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Recent Studies of Agricultural Trade Liberalization

The governments of the industrial countries seem to have agreed on the desirability of a general liberalization of agricultural trade. More precisely, each government sees the desirability of all other countries liberalizing trade. They apparently do not see the desirability of liberalizing their own trade, since it is in their power to do so and, except for New Zealand, they have not done it. So the hypothesis of each government regarding general liberalization must be: the good results of everyone else's liberalizing will outweigh the bad results of our liberalizing. But each country wants evidence on this hypothesis. Hence the studies on who gains how much from different kinds of liberalization. (If it were only global-viewing economists who were concerned, we would not need these studies, because economists tend to be convinced *a priori*, in ways that would be built into every study, that there would be net worldwide gains from free trade.)

So some studies have been done. What I will discuss in this paper is the results and the believability of the results. How confident can each country be in using the results?

ANALYTICAL METHODS AND RESULTS

Research on effects of trade liberalization has followed a different line from the usual theoretical and empirical work that is reported in journals. The usual theoretical papers attempt to develop rigorous and intuitively plausible explanations for puzzling observed phenomena; and in empirical work, they explain phenomena econometrically. But the only way to obtain quantitative empirical estimates of the counter-factual situation of liberalized trade has been to simulate events in a model of the world economy so simplified as to bring the research well back from the academic frontiers. This area has a frontier of its own, however: making progress in choosing the right simplifications, in being able to handle ever increasing computational complexity (so as not to have to make so many simplifications), and in getting better evidence on parameter values needed for simulation. A major problem arises in comparing the scientific merit of alternative ways of making progress on this frontier, because we do not have enough data on what actually happens under trade liberalization to judge which of several alternative estimates is most plausible.

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Of the many contentious modelling choices, the following general ones seem most important: how far to go in (1) bringing in general equilibrium considerations, (2) incorporating dynamics of liberalization – the time path of effects, (3) recognition of imperfect competition and multiple contemporaneous prices for the 'same' commodity, and (4) endogeneity of each country's economic policy choices. Equally important for simulated outcomes are more particular modeling choices, notably, (5) which countries to model (and which to aggregate into regional entities), (6) which commodities to cover, (7) how to measure initial protection levels, (8) what time period to use as a base from which to liberalize, and (9) how to obtain the parameter values for the equations of the model.

The most straightforward and, to me, appealing way to simulate liberalized trade is to link country-specific supply-demand models of the main agricultural commodities – a many-country generalization of the simple two-country diagrams one sees in elementary trade tests. The equilibrating principle is to find a price vector such that worldwide excess demands are zero. Then the quantities consumed, produced and traded in each country can be calculated, and the welfare effects of price-quantity changes for each country can be estimated. This is the basic approach taken in the first major modelling effort of Valdes and Zietz (1980) and of the five others considered here, those of Anderson and Tyers (1987), the OECD (1987), IIASA (Parikh, *et al.*, 1986), USDA-ERS (Roningen, *et al.*, 1987), and Burniaux and Waelbrock (1984). But various complications are added in each of the studies.

Table 1 summarizes some of the modelling choices made in recent studies. All the models determine world prices by finding competitive market clearing prices after existing trade distortions are removed. All measure existing distortions principally by estimating a scalar price wedge for each commodity in each country, for example, a tariff equivalent in the IIASA model, or producer subsidy equivalent (PSE) in the OECD study. The main respects in which the studies differ are: (a) introduction of nonagricultural sectors to make the model general equilibrium; (b) the handling of dynamics and length of run of simulations; (c) the measurement of existing protection; and (d) the estimates of elasticities. The issues raised by these differences are discussed for each item in turn.

General versus partial equilibrium

It seems obvious at first glance that general equilibrium is preferable to partial equilibrium modelling. General equilibrium modelling is more rigorous in that it satisfies more neoclassical restrictions: for example, the adding-up properties of a system of demand equation. It also satisfies the constraint that income equals expenditure, so that if protection changes farmers' incomes, this is fed back through the demand side of the model to obtain price effects that a partial equilibrium model would omit. In a more practical vein, Burniaux and Waelbrock argue for the use of a general equilibrium model to analyse the effects of reform of EC agricultural policies on the grounds that partial models omit 'the influence of increasing food prices on competitiveness of the industrial exporting sector of LDCs, and their impact on Europe's industrial growth' (p. 3). Moreover,

TABLE 1 *Modelling approaches in studies of trade liberalization*

Element of Model	Tyers and Anderson	IIASA[1]	OECD	USDA-ERS2	Burniaux and Waelbrock
Supply	econometric equations	optimization and estimated equations	elasticities	elasticities	production functions
Demand	"	demand system	"	"	demand system
Market clearing	multi-market partial	general equilibrium	multi-market partial	multi-market partial	general equilibrium
Competition assumed	yes	yes	yes	yes	not in manufacturing
Number of 'countries'	30	20	11	7	10
Number of agricultural commodities	7	9	14	6	13
Reference level of protection	1980–82	1980	1979–81	1984	1978
Date of simulated results	1980–82	1980–2000		1984	1995

NOTES: [1]Parikh, *et al.* (1986)
[2]Roningen, *et al.* (1987)

increasing agricultural imports of Europe 'will force that area to sell more manufactured goods, and hurt other exporters of these goods' (p. 4).

