole new technologies, but is limited to incremental improvements in productivity of existing technologies. The approach also does not allow for changes in substitutability-complementarity relationships among inputs. For example, as constraints on greenhouse gases become more stringent, producers might substitute away from energy-using capital inputs (e.g., fossil fuel combustion), and towards to energy-saving capital inputs (e.g., solar energy, hybrids, insulation, etc.). It would seem fairly straightforward to adopt a simplified means of incorporating a broader concept of productivity change. This could be a relatively simple, empirically based approach, using historic levels of changes in overall productivity. If nothing else, this could be implemented for examining the sensitivity of cost estimates to alternative assumptions about technological change.

► I noted only non-neutral technical change worked in the model. Although not a high priority item, I would appreciate more intuition about this factor and why labor productivity is the major driver of growth. Does that have a historical basis?

► Technological Change is modeled in the SGM through energy efficiency, labor productivity, and increase in long-run elasticity of substitution. This approach is appropriate and exists in other models of the similar type.

- <u>Dynamics; short- and long-run adjustments</u>

► Sophisticated use of vintaging model. I wonder if the short run elasticity of substitution is as high (.10) for most sectors and the long run elasticities might be greater than represented.

► The strength of the SGM is a rich representation of investment process and the usage of short-run and long-run response. An explicit representation of investment decisions bears its cost. It requires the initial data on capital stock of different vintages. For many countries investment and capital stock data are of a very poor quality or non-existent.

► 1. Putty-clay assumptions and depreciation schedule. The putty-clay assumption underlying the model, coupled with the fixed 20-year life span for all capital and 5-year time steps, seems unusual. In particular, it implies that 25% of the capital can be replaced in a given period, and applies a short-term elasticity to that capital. While I commend PNL for tackling the problem of capital vintaging, I am concerned about the particular implications of these assumptions.
I would recommend analysis to compare these assumptions to others in the literature (e.g., putty-putty or putty-semi-putty) as well as the responsiveness of the model to historic data and other models. For example, what is the path of change in inputs and overall price associated with a permanent 10% increase in primary energy prices in SGM versus other models and a crude time-series data analysis?
2. Interest rate responsiveness. It is unclear from the documentation how real investment supply and demand responds to changes in the real interest rate. This is

critically important to understanding how increased investment needs are met over time—less investment in elsewhere or decreased consumption.
Again, I would like to see this compared to other models and historical data.

► Analogous to trade is factor mobility. Modeling factor mobility is challenging, but it is an important economic phenomenon with implications for the issues at hand. Here I would propose an evaluation of options and their merits for capturing capital and labor mobility.

► I would recommend against introducing climate-change-related damages in the model. I feel that bringing in damages would introduce a huge number of new challenges. In my view, there are many insights to be offered from cost-effectiveness exercises, which the model is meant to perform. That is, a great deal of insight can come from evaluating the costs of achieving given reductions in emissions or concentrations of greenhouse gases or a subset of these gases.

- Emissions Modeling; Multiple Gases

► All Kyoto greenhouse gases (CO2, CH4, N2O, HFC, PFC, and SF6) are modeled. However, endogenous mitigations options available for CO2 emissions only. Other models of the similar type allow endogenous mitigation for other greenhouse gases. This is another area of a potential improvement for the SGM model.

► Tying agricultural emissions to agricultural output is unduly gross. This might be ok for animals, but for crops and forests, some indicators related to land and other inputs, as well as to output may be appropriate.

- Agents' Expectations, Uncertainties, etc.

► It is stated that the default assumption for expectations is that current prices are anticipated to remain fixed, but that future policy interventions are known with perfect foresight. If the future policy is known, why agents do not adjust their prices and quantities?

It is not clear why the experiment with "full perfect foresight" about future prices never been run. If the model has never been tested with full perfect foresight, why the option is mentioned in the SGM documentation?

► **Lower Priority.** I would recommend against:

a. trying to incorporate perfect foresight expectations

Incorporating profit-maximizing investment or capital-removal decisions in every sector would be extremely difficult. You would need a different shadow price for capital of every vintage in every sector. A simpler alternative would be various forms of adaptive expectations, and examining sensitivity of results to these variants.

- Representation of Climate Policies

► It is questionable that the SGM model can impose the sectoral emissions targets and sector-specific policies correctly.

A very weak representation of the international trade in the SGM model makes it questionable to use of the model for determining the world carbon price and simulating international emission trading. The Joint Implementation (JI) and Clean-Development (CDM) mechanisms of the Kyoto Protocol are not modeled in the SGM.

- Resolution/Disaggregation of Sectors, Regions, Resources

► Land markets and policies are important to emissions and sequestration given that agriculture and forestry are by far the largest land uses. Land supply is modeled as determined by a competitive land market. Yet, at least in the US and Western Europe, and I would suspect in other regions as well, government policy is an important variable in land markets. If land policies are to be considered, or if the effects of land markets on the economics of agricultural/forestry emission/sequestration polices are of interest, then a more realistic treatment of land markets should be considered.

► Biofuels – nonexistent – but related to ag, forests, and land markets.

► Given the importance of informing many stakeholders on the advantage of fine-tuning policies, the model output should be presented at a finer degree of resolution. Recommendation: The model results should be presented at a higher degree of resolution. Tables of results should be provided to show impacts by sector and socioeconomic group.

► Clarify Treatment of "Everything Else Sector" Prices

The October 2004 "Overview" paper indicates that the EES price is the numeraire. At the same time, the detailed appendix indicates that this price can either be specified exogenously or allowed to be endogenous. I'm puzzled about this. If the EES good is the numeraire, its price must be exogenous in nominal terms. Is the appendix also suggesting it might be exogenous in real terms, that is, relative to a price index of commodities? What is the meaning here? How could the model fix the real price of EES?

2. Model Inputs

- Data

► The baseyear of the SGM is 1990. Many counties and regions have had substantial changes in economic accounts, market structure and sectoral shares since 1990. The baseyear should be updated. The modelers should also analyze existing economic-energy datasets, such as GTAP, and compare their data with alternative sources of the data.

