

Comments on Selected Papers
Session 3T: Agricultural Trade and Policy Analysis
Monday, August 3, 1998 1:30-3:00 pm
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I appreciate being asked to provide some thoughts on the four papers presented in this selected papers session. I enjoyed reading the papers and learned something from each of them. I recommend all four papers to all those who may read these remarks.

Although grouped into one session, each paper is quite unique in focus and subject matter and are only peripherally related. The papers by Giannakas and Fulton (GF) and by Bhuyan explore very different aspects of agricultural policy. GF investigate the theoretical ramifications of farmer misrepresentation of payment area for the economic analysis of income transfers to agricultural producers through the use of decoupled payments. Bhuyan, on the other hand, examines the welfare effects of efforts by the U.S. food and tobacco industries to influence agricultural policy through rent-seeking activities. GF stop short of providing an empirical test of the hypothesis they develop while Bhuyan is content with plugging some admittedly weak data into a formula and drawing some rather strong conclusions on rent dissipation and the social costs of rent-seeking. The papers by Brewster and Spreen (BS) and by Lee, Wailes, and Hansen (LWH) are more oriented to trade issues. BS provide a conceptual specification of a multi-commodity spatial equilibrium model for the world processed orange juice market which is long on model specification and short on empirical application. LWH, on the other hand, provide an analysis of the effects of the tariffication of Japanese and South Korean rice import controls which is long on empirical application and short on model specification. For each of the four papers, I would like to briefly summarize what I think I learned and provide some thoughts and suggestions that the authors might find useful.

In their paper, LWH take a look ahead at potential impacts of the next round of multilateral trade negotiations on the world rice market. They focus specifically on Japan and South Korea and give us some idea of what might happen in those two rice markets if their minimum access import requirements were replaced by their tariff equivalents and then reduced by 6% annually between the years 2001 and 2010. They utilize a non-spatial price-equilibrium econometric simulation model of the world rice market for the analysis. The paper is well-

written and the analysis straight-forward. The results teach us, once again, that the net economic welfare effects of freer trade are usually positive for both trading countries and the global economy. In this study, rice consumers gain more than producers lose from tariffication in both Japan and South Korea compared to the current minimum access policy so that both countries are better off with freer trade. Not only that, the gains from freer trade are greater for both Japan and South Korea, the two countries that free up their markets, than for their trading partners. One might wonder why, then, both countries have been so reluctant to open up their markets. The answer, of course, is that the political and social costs associated with the economic welfare losses that would be suffered by rice producers in both countries have been perceived to be greater than the economic welfare gains that might accrue to rice consumers from more open markets. So whether the interesting analysis offered by LWH is at all a plausible future scenario for either country, and therefore, whether the paper is at all relevant, depends critically on political, social, and other non-economic factors not considered by the model or in the subsequent analysis of the results. LWH admit that Japan has refused to consider tariffication but then blithely conclude from their analysis that Japan would be better off in an economic welfare sense with freer trade, a point that is already well understood by the Japanese government. What the Japanese government also understands but which is ignored in this analysis is that, for them, the political and social benefits of the welfare redistribution through the current minimum access policy exceed any net national welfare gains that might be achieved by freer trade. Consequently, for me at least, some quantitative treatment of the political economy aspects of a proposed tariffication of the rice import quotas of Japan and South Korea would be a highly useful addition to this analysis. At a minimum, some recognition and discussion of the political plausibility of the scenario being analyzed would be helpful. A few other ideas might be useful for the authors to consider. First, the “typical” country submodel presented on page 3 is for a net exporting country and differs substantially from the description of the Japanese and South Korean net importing country models described on pages 4 and 5. I have a number of severe reservations about the specification of the country models and the empirical estimation of the parameters. Time does not permit me to explore all of them in detail so I will mention only a few of the more salient problems that I see. In the “typical” submodel, the net export equation is specified as a hybrid between an export supply and an export demand equation so that the theoretically expected sign of the export price is not clear. For the model to simulate

properly, however, the sign must be negative so that this is an export demand equation specified using domestic market variables, a specification rejected by Thompson nearly 20 years ago. Also, the LWH specification of the Japanese and South Korean rice markets is quite interesting. I don't think I have ever seen "area harvested" used as the residual in a market clearing identity before. I find it difficult to believe that the rice land diversion policy of either country requires the harvested rice area to be determined in such a way. In fact, it would be impossible to do so since, according to the formula given, one would have to know the level of stocks at the end of the year as well as the levels of consumption, trade, and yields during the year to be able to determine how much area to allow to be planted to rice at the beginning of each year. In fact, this specification appears to simply be a way of avoiding the need to estimate a rice acreage equation for each country which would require detailed knowledge and data on rice policy and rice producer behavior in each country. Also, since a number of the behavioral equations in the country submodels include endogenous variables on the right hand side, the use of OLS to estimate the parameters seems inappropriate. Finally, the results and conclusion sections of the paper are disappointing, focusing mostly on numbers with little insight provided on what the simulation results and welfare measurements mean for Japanese and South Korean consumers and producers and their trading partners. Also, with a little additional work, LWH could offer a highly useful and, perhaps, more relevant analysis of alternative trade liberalization schemes and domestic policies that might allow each country to achieve domestic welfare objectives while allowing greater access to their rice markets.