So is it only through laziness or ignorance that some authors stick with partial equilibrium approaches? Not necessarily. A pitfall of general equilibrium modeling has been described as follows:

> The sense of general equilibrium analysis is that it is important to explain everything at once in a way that adds up (that is, in an internally consistent fashion). Furthermore, if a general equilibrium model is taken seriously econometrically, it naturally instructs the user to employ all of the available data on all of the model's variables . . . the model itself does not say that its predictions are to be taken more seriously on some directions than in others. On the other hand, in order to make general equilibrium models tractable, their preferences, technology, and endowments have typically been so simplified, and so much has been abstracted, that it is often difficult to take their predictions in some directions seriously. The internal logic of general equilibrium modeling then creates a difficulty in taking *any* of the model's predictions seriously. (Sargent, 1987, p. 7)

The corresponding problem in trade liberalization models is not limited to simplification and abstraction. In addition parameter detail has to be specified, about which we know little and cannot judge the likelihood of error well. For example, Burniaux and Waelbrock specify multi-input, multi-output production functions for crops and livestock. This requires knowledge of (the equivalent of) many Allen elasticities of substitution among inputs and elasticities of transfor-

mation among crops – parameters which have proven impossible to estimate with any confidence, even for the most-studied industries of a much-studied country like the United States.

Moreover, any general equilibrium model must specify the terms of input supply to the whole economy and of input transfer between sectors. Modellers often resort to assuming that some factors are fixed in supply to the whole economy and some to specific sectors, and that variable factors are perfectly mobile between sectors so that, for example, the wage rate is the same in all sectors. Efforts have been made to relax these assumptions but our econometric information is weak to nil on these matters. An example of a prediction of general equilibrium models applied to agricultural policy that is difficult to take seriously is that a change in agricultural tariffs will change the relative prices of agriculture and other non-traded goods and services, but it will not change the relative factor prices of variable inputs in the different sectors – for example, the ratio of farm to non-farm wage rate – and quantities of variable inputs are immediately reallocated by sector as demand conditions warrant. At the same time, the quantities of other factors are assumed constant, for example, the quantity of farm land does not change when tariffs are changed.

At the macroeconomic level general equilibrium models cause all income received through factor payments on the supply side to be spent on the demand side. Savings and investment are not usually made endogenous. But a shock such as trade liberalization could well change the savings-investment picture. Therefore, a model which forces all income received as a result of trade liberalization to be spent may be misleading, for the short run particularly. With respect to international trade, the general equilibrium condition most commonly employed is a balance of payment (BOP) constraint that makes the aggregate value of exports equal the value of imports for each country. This is a sensible constraint but in fact we know it is violated for sustained periods by many countries. To handle this the model would have to incorporate international capital flows which in turn suggests incorporation of financial and money markets for each country. But empirical models of such complexity are not in the cards presently. As matters stand, it is not clear that incorporating a BOP constraint is an improvement, at least for short-run effects, over a partial equilibrium approach that did not even consider what would happen to nonagricultural imports of a country whose agricultural exports were stimulated by trade liberalization.

Defenders of the partial equilibrium approach can claim that they do not require assumptions about macroeconomic identities holding, underlying resource constraints for the whole economy, or the mobility of variable factors between sectors; but proponents of the general equilibrium approach will claim in turn that the defenders are indeed making such assumptions, they just do not realize it. What is being assumed by Anderson and Tyers or USDA-ERS when they fail to specify what happens to non-agricultural imports when a country's agricultural exports increase? Not any particular assumption, but a general one that the effects are insignificant in assessing the effects of trade liberalization. Similarly for the macroeconomic income-expenditure linkage. With respect to the supply side, in a partial equilibrium model factor market adjustments are subsumed in the elasticities of supply of the commodities. Inputs and nonagricultural product supply are not modelled. This approach has the benefit of

concentrating our forecast on a conceptual and empirical construct that we have a relatively good notion of – not that supply elasticities and cross-elasticities are known with precision, but that we have a feel for their likely range. Moreover, the consequences of the range of error can be explored easily using sensitivity analysis. Granted, we remain ignorant of the determinants of supply elasticity, but this can be lived with as long as supply elasticities are stable.

One area where the partial equilibrium commodity supply/demand approach leaves us in the dark is the income distributional effects among factor owners. By virtue of its specification of fixed and variable factors, general equilibrium models tell us more about which factor owners gain most. But this information, even more than commodity price effects, depends on the assumptions made. The partial but vertical multimarket approach of Floyd (1965), and recent extensions of it, seems to me the most promising way at present available to model factor market effects.

Choosing between partial and general equilibrium models of trade liberalization is like the choice between two imperfect cakes. The partial cake is made from a receipt that is known to omit ingredients that could be important. The general cake is made from a complete gourmet recipe but the chef has had to put in certain ingredients blindfolded from unmarked boxes. Neither cake is likely to be highly palatable but we have to choose one or the other. Fortunately there are many investigators, so as a group we can honour the Mae West rule of choice under uncertainty. ('Whenever I have to choose between two evils, I always pick the one I haven't tried before.') That is, economists should be trying both approaches.

Dynamics and length of run

Simulation of liberalized trade compares an actual situation with a counterfactual, constructed one. Ideally we want the constructed situation to be just the actual one as it would be without agricultural protection. This is easiest to imagine if we have a static supply-demand system, remove the wedges caused by protection, and move to the supply-demand intersection points. This is what the SWOPSIM (USDA-ERS) model does, elaborated for cross-elasticities, and it works nicely in a personal computer for easy sensitivity analysis that makes it fairly transparent how the model works.

