Canadian Pork Imports and the Australian Pigmeat Market

Review of Modelling Reports for the Productivity Commission

Brett Inder
Department of Econometrics and Business Statistics
Monash University
Clayton 3168
Vic

Email: Brett.Inder@monash.edu.au

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1. Introduction

This report presents an evaluation of the econometric methodologies of the studies prepared for the Productivity Commission in its inquiry into the effect of Canadian imports on the domestic pigmeat market. In assessing the validity of the methodologies, the report will also offer comment on the extent to which the results and conclusions presented in each study are reliable, given the particular econometric approach adopted.

The outline of this report is as follows. Section 2 reviews the work of G.R. Griffith as reported in "The Impact of Canadian Pork Imports on New South Wales Pigmeat Prices", October 1998. Section 3 focuses on "Quantitative Analysis of the Impact of Imports on the Domestic Pig and Pigmeat Industries", October 1998. The submission by Purcell and Harrison is discussed in section 4, and the later submission by Purcell and Beard is discussed in section 5. A very brief summary follows in section 6.

2. Review of *The Impact of Canadian Pork Imports on New South Wales Pigmeat Prices*, by G.R. Griffith

This study explores the causal relationships between imports and prices of pigmeat. The author looks at several measures of imports - volume of imports, and value of imports. Several pigmeat price variables were also considered - retail, wholesale, and farm gate. Other variables were included in the models to capture "all other relevant information" - pigmeat production, retail prices for beef and lamb.

Several methods were used for analysing causality. All were seeking to ascertain whether there was any significant causal link between the import variable and the domestic price variable. Causality from imports to prices would imply that imports were indeed impacting on the domestic market. Causality in the other direction - from domestic price to imports - would suggest some price feedback from the domestic market to import levels.

2.1 Unit Root Tests

The first step in the analysis was to perform unit root tests on the various price and imports variables. Standard, up-to-date tests for unit roots were performed. Whilst there was some ambiguity in results between the tests and between the variables, I concur with the general conclusion that all variables should be treated as non-stationary, containing only one unit root.

2.2 Bivariate Causality Tests

The next step in the analysis involved a series of bivariate causality tests between an import variable and a price variable - all variables being first differenced. Granger causality and Sims causality was considered. In each model, several other "predetermined" variables were included. Granger causality of imports on price is easily tested for by looking for the joint significance of the lags of the import variable in a regression of price on up to 9 lags of price, and up to 9 lags of imports, as well as the predetermined variables. Causality in the opposite

direction is found in a similar regression with imports as the dependent variable. Sims causality uses a similar approach, but includes leads of variables as regressors - see the report for details.

The Granger and Sims causality tests were performed for each combination of import and price variables, with a range of number of lags included in the regressions, over the full sample (1990-1998) and a subsample of the data (1993-1998). The report found limited evidence of causality in either direction - some significant results were obtained for certain lag lengths and particular variables, but the evidence for causality in either direction was certainly weak.

I have one serious concern with the approach taken to causality testing here. I do believe this concern calls into question the reliability of the conclusions. I cannot say what impact addressing my concern would have - whether it would make it more likely to find causality or less likely. Let me outline my concern as follows.

Consider a simplified framework with just the two variables of interest y_t and x_t. We can, quite generally, write the data generating process (DGP) for (y_t, x_t) jointly as a Vector Autoregressive (VAR) process. The first equation of that VAR representation could be written as follows:

(1) $$A(L)y_t = B(L)x_{t-1} + e_t$$

where L is the lag operator, and $A(L)$ and $B(L)$ are polynomials in L. For (1) to adequately represent the relationship between y and x, e_t is usually assumed to be white noise, or at least stationary. It is easy to show that (1) has an error correction (ECM) representation

(2) $$\Delta y_t = \alpha(y_{t-1} - \beta x_{t-1}) + A^*(L)\Delta y_{t-1} + B^*(L)\Delta x_{t-1} + e_t,$$

where $\alpha, \beta, A^*(L)$ and $B^*(L)$ are simple rearrangements of the original polynomials (see Wickens and Breusch, *Economic Journal* Conference 1988, for example). From (2), it is easy to see that for there to be noncausality from x to y requires $B^*(L) = 0, \alpha\beta = 0$. Joint testing of this kind of hypothesis is quite difficult - see Toda and Phillips (*Econometrica*, 1993).

