Rice in Asia: Is It Becoming an Inferior Good? Reply

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Given the prominence of rice in Asian economies as well as the importance of Asia for world rice markets, it is not surprising that the results we reported in our article (Ito, Peterson, and Grant) were somewhat controversial. We concluded that declining income elasticities of demand for rice in Asia mean that predictions based on conventional elasticity estimates are likely to overestimate total demand. Although the authors of the two comments reach their conclusions using very different approaches, they appear to agree that our predictions underestimate future rice demand in Asia. This debate is important because certain policy decisions in Asia, as well as in other nations exporting or importing rice, may depend on how actual Asian rice demand evolves. In our reply, we respond to each of the comments in turn.

Reply to Huang, David, and Duff

Huang, David, and Duff raise a number of issues concerning data and method. In particular, they argue for the following procedures: (*a*) use of private consumption expenditures expressed in a common currency rather than per capita gross domestic product (GDP) expressed in national currencies, (*b*) use of domestic price data instead of world price data, and (*c*) the use of prior information in specifying the ridge regression. Using these procedures, they reestimate our model and obtain results that differ from the results we reported. However, their results do not differ greatly from those we reported. While the income elasticities estimated by Huang, David, and Duff decline more slowly than those we estimated and remain positive in two countries where our estimates indicate that rice has become an inferior good, the general tendencies predicted in our study are replicated in theirs.

There will always be differences in elasticity estimates derived from different research approaches and data sets (Gardiner and Dixit). Choosing between two sets of estimates is complicated in this case because, due to the use of ridge regression, the diagnostic statistics associated with each model are not reliable. Although the estimated standard errors in our model are substantially smaller relative to their corresponding coefficients than is the case for the model reported by Huang, David, and Duff, this cannot be taken as an indicator of statistical superiority because of the biases introduced by using ridge regression. Price has developed a formal procedure for comparing two models of this nature, but such a test requires more data, space, and time than we have available. However, the challenge to our results presented by Huang, David, and Duff is based critically on the procedural points they raise. In the absence of statistical indicators, an examination of the validity of these points is a reasonable way to approach the evaluation of their criticisms.

The first procedural point raised by Huang, David and Duff concerns the data series used to represent income. Huang, David, and Duff argue that per capita private consumption expenditure (PCE) is a better measure of income than gross domestic product (GDP) because it does not include government expenditure which, they suggest, is likely to grow more rapidly than the private consumption components of GDP. Although it may indeed be the case that, over time, a greater proportion of GDP is accounted for by the governments of these countries, this part of the argument is an empirical question for which the authors provide no support. If the change in composition of GDP is not great, it is likely that the two series will be highly correlated, so it makes little difference which is used. Correlation coefficients between PCE and GDP were computed for all the countries except Bangladesh, Nepal, the PRC, and Taiwan for which data were not available from the International Monetary Fund (IMF). These coefficients as well as the results of regressing PCE on GDP are shown in table 1. The results indicate that the two series are very highly correlated (most of the correlation coefficients are either 0.999 or 0.998), and we conclude that it probably makes little difference which series is chosen.

In addition to criticizing the data series chosen to represent the income variable, Huang, David, and Duff suggest that income should be expressed in a common currency rather than in individual national currencies. According to the authors, their income variable, *PCE*, is expressed "in real terms (deflated by consumer price index) . . . in US dollars converted by the purchasing power parity (PPP) exchange rate for 1980." The PPP exchange rate should account for relative inflation rates between the individual countries and the *numéraire* country, presumably the United

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Table 1. Statistical Relationship between Gross Domestic Product (GDP) and Private Consumption Expenditure (PCE) in Asian Countries, 1961–85