But on reflection it is not clear what question is answered in such an exercise. If the liberalization were announced and implemented in 1984, the base year for this model, the shore-run supply and demand responses would be quite small, and the price changes therefore large. This would overstate the price changes that would occur if more time were allowed for adjustment. Suppose there exists more time for adjustment – looking at, say, results in 1986 from liberalization in 1984. But 1986 is different from 1984 in ways other than the trade regime change; for example, worldwide commodity markets were weaker in 1986.

Another problem is that the policies of 1984 entailed the holding of large quantities of government stocks of grains. If we not only remove price wedges but dump the stocks, there will be further transitory price effects. Typically these are ignored, and properly so. On the other hand, a simulation that involved continued holding of government stocks would be unsatisfactory because we have not fully liberalized in this scenario.

IIASA, and Burniaux and Waelbrock, take the approach of explicitly building dynamics and projections of underlying supply-demand trends into their model. Then they project simulated liberalized conditions into the future also, generating cycles or at least fluctuating prices up to the year 2000 which for some commodities are not clearly converging to any long-run equilibrium.

The problem created by dynamic projections is analogous to the problem in general equilibrium models: theoretical improvements are made but the empirical implementation requires enough conceptual simplification and structural parameter values of dubious reliability that we do not know if the simulated dynamic liberalization effects are better estimates than we would get from a simple comparative static simulation. In particular, estimates of lags in adjustment and of future demand or productivity trends are likely to be even less reliable than our estimates of elasticities. The problem of unknown trends can be circumvented by the following method, used by IIASA and by Burniaux and Waelbrock: simulate a dynamic reference scenario under current protectionist policies, then simulate a liberalized-trade run over the same period. Comparing the reference and liberalized scenarios in each simulated year, say between 1985 and 2000, gives a dynamic representation of liberalization effects. Comparing the results for a particular year, say 1995, gives liberalization effects that are not, to a first approximation, influenced by errors that may exist in projected underlying conditions since these will be the same in both scenarios. Unfortunately, IIASA partly abandons this advantage by projecting even in the reference scenario future protection rates different from reference-period ones.

When the liberalization effects vary from year to year, as they do in the IIASA simulations, the question arises of what year or years to use in constructing a table like Table 2, which shows a single estimated price effect. The choice appears arbitrary, and in fact is so; but the dynamics do provide a suggestion of the sensitivity of estimated price effects. For example, if a simulation shows a 5 per cent effect in 1990 and a 15 per cent effect in 1995, we may choose a particular year's result for Table 2, but an average of several years might represent the long-run effect better.

When all is said and done, it is difficult to be more confident in the IIASA column of Table 2 than in the USDA-ERS column, even though IIASA's model is theoretically more satisfactory, incorporating general equilibrium and dynamics while USDA-ERS is simply comparative statics using elasticities.

Lucas critique

The 'Lucas critique' (Lucas, 1976) says, adapted to the liberalization simulations, that the supply-demand structure is different under current programmes than under the liberalized regime; so we are likely to err when we simply remove wedges and keep the supply-demand parameters unchanged to simulate the liberalized regime. In some respects the Lucas critique does not cut so deeply in the models discussed here as it does in macroeconomic modelling. The consumer demand elasticities and underlying input supply and cost conditions which determine supply elasticities are to a first approximation unaffected by agricultural protection. The demand for stocks of commodities is however

TABLE 2 *Simulated results of OECD trade liberalization: percentage change in selected world prices*

	Tyers and Anderson	IIASA	OECD[1]	USDA-ERS
Wheat	10	18	−1	29
Rice	11	21	1	32
Coarse grains	3	11	−3	23
Beef	27	17	15	17
Dairy products	61	31	44	53
Mean of Agricultural Commodities	16	9	–	10

Note: [1]Results for 10 per cent *ad valorem* liberalization multiplied by 10. No mean price effect reported by OECD. Date of liberalization as shown in Table 1.

Sources: Tyers and Anderson (1988, Table 2), IIASA (1986, Table 5.1), OECD (1987, Table 7), USDA-ERS (Roningen and Dixit, 1988, Table 5).

strongly affected, in that the returns from holding stocks are much changed by simulating the dynamics of adjustment to and price fluctuations within a liberalized agricultural policy regime. The paper that goes the furthest in simulating price fluctuations, Tyers and Anderson (1988), does not give a satisfactory account of its handling of private speculative storage under liberalization. But results like those in Table 2 do not depend on such dynamics.

The Lucas critique does point up an econometric problem for analysing some countries' policies. The United States, for example, has been fairly regularly restricting acreage of key crops for the last 25 years. How in these circumstances can we estimate the underlying supply elasticity? The simulation models generally avoid this problem by not estimating supply functions but using supply elasticities from other studies, or in the case of IIASA, by deriving supply reactions via optimization. The ERS-USDA model contains no econometrics of its own. Can the simulation studies be accused of doing improperly what they do not at all? If you criticize the stick figures in my painting as not being lifelike, I can respond that my stick figures are not intended to be lifelike. I have rebuffed the criticism but revealed a possible major weakness in my approach to painting. The scarcity of econometrics in the simulation studies also reveals a weakness even if that very weakness allows the studies an escape from the Lucas critique.