► Given the prominence of the role of energy as the major source of GHG emissions and the major focus of mitigation policy, it is imperative that the energy data be current and accurate. Recommendation: Annual updates of energy data should be incorporated (including fossil fuel reserves, energy commodity prices, and current and future technology costs). Also, improve on the use of the standard ratio estimator approach to specification of energy flows in the hybrid I-O table (this results in a uniform price of energy to all sectors, and thus omits differentials due to variations in quality, location, and technology). Primary or government tabulated data should be used as much as possible for major energy using sectors.

- Parameters

► The documentation does not make it clear how any of the parameters of the model are determined, nor are there confidence intervals on any parameters. This could be enormously important. At a minimum, some sensitivity analysis should be done to find how sensitive the results are to alternative reasonable values for the various parameters. For example, one could do model runs doubling and halving all elasticities. If the results are found to be particularly sensitive to particular parameters, then it would be worth while to spend considerable effort to identify narrow down the reasonable range for these parameters.

► Every application of the model should include significant sensitivity analysis. The huge number of parameters in the model leaves one wondering whether any desired policy result could be generated with "fine-tuning" of hidden parameters. Explicit sensitivity analysis is crucial.

► **Indicate Empirical Basis for Parameters.**

The empirical basis for key parameters is not clear. In particular:

-- the putty/semi-putty nature of production is captured by using successively lower elasticities of substitution with successively older vintages of capital. How is this calibrated? That is, how is the time-profile of elasticities of substitution determined?

-- what is the empirical basis for the choices of the technological change parameters?

The data appendix should indicate the central case values for every parameter used in the model. This should also be a top-priority item.

3. Model Outputs

- Reporting of Prices, Quantities

- Measurement of Costs; Welfare Measures

► Consumer welfare measures should be included. Several alternative approaches are possible. First, one could assume a "representative" utility function, and provide measures of compensating variation (CV) and/or equivalent variation (EV). If this approach is used, it would be best to create utility functions for different groups (e.g., for different income groups).

I do not favor this approach, because of the limitations that are required for specifying the utility function in order to have a representative consumer. . Theory tells us that aggregate market demand function should not be integrable except under very restrictive conditions. In order to have a representative utility function underlie market demand functions, ALL individuals in the economy must have demand functions (and equivalently, the indirect utility functions) that are of the Gorman form:

$$x_j^i = \alpha^i(\mathbf{p}) + \beta(\mathbf{p})m$$

where x_j^i is the demand function for commodity j by consumer i; $\alpha^i(.)$ is a function that can vary across consumers, and is a function of the price vector, **p**, but is independent of income; $\beta(.)$ is a function the vector of prices, but must be the same for all consumers; and m is income. This means an additional dollar of income results in the same change in quantity demanded for all consumers, irrespective of their attributes, such as income, age, gender, location of residence (Hawaii vs. Alaska), etc. This is very restrictive, as it does not allow for the existence of luxury goods or necessities.

I believe that a better approach is to use a more realistic, empirically-based approach. That is, aggregate market demand functions are based on the best empirical formulations that can be identified, without restricting them to be of the Gorman form. One could then either use consumer surplus (CS) as an approximation to CV or EV. Since CS always falls in between CV and EV, this might not be too bad, especially if we don't have a distinct preference between CV and EV.

Alternatively the model could base welfare measures on Paasche and Lespeyres compensation measures. These have the advantage not being dependent upon the functional form that is selected for demand, unlike CV, EV or CS, which can be extremely sensitive to the selection of functional form (log vs. linear). Also, since one of the two measures (Paasche or Lespeyres) will always overstate welfare effects, and the other will always understate welfare effects, one could use the mean (or geometric mean) of the two as a point estimate of welfare.

► If welfare effects are considered, distributional impacts could be an important consideration. Consumers could be differentiated by income, demographics, residence, etc.

► Carbon price is not a good measure of welfare cost of a carbon policy. Interactions with other pre-existing distortions play an important role in determining economic costs. Change in welfare (or change in consumption) is a better indicator of economic cost of a policy. Representation of taxes and representation of final demand in the SGM model does not allow to calculate interactions with pre-existing distortions and welfare costs of climate policy (which is one of the stated objectives of the SGM).

- Treatment of Uncertainties in Outcomes; Sensitivity Analysis

► All aspects of the model should be validated against historic data to the extent feasible.

4. Solution Method

► It is not clear why GAMS solvers (which are well-known and well-established in the CGE modeling community) are not used for the SGM. On page 38 of the model overview (Edmonds *et al.*, 2004) it is stated that the SGM is expected to move from Fortran to C++. It is not clear what are the advantages of C++ for economic modeling. Rather, a switch to the GAMS modeling language would increase the model transparency for other CGE modelers and would allow the SGM modelers to implement many techniques specifically developed for a CGE model solution.

A very important check that the model is consistent with the underlying data is a benchmark check with a zero limit on iterations. It is not clear if such a check has been performed. As it has already been mentioned, it is not clear if the SGM formulation is adequate for a global CGE model. It is not clear if all necessary conditions are satisfied on a production side of the economy. Utility maximization is not represented. International trade representation is extremely limited. It looks like the SGM solution method might be a major obstacle in the future development of the detailed representation of the household sector and international trade.

► **Check for Walras's Law**

Given the great complexity of the model, it is important to check that no flows of income or expenditure are lost from the system. In this regard, Walras's Law should be tested at every iteration of the solution algorithm (i.e., out of equilibrium). The computer model should engage this feature always. This seems crucial to checking the consistency of the existing model structure.

5. Other

►

Charge Questions

I shall take the liberty of re-interpreting the charge questions in a non-binary form, and express my summary answer in a Likert scale. A score of 5 means I strongly agree with the proposition in the question, 3 means that my feeling is equivocal, while 1 indicates that I strongly disagree.

II. Are the model's structure and fundamental assumptions reasonable and consistent with economic theory?

2a. Are the number and type of agents in the model (firms, households, governments, and regions) appropriate for the problem, and are they adequately represented?

