International Agricultural Trade
Research Consortium

A Critique of Computable General Equilibrium Models for Trade Policy Analysis

by

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Working Paper # 92-4

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April 1992

Preface

This paper by Dr. Tim Hazledine was presented at the IATRCs annual meeting held in New Orleans, La., December 12-14, 1991 as a part of the "theme day" program devoted to the topic of "Applied General Equilibrium Analysis of International Trade". Dr. Hazledine was asked to provide a set of critical comments on the use of computable general equilibrium models in addressing trade policy issues. Following the meeting he kindly agreed to put his comments in writing. Several examples of applied general equilibrium modelling were provided in other papers presented during the theme day program. These were:

- "Overview of CGE Modelling and International Agricultural Trade" - Thomas Hertel, Purdue University
- "Agricultural Liberalization in the Context of a U.S.-Mexico Free Trade Agreement" - Sherman Robinson, University of California, Berkeley; Mary Burfisher, ERS, USDA; Raul Hinojosa-Ojeda, UCLA; and Karen E. Thierfelder, ERS/USDA
- "Trade Liberalization in a Multinational-Dominated Industry: A Theoretical and Applied General Equilibrium Analysis" - James Markusen, University of Colorado; Linda Hunter, San Diego State University; and Thomas Rutherford, University of Western Ontario
- "Some Conceptual Issues in the Modelling and Computational Analysis of the CUSTA Agreement" - Robert Stern, University of Michigan
- "Analysis of Trade Liberalization Under Induced Technological Improvement" - Antonio Brandao, World Bank; Marinos Tsigas, World Bank; Dominique van der Mensbrugghe, OECD; and Ian Goldin, OECD
- "The Contribution of CGE Analysis to Policy Reform in Australia" - Alan Powell, Monash University, Australia

Copies of these papers, some of which have been published, can be obtained by writing to the senior authors.

Karl Meilke
Chair, IATRC

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This paper is developed from the author's presentation to the International Agricultural Trade Research Consortium, December 12, 1991, New Orleans LA. Comments received at that time are gratefully acknowledged.

1. Introduction

The building of computable general equilibrium (CGE) models is a vigorous and successful branch of applied economics. The modellers set, and generally meet, high technical standards, and the basic idea behind CGE is possibly important enough to be of Nobel quality. The field is largely problem- not tools-driven. That is, as the titles of the papers presented at the IATRC conference demonstrate, CGE models are built to answer specific and usually important policy-oriented questions, such as the impact of trade liberalization. Such a focus contrasts with the often sterile technique- or tools-driven approach found in so much of the work promulgated by the top U.S. and Canadian schools. One thinks, for example, of the near-mania for game theory that swept through the best graduate programs in the 1980s.

Nevertheless, the leading CGE modellers have significant intellectual capital tied up in their work, and perhaps cannot be relied on to always assess with absolute objectivity the appropriateness of CGE to the problems at hand. I will suggest below that simpler or narrower research strategies may indeed often be more cost-effective. The basic points are, first, that the extra work required to achieve 'general' results may not be worthwhile if the sector or problem being focused on is small enough not to generate significant general equilibrium feedbacks, and, second, that rigidity of the specification of CGE models can impose a disfunctional theoretical straitjacket on the analysis of a particular industry. A 'partial' model may better capture the nuances of specific market behaviour.

The paper will deal in turn with three sets of modelling issues: the question of 'data'; the 'micro' problem of specifying market behaviour, and the 'macro' issue of 'closing' the models in aggregate. I will conclude with some suggestions for future research. The basic theme of the paper is this: CGE modelling is essentially a conservative or 'neoclassical' scientific endeavour, and exhibits the strengths and weaknesses of neoclassicism. And as for the recent injection of apparently non-neoclassical imperfect competition or industrial organization (IO) concepts into CGE, though, as an IO specialist myself I certainly welcome this in principle, I have doubts about the usefulness of the practice.

I do not claim originality for my observations, with the possible exception of those relating to IO specification. Thus, the paper is directed more towards the user or consumer or client of CGE models than at producers of the models. The paper focuses exclusively on the use of CGE for the analysis of trade policy.

2. Data

First point: CGE models are not 'empirical' in the usual sense. They are, of course, quantitative, but the models are essentially consumers, not producers, of empirically tested propositions. This is not in itself a criticism, but does seem that dealing with shortages or weaknesses of data take up much of the time and ingenuity of the model-builders, and certainly are not neutral with respect to results.