Measurement of existing protection

The different studies have obtained quite different estimates of the initial wedges, even though all use a similar idea – comparing the internal producer or consumer price with a border price. This is troubling, since it obviously makes a big difference in simulated effects of liberalization and because this is an area where it ought to be easy to obtain general agreement on the correct value. An important problem is that an accurate measure of the underlying internal/border price

margin is required. Even with zero protection the internal farm price differs from the border price, and it is wrong to include this difference in one's protection measure. However, this margin is quite difficult to measure and tends to be unstable from year to year.

In considering more complicated policies for large countries, problems in estimating existing protection are a more difficult order of magnitude. US wheat policy places no wedge between the internal market price and the border price, yet the policy does protect US producers and moreover it influences the border price. I believe it is quite impossible to represent US grain policies by any single indicator of protection in the price dimension, for example a 15 per cent PSE. Moreover, no simple correction of it is satisfactory either. For example, US policy has guaranteed corn producers a price of about US $120 per ton for most of the 1980s. But at times this has been accomplished by almost pure subsidy payments while at other times (1983) acreage controls have driven the market price above US $120 per ton. In the former case the PSE is quite large but in the latter case it is zero (there is no wedge between the US producer and world price). Actually, the producer protection in terms of income would be about the same (although this depends on farmers being paid for the cropland they hold idle). Yet the effects on world markets are just the opposite; the subsidy increases world market supplies and acreage controls reduce world market supplies. So even if we had 'true' PSE in terms of producers' benefits, we would have an unsatisfactory measure to relax to obtain simulated world price effects, even in qualitative terms.

One who was convinced by the preceding line of argument might respond by replacing the PSE indicator by a world-supply-effect (WSE) indicator. This would give the percentage excess supply created by acreage subsidy, a quantity wedge, and a percentage excess demand (negative WSE) created by acreage controls. Constructing a WSE would be a difficult analytical task. And even if it were properly accomplished it would not be at all helpful about producer gains. So in our simulations using WSEs we would not be able to provide a believable simulation of producers' gains or losses.

Moreover, even if both the PSE and WSE were used, thereby giving up the scalar approach to measuring protection, this would not be sufficient if consumer welfare were of interest, as it is in the simulation studies. A simple example is that an export subsidy programme and, alternatively, a domestic producer subsidy programme can be constructed such that each has the same effects on producer welfare and world supply (equal PSE and WSE). Yet the two policies would have opposite effects on domestic consumer welfare; the export subsidy reducing it and the production subsidy increasing it (or leaving it unchanged in the small-country case). So we need a CSE, too.

If a simulator of liberal trade were only interested in world price effects, then it might be possible to get by with a scalar indicator. But it would be the WSE, not the PSE or CSE. (For example, lump sum or 'decoupled' payments would give a zero WSE.)

To reinforce the difficulty of the problem, note that it is not even in principle soluble by aggregation, because the PSE and WSE are measuring basically different things. The search for an all-purpose scalar indicator of protection might be likened to aggregating blood pressure, cholesterol level, body weight,

and so forth to obtain an indicator of health. But the protection measurement indicator has a worse problem. It's like trying to find a scalar indicator of both your health and how full your gas tank is. In fact the modellers have not been content to use a single PSE-like indicator but have attempted, for example, to shift US grain supply functions with the removal of acreage control programmes under liberalization. The methods used have been crude, however.

Elasticities and 'world' prices

Problems of appropriate elasticity measures were discussed earlier with reference to the Lucas critique. A further problem exists at an even more fundamental level, the assumption that under liberal trade the law of one price would hold. Thus, if policy is liberalized, the border prices of rice for all countries rise 1, 11, 21 per cent (depending on which result in Table 2 is your favourite). In the case of rice we have an actual liberalization experiment, the US eliminating its support of the market price of rice in 1986. This experiment, as would be the case in any actual policy change, does not correspond exactly to the scenarios considered by the modelers. But the Tyers-Anderson (1988, p. 205) and IIASA (1986, p. 6.27) studies did report results for unilateral US liberalization. Anderson and Tyers estimated a long-run world price effect of –1 per cent and a short-run effect of –13 per cent, that is, US policy is holding the world price of rice up. IIASA estimates a world 1 per cent price decline in 1990 and a 0.2 price increase in 2000. These scenarios liberalized all US agricultural commodities, not just rice, but in the case of rice commodity interactions are less than for feed crops or wheat.

When the US actually eliminated its rice support price, the world (US Gulf export) price fell substantially. The enabling legislation, The Food Security Act of 1985, became law in late December 1985 and market support mechanisms ceased operating in April, 1986. Between February and September, 1986, the price of US rice in world trade (f.o.b. Houston) fell from US$17.50 to US$13.00 per hundredweight, a decline of 26 per cent (and the corresponding Rotterdam c.i.f. quotation fell 37 per cent), far greater than any of the models would have predicted. Yet by December 1987 the Houston price had rebounded to US$21.00 per hundredweight. Since the worldwide supply-demand picture had changed by 1987, it is difficult to say how the 18-month experience could be reconciled with the models.

The more fundamental problem is that in the February–September 1986 period, the price of Thai rice (100% firstgrade; f.o.b. Bangkok) fell from US$273 to US$256 per metric ton, a decline of 6 per cent, over the same period when the US (f.o.b. Houston) price fell 26 per cent. It is troubling that the US and Thai rice prices did not come closer to moving by the same percentage in the liberalization period. The models have no room for changes in relative prices of the same commodity at the borders of different countries.

In short, we have a case where actual policy changes in rice produced an event similar to what had already been simulated. What contribution did the simulation results make toward an understanding of what occurred in this case? Zilch. What could have been predicted, the direction of the immediate price change, could have been predicted without the models. On what could not have been predicted,

the magnitude of the price change, the models are not helpful. Indeed, their one clear prediction, that border prices of rice at different locations would change by roughly the same percentage, proved false.