Country	Regression Coefficients[a]	R^2	Correlation Coefficient[b]
Japan	0.592	0.998	0.999
	(0.005)		
Malaysia	0.493	0.997	0.999
	(0.005)		
Singapore	0.425	0.987	0.994
	(0.010)		
Thailand	0.652	0.999	0.999
	(0.003)		
India	0.671	0.999	0.999
	(0.004)		
South Korea	0.603	0.997	0.999
	(0.007)		
Sri Lanka	0.769	0.997	0.999
	(0.008)		
Burma	0.860	0.996	0.998
	(0.010)		
Indonesia	0.573	0.998	0.999
	(0.006)		
Philippines	0.736	0.995	0.998
	(0.011)		

Data Source: International Monetary Fund.
[a] The regression coefficients are the slopes from equations with PCE as the dependent variable. The figures in parentheses are standard errors.
[b] Correlation coefficient between PCE and GDP in each country.

States, so it is difficult to see why the income series should also be deflated by some unidentified consumer price index. In addition, the use of the PPP exchange rates introduces variables (e.g., the U.S. inflation rate) that may have little to do with domestic demand in these countries. The income variables used by Huang, David, and Duff may be appropriate for countries such as Singapore, where all of the rice consumed is imported. For the other countries where imports generally make up less than 10% of domestic consumption (except in Malaysia where they account for 20%), the distortions introduced by this conversion may be worse than the statistical problem signalled in their comment.

The second procedural point raised by Huang, David, and Duff concerns the price data. It is, of course, preferable to use domestic price data rather than world prices if domestic prices are accurately reported. The reason we chose to use world prices is that we were unable to locate reliable national price series for rice and wheat. Huang, David, and Duff apparently have access to such series, although they do not document their sources. The authors indicate that their domestic price series and the ratios they discuss in the text of their comment are taken from "individual country sources" (p. 4), none of which is cited in the references. Although government policies can drive a wedge between domestic and international prices, it is difficult to isolate the domestic market completely from world market conditions. Even in Japan, with its highly protectionist rice policies, internal rice prices are not completely insulated from changes in world prices. The correlation coefficient between the Japanese government procurement price (deflated by the Japanese consumer price index) and the Thai rice export price converted to yen (also deflated by the Japanese consumer price index) and lagged one year is 0.76 over the period 1961–89 (based on data from Food Agency and IMF). This suggests that the use of world prices may not seriously undermine our results. Further, if the domestic prices obtained from individual country sources are not accurately reported, the statistical results could be worse than those based on the more reliable world price series taken as proxies for domestic prices.

The third procedural point raised by Huang, David, and Duff concerns the use of prior information in ridge regression. They suggest that the following criteria should be used in choosing the k-values for the ridge regression: (*a*) the k-values should be small; (*b*) they are expected to give rise to a negative coefficient for the inverse of the income variable; (*c*) the coefficients from the model should lead to elasticities that are close to the constant elasticities obtained from a double-log specification. The first criterion is not controversial. In specifying our model, we specified small k-values (0.00002, 0.05, and 0.1 compared with 0.002, 0.01, and 0.0005 in Huang, David, and Duff). With respect to the second criterion, we also expected the coefficients for the inverse-income variables to be negative in line with our expectation that income elasticities in Asia are declining as incomes increase, as we stated in the text. We did not constrain the coefficient estimates to be negative and, apparently, Huang, David, and Duff did not do so either because they report positive coefficients for the inverse-income variable in two countries, Bangladesh and South Korea. In terms of including the "prior information" embodied in the first two criteria suggested by Huang, David, and Duff, there does not appear to be much difference between the two models.

The third criterion really is not very useful. It does not provide a guide for determining what constitutes an estimate that is "not very far from the constant elasticities . . ." (p. 4). The justification for attaching particular weight to constant elasticity estimates appears to be statistical (see their footnote 5). There is no theoretical reason for taking elasticity estimates from a double-log specification as benchmarks for the empirical analysis. The double-log specification does not satisfy the adding-up condition of theoretical demand analysis and, thus, makes no more sense than some other specification as a reliable indicator of the order of magnitude of the elasticities (Phlips). The justification in footnote five would apply equally well to an ordinary linear model.