Now, the problem with the Griffith analysis is that it is performed on first differences of the data alone. That is, the first term in (2) - $\alpha(y_{t-1} - \beta x_{t-1})$ - is omitted, and noncausality is accepted if $B^*(L)$ is zero. Thus the Granger causality regressions are misspecified in an important way - omitting important variables from a model leads to biased estimates and inconsistent test procedures. The procedure will only be valid if indeed $\alpha\beta = 0$ - a restriction not tested in the analysis.

It is unclear in which direction this will affect the results. All we can say is that the results are likely to be affected reasonably seriously.

2.3 Multivariate VAR modeling

The next phase of the report considers multivariate vector autoregressive (VAR) modelling with the three pig price variables (farm, wholesale, retail), import volume, and domestic pig production. This approach was motivated by the lack of a clear picture emerging from the Granger causality and Sims results. Again, all variables were included in first differences. It is unclear whether other predetermined variables such as beef price were included.

In the context of this VAR model, the report performs a test of the significance of imports in explaining each of the price and domestic production variables. Impulse response functions were also used to measure the impact of a unit shock in imports on the various prices and on production. The results generally suggest that imports made little impact on any of the prices (except one marginally significant result for retail prices over one sample period, where the long run coefficient seems to have the wrong sign). The only consistent result was a significant positive effect on domestic production. Contrary to intuition, this positive effect suggest higher imports lead to greater domestic production.

My comments in relation to modelling with first differenced variables also apply to this analysis. Again, this VAR would most likely be misspecified , not only because of the omission of the "levels" term as in equation (2), but also because of the seeming omission of Beef price. I suspect, however, that the omission of this latter variable would have little effect on the outcome - if anything, this may overstate the impact on imports on pork prices, and since the impact is generally insignificant anyway, including Beef prices would likely only confirm the insignificant effect.

However, the omission of the levels term is more critical. It does, as with the bivariate causality results, bias the test outcomes in an unknown direction. I suspect that the conflicting and often counter-intuitive results from this VAR analysis could be due to the misspecification.

2.4 Levels Multivariate VAR Causality Testing

The final piece of analysis was based on a VAR with the same variables as described in section 2.3 above, but with all of the variables included in levels form, not first differences. Recent work by Lutkepohl and others shows a simple way of doing the statistical analysis which is valid in the current framework. The results show the long run coefficient of imports in each of the equations for price - farm, wholesale and retail - and for volume, with a couple of different sample periods. Generally these results show minimal effect of imports: they are not significant in the production equation, nor the farm price equation. Over one sample period, imports had a significant positive effect on wholesale price, and a significant negative effect on retail price.

I have one comment on the interpretation of these results. Being a multivariate VAR, the effect of one variable on another cannot be simply captured by looking at coefficients in that equation in isolation. For example, the results as reported suggest that imports affect retail prices but not farm prices in the sample from 1993-1998. However, once imports impact on the retail price, this may translate into an effect on farm price via the coefficients on lags of retail price in the farm price equation. The tests reported do not allow for that effect. In fact, the seemingly counter-intuitive result for wholesale price (higher import increasing the wholesale price) may be misleading - if the retail price has a strong positive effect in the wholesale price equation, then the higher imports may operate to at first push retail prices

down and wholesale prices up slightly, with a secondary effect of the lower retail prices pushing down wholesale prices so they are eventually lower overall as a result of the initial import increase. Such issues of the interpretation of causality in multivariate models are addressed in Dufour and Renault (*Econometrica*, September 1998).