CONCLUSION

The introductory question was what confidence we can have in the point estimates of estimated price effects and welfare consequences of agricultural trade and policy liberalization that the large-scale simulation studies have generated. The answer is 'not much'; based on some admittedly sketchy considerations. Nonetheless the studies are useful in showing the range of billions in gains and losses being fairly narrowly circumscribed by even a wide range of uncertainty about price effects. There is no way for agricultural liberalization to cause any of the OECD countries to lose or gain a significant percentage of their GNP, and indeed it is difficult for them to lose at all. This may seem obvious given the sizes and net trade positions of the sectors involved, but it is still helpful to see the calculations actually carried out. This can be helpful even without an explicit supply-demand structure, as in the Quizon, *et al.* (1988) study of trade liberalization impacts on developing countries. More generally, the simulation studies may be valuable in the GATT negotiating process (and in getting countries to accept domestic reforms that a GATT agreement may entail) by placing policy makers or their staffs in a way of thinking about trade policy that brings the calculation of economic benefits and costs out in the open. The risk in placing such studies at centre stage is that negotiators and their constituents may be discouraged by the relatively small size of global benefits attained by liberalizing agricultural trade only, with even losses for food importing developing countries. The big potential benefits are in the realm of removing one area of troublesome recalcitrance in pushing the world toward an overall liberal trade regime in both agricultural and nonagricultural goods in which worldwide economic growth will be less shackled than it now is.

REFERENCES

Anderson, Kym, and Tyers, Rodney, 1987, 'Global Effects of Liberalizing Trade in Agriculture', forthcoming as a Thames Essay, Trade Policy Research Centre, London.

Burniaux, Jean-Marc, and Waelbrock, Jean, 1984, 'Agricultural Protection in Europe', mimeo.

Floyd, J.E., 1965, 'The Effects of Farm Price Support on the Returns to Land and Labor in Agriculture', *Journal of Political Economy*, 73 pp. 148–58.

Lucas, Robert E., Jr., 1976, 'Econometric Policy Evaluation: A Critique' in Brunner, K. and Meltzer, A. (eds.), *The Phillips Curve and Labor Markets*, North-Holland, Amsterdam, pp.19–46.

Organization for Economic Co-operation and Development (OECD), 1987, *National Policies and Agricultural Trade*, OECD Paris.

Parikh, K.S., Fischer, G., Frohberg, K., and Gulbrandsen, O., (IIASA) 1986, *Towards Free Trade in Agriculture*, International Institute For Applied Systems Analysis, Laxenburg, Austria.

Quizon, J., Gardner, B., and Quinn, L., 1988, 'Consequences of Agricultural Trade Liberalization for Developing Countries Assisted by AID', Wharton Econometrics, Balla Cynwyd, PA.

Roningen, V., Sullivan, J., and Wainio, J., (USDA-ERS), 'The Impact of the Removal of Support

to Agriculture in Developed Countries,' paper presented at the American Agricultural Economic Association meetings, August 1987.

Roningen, V., and Dixit, P., 'Economic Implications of Agricultural Policy Reform in Industrial Market Economics,' paper presented at the International Agricultural Trade Research Consortium Symposium, Annapolis, Md., 19–20 August 1988.

Sargent, Thomas J., 1987, *Dynamic Macroeconomic Theory*, Harvard University Press.

Tyers, Rod, and Anderson, Kym, 1988, 'Liberalizing OECD Agricultural Policies in the Uruguay Round: Effects on Trade and Welfare,' *Journal of Agricultural Economics* 30 (May 1988) pp. 197–216.

Valdes, A., and Zietz, J., 1980, 'Agricultural Protection in OECD Countries: Its Cost to Less Developed Countries,' Research Report No. 21, International Food Policy Research Institute, Washington, DC, December.

PANEL DISCUSSION

KYM ANDERSON and ROD TYERS

A great deal of time and money has been spent in the past decade on modelling agricultural trade and estimating the global effects of agricultural policies in different countries. It is appropriate, therefore, to ask whether the effort has been worthwhile. Professor Gardner's paper does not provide us with an assessment of the overall benefit/cost ratio from this research effort. Instead, it addresses a much simpler yet fundamental question, namely, how believable are the results generated with these models. Attention is focused both on the extent to which the models incorporate the complexities of the real world and on the estimated effects of policies on international food prices.

On the modellers' simplification of reality, Professor Gardner has much to say that is very sensible. His summary of the pros and cons of general equilibrium versus partial equilibrium modelling exposes well the trade-offs involved in choosing between these two approaches. He has reminded us of the point made by Lucas and others that econometrics cannot provide us with reliable estimates of what the model parameters such as elasticities would be in a deregulated environment. And he has pointed out the difficulties of having non-homogeneous products, of representing policies accurately in the model, and of deciding which years to choose in presenting results.

These difficulties are not reasons for abandoning modelling efforts, however. In principle it is a simple matter to include two or more types of rice in a world food model, for example. The main practical difficulty is to obtain reliable estimates of the elasticities of substitution between different varieties.