Score: 5 on the first part of the question, 2 on the second part of the question.

The overall structure of the economic agents in the model is appropriate. Several aspects of their representation appear questionable, however. A particular problem is the representation of the household and associated final demands in each region. There is no well-defined utility function (e.g., a CES aggregate of the use of commodities for consumption), and this shortcoming limits SGM's potential usefulness for making welfare assessments. I deal with this issue in greater detail below. Moreover, the description on pp. 66-79 of the documentation of how of the demands for the various categories of investment are calculated is very complicated, leaving me unable to adequately judge its basis in economic theory.

This is a common theme throughout my comments. Even where there is sound theoretical guidance as to how to correctly model an economic process, the SGM model equations seem to be documented in a manner which is needlessly obfuscated. Maybe I am being hyper-critical on this point, but I wonder whether the SGM documentation—and the model itself—could not be improved by replacing some of the existing formulations with more parsimonious and transparent assumptions, perhaps borrowed from the experience of other modeling studies. This feeling stems from my considerable frustration at finding it impossible to gain a firm grasp of SGM's structure and operation of from the documentation. While I realize that the model equations seem to generate tractable numerical behavior, the lack of clear and transparent linkages between the model's functioning and the results it generates threatens to undermine the SGM's credibility as a tool for rational, transparent analysis.

2b. Is each agent's optimization problem appropriately specified, and the implied behavioral equations used correctly?

Score: 3 on the first part of the question, 2 on the second part of the question.

The firms' production functions are CES, which is straightforward. The solution to the each firm's intra-temporal profit maximization problem is a dual cost function whose arguments are the firm's activity level and the prices of intermediate material and primary factor inputs. Chapter 3 of the documentation does not explicitly set up and solve the profit maximization problem, and I could not find in the documentation readily-identifiable expressions for the firms' CES cost functions and associated input demands. Instead, eqs. (3) and (5) specify production in *primal* terms. While this not a problem, it does raise the question of how the primal production function, which gives the level of firms' output or activity, is employed to recover the price of output and the demands for inputs.

However, the determination of prices in Chapter 4 is difficult to understand from a strict production theoretic standpoint. The crucial equation in this regard is eq. (8), which appears to specify the output price as the sum of the sum of the demands for inputs net of the value of tax payments by the firm, divided by the sum of the demands for inputs. If I understand this scheme correctly, it is a kind of tatonnement mechanism in which firm's output prices attributed to their scaled excess profits. The issue is that as far as I can tell the documentation makes no reference to a recognizable CES unit cost function.

Consider a situation in which there are i firms, each operating using CES technology to produce a single output q_i using a single intermediate input, say energy e_i, and inputs of primary factors labor l_i and capital k_i. Let the elasticity of substitution be σ_i, and let the technical coefficients of the production function, calculated as the input cost shares from macroeconomic data be $a_{e,i}$, $a_{l,i}$ and $a_{k,i}$. By eq. (5), SGM correctly computes the firm's activity level as

(ISW-1) $$q_i = \left(a_{k,i} k_i^{\frac{\sigma_i - 1}{\sigma_i}} + a_{l,i} l_i^{\frac{\sigma_i - 1}{\sigma_i}} + a_{e,i} e_i^{\frac{\sigma_i - 1}{\sigma_i}} \right)^{\frac{\sigma_i}{\sigma_i - 1}}.$$

But by eq. (8), SGM approximates the price of output, p_i, using only primal variables and the tax rate on output, τ_i:

(ISW-2) $$p_i = \frac{k_i + l_i + e_i - \tau_i q_i}{k_i + l_i + e_i}.$$

It is conceivable that I have misunderstood eq. (8), so that the variable X that the documentation uses to denote inputs are in fact input values, i.e., input quantities which are scaled by their prices. Denoting the wage by w, the rental rate of capital by r, and the price of energy by ε yields:

(ISW-3) $$p_i = \frac{rk_i + wl_i + \varepsilon e_i - \tau_i q_i}{rk_i + wl_i + \varepsilon e_i}.$$

But this does not change the point that neither eqs. (ISW-2) nor (ISW-3) accurately represent the unit cost of output for a CES technology. Rather, they look more like the inverses of the benchmark tax shares than a price. The correct specification of the unit cost function is well known:

(ISW-4) $$p_i \le \tau_i + \left(a_{k,i}^{\sigma_i} r^{1-\sigma_i} + a_{l,i}^{\sigma_i} w^{1-\sigma_i} + a_{e,i} \varepsilon^{1-\sigma_i} \right)^{\frac{1}{1-\sigma_i}},$$

but this formulation assumes that the technical coefficients are constant parameters computed from the SAM. However, eqs. (18), (22) and (23) in Chapter 5 of the documentation specify these coefficients as complicated nonlinear functions of prices.

These expressions are very reminiscent of CES demand functions, e.g. the demand for energy associated with eq. (ISW-4):

(ISW-5) $$e_i = q_i \left(a_{e,i} (p_i - \tau_i) / \varepsilon \right)^{\sigma_i} .$$

But the documentation curiously refers to the analogue of this expression as an input-output coefficient, *not* a commodity demand, and is it not clear from the equation listing at the end of the documentation whether this coefficient is used as an input to any other equation in the model.

The symmetry between this point and my observations about eqs. (ISW-2) nor (ISW-3) above is puzzling. It is obvious that substituting the demand for each input analogous to eq. (ISW-5) into the production function (ISW-1) will yield the unit cost function (ISW-4). But it is not clear whether this is what SGM's authors intended to achieve.

I am unsure how robust the analogues of eqs. (ISW-2) or (ISW-3) in the documentation are in terms of their converge to an equilibrium price vector under an iterative scheme. [Perhaps Glenn Harrison can help clarify this point.] Reasoning intuitively, however, SGM's tatonnement algorithm will squeeze the values of these parameters toward zero at each successive iteration, which will make the magnitude of the scaled excess demands in each market look like the marginals of the quantities, in which case what SGM is actually solving is the primal problem.