In addition, applying this criterion to the results presented in the authors' table 5 does not unambiguously favor the k-values they selected. For example, consider the estimates for 1975. Only four of the es-

timated elasticities in the column under k^* are closer to the constant elasticity estimates than estimates recorded in other columns. Seven of the estimates based on a k-value of 0.00001 are closest to the constant elasticities, with one each in the other columns. In fact, out of thirty-nine possibilities in table 5, only twelve of the estimates in the k^* column are "better" in the sense of being closer to the constant elasticities. One wonders exactly how the "prior information" contained in the constant elasticities was used to select the preferred k-values.

The comment by Huang, David, and Duff is based primarily on these procedural points and a few unsupported assertions. We have responded to the procedural points and leave it to the reader to decide whether the criticisms of Huang, David, and Duff are telling. On the whole, their analysis appears to be a somewhat "ad hoc" attempt to generate outcomes consistent with certain preconceptions. For example, the authors argue that negative income elasticities are implausible in low income countries. This is an empirical issue, and it is misleading to assume at the outset that a commodity can never be an inferior good in low-income countries. Daly et al. reported a negative income elasticity for rice in rural Vietnam in 1973, a time when that region would certainly have qualified as "low income." Another example of an unsupported assertion is the suggestion by Huang, David, and Duff that income elasticities in Thailand can be "expected to become negative only sometime in the 1980s." According to their own estimates presented in their table 3, the Thai income elasticity became negative in 1967.

Finally, Huang, David, and Duff argue that, based on their results, the countries with negative income elasticities account for only 10% of Asian rice consumption. They speculate that income elasticities for rice in most Asian countries will remain positive throughout the 1990s and that rice demand in the region will remain strong. In another study (Peterson, Lan, and Ito), we obtained estimates indicating that the income elasticity of demand for rice in the People's Republic of China became negative in the early 1980s. The income and price data used in this study were from Chinese sources, and the analysis was based on time-series estimates of the log-inverse-log model. These results are not subject to any of the criticisms Huang, David, and Duff raise because the data meet all of their recommendations, ridge regression was not used, and the Chinese account for more than one-third of total world rice consumption.

Reply to Bouis

In the other comment, Bouis argues that changing patterns of rice consumption in Asia may be due primarily to structural changes in Asian economies, notably those related to rural migration and the commercialization of rice production. These results actually complement ours in that they provide insights into the processes behind declining per capita rice consumption in some countries. Bouis's results appear to support his conclusion that aggregate declines in rice consumption may occur as "agriculture becomes more commercial and especially as rural population densities decline." This conclusion can be linked to our results if it is true that income increases in Asia are associated with agricultural commercialization and declines in rural population densities. While this association seems plausible, an empirical test is still needed. At any rate, Bouis's focus on country-specific explanations for aggregate changes is a useful contribution to the discussion of Asian rice demand.

Bouis does recognize some limitations in his analysis. We would add a few procedural and conceptual points to his list. In discussing rice consumption, Bouis relies primarily on expenditure data. Rice expenditure is not the same as rice consumption because different qualities of rice mean that there will be different prices at any given time. In Japan, for example, rice prices range from around 300 yen per kilogram to over 600 yen per kilogram depending on the quality (Ito, Wailes, and Cramer). Ito, Wailes, and Cramer have shown that higher-income consumers tend to purchae higher-quality rice rather than larger quantities. The figures in Bouis's table 1 do not reflect shifts of this nature, and his inferences concerning the quantities consumed by higher-income consumers may be misleading. Similar comments may apply to his second table depending on whether the quantities shown were obtained by dividing expenditures by average prices or whether data on quantities consumed were also collected in the expenditure surveys cited.