Specifically, the models require three types of data. in descending order of apparent reliability, these are:

(a) national accounting data
(b) policy data
(c) elasticities

National accounting data are those produced by official national statistical agencies. They cover the nominal currency flows of output, input, consumption, exports, imports and so on. I will not have to say about these data, except to note that some variables are better measured than others. For example, data on trade in goods are much more reliable than on trade in services. Labour input is easier to measure than capital. Some important economic magnitudes are not measured at all in the official accounts: the value of leisure; the value of household production and (most) childcare; illegal activities; and accounting for environmental impacts and depletion of natural resources.

Clearly, these omissions must limit the generality of general equilibrium models. But they may also bias the results, if the models are somehow distorted to fit the available data. I will return to this point in section 4.

Policy data, in the present context, are mostly measures of protection of domestic industries from import competition and/or subsidization of exports. Information on tariff rates are readily available; on non-tariff barriers and subsidies less so, though these usually yield to the determined investigator. However, it is rarely possible to plug even tariff data into a model without serious further processing by explicit or implicit theoretical assumptions.

As an important example, take the aggregation of individual commodity 'tariff items' to get a figure for the tariff protection afforded the industry which produces the commodities. Typically, there will be a range of tariff rates, including some set at zero. How should they be combined into a single number?
There are at least four ways of doing this:

(a) take an arithmetic average of scheduled tariff rates;

(b) take a weighted average of tariff rates, with domestic production values as weights;
(c) take a weighted average of tariffs on dutiable imports with import values as weights;
(d) take import value-weighted average of tariffs on all imports (dutiable and duty-free).

The listing goes in descending order of likely magnitude. Methods (c) and (d) are most popular, since they are most simply measured, by dividing the total (over all the commodities assigned to the industry) value of tariff duty collected by the the total value of, for (c), dutiable imports, or, of all imports, for (d). It does matter which method is chosen: for Canadian manufacturing, for example, weighted average tariffs on dutiable imports before the implementation of the Canada-US Trade Agreement (CUSTA) were about 11%; on all imports, nearer 6%. In a model driven by tariff cuts, the choice of starting tariff will obviously make a major difference to the results.

So which method is right? Well, none of them, in principle. The aggregator should depend at least on elasticities of substitution in consumption and in production between the commodity items. In practice, too, the political economy of tariffs is important. If tariffs are set randomly (or for ancient historical reasons that no longer have any systematic relevance), then (d), which averages all tariff rates, including zero, might be best. But if, however, duty-free status is only given to commodities which are not produced in significant quantities domestically, then method (c) is preferable as a measure of the protection actually granted to domestic industry.

Non-tariff barriers are even trickier. If markets are competitive, and supply and demand elasticity estimates are available, then an unambiguous tariff-equivalent can be calculated. But if markets are imperfectly competitive, a tariff-equivalent does not exist even in principle. That is, a tariff could mimic the price effect of the NTB, or one of the quantity effects, but not all of them. The situation needs to be explicitly modelled.

The above illustrates a point I will make again: given finite time and resources, the breadth of coverage of a CGE model must come at a cost in depth and detail. The analyst concerned with a specific industry or small set of industries should consider carefully whether the likely general equilibrium feedbacks are big enough to make worthwhile the loss of accuracy at the micro level.

As for <u>elasticity</u> data, inadequacies in these concerned Shoven and Whalley in their authoritative 1984 survey paper. Most model builders recycle the estimates of others, though often modifying them for one reason or another.[1] Of course, this is quite understandable --large scale econometric modelling is possibly even more time-consuming than building CGE models.

The best available estimates are without doubt of own-price demand elasticities. Consumers, especially of household goods and services, are closest to fitting the pre-condition of price-taking behaviour that is needed for reliable (single-equation) demand curve estimation. But it has proven terribly difficult to get the data to cough-up plausible cross-price elasticities. As for the supply side, outside of primary resource-based industries these cannot be estimated at all, for the very good reason that they do not exist under the imperfectly competitive conditions that typify manufacturing and service industries. Instead, modellers build up from data or estimates of underlying production technologies.

Holes in the base data can be filled by applying theoretical 'restrictions' (such as homogeneity), by ad hoc assumptions (eg, that demand is 'Armington'), or by simply plugging-in numbers that the researcher considers to be reasonable. In practice, many model-builders seem to be quite ready to 'fix' estimates even when they 'ain't broke' -- that is, to adjust estimates without providing any critique of them, except perhaps some general statement about econometric elasticity estimates always being 'too small.'