However, this desired result only arises once the algorithm does in fact converge. In general the fact that all markets clear does not *per se* indicate the existence of equilibrium. It is well known that the quantity of a good supplied equals the quantity demanded even in *dis*equilibrium, by the mere fact that under an accurate accounting system product will be conserved, with the sum of sources and sinks being zero. General equilibrium imposes stricter conditions on prices, however, with (ISW-4) guaranteeing that firms make zero profits in equilibrium, and demands and supplies specified according to well-posed functions of prices, such as eq. (ISW-5), being equalized. The main reason I raise this point is the magnitudes of the excess demands at the final iteration documented in Table 4 (pp. 107-108), which is for me a source of considerable unease. I have not run SGM at tighter tolerances to verify whether this behavior is an artifact of the model structure or the solution procedure. This is something I plan to do.

What I take away from all this is that the robustness of SGM's operation might benefit from more careful attention to representation of the *dual* problem, as it is not immediately apparent from either the documentation or the code that this in currently done in a way that is fully consistent with the accepted tenets of production theory.

My impressions are similar when it comes to the representative consumer's utility-maximization problem, only here instead of recognizable utility function such as CES or Cobb-Douglas, demand functions for goods of the (Edmonds-Reilly 1985) variety are specified directly (eq. 185). This is definitely a problem, because the consumer's utility maximization implies an associated dual expenditure minimization problem, from which an expenditure function can be derived and used to derive the change in expenditure due to the imposition of policy (e.g., a carbon tax), which is the generally-accepted measure of the change in aggregate economic welfare induced by the policy. My sense is the fact that eq. 185 is non-homothetic and unable to support welfare analysis is a serious limitation.

I would be interested to hear Michael Hanneman's opinion on whether it is still possible to use the demands specified in the documentation to recover a utility function which may be useful for aggregate welfare analysis. As it stands, the model computes total consumption expenditure, so as a practical matter it is possible to calculate equivalent variation. However, my knowledge of applied welfare analysis is insufficient to make a recommendation in this regard.

Additional shortcomings on the household side are, first, the apparent lack of a recognizable market clearance condition for commodities which explicitly includes the demands by households (see my response to question 2(d) below), and second, the confusing specification of the household income-expenditure balance condition in eq. (191). Regarding the latter point, the documentation of the households' consumption problem is difficult to understand. A case in point is eq. (194), which declares the variable *DemTot* to be the "Total household demand for variable factor inputs". As far as I can tell, *DemTot* is actually the value of the total demand by *firms* for the households' aggregate endowment of variable factors, or, more simply, aggregate gross factor returns. I cannot understand how this variable differs from "household income", *Pinc*, or how eq. (191) thereupon represents the simple intuition that for i goods, with prices p_i and a quantities of consumption c_i,

$$\text{Factor Income} + \text{Tax Revenue} = \sum_i p_i c_i \ .$$

The fact that the documentation does not coherently and transparently specify this most basic of equilibrium conditions is just one of the more glaring instances of the general lack of transparency that bedevils fact-finding efforts. Fortunately, the issue of clearer documentation can be easily remedied. The real underlying question is whether these processes are treated coherently within the model.

2d. Are the market-clearing equations appropriate?

Score: 3

There is little specific discussion of market clearance conditions in either the SGM documentation or the model code listing. Eqs. (1) and (2), which specify the row- and column-balance conditions in the SAM which are equivalent to market clearance and zero profit, respectively, are straightforward. However, as with much of SGM, these are *primal* conditions. The issue of appropriateness turns on whether each of the terms of the right-hand side of these expression is consistently expressed in terms of the *dual*, i.e., as functions of the prices of commodities and primary factors, firms' activity levels and the consumer's income. To understand whether this is in fact the case, one must inspect the demand functions for commodities by firms and the household. But, as I discuss in my response to question 2(b) above, the closest thing in the SGM documentation to demand functions for intermediate commodity are the input-output coefficients in eqs. (18), (22) and (23) of Chapter 5. Thus, it is difficult to discern what the actual demand functions are, and whether they are linked within the model to the inter-industry demand variables in eq. (1). Likewise, although price-responsive household demands are clearly stated in eq. (185), it is difficult to understand from the documentation whether the variable with which these are associated, ED_i is actually linked to the corresponding household demand variable X in eq. (1).

2e. Does the model satisfy basic tests for consistency with general equilibrium theory?

Score: 2

The SGM works in practice. However, in light of my response to question 2(b), I have questions as to whether it operates in a manner consistent with general equilibrium theory, at least as in terms of the clear specification, expression and numerical solution of its general equilibrium problem in the now-standard complementarity format. Again, the biggest problem from a fact-finding point of view is that it is virtually impossible to tell from either the documentation or the listing of computer code how exactly the excess demand correspondence of the economy is specified as a function of prices.

This is particularly unfortunate, because the SGM actually seems to be a model which is structurally simple and well-defined. The main issue is that it is just does not seem to be either documented or coded in a way that separates the essence of the general equilibrium problem from the details of the solution procedure.

Going forward, then, a key recommendation would be to streamline the documentation in conjunction with re-casting the model code in a form that first clearly specifies the conditions of market clearance, zero profit and income balance, as well as their associated dual variables, second, presents analytical expressions for the excess demand correspondence of the economy, and finally applies the solution algorithm to this last.

A specific example will clarify this point. Sue Wing (2004) derives the excess demand correspondence for the general equilibrium of an economy whose institutional setup is as follows. There are N firms (indexed by j), each of which uses Cobb-Douglas technology to produce a single output good (indexed by i). Households are represented by a single representative agent who has Cobb-Douglas preferences and is endowed with quantities of f distinct types of primary factors.