Similarly, with more effort one can narrow our differences in estimates of the extent to which policies are distorting domestic prices. In this connection the ongoing efforts of OECD and USDA are to be commended. If policies interfere with more than just prices, multiple measures of distortion may be needed by the modeller. In the case of United States grain policies, for example, we have used a combination of three instruments: price-raising measures, production-restricting measures and stock-changing measures. We certainly do not pretend to have accurately represented all aspects of US policy, but we believe we are closer to it than if we had used only a price-raising measure. Incidentally, there is a lesson here for trade negotiators: agreements may need to be sought on reducing a number of distortions, not just price distortions.

What about the problem of deciding which year to choose in presenting results? Professor Gardner feels it is unfortunate that IIASA (like us) projects future protection rates in the reference scenario and then compares that scenario with scenarios in which that protection is reduced or removed. He would rather see today's protection rates held constant through the projection period. However, this is not appropriate if we are interested in the effects of today's *policies*. This is because few countries have agricultural policies which simply alter border prices by a fixed percentage, so when international prices or domestic market conditions change, the rates of protection also change. Even in the long run most industrial countries do not fully transmit changes in international prices

to their domestic market. This raises both a conceptual difficulty and a practical difficulty. The conceptual issue is that no useful model of world agricultural markets – including comparative static models – can afford to omit this market-insulating aspect of farm policies. Our model includes this behaviour in the rudimentary form of econometrically estimated price transmission equations that vary by country and commodity and with the length of run being considered, although with further effort a variety of more complex configurations could be included. Whatever the mechanism used, however, protection rates should be endogenous even in the reference scenario of any model used for forecasting. To placate concerns about what is the appropriate reference scenario, a number of different ones could be used and a range of results reported. There is also a practical difficulty that results from the fact that protection rates vary widely from year to year even when there are no policy changes, simply because international prices are fluctuating in response to seasonal weather conditions and the like. It is that estimates of the effects of policies will vary not only according to whether they refer to the short run or allow some time for adjustments to occur, but also according to which year the protection rates refer. It is therefore misleading, to say the least, to compare results, as in Gardner's Table 2, when they refer to protection rates in different years (from 1979 to 1984) and to different adjustment periods (from medium term to long term). Given these (and many other) differences in model specification, it would be worrying indeed if the estimated price effects reported in Table 2 *were* the same.

In a similar vein, we are concerned with Professor Gardner's comparison of published model results with an actual change in international prices following a change in US rice policy in order to evaluate the predictive capability of the models. The published result he refers to from our work was the average estimated change in the international price of rice in 1988–90 following a liberalisation of *all* US grain, livestock and sugar policies phased in over five years from 1988 to 1992. This is hardly the same as the actual policy change he refers to in 1986. (Even so, the trade-weighted average change in the prices of US and Thai rices in international markets of 14 per cent in 1986 is very close to our published 13 per cent result for 1988–90.)

What is worrying about this paper is that if someone with Professor Gardner's competence can apparently misinterpret model results to this extent, then there is little chance that the interpretation by the average trade negotiator will be error-free. Clearly what is required for Professor Gardner's purpose are model results which address precisely the same question, using the same base period and the same measures of distortions to be liberalized. Only then will it be possible to assess the extent to which results from these models differ, and to identify the sources of the differences. Similarly, negotiators should not expect to be able to find model results in published papers which always suit their needs; specific runs typically will need to be made. Fortunately, the marginal cost of making such runs is relatively minor.

One final point. As mentioned at the beginning of these comments, the paper focuses on the believability of the model results mainly with respect to the estimated effects of OECD agricultural policies on international food prices. Yet the main contribution of these modelling efforts has been in drawing more of the world's attention to the growing welfare cost of those policies, to their ineffi-

ciency in transferring income to poor farmers, and to the greater economic gains and the smaller political costs associated with multilateral, as compared with unilateral, liberalizations. If through providing quantitative information of this sort they are able to alter even slightly the climate of opinion in favour of freer agricultural trade, they will score a very high benefit/cost ratio, given that in the 1980s these policies of industrial countries have been costing the world economy – if you believe *our* results – something between US$20 billion and US$40 billion per year.

REFERENCES

Anderson, K. and Tyers, R., 1988, *Global Effects of Liberalizing Trade in Farm Products*, Thames Essay No. 55; Gower, Aldershot (for the Trade Policy Research Centre).

OECD ,1988, *Monitoring and Outlook of Agricultural Policies, Markets and Trade*, OECD Secretariat, Paris.

Tyers, R. and Anderson, K., 1988, Liberalising OECD Agricultural Policies in the Uruguay Round: Effects on Trade and Welfare', *Journal of Agricultural Economics* 39(2): pp. 197–216, May.

Tyers, R. and Anderson, K. (forthcoming), *Distortions in World Food Markets*, Cambridge University Press.

US Department of Agriculture, 1988, *Estimates of Producer and Consumer Subsidy Equivalents: Government Intervention in Agriculture, 1982–6*, ERS Staff Report No. AGES 880127, Washington, DC.

H. BRUCE HUFF

The paper by Professor Gardner has two themes. First, it develops and discusses a number of contentious modelling choices with respect to model specification, structure and experiments that researchers have adopted in evaluating the impact of trade liberalization. Second, Gardner discusses the credibility of results from selected studies which involve three different types of models (OECD and USDA are very similar) of varying size and complexity, all of which are world, multicommodity models. These are only a few of the many studies on trade liberalization, but most of the others are less comprehensive.[1]

The author concludes that the simplest approach is probably best but still confusing to policy makers, that the results have little credibility but are still useful in the GATT, and that we need to do more modelling work. These latter two conclusions appear to be mutually inconsistent and moreover are inconsistent with the assessment carried out in this paper. Moreover, the author provides little guidance as to what type of modelling work we should be doing and why.