Such massaging of the numbers may, at the hands of an experienced and honorable researcher, improve them. But there are obvious dangers that the model-user should keep very much in mind, especially when one of the criteria for settling on elasticities or other data is whether the **results** that they generate in the model are to the model-builder's liking.

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[1] For example, Harris and Cox (1984, Table B2A and p178) show, for their 29 2-digit SIC industries, export and import price elasticities of demand for Canada. The export elasticities they credit to the survey of Stern, Francis and Schumacher (1976). The import elasticities are supposedly averages of 4-digit elasticities listed in an unpublished 1981 paper by Hazledine (for which a totally inaccurate citation is given in the Harris-Cox bibliography). But (i) my numbers did not cover nine of the 29 Harris-Cox industries; (ii) my numbers were themselves largely taken from Stern et al (ie, were not an independent source), and (iii) the numbers actually used by Harris-Cox are 'adjusted' -- with no explanation -- so that they are all equal to 2 or 3.

3. Micro Foundations

In his very useful introduction to the use of CGE in agricultural policy analysis, Hertel (1990, p7) claims that 'well specified' CGE models have a structure which 'adheres strictly to standard neoclassical theory'. The reasons given by Hertel for this statement are really practical rather than principled: because neoclassicism provides a well-understood language of discourse, it is relatively easy for the model user to understand, and for the model builder to explain how results are generated.

Quite so -- well-trained economists are intimately familiar with how neoclassical models work. But what if the neoclassical model is wrong? Perhaps real-world markets should not be so easy to understand. It is this issue that I will explore below.

First, a definition of neoclassicism. In my book, it is the economics of perfect competition, meaning:

(a) agents optimize (consumers maximize utility; firms, profits)
(b) agents are small (price-takers)[1]

Now, they (used to) say that no purchasing manager ever got fired for buying IBM. Likewise, no academic economist ever was refused tenure for writing down models based on assumptions (a) and (b). But some of the most interesting recent work in trade policy modelling has broken -- albeit somewhat half-heartedly -- from neoclassicism. Before getting to this, note just how problematic can be the predictions generated by atomistic optimization.

Smallness is generally taken to imply constant returns technology and free entry. This means that price is determined solely in factor markets -- equal to average cost. Suppose that an imported commodity that competes with domestic output has its 10% tariff removed. The landed price of the import will drop by this amount. What will happen to the price of the domestic substitute? In partial equilibrium: nothing; in general equilibrium, perhaps a tiny fall as factor prices adjust throughout the economy. This is so no matter how closely substitutable are imported and domestic commodity.

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1 In his paper for the IATRC meeting, Hertel implies a definition (1991, p11) of neoclassicism that only uses the optimization assumption (a). I do not think that this is helpful -- too much that is fundamental changes when agents become large. For example, it is in fact quite problematic under imperfect competition to have firms' pricing representing the 'solution to a well-defined profit maximization problem', and, unemployment need not be the result of a 'distortion, such as minimum wage legislation'.

Such an inference surely offends our sense of how actual import-competing firms do respond to lower-price imports. It can be avoided only by assuming the law-of-one-price to hold (so that domestic prices are arbitraged to follow import prices **exactly**), and assigning, perhaps arbitrarily, a fixed input to the domestic sector. This input earns the residual rents generated by import protection.[1] This specification is also extreme.

The assumption that factor market competition prevents domestic prices from responding directly to import prices, apart from its inherent implausibility (as a general proposition), has serious implications. Since domestic price is almost unchanged, the **relative** price of imports to domestic changes a lot, inducing a large shift in demand away from domestic output. This requires adjustment of the exchange rate to maintain overall trade balance. The size of the adjustment depends very much on the size of the demand elasticities; the net result, in a bilateral trade liberalization scenario such as CUSTA, can be that the country with the highest tariffs comes out a net loser from free trade, because of terms of trade effects (cf Brown and Stern, 1991).

This 'problem' is usually blamed on the 'Armington' assumption of commodities differentiated in demand by their country of origin and the 'low' price elasticities plugged in to the Armington demand equations. But it is perhaps more fundamentally attributed to the neoclassical pricing model. With more direct and bigger responses by domestic firms to the landed price of their import competition, relative price shifts would be smaller, and so too the shifts in demand that require real exchange rate adjustments.