The representative agent maximizes utility U by choosing levels of consumption c, subject to the constraints of her income, m, ruling commodity prices p. The agent may also demand goods and services for purposes other than consumption—in the present example saving s—which are assumed to be exogenous and constant. The agent's problem is thus:

(ISW-6) $$\max_{c_i} U = A_C c_1^{\alpha_1} c_2^{\alpha_2} \ldots c_N^{\alpha_N} = A_C \prod_{i=1}^{N} c_i^{\alpha_i} \text{ subject to}$$

$$m = \sum_{i=1}^{N} p_i (c_i + s_i) .$$

with $\alpha_1 + \ldots + \alpha_N = 1$. The solution to this problem is the representative agent's demand function for the consumption of the i^{th} commodity:

(ISW-7) $$c_i = \alpha_i \frac{\left(m - \sum_{i=1}^{N} p_i s_i \right)}{p_i}.$$

Each producer maximizes profit π by choosing levels of N intermediate inputs x and F primary factors v to produce output y, subject to the constraint of its production technology ϕ. The j^{th} producer's problem is thus:

(ISW-8) $$\max_{x_{ij}, v_{fj}} \pi_j = p_j y_j - \sum_{i=1}^{N} p_i x_{ij} - \sum_{f=1}^{F} w_f v_{fj} \text{ subject to}$$

$$y_j = A_j \left(x_1^{\beta_1} x_2^{\beta_2} \ldots x_N^{\beta_N} \right) \left(v_1^{\gamma_1} v_2^{\gamma_2} \ldots v_F^{\gamma_N} \right) = A_j \prod_{i=1}^{N} x_{ij}^{\beta_{ij}} \prod_{f=1}^{F} v_{fj}^{\gamma_{fj}}.$$

with $\beta_{1j} + \ldots + \beta_{Nj} + \gamma_{1j} + \ldots + \gamma_{Nj} = 1$. The solution to this problem is producer j's demands for intermediate inputs of commodities:

(ISW-9) $$x_{ij} = \beta_{ij} \frac{p_j y_j}{p_i},$$

and for primary factor inputs:

(ISW-10) $$v_{fj} = \gamma_{fj} \frac{p_j y_j}{w_f}.$$

The conditions for general equilibrium are as follows. Market clearance specifies that the quantity of each commodity produced must equal the sum of the quantities of that commodity demanded by the j producers in the economy as an intermediate input to production, and by the representative agent as an input to consumption and saving activities:

(ISW-11) $$y_i = \sum_{j=1}^{N} x_{ij} + c_i + s_i.$$

The quantities of primary factor f used by all producers must sum to the representative agent's endowment of that factor, V_f:

(ISW-12) $$V_f = \sum_{j=1}^{N} v_{fj}.$$

Zero profit implies that the value of output generated by producer j must equal the sum of the values of the inputs of the i intermediate goods and f primary factors employed in production. This condition is derived by setting the right-hand side of eq. (ISW-8) to zero and rearranging:

(ISW-13) $$p_j y_j = \sum_{i=1}^{N} p_i x_{ij} - \sum_{f=1}^{F} w_f v_{fj}.$$

Income balance implies that the income of the representative agent must equal the value of producers' payments to her for the use of the primary factors that she owns and hires out:

(ISW-14) $$m = \sum_{f=1}^{F} w_f V_f \ .$$

I find it hard to see how the preceding four expressions would not appear at the core of *any* CGE model, including SGM.

To actually compute the solution to such a model, the endowment of the representative agent needs to be fixed, and substitutions made of eqs. (ISW-7) and (ISW-9) into eq. (ISW-11), and eq. (ISW-10) into eq. (ISW-12). The result will be two excess demand vectors that define the divergence Δ^C between supply and demand in the market for each commodity and the divergence Δ^F between supply and demand in the market for each primary factor. The absolute values of both of these sets of differences are minimized to zero in general equilibrium. There are *N* such excess demand equations for the commodity market:

(ISW-15) $$\Delta_i^C = \sum_{j=1}^{N} \beta_{ij} p_j y_j + \alpha_i \left(\sum_{f=1}^{F} w_f V_f - \sum_{j=1}^{N} p_j s_j \right) + p_i s_i - p_i y_i$$

and *F* equations for the factor market:

(ISW-16) $$\Delta_f^F = \sum_{j=1}^{N} \gamma_{fj} \frac{p_j y_j}{w_f} - V_f \ .$$

The zero profit condition implies that the absolute value of producers' profits is minimized to zero in general equilibrium. Thus, substituting eqs. (ISW-9) and (ISW-10) into the production function allows us to write *N* pseudo-excess demand functions that specify the per-unit excess profit (i.e. excess of price over unit cost) Δ^π in each industry sector:

(ISW-17) $$\Delta_j^\pi = p_j - A_j \prod_{i=1}^{N} \left(p_i / \beta_{ij} \right)^{\beta_{ij}} \prod_{f=1}^{F} \left(w_f / \gamma_{fj} \right)^{\gamma_{fj}} \ .$$

Finally, the income balance condition (ISW-14) can be re-written in terms of the excess of income over returns to the agent's endowment of primary factors, Δ^m:

(ISW-18) $$\Delta^m = \sum_{f=1}^{F} w_f V_f - m \ .$$

The computation of general equilibrium is thus the joint minimization of Δ^C, Δ^F, Δ^π and Δ^m.

A solution to the general equilibrium problem may be found using eqs. (ISW-15)-(ISW-18). These expressions form a system of $2N + F$ equations in $2N + F$ unknowns: an *N*-vector of industry output- or "activity" levels $\mathbf{y} = [y_1, \cdots, y_N]$, an *N*-vector of commodity prices $\mathbf{p} = [p_1, \cdots, p_N]$, an *F*-vector of primary factor prices $\mathbf{w} = [w_1, \cdots, w_F]$ and a scalar income level *m*. The problem of finding the vector of activity levels and prices that supports general equilibrium therefore consists of choosing values for these variables to solve the problem

(ISW-19) $$\xi(\mathbf{z}) = \mathbf{0} \ ,$$

in which $\mathbf{z} = [\mathbf{p}, \mathbf{w}, \mathbf{y}, m]'$ is the vector of stacked prices, activity levels and level of income, and $\xi(\cdot) = [\mathbf{\Delta}^C, \mathbf{\Delta}^F, \mathbf{\Delta}^\pi, \Delta^m]'$ is the system of stacked pseudo-excess demand equations, which forms the production-inclusive pseudo-excess demand correspondence of the economy.