In my comments I would like, first of all, to review some of the assessments the author makes about the contentious modelling choices; second, to comment on the author's basis for his lack of confidence in model results by referring to US liberalization impacts on the rice market; and finally to suggest some areas of future activity that are missing in this review paper.

The author is particularly critical of general equilibrium modelling, noting that some of its assumptions are very restrictive and that some of its required coefficients have proved to be very difficult to estimate even in the United States.

Many of these arguments, of course, are true for any model, regardless of its underlying economic structure. It is difficult to accept the author's argument that simpler models with even more restrictive assumptions can provide more reliable results. The author appears to base much of his argument on the premise that demand and supply elasticities have been estimated with greater statistical significance. The author does not make it clear whether these weaknesses of general equilibrium models are so critical that results can be misleading or simply have low reliability. Moreover some of the assumptions that Gardner criticises have been relaxed in other recent studies. While I agree with his concern that many assumptions determine the results, I feel that the author should be emphasizing where we need to improve our analysis and not simply dismissing all general equilibrium modelling.

On the issue of trade distortions, the author appears to misrepresent how these are treated in each study. In his Table 2, the author notes that the studies determine prices after removal of trade distortions. It is important to note that these results are *only* for the liberalization of the agricultural sector, not all sectors of the economy. As well, contrary to the author's statement, these studies define trade distortions quite differently. For example, IIASA and Tyers-Anderson use world-domestic price differences (price wedge), while OECD-USDA include these plus government income transfers. The importance of these types of support vary widely by country. For example in 1984–6, in the United States, government expenditures accounted for about 76 per cent of total agricultural support whereas these were only 38 per cent of total EEC and 27 per cent of total Japanese agricultural support (Table III.3, p.39, OECD, 1988). The OECD-USDA studies also relax any direct consumer subsidies. The OECD also includes relaxation of US and Japanese acreage set aside programmes and US and EEC stock holding for dairy and grains. Hence the results are individually interesting but are not strictly comparable between studies because of these differences in assumption. Small wonder that the author is troubled by differences in Table 2 and readers are left with the obvious conclusion that results differ widely because of model structure only.

The author does not define PSEs, but he assumes that the PSE for US grains is only the government deficiency payment. The OECD estimates of the US coarse grains PSE in 1983 was US $60 per tonne, not zero as the author states – as there were substantial government payments under the Payment In Kind programme (OECD). The author states 'we need a CSE, too'. The author has not read the USDA and OECD studies or he would know that these exist and are part of the liberalization experiments.

In assessing the credibility of model results, Professor Gardner uses some invalid comparisons of events in the 1985–7 world rice market. Gardner notes that under the 1985 Farm Bill the United States eliminated its market support. Gardner suggests that the results in Table 2 for rice should be equivalent to this change. In fact, these are medium-term results if all countries and all commodities were liberalized. He also cites the results from Tyers-Anderson and IIASA unilateral US reduction in *all* commodities as more comparable. Since the author acknowledges that seasonal and other supply-demand factors also influenced the rice market, his observations of three price points over 18 months is difficult to interpret as only policy impacts. Nevertheless, he concludes that the short-run

decline was greater than model predictions. From this comparison of model and market results, he concludes that all models have little value ('Zilch') in explaining multilateral, multicommodity impacts. This is a surprisingly unscientific test to reach the sweeping conclusions of the paper.

He also observes that the law of one price appears not to hold for rice in that relative rice price movements in 1985–7 between countries differed in the short run. Whether this invalidates the law of one price or not is questionable. Short-run price variability between different grades of rice in different countries in a period of extreme changes in policy combined with normal variability in marketing and transport costs and differential demand and supply by product type would be expected. Annual price movements among types of rice are likely to be much more stable. The author notes that the internal/border price 'margin is quite difficult to measure, and tends to be unstable from year to year'. Surely some of the same variability exists between f.o.b. Houston and Bangkok prices on a monthly basis.

Finally, I would like to make some comments on what the author does not say. Based on his experience as a policy advisor, I was disappointed that Professor Gardner makes little reference to the variations in the information content among models and what type of information is required by decision makers. His analysis focuses mainly on world price comparisons. The three types of models reviewed provide quite different information. For example, the IIASA model provides considerable detail on input use and price of assets. The dynamic models are criticised for their 'dubious reliability'. No mention is made of whether this information is important. Should modellers concentrate on improving the dynamic specification? No assessment is made about how policies are incorporated in the model and whether more specific policies might improve both the information and credibility of the results. In sum, the author is highly critical of recent work without providing much direction for future improvements in the analysis.

NOTE

[1]For examples, see review article by Winters (1987).

REFERENCES

OECD, 1988, *Monitoring and Outlook of Agricultural Policies, Markets and Trade*, Paris, May.
Winters, L.A., 1987, 'The Economic Consequences of Agricultural Support: A Survey', *OECD Economic Studies*, No. 9, Autumn.

STEPHEN L. MAGIERA

There are two themes in Dr. Gardner's paper. First, the paper questions the credibility of the quantitative results obtained by various studies of agricultural trade liberalization. Second, it examines some of the methodological choices made by modellers.

Model credibility

Dr. Gardner appears to have little confidence in the models for two reasons: (1) the world price impacts resulting from trade liberalization differ across the models; (2) the models were unable to predict the effects of recent policy changes in the United States on the international rice market.

