

Thank you for this very helpful feedback.

To the best of my knowledge an integrative theory that explains the co-behaviour of two markets for the same product but with different hedonic characteristics is lacking and therefore I apply bits and pieces from various backgrounds. This admittedly weakens the link between theory and the empirical results.

Dick Durevall has some very helpful comments on our hypotheses. Considering the weak link mentioned above, I have to make clearer how our hypotheses are derived. Here are some quick responses. On the first hypothesis: of course production costs should be similar but their changes not necessarily when they are grown in different places and have different pest and disease challenges. It is true that I don't know if there is also a lag in retail prices. The reference to consumers at this point is clearly confusing. Nevertheless, on the reverse, it may well be that wholesale buyers behave in the same way. Why there could be a lag in wholesale prices and also in retail prices is explained on page 8. Concerning the second hypothesis, hedonic demand theory provides the basis for the existence of the two related demand curves, and the hypothesis follows from imperfect information and sluggish demand response. Hedonic demand theory also provides the basis for the third hypothesis. Using hedonic demand theory helps us to explain the existence of different consumer groups that value the organic attribute differently. These groups can be ranked according to their valuation of the attribute and this explains why the size of the premium depends on the relative size of the two markets. To arrive at the nonlinearity we have to add two ingredients. The first is the fact that supply response to increasing demand is necessarily lagged due to the production cycle and the conversion period to organic production (usually three years). Secondly, we have to assume that consumer valuations can change over time and this is based on empirical observations of markets. Hence, all hypotheses are consistent with hedonic demand theory, but they cannot be fully explained using only this theory.

Among the possible existing theories that help to explain our findings, I still consider hedonic demand theory as the most relevant one, even though the link between theory and empirical analysis is not fully satisfactory.

As highlighted by Björn van Campenhout, most of the literature on price analysis is looking at the market for a single homogeneous product and relations over space. Hence our approach is fairly new. It is also relevant since organic and other certified labelled products are gaining market share. There is quite a lot of research on both the production and the consumption side related to this trend, but price correlations between this emerging market and the mainstream market have not been looked at so far. One reason is probably the lack of reliable data and so I made a first exploratory attempt at assembling and analysing the limited data that there is. I therefore believe that this paper is an early stage contribution to a promising field.

Because of the short period of time for which data is available the conclusions can necessarily be only tentative and I should add that the thresholds and the corresponding price adjustment behaviour may change over the longer term. This includes the notion that for the available time period the study shows that price premia do not decline and hence I am confident that there is a larger potential of the organic market. There would only be no larger market potential, if there were no further consumers that would buy organic at a slightly lower price. In all other cases, even if consumer demand for organic food is not increasing, which is unlikely given available trend projections, any increase in supply would decrease the premium but also increase the market.

On how to best estimate the model, Bjorn von Campenhout has proposed an alternative estimation strategy, a smooth transition threshold autoregressive model. Since I still have difficulties explaining the size of the threshold, a model with a smooth transition might indeed make sense and I am happy to apply it in a revised version of the paper. With theory lacking I still believe that our explanation has some value to it and I would opt for testing which model works better.

As Dick Durevall rightly says in fact there is some information lost by not estimating a panel model. I appreciate the comments concerning the validity of the unit root tests and I agree that I can do more to verify our results, for instance reporting estimated unit roots and in improving and widening the discussion about the results and the persistence of the prices. In addition, we are happy to employ also the multivariate Johansen approach for testing for unit roots. Since it is rarely used and also not unproblematic in particular with respect to the influence of deterministic components in the specification, we would see it as an additional test rather than an alternative.

Concerning seasonality, there are two problems with seasonal dummies. The first is that seasons are not clearly enough defined to certain months. What I could do of course is include dummies for positive and negative deviations from the mean price for each year. Second both organic and conventional prices experience exactly the same kind of seasonality.

On the question on the data, specifically who are buyers and sellers, I cannot identify them. The data is from International Trade Centre market reports. They state "major importers and wholesalers based in Europe". This is all the available information.

On the aggregation of data over several countries: concerning countries of origin, for Latin American pineapple Costa Rica has a dominant market share of over 70%. For African pineapple the data could in principle be split into Côte d'Ivoire and Ghana, which are the main sources recorded and here the prices are highly similar. Concerning destination countries, the question is a bit more complicated since wholesalers are often active in several countries and the country recorded is often, but not always the country where the harbour is. I tested for differences in the time series data of different countries and did not find significant differences in the mean prices. There are however some differences in the maxima and minima. I also repeated the analysis several times leaving out various countries without detecting noticeable differences. The raw data is of course available from us.

Finally I have to add that the unit root tests are reported in levels, but results do not change if done in logs; and Table 3.5. is not correct, it should read intercept in CE, no trend.