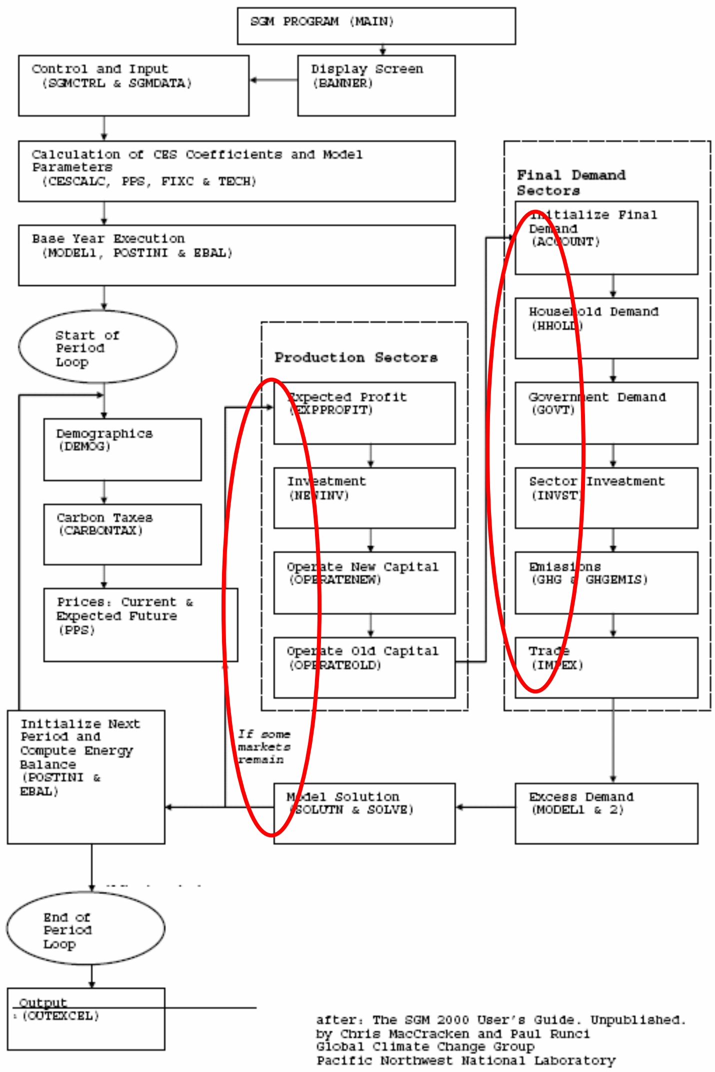
Eq. (ISW-19) is the expression of general equilibrium in a complementarity format, so named because of the important complementarity that exists between prices and excess demands, and between activity levels and profits, and that is a critical feature of general equilibrium. For the equilibrium above to be economically meaningful, prices, activity levels and income are all positive and finite ($\mathbf{0} \leq \mathbf{z} < \infty$). In the limit, as **z** approaches zero, eqs. (ISW-15), (ISW-17) and (ISW-18) all approach zero, and eq. (ISW-16) tends to $-V_f$, implying that $\xi(\mathbf{0}) = [\mathbf{0}, -\mathbf{V}, \mathbf{0}, 0]' \leq \mathbf{0}$. If **z*** is a vector of prices and activity and income levels that supports general equilibrium, it must be the case that $\mathbf{0} \leq \mathbf{z}^*$ and $\xi(\mathbf{z}^*) = \mathbf{0}$. Thus, the problem in eq. (ISW-19) may be compactly re-specified as one of finding

(ISW-20) $\quad \mathbf{z} \geq \mathbf{0} \quad$ subject to $\quad \xi(\mathbf{z}) \geq \mathbf{0}, \quad \mathbf{z}'\xi(\mathbf{z}) = \mathbf{0}$,

which is a mathematical statement of Walras' Law (Varian 1992: 343).[1]

[1] Varian, H. (1992). Microeconomic Analysis, W.W. Norton.

Figure 1 A Basic Flowchart of the SGM



It would be ideal if both the SGM documentation and the computer code could lay out the format of the model in as transparent a way as I have just done, and only then discuss methods for solving the general equilibrium problem in (ISW-20). This would definitely be my recommendation for improving the overall implementation of the model. With such an improvement, the SGM computer code could algebraically specify the excess demand correspondence $\xi(\mathbf{z})$, and then employ the Newton-type algorithms discussed in Appendix D to solve for general equilibrium.

On this point, I fully acknowledge the SGM team expertise, and defer to their particular, specialized expertise in FORTRAN. Nevertheless, in Figure 1 of the SGM program flow chart (p. 205), reproduced above, my sense is that in terms of model specification there is scope for efficiencies in the areas indicated by the bold ovals.

Perhaps the key feature of SGM's solution process that only emerges from reading the model code listing is that the model's tatonnement algorithm operates in a market-by-market fashion. It is therefore necessary to *sequentially* process the subroutines that determine the excess demands in each market, going around the main intra-period program loop, iterating to solution one market at a time in a round-robin fashion. My impression is that it is the need to conform to this logical structure which is at the root of all the complexity in the specification of the equilibrium problem in both the code and the documentation. This approach may be contrasted with the complementarity format of equilibrium which I define above, in which the *entire* excess demand correspondence is specified in a *single* step, and then an iterations performed on the space of $2N + F$ prices and activity levels, **z**.

I get the sense is that it is possible in FORTRAN to leave the existing structure of SGM unchanged, but replace the existing set of subroutines with an algebraic expression of the general equilibrium problem in the form of eq. (ISW-20), and then employ a modified tatonnement or Kimbell-Harrison (1986) Factor-Price Revision Rule algorithm to iterate to equilibrium.[2] Although these kinds of solution schemes are no longer state of the art, my sense it that it would nonetheless constitute a definite improvement over SGM's existing "sequential markets" computational implementation, and also tremendously streamline the formulation and expression of the model, making SGM easier to document in ways most economists can understand.