The fact that the models predict different world price impacts from trade liberalization is easily explained. Our analysis at USDA indicates that these differences are due in large part to the different support levels used in the models (Magiera and Herlihy, 1988). If a model predicts relatively large price impacts from trade liberalization, it generally uses high support levels, and vice versa.

TABLE *World price effects of full agricultural trade liberalization by the industrial market economies using the USDA model*

	OECD 1979–81	T/A 1980–2	T/A 1995	IIASA 2000
	Per cent change in world prices			
Ruminants	18(16)*	29(27)	44(51)	15(17)
Non-ruminants	6(3)	8(8)	11(9)	5(0)
Dairy Products	61(48)	71(61)	91(95)	37(31)
Wheat	4(–1)	12(10)	26(25)	19(18)
Coarse Grains	2(–3)	7(3)	10(3)	9(11)
Rice	8(.5)	15(11)	25(18)	16(21)
Sugar	12(10)	24(11)	52(22)	n.a.

Notes: n.a. = Not available.
T/A = Tyers and Anderson. The dates are the reference periods over which the models were run.
*Numbers in parentheses are the actual price impacts generated by the models. These numbers may differ from those in Table 2 of Dr. Gardner's paper because different sources have been used.

In one test of this conclusion, we have used the USDA model to predict world price changes from trade liberalization based on support levels from the other models (See Table below). In general, the USDA model predicts world price effects which are very similar to those of the other models – even the IIASA model which is structurally very different. One exception is sugar. The USDA model predicts much higher sugar price increases than the Tyers and Anderson model for both 1980–2 and 1995.

We have not yet analysed why support levels differ across the models, but two obvious reasons are that the comparative static models use different reference periods for measuring support and the projection models use different assumptions regarding future market conditions and government policies.

Although many of the detailed commodity results from these models probably could be questioned, the paper's attempt to validate the models based on a short-run disequilibrium situation in the international rice market is inconclusive. That market is very thin and the annual variability of international rice prices is among the highest of those products included in the models. The paper does not attempt to disentangle the price changes induced by policies in the United States from other factors which might have affected prices in the years under consideration.

Dr. Gardner's paper indicates that the rice price changes predicted by the models are relatively small compared with the changes actually observed in 1986. Rice protection is high in the industrialized countries, yet international rice prices are little affected by liberalization. This apparent contradiction is explained by the fact that production and consumption of rice in the industrialized countries constitute a relatively small proportion of world production and consumption. Therefore, policy changes in these countries have little effect on international rice prices. The real issue is whether or not subsistence rice farmers in developing countries will adjust to trade liberalization by the industrialized countries. If these producers do not adjust, the impact of liberalization on rice prices will be greater.

Model methodology

The paper's decision criteria regarding modelling choices are primarily technical in nature with a preference for models which are easy to interpret. Missing from these criteria are a list of issues requiring analysis and a discussion of those factors important to the analysis. While an analyst may agree that comparative static models based on an historical reference period are easier to interpret and thus preferred to dynamic models, policy makers may have a different view. They may wish to know the implications of trade liberalization in the future and the adjustment path to liberalization.

Similarly, the paper indicates a preference for elasticity models like those of the OECD and USDA over more complex general equilibrium models. The OECD and USDA models are based on reduced-form elasticities which subsume all factor market adjustments. However, the gains from trade liberalization ultimately depend on factor mobility. Therefore, some analysts might prefer a more general equilibrium model in which factor mobility assumptions are explicit. This information is also necessary if issues regarding compensation for changes in asset values are to be analysed.

Elasticities in the USDA and OECD studies are synthesized from literature surveys. The OECD reports that some model results depend more on the difference in elasticities across countries than on their overall level. However, we often do not know what is subsumed in the elasticities and whether or not elasticities from different studies are at all comparable. Thus, the theoretical

rigour placed on the elasticities in general equilibrium studies may have some advantages even though these elasticities are difficult to estimate.

The paper points out several problems with the use of a price wedge as a single measure of policy effects. There appear to be three issues in this regard. Are price wedges adequate: (l) as descriptive devices for comparing protection across countries; (2) as negotiating tools in the GATT; (3) to capture all policies in a model? Only the third issue is relevant in a discussion of modelling. USDA (and OECD) would apparently agree with the paper's conclusion that a single price-wedge measure is not always adequate. The USDA (and OECD) model appears consistent with the paper's recommendations of using PSEs to capture the effects of US deficiency payments, supply shifters to capture the effects of US acreage reductions requirements, and CSEs to capture the differential effect of US programmes on producer and consumer prices. How US grain programmes are modelled has a tremendous impact on model results for world grain markets and US agriculture. Therefore, Dr. Gardner's assessment of the difficulty of measuring the impact of programmes which have been in place for twenty-five years is very relevant. However, these difficulties arise in any study of US policy, not just in studies of trade liberalization.

Finally, the paper seems overly concerned with the point-estimates obtained from studies of trade liberalization. One general conclusion apparently reached by all the models is that the overall gains from agricultural trade liberalization are likely to be small relative to the dislocations caused in agriculture. A major question, which is not addressed in the paper, is how robust is this conclusion with respect to the methodological choices made by modellers.

REFERENCES

Magiera, Stephen L. and Herlihy, Michael, 1988, 'Comparing World Price Changes from Trade Liberalization Models,' Background paper for the IATRC Symposium on 'Bringing agriculture into the GATT,' 19–20 August, Annapolis, Maryland.