Comments on "Knowing Where Organic Markets Move Next – An Analysis of Developing Countries in the Pineapple Market"

The paper examines the relation between conventional and organic markets using the pineapple market as an example. The purpose is to test three hypotheses: 1) organic prices move along with conventional prices but with a lag 2) cross-price elasticities are low within a certain range of price changes, and high when crossing a certain threshold 3) the organic premium, and hence the willingness to pay for organic products, depends on the relative size of the two markets in a non-linear way.

The author concludes that the conventional market seems to act as a price leader for the organic market while being unaffected by organic price behaviour, and that there is a non-declining price premium for organic products. Moreover, there are thresholds below which organic prices are unaffected by conventional price changes. These thresholds and the corresponding price adjustment behavior do not change over time. Hence, there is a "potential of the organic market and the number of farmers in developing countries who can potentially benefit from growing organic products."

The topic of the paper is interesting and the empirical results are credible. However, the paper can be improved considerably; the empirical analysis is not consistent, the comments on the results are shallow, and the author draws too strong conclusions from the small sample. Moreover, it is unclear (to me at least) how the theory presented generates the hypotheses

Detailed comments

I find the links between the relatively long theory section and the hypotheses weak, or poorly explained. Is there really a need for a hedonic approach and an equation with utility functions to motivate the hypotheses? The first hypothesis, i.e., organic price moves along with conventional price but with a lag, follows partly from profit maximization and the fact that the costs of production should be similar for conventional and organic pineapple. The second part of the hypothesis, the presence of a lag, is unlikely to be related to consumer behaviour; in fact, we do not know if there is a lag in retail prices since wholesale prices are used in the analysis. The second hypothesis, cross-price elasticities are low within a certain

range of price changes, does not follow from the theoretical model either. There should be many consumers with different cross-price elasticities, and when aggregated there is nothing in the theory that says that elasticities are low within a certain range of price changes. The reason the hypothesis might be correct is probably that consumers do not bother about small price differences; they might not even notice them. Moreover, there is certainly imperfect information, as the author mentions. The third hypothesis, that the organic premium and the WTP for organic products depend on the relative size of the two markets in a non-linear way, is even more distant from the theoretical model. At the end, the premium should depend on supply and actual costs of production, and how preferences for organic relative to conventional pineapple develop. Does the model really have anything to say about these changes and their relative importance?

The author describes the data well but some essential information is unclear. First, exactly what prices are analysed? The term 'wholesale prices' is not defined and there is no information about who the sellers and buyers are. This matters because the theory section discusses consumer behaviour, but it might not relevant if buyers are importers or retailers, who might have market power. For instance, the arguments in support of the about the lagged impact of conventional prices on organic prices hypothesis, are partly based on consumer behaviour, but consumer prices might differ from wholesale prices, both in levels and dynamics. (The other argument for a lagged effect is based on wholesalers' lack of information about conventional prices, but this is not credible since monthly data is used).

Second, the data is aggregated/interpolated over several countries, due to missing observations. It would have been interesting to have some information about how similar prices and price trends are across countries, since aggregation can distort the original series. In other words, the aggregation/interpolation should be defended.

There is a relatively long description about single-equation unit root tests and arguments for and against different approaches, as well as how to determine the number of lags. This is odd for several reasons. First, the Johansen approach is used when testing for cointegration. It tests for the presence of eigenvalues larger than zero, which corresponds to roots smaller than one. Hence, the tests are about unit roots. To use the Johansen approach to test for cointegration, without testing if the variables are stationary or not with the Johansen

approach is thus peculiar, particularly since the single-equation unit root tests provide conflicting results. Moreover, we cannot be certain that there is cointegration with the Latin American prices simply because the cointegration tests are significant, the Johansen approach might have found that one variable is stationary and the other one is non-stationary. It could also show that the African prices are non-stationary, even if the single-equation unit root cases show that they are stationary (in fact, all the price series in the data are border-line cases, as evident from the graphs). If the Johansen approach is believed to be accurate when testing for cointegration, it should be trusted when testing for unit roots as well. I thus suggest that all prices series are tested with the Johansen approach.

Second, in a normal sample it is not possible to draw a sharp dividing line between a series with a unit root and a highly persistent series. Hence, the results of the tests should be supported with point estimates of the roots, but these are not reported or discussed in the paper. Since a visual inspection of the Latin American and the African prices indicate that they have more or less similar persistence during 2005-2011, the results from the unit roots can be questioned. I suspect that the estimated roots are quite similar (the VEC models indicate this as well).

The specification of the model reported in Table 3.5 is not correct. There should be an intercept in the model, or an 'implicit' trend should be allowed for. The specification 'No intercept, no trend is wrong, since the means of the series are not zero. A follow-up question is why the cointegrating vector, eq. 3.3, then has a constant?

It is mentioned on page 16 that the variables are in logs. Are the unit root tests carried out with variables in levels? It might give a different result than when log-levels are used.

It is mentioned on page 12 on page that there is seasonality in the price series. However, no model appears to have seasonal dummies.

It is claimed on page 13 the there is no point in estimating panel data models with two series, which is correct. However, from this it does not follow that the markets should be analysed separately, as implied by the following sentence. The two regions are likely to be interdependent, so SUR might be a better approach. I don't' think it is worthwhile estimating

a SUR, but the statement about no loss of information in the second sentence should be removed.

The discussion about the results obtained with the VEC models can be improved. What is the long run relationship between African prices? It is not reported (there is one even if both series are considered stationary). Some sentences are unclear, for example "This effect is larger than the effect of the organic price AR term (0.28 in Africa, not significant in Latin America). The highest and most significant effect is of lagged on current conventional prices." (p. 17). And why are the lagged coefficients on conventional prices clearly smaller than -1? Maybe the models should be specified in log-levels, after all. Finally, doesn't the similarity between the models indicate that the results of the unit root tests are dubious or irrelevant?

The conclusions should be weakened since only four years of data are analysed. It is quite possible that the thresholds and the corresponding price adjustment behaviour do change over time. And does the study really show that "there is a larger potential of the organic market and the number of farmers in developing countries who can potentially benefit from growing organic products"?

Two minor comments. The use of 'we' in the paper is not always correct; when there is only one author 'we' should include the reader. Several sentences sound strange and should be reformulated. Moreover, remove 3 in the numbering of figures and tables.

Dick Durevall
Professor
Dept. of Economics, School of Business, Economics and Law
University of Gothenburg
Box 605, SE 405 30 Gothenburg
Sweden