One option for a more up-to-date solution method is the sequence of linear complimentarity programs (SLCP) algorithm employed by Mathiesen (1985a,b) and Rutherford (1987) to solve CGE models. The SGM's numerical calibration of the coefficients of cost and expenditure functions appears to be correct. The existing procedure can be retained to transform (ISW-20) into a square system of numerical equations known as a *nonlinear complementarity problem* or NCP (Ferris and Pang, 1997), which may be solved using the SLCP algorithm, which is similar to a Newton-type steepest-descent methods. SLCP iteratively solves a sequence of linear complementarity problems or LCPs (Cottle et al 1992), each of which is a first-order Taylor series expansion of the non-linear function ξ. The LCP solved at each iteration is thus one of finding

(ISW-21) $\quad \mathbf{z} \geq \mathbf{0} \quad$ subject to $\quad \overline{\mathbf{q}} + \overline{\mathbf{M}}\mathbf{z} \geq \mathbf{0}, \quad \mathbf{z}'(\overline{\mathbf{q}} + \overline{\mathbf{M}}\mathbf{z}) = \mathbf{0}$,

where, linearizing ξ around $\mathbf{z}_{(k)}$, the state vector of prices, activity levels and income at iteration k, the matrices in (ISW-21) are given by $\overline{\mathbf{q}}(\mathbf{z}_{(k)}) = \nabla\xi(\mathbf{z}_{(k)})\mathbf{z}_{(k)} - \xi(\mathbf{z}_{(k)})$ and $\overline{\mathbf{M}}(\mathbf{z}_{(k)}) = \nabla\xi(\mathbf{z}_{(k)})$. The value of **z** that solves problem (ISW-21) at the k^{th} iteration is $\mathbf{z}^*_{(k)}$. Then, starting from an initial point $\mathbf{z}_{(0)}$, the algorithm generates a sequences of vectors **z**, updated according to the line search:

(ISW-22) $\quad \mathbf{z}_{(k+1)} = \mu_{(k)}\mathbf{z}^*_{(k)} + (1 - \mu_{(k)})\mathbf{z}_{(k)}$,

where the parameter $\mu_{(k)}$ controls the length of the forward step in **z** that the model takes at each iteration. The convergence criterion for the algorithm consisting of eqs. (ISW-21)

[2] Kimbell, L. and G.W. Harrison (1986). On the Solution of General Equilibrium Models, Economic Modeling 3: 197-212 < http://www.bus.ucf.edu/gharrison/papers/On%20the%20Solution%20of%20General%20Equilibrium%20Models.pdf >.

and (ISW-22) is just the numerical analogue of eq. (ISW-19): $\left\|\xi(\mathbf{z}_{(k)})\right\| < \varpi$, in which the scalar parameter ϖ is the maximum tolerance level of excess demands, profits, or income at which the algorithm is deemed by the analyst to have converged to an equilibrium.

Although straightforward from a computational standpoint, implementation of this algorithm requires some effort to algebraically linearize the matrix ξ, which is a straightforward but tedious task. The feasibility of this method of solution depends on the ability to solve the LCP in (ISW-21). For the large-scale, highly nonlinear system of equations that defines CGE models such as SGM, the authors might want to investigate computational experience with the homotopy methods in freeware optimization code libraries such as HOMPACK or NLEQ.

4*a*. One of the important features of the SGM is its ability to track energy balances throughout the model's time horizon. This is accomplished, in large part, by the creation of hybrid input-output tables, which combine energy balance data from the International Energy Agency (IEA) with national economic input-output data. Was the merging of these two data sets reasonable to create the hybrid input-output tables? Are these hybrid input-output tables logical and useful tools?

Score: 2

From a conceptual standpoint the merging of the two datasets is logical, reasonable and to some degree necessary. Often, the simplest solution is not to modify the SAM, but to multiply the output of each primary energy supply sector by an energy-output conversion factor. The value of this coefficient is the aggregate use of that type of energy in exajoules or quadrillion BTUs divided by the value of the gross output of that energy industry in the SAM. Where this procedure founders is in the electricity sector, as to avoid double counting it is necessary to separately resolve primary electric generation activities (nuclear, hydro and renewables) from generation which uses fossil fuels. Note that while this is an issue for energy accounting, it is inconsequential for tracking flows of carbon. Indeed, the stoichiometric coefficients tabulated in the SGM Appendix track carbon emissions upstream from the fossil-fuel supply sectors.

Speaking to the question of usefulness, it is difficult to understand from the documentation how the adjustment of the I-O tables in SGM provides a significant advantage over the much simpler, coefficient-based approach described above. The electric power sector is disaggregated into discrete technologies, which facilitates accounting for primary electricity. In my view, a general rule for CGE modelers should be to only adjust the input-output structure of the SAM where it is absolutely necessary, leaving the compilation of economic accounts to the statisticians whose substantive area of expertise it is to develop social accounting matrices. This was the reason for the creation of the Global Trade Analysis Project (GTAP) in the first place. The GTAP database provides a set of national and regional social accounting matrices which are consistently linked with inter-regional trade flows. Furthermore, thanks to several man-years of work by other modelers such as Dominique Van Der Mensbrugghe, Tom

Rutherford and Mustafa Babiker, these economic data are now consistently merged with the IEA Energy Balances as part of each new GTAP release.

My comments should in no way be seen as impugning what must have been a tremendous amount of blood, sweat and tears expended by the SGM's authors to create what was essentially their own version of the GTAP in the early 1990s—at a time when no such database was available, commercially or otherwise. However, by the same token, it is incontrovertible that the 1990 database used in the SGM is showing its age and has been superseded by GTAP. I would therefore strongly encourage the SGM's authors to make a concerted effort to shift the model fully to the GTAP dataset. The just-released GTAP version 6 has a base-year of 2001, which will bring SGM to absolutely up to date in terms of the structural characteristics of the economies which its authors wish to analyze. Furthermore, such a move will bring the added benefit of being able to automatically adjust the SGM's structure to endogenously simulate international commodity trade flows, the absence of which is one of SGM's biggest limitations.