Alternatives to the neoclassical model are now to be found. Interestingly, though, their innovation into CGE was driven not by worries about the product market pricing model, but by the perceived need to incorporate different production technology -- specifically, increasing returns to scale, which many believe to typify modern manufacturing. Large-numbers equilibria are not sustainable under increasing returns, so it becomes implausible to maintain the assumption of price-taking behaviour. With a rather small number of firms in an industry, each must be aware that its actions will affect price.

In their pioneering and famous introduction of imperfect competition into a CGE model of the CUSTA, Harris and Cox (1984) calculated two pricing formulas for each domestic Canadian industry. One had price set as a profit-maximising markup on marginal costs, given a perceived elasticity of demand; the other was the 'focal point' price equal to the exogenous world (or US) price inclusive of the Canadian tariff. The latter requires implicit collusion on the part of the domestic oligopolists,

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[1] It is not possible to maintain both free entry (zero profits) and perfect output market substitutability -- price will be over-determined.

and is often known in Canada as the 'Eastman-Stykolt Hypothesis'. It is sometimes also called the 'tariff limiting' price, meaning that it is the price which will just limit competition from imports, though in an Armington world of nationally differentiated products this is not accurate, since imports can occur even if the domestic price is lower than the landed import price.

In any case, Harris and Cox end up using a simple arithmetic average of the two price formulae. In practice, since the perceived elasticity formula is very close to a constant markup rule (ie, just like a competitive price), their pricing equation causes about one half of a tariff cut to be matched directly by cuts in competing domestic output prices.

By themselves, such cuts would not cause anything spectacular to happen -- indeed, as noted above, the market share and output shifts induced by trade liberalization would be smaller than in the neoclassical model because the relative price changes are smaller.

So from where do Harris and Cox get their gigantic (8% of GNP; or around 25% of tradable sector value added) efficiency gains from free trade? The trick lies in retaining the neoclassical free entry assumption. It is assumed that, just because efficient technologies are large-scale, they are not thereby unavailable to potential competitors. So, any rents from tariff protection will induce entry, and this will dissipate the rents, as the domestic industry becomes 'crowded' with firms, each producing at sub-optimal scale. Remove the tariff; the pricing formula gives a lower domestic price at which the current set of domestic firms will all be making losses, so that some must exit, until output per surviving firm increases enough to restore zero profitability.

Thus, the tariff cut has induced a 'rectangle' of efficiency gain, covering all units of output, in contrast to the neoclassical 'triangles' of allocative improvements when only marginal units are affected. Still, though, we are short of the Harris/Cox numbers. This is where the general equilibrium effects come in: lower domestic costs reduce export prices, which increases export demand, requiring a balancing increase in the exchange rate, which makes imports even cheaper than with just the tariff cut, which forces a further round of domestic price decreases, and so on.[1]

The Harris/Cox mixture of imperfectly competitive product markets (price above marginal cost), perfectly competitive input markets (price equals average cost), and similar technologies with scale economies (generated simply as fixed costs plus constant marginal costs) has become something of a standard for modellers

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[1] The mechanism is explained, and the results illustrated in a stripped-down CGE model, in Hazledine (1990).

anxious to inject IO concepts into CGE. Refinement has focused on the pricing equation. In their paper for the IATRC symposium, Hunter, Markusen and Rutherford (1991) bring pricing into the IO mainstream by assuming non-cooperative conjectural variations. This means that each firm, operating independently (non-cooperatively) of other firms, but conscious of its interdependence with them, will choose its price (or output) to maximize profits subject to the expected (conjectured) change or variation in the output of its rivals that would be thereby induced.

The conjectural variations (CV) approach seems attractively general. It can encompass, as extremes, both perfectly competitive pricing and monopoly pricing. Pricing in between the extremes is related to the number of firms in the industry, which industrial organization specialists find realistic. Although there is some dissent (eg, from proponents of 'contestable' markets), it is reasonable to claim that CV matches up with the mainstream learning in IO: in 'lumpy' industries firms do set prices above marginal cost, with the size of the mark-up (the degree of imperfect competition) dependent on 'structural' parameters such as industry demand elasticity and the number of firms.

But there are problems. Note that it is just about impossible to have the oligopolists' pricing behaviour represent the solution to Hertel's 'well-defined profit maximization problem'. The conjectures on which firms base their output decisions turn out, in general, to be wrong. This is due to the immense complexity of the action/reaction/action sequence when more than a very small number of firms (say, two) are involved.