GF provide useful insight into the relationship between cheating by farmers and the costs of monitoring and enforcement of agricultural policy. The paper does a good job of motivating the need to incorporate the possibility of farmer misrepresentation of payment area into the analysis of decoupling. The model is well laid out and justified although some assumptions made might be questioned. For example, they assume that the probability of farmer cheating being detected is a linear function of the area reported as eligible for payments above the actual historical acreage. In other words, the bigger a cheater you are, the more likely you will be caught. I am not sure why this is necessarily true. It might be interesting to consider the possibility of payoffs to regulators and enforcers in the game so that the bigger the potential return to cheating, the more likely enforcers and regulators will be paid off, reducing the probability of detection as the misrepresented area increases. Even if the relationship between the

probability of detection and cheating is positive, it is more likely to be nonlinear than linear because of the effort and cost required to catch many small cheaters as compared to a few large cheaters. The main drawback of the paper is the lack of any empirical tests which the authors indicate “could make the analysis of [decoupling] under costly enforcement more useful in practical policy settings.”

Bhuyan presents a straight-forward approach to identifying and measuring the welfare impact of rent-seeking in the U.S. food and tobacco industries with an emphasis on the extent of rent dissipation that occurs. He assumes a demand function for the industries, uses the Lerner index of oligopoly power to calculate the oligopoly price and output, and then calculates the standard welfare loss due to oligopoly and the oligopoly rents for the food and tobacco industries. He then attempts to determine the total expenditures on rent-seeking by U.S. food and tobacco industries, including the cost of lobbying, political contributions and related costs, which he subsequently divides by his estimate of oligopoly profits to determine the rate of rent dissipation in those industries. Bhuyan admits “extreme difficulty” in collecting data on rent-seeking expenditures so that his final estimate of such expenditures likely “underestimates the actual amount of money spent in such activities.” Although there is no way to tell for sure by how much his data underestimates the true level of expenditures on rent-seeking activities, we get one clue in the extremely low measures of rent dissipation in the food and tobacco industries he calculates (0.4%), compared to the much higher estimates normally reported in the literature ranging from 100% dissipation (exact rent dissipation) to something less than that. By comparison, Bhuyan’s estimates are extremely low and, therefore, suspect. Nevertheless, he forges ahead and draws the strong, and perhaps, unwarranted conclusions that “there was only a slight increase in social cost due to rent seeking in the food and tobacco industries” and that the “low dissipation imply (sic) that the food and tobacco firms were effective in protecting their oligopoly rents (by blocking entry or competition).” More likely, Bhuyan’s estimates of expenditures on rent-seeking in the food and tobacco industries were simply much too low. I would have expected a strong caveat to that effect in the discussion of the conclusions.

Finally, BS conceptualize a multi-commodity, spatial equilibrium, mathematical programming model for the world processed orange juice market which could be adapted for use with other commodity markets like coffee with multiple producing and consuming countries in which a number of inputs must be blended to produce a

number of closely related final products. The paper does a good job of laying out and explaining the conceptual model. However, on the first page, the authors promise to focus on the “application” of the model and not just the conceptual formulation of the model. Nevertheless, the “Empirical Application” section offers nothing more than some loosely connected, perfunctory comments on problems related to empirical application and data availability and adequacy. In fact, the “Empirical Application” section effectively argues against that the “attractiveness of these models for policy analysis.” They also argue that data needed for the model are “difficult to acquire” and that what data are available “mis-state (sic) the case” at times. The authors suggest that the conceptual model they present does a better job of describing the world orange juice market than the available data. Although I am not exactly sure what that means, the statement seems to suggest that the available data are not adequate for attempting an empirical application of the model. Undaunted, they then indicate that empirical testing of the model is underway. The reader is left to wonder how empirical application is possible with such poor or unavailable data. In any event, one conclusion that I came away with from this paper was that the authors apparently ran out of time in attempting to empirically test the model they develop and so opted to include some hand waving in the paper in place of the promised empirical application.

In summary, the four papers presented in this session are quite different in focus, approach, and presentation. They are similar in that they are each quite interesting and teach important concepts. Again, I enjoyed reading the papers and hope the authors find these comments useful.

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