Bouis's empirical analysis is based on an equation relating changes in aggregate rice demand to a set of proxies that are supposed to measure changes in semisubsistence rice production and farm size, controlling for differences in rural and urban consumption. This equation does not appear to have a basis in economic theory because prices and incomes, variables normally associated with the theory of demand, are not included. Real retail rice prices are shown for most of the countries under discussion in table 2. In many cases these prices fell considerably between 1961 and 1981, a fact which may be important in accounting for consumption patterns. Even if one accepts Bouis's specification, it is not clear that the proxies used in the statistical exercise are good measures of the target variables. For example, the first variable is supposed to measure the "contribution of semisubsistence production of rice to total agricultural production." To measure this, however, Bouis uses total rice production and agricultural GDP normalized on the percentage of the population that was rural in 1961. This might make sense as a proxy if all changes in rice production took place on semisubsistence farms. This may be true in some countries but probably not in all of them.

In motivating his comment, Bouis suggests that we would explain changing consumption patterns by claiming that "urban incomes are higher and that higher

Table 2. Comparison of Real Retail Prices of Milled Rice in Asian Countries between 1961 and 1981

Country	1961[a]	1981[b]	Real Price of 1961 Rice[c]	Change[d]
				(%)
Bangladesh (taka/ton)	796 (11.5)[f]	7,613 (116.2)	8,043	−5.4
Burma (kyat/ton)	371 (27.3)	1,286 (100.3)	1,361	−5.5
India (rupee/ton)	608 (26.1)	2,272 (113.0)	2,632	−13.7
Indonesia (rupiah/kg)	44 (20.0)	212 (112.2)	247	−14.2
Japan[e] (yen/60 kg)	4,351 (24.2)	16,381 (104.9)	18,838	−13.0
South Korea (won/kg)	23 (6.6)	811 (121.3)	423	91.7
Malaysia (M$/ton)	463 (51.4)	990 (109.7)	989	0.1
Philippines (peso/ton)	470 (15.4)	2,660 (113.1)	3,451	−22.9
Sri Lanka (rupee/t.)	1,277 (32.9)	6,250 (117.9)	4,576	36.6
Taiwan (NT$/ton)	5,390 (29.3)	19,600 (116.3)	23,378	−16.2
Thailand (baht/ton)	2,320 (33.9)	7,900 (118.6)	8,125	−2.8

Sources: IRRI, IMF, Republic of China, and Food Agency (MAFF, Japan).
[a] The Indonesian and Japanese figures are for 1970 and 1960, respectively.
[b] Figures for Taiwan are for 1979.
[c] Calculated using the consumer price index figures and expressed in real 1981 values.
[d] 1981 rice prices relative to real 1961 prices.
[e] Brown rice.
[f] Numbers in parentheses are consumer price indices, 1980 = 100.

income consumers prefer to substitute more preferred foods for rice." In fact, we make no claims with respect to urban incomes. Our study relates national consumption to national income, focusing on aggregate income elasticities of demand. Thus, Pitt's elasticity estimates for rural areas are not strictly comparable to ours. Rural and urban demand elasticities often differ, as we noted (see also Moon, KREI, and Daly et al.). Further, we would emphasize that our estimates, as in the case of any elasticity estimates, pertain to marginal changes, not the large change Bouis suggests in his thought experiment on Japanese consumption at the beginning of his comment.

Conclusion

The authors of both comments have raised interesting points. In the conclusion of our original article we commented on some of the limitations of the analysis. Huang, David, and Duff, and Bouis suggest other limitations and offer further recommendations for an improved understanding of Asian rice demand. Their contributions are useful in refining the issues related to this important topic and offering alternative visions of the changing markets for rice in Asia. After reflecting on their comments, however, we see no reason to alter the overall conclusions we presented in our article. The results of our study on China (Peterson, Lan, and Ito) support and strengthen those conclusions, although we are more willing to defend the trends we identified in the two studies than the exact magnitude of the estimated income elasticities in specific countries at specific times. Bouis emphasizes the potential social costs of insufficient investment if future rice demand is underestimated. It is important to note that there would also be social costs if rice demand is overestimated and too much is invested in this sector at the expense of other economic activities.

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