What this means is that there is an inevitable ad hoc-ness to oligopoly pricing models. They must be assessed on their 'reasonableness': does price fall if elasticity increases? if costs fall? if the number of competitors increases? The 'theoretical consistency' beloved of neoclassicists just is not available. But this is a price worth paying, in my opinion. The point is this: in analysing firms' market behaviour, theoretical consistency comes at the price of inconsistency with the facts.

Thus I support the basic idea of injecting IO concepts into the modelling of (most) tradable goods industries. The execution is problematic, however. Once the researcher leaves the tight and narrow path of perfect competition, the modelling territory becomes a lot broader and less well charted. Choice of assumption about pricing rule or entry behaviour makes a big difference to the results (cf. Hazledine, 1990; Hertel, 1992), but most model-builders -- certainly, most CGE modellers -- have little expertise or information to guide their choice.

The particular set of assumptions favoured by Harris and Cox and others since them generates, in my opinion, both implausibly large macro gains-from-free-trade numbers, and unsatisfactory micro predictions of the impacts on particular industries. For example, Harris-Cox (1984, table 14) predict that the Canadian transportation equipment industry -- already a massive exporter under near-free trade conditions through the AutoPact, would increase its exports to the U.S. by 157% after general free trade was implemented!

What goes wrong? I expect that the problem lies in the input market specification, not the product market. The pricing rules, with their general predictions that some but not all of a tariff cut will be passed on in lower domestic output prices, are reasonable, and cannot do much harm. But free entry is not innocuous. It comes from the postulates, first, that oligopolies exist because scale economies limit the number of firms, and, second, that the technologies that generate these scale economies are widely available.

That is, all the firms in an industry have the same cost curve, and this is also freely available to outsiders, who therefore prevent any 'excess' profits being earned, just as in perfect competition. These assumptions often result in large structural changes as a result of trade liberalization, with many firms forced to exit, as well as the hefty reductions in industry average costs, noted above.

Now, to an empirical IO person, this is not a readily recognizable picture of what actual industries look like. Specifically, we do not observe many industries in which all the firms are (approximately) the same size. Instead, the typical Canadian manufacturing industry has two-to-four big firms, perhaps a similar number of medium-sized enterprises, and a 'fringe' of many small operators.

Much of the most interesting recent work in IO has focused on analysing these persistent size differential within industries (eg. Farrell and Shapiro, 1990). The simplest way to explain them is to endow firms with different cost curves. These, in turn, come from firms having different endowments of some rent-yielding inputs, such as a superior management team, or control over raw materials. Heterogeneous costs do not necessarily alter much the pricing model[1], but they make a big difference to entry, which now must be expected to occur, if at all, only at the fringe, where firms with no special rent-yielding assets operate.

Then, loss of tariff rents will not force large-scale exit, and will not, therefore, result in much rationalization of the industry to exploit scale economies. Indeed, the story is rather anti-climactic: we are close to being back to the neoclassical prediction of only 'marginal' efficiency gains from trade liberalization.

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[1] Indeed, under linear Cournot-Nash assumptions (this being the most popular model), price is only a function of the arithmetic **average** of firms' costs. That is, the distribution does not enter the pricing formula.

If forced to choose a single IO model to represent behaviour in all markets, I would plump for the differential-cost model over free-entry. But I believe that the real lesson to be learned from the flightiness of results under varying IO assumptions is that some fairly detailed knowledge of the actual characteristics of an oligopolistic industry is needed before reliable policy simulation can be undertaken. If these informational requirements are too severe to be satisfied in big multi-sector CGE models, then perhaps we should not build such models, or, at least, not rely on them for policy analysis.

4. Closure

The G in CGE stands for 'general'. This means that the models are supposed to capture all the impacts, throughout the economy, of an event such as a tariff cut. Specifically, models are 'closed' by requiring that the total supply of an input or output must equal the total demand, with whatever price and other adjustments are required to effect this. In contrast, partial equilibrium models take some prices as given, on the assumption that the industry or sector being studied is too small to have a significant impact on total supply or demand.

The point to be made here is simply that there is, in practice, no such thing as a truly general model -- all operational models must limit the range of phenomena they can deal with in full generality. The choice of closure rule is often arbitrary, and it can affect the results.