4d. Very often, the SGM is run using energy quantity and price projections from the NEMS outputs in the Department of Energy's Annual Energy Outlook (through 2025) as inputs to SGM and then extended to 2050. Is the use of NEMS model outputs as SGM input data an appropriate modeling strategy?

Score: 3

I am equivocal on this point. This exercise may or may not be a reasonable thing to do—but I have some concerns given that SGM is a price-endogenous model. The repercussions depend on whether prices and quantities are actually fixed according to the trajectories generated by NEMS, or whether SGM's input parameters are adjusted to make the trajectory of its unfettered equilibria consistent with NEMS' results. Although the latter technique is standard in model inter-comparison studies (e.g., Viguier et al 2003), the former can have potentially serious negative consequences depending on the extent to which the business-as-usual (BaU) unconstrained forecasts of energy use and energy prices of NEMS and SGM diverge from one another.[3]

Where the divergence of the models' baseline solutions is large, the imposition of energy price and quantity trajectories in SGM is equivalent to introducing a series of energy-market distortions. Relative to the undistorted equilibrium solution of the model, these price and quantity change induce an array of substitution responses in forward markets for energy commodities, which ultimately affect factor prices, income and consumers' welfare along SGM's baseline path. The general caution here is that the gain from being able to coordinate SGM's analyses with those of other agencies should always be carefully weighed against the sacrifice of potentially biasing one's results for the sake of consistency.

[3] Viguier, L., M. Babiker and J. Reilly (2003). The Costs of the Kyoto Protocol in the European Union, Energy Policy 31(5): 459-481.

The problem stems from the fact that NEMS is a partial equilibrium energy model with relatively inelastic demand for energy goods, but relatively elastic supply responses due to its highly detailed representation of the energy system. By comparison, I would guess that SGM's supply response is less elastic and its demand response is much more elastic. Thus, there is a fundamental inconsistency between the structure and behavior of the two models, which is the natural consequence of their being used for two very different analytical purposes.

The differences in the structure and scope of the two types of models imply that each has a comparative advantage in analyzing different research questions in energy and climate policy. Top-down models such as SGM are best employed to assess the macroeconomic costs of GHG emission limits and the feedbacks of these policies on prices, commodity and factor substitution, income and economic welfare. Bottom-up models such as NEMS are used to investigate the impacts of GHG emissions constraints on the portfolio of technologies that make up the supply and demand components of the energy system, in order to identify low-cost abatement opportunities or design technology-based subsidies or emission standards. While the analytical contributions of these two approaches are complementary, their results are often difficult to reconcile, precisely because bottom-up models like NEMS only imperfectly capture the effects of changes in supply composition on wages and capital rental rates, and the consequences for income and final energy demands.

In sum, imposing the results of a partial equilibrium model on the general equilibrium system of a top-down model seems wrong, because the latter logically encompasses all of the results of the former. This problem is not merely conceptual but it is rooted in the data: energy industries make up less than 3 percent of aggregate economic activity, so the ability to control for the effects of the substitution responses which occur in the much larger remainder of the economy is crucial when undertaking this type of analysis.

4e. As noted above, the model does not make use of any assumed "backstop" technologies. The consequence of this choice is that the model's energy use projections are of necessity based on an existing set of technologies (though non-commercially available technologies can be and are modeled). Given the timeframe of the climate problem and SGM's time horizon, as well as the model's feature of tracking energy balances, is this an appropriate modeling choice? Is it the best modeling choice?

Score: 5

I particularly like the way in which SGM's authors perform a column disaggregation of the SAM to create a macroeconomically consistent representation of discrete electricity generation technologies. This approach is a good choice in a CGE model because it constrains technologies' inputs and outputs to fit within the general equilibrium framework of excess demands.

The absence of backstop technologies—i.e., activities which supply energy in an infinitely elastic manner at a constant, albeit high, marginal cost—is inconsequential once the model specifies energy supply activities which are not operated at benchmark equilibrium prices. The key feature of the latter is that they do not instantaneously capture a large share of the supply in the relevant market, but penetrate smoothly over several periods. The reason is that once energy prices reach the threshold level that allows these technologies to operate competitively (a point which is usually determined by the engineering data on which they are calibrated), they must then compete with other activities for factor inputs.

However, what makes this smooth behavior possible is the specification of a nested production function in which the outputs of individual Leontief technologies are aggregated according to a CES function. This is achieved by permitting the unit costs of electricity generation by the different technologies to diverge from one another, which represents the proliferation of market niches for generation with different characteristics. But while this is a neat solution in terms of economic characteristics, it creates a problem for energy accounting. In such a model, the aggregate quantity of electricity generation at the top level of the CES nesting hierarchy is a nonlinear function of the quantities of electricity generated by the different technologies, whereas in reality the total generation is just the linear sum of he components. While this problem is well known (e.g., McFarland et al 2004; Sue Wing 2004), there appears to be little that can be done to remedy it.[4]

As far as evaluating how good a modeling choice this is, in my response to question 4(a) above I noted that the complexities of energy accounting are inconsequential for tracking flows of carbon, once the model employs an upstream emission accounting system like the one used in SGM . Therefore, even though it might be difficult to reconcile the quantity of output of the electric power sector and the levels of generation by the discrete technologies specified within SGM, I can see no down-sides in terms of the ability to accurately capture the general equilibrium effects of climate-change mitigation policies.

[4] McFarland, J.R., J.M. Reilly and H.J. Herzog (2004). Representing energy technologies in top-down economic models using bottom-up information, Energy Economics 26: 685-707; Sue Wing, I. (2004). The Synthesis of Bottom-Up and Top-Down Approaches to Climate Policy Modeling: Electric Power Technologies and the Cost of Limiting U.S. CO_2 Emissions, mimeo, Boston University.