We can distinguish three classes of closure:

(a) geographic/temporal limits
(b) market responsiveness limits
(c) exogenous parameters

In turn:

(a) All national or regional models are obviously only 'partial' in a global context. Sometimes, this is justified by a 'small country' assumption, implying perfectly elastic supply or demand curves linking the country to the rest-of-world (RoW). Such can have large effects. For example, Harris and Cox assume an elastic supply of capital to Canada, and free trade results, in their model, in a huge (nearly seven-fold) increase in capital imports (1984, Table 9, p.94). It must be doubted whether Canada could actually finance such inflows without affecting the supply price of capital.

A common heuristic way to soften the small country assumption without actually explicitly modelling the RoW is to assign an elasticity of supply or demand--

for example, a world demand elasticity for the region's exports.

Most CGE models are 'static' (single period). This leads to some awkward decisions about how to deal with decisions, such as savings and investment, involving inter-temporal trade-offs. Again, a heuristic behavioural rule -- such as a savings function -- can help make the numbers more reasonable.

(b) Most models have some 'vertical' quantitative constraints imposed, usually, on the supply side; for example, Hunter et al assume that the total labour endowment in each country is some fixed number. This amounts to 'giving up on the market' -- ie, to imposing a limit on the scope of price in encouraging supply. The result will be an over-estimate of the price effects of changes in demand.

A related issue is the imposition of what could be called 'zero' constraints -- that is, the total omission of some economic activity. Few CGE models incorporate illegal activity, or unpaid household production, to give two quantitatively significant examples. I do not know how important are these zero constraints, but they might be worth thinking about.

(c) CGE models do not push the generally accepted limits of endogeneity. Notably, tastes and technologies are taken as exogenous. Some very interesting recent work in microeconomics has tried to explain the process whereby consumer and producer technologies are fashioned: I will just flag this as an issue of possible merit.

5. Recommendations

I will close with a terse list of recommendations, which follow (more or less) from the arguments made above:

1. Modellers must be willing to escape from the neoclassical straitjacket. The seductive power of the neoclassical paradigm is no use if it drives you in the wrong direction, as -- I believe -- it all too often does, when realistic analysis of actual economies is required.

2. Specifically, perfect competition doesn't fit most markets. But modelling imperfect competition is highly non-trivial, as there is no unique model that can be plugged-in. Careful theoretical and empirical analysis of the actual market(s) is required.

3. Also, modellers should not be so sanctimonious about grounding all their behavioural equations in 'rational optimizing behaviour by self-seeking atomistic agents'. The aggregation problem involved in using representative agent assumptions with aggregate data is, alone, enough to thoroughly mess up methodological purity. But, as well, we have the limitations in the scope of modelling, noted above. What biases are introduced by, for example, 'rigorously' modelling household labour supply in a framework that excludes household production?

I am not suggesting throwing first-order conditions out the window; but, at least, modellers could be content to make do with rules of thumb or empirical regularities in situations where reliable theoretical restrictions are not forthcoming.

4. More time spent getting things right at the micro level means less time available for other activities, such as closing the model in general equilibrium. So be it -- I expect that the returns to closing a model (ie, making it 'general') may often be exceeded by the returns to more scrupulous specification of the particular sector(s) that are the focus of interest.

In closing, let me say that, despite the generally negative tone of the above comments, I really do believe that CGE modellers have a valid and exciting research program, and I look forward to seeing how they develop it over the years ahead.

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Endnotes

1. For example, Harris and Cox (1984, Table B2A and p178) show, for their 29 2-digit SIC industries, export and import price elasticities of demand for Canada. The export elasticities they credit to the survey of Stern, Francis and Schumacher (1976). The import elasticities are supposedly averages of 4-digit elasticities listed in an unpublished 1981 paper by Hazledine (for which a totally inaccurate citation is given in the Harris-Cox bibliography). But (i) my numbers did not cover nine of the 29 Harris-Cox industries; (ii) my numbers were themselves largely taken from Stern et al (ie, were not an independent source), and (iii) the numbers actually used by Harris-Cox are 'adjusted' -- with no explanation -- so that they are all equal to 2 or 3.

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3. It is not possible to maintain both free entry (zero profits) and perfect output market substitutability -- price will be over-determined.

4. The mechanism is explained, and the results illustrated in a stripped-down CGE model, in Hazledine (1990).

5. Indeed, under linear Cournot-Nash assumptions (this being the most popular model), price is only a function of the arithmetic average of firms' costs. That is, the distribution does not enter the pricing formula.

April 24, 1992

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