Methodologically, this analysis does not suffer from the misspecification problem inherent in models with first differences. The results are likely to be valid and reliable. My only hesitation is that the procedure is notoriously low in power. The simple procedure used to perform the tests involves deliberately overspecifying the model in order to get standard statistical distributions for the tests. Overspecification does tend to hurt power, as some simulation evidence in the literature on this procedure has shown. This is likely to lead to a bias towards finding no significant effects, when they may be there. Power will also suffer because of the dimension of the model (a 5-variable VAR) and the strong collinearity of variables - the price variables will tend to move together, making effects on price "dissipate" between the three variables, producing a tendency towards not finding significant effects.

This levels multivariate analysis does, therefore, offer some suggestions that higher imports have driven prices down. The methodology is sound, and some significant and plausible effects are found. The fact that consistently significant effects were not found for all variables and all sampling periods may reflect lack of power in the procedures. This is still an open question.

2.5 Overall Assessment

The last paragraph of the Griffith report suggests that "if a position had to be taken", the VAR results would provide the most reliable guide. I would agree with this for the reasons given in the report, but also because the second set of VAR results are the only results which do not use first differences, as explained above. The misspecification implied by first differencing is likely to have a serious and unknown effect on the results.

Based on the levels VAR, then, the results indicate a strong possibility that imports affect prices. The Griffith report suggests that this is only on retail prices, and maybe on wholesale prices. The above discussion on interpreting causality analysis in multivariate systems indicates that the result is stronger than this - imports most likely affect all prices, some directly, and some through the other pricing levels.

It is difficult, given the results included with the report, to quantify these effects with elasticity estimates; however, indications are that the effects are not substantial. If effects were particularly strong, then significant results would tend to show up more consistently with the other sampling period, and even with the misspecified differenced models - strong effects are unlikely to simply disappear because of a partly misspecified model.

3. Review of *Quantitative Analysis of the Impact of Imports on the Domestic Pig and Pigmeat Industries,* from the Curtin University of Technology.

This study looks at multivariate cointegration results between certain key variables related to pricing and production in the industry. The authors find that the data contains unit roots, and use appropriate techniques to estimate cointegrating relationships. Within the multivariate error correction modeling of the series, tests for the impact of imports on other variables are performed. These tests look for both short run and long run impacts, and generally find little evidence of any impact.

3.1 Unit Root Tests

The report shows the results of standard Dickey-Fuller related unit roots tests, as well as tests for seasonal unit roots. They find that virtually all series have unit roots, but no seasonal unit roots. This suggests standard cointegration analysis is the appropriate way forward. The only notable exception to this is with the finding of a seasonal unit root in the import price for pigmeat. This led the authors to omit this variable from the subsequent analysis; its long run properties are incompatible with that of the other variables, and hence to include it would introduce spurious results.

This stage of the research has been undertaken well, and there are no real problems or surprises with the results.

3.2 Multivariate VAR / Cointegration Modeling

The main empirical component of the report is centred around a VAR model in five variables: Saleyard price of Pigmeat (PS), Retail price of Bacon (PR), Saleyard price of Beef (BS), Domestic production of pigmeat (PP), volume of pigmeat imports from Canada (CV), all in logarithmic form. Initially the analysis was performed without the import variable, CV, and then this variable was included. It is the significance of CV which is of primary interest in the research agenda.

The methodology involved first testing for cointegration based on the VAR model without CV, using Johansen's likelihood ratio test for cointegration. The study found evidence for one cointegrating vector. Similar coefficients in the cointegrating vector were obtained using the Fully modified least squares single equation method, with pig saleyard price as the dependent variable.

The methodology to this point of the research is fine. However, the finding of one cointegrating vector is contentious. Depending on which critical values are used, the test could easily have suggested no cointegration. This conclusion seems plausible given Figure 5.1. This graph of the purportedly stationary linear combination of the four variables implied by the cointegrating vector seems to be non-stationary. From simply looking at the graph, there appears to be a general downward drift in the series, with particularly large drops around 1990 and 1997. I would conclude from this graph that no cointegrating relationship has been found. This conclusion is partly supported by one slightly counterintuitive coefficient in the "cointegrating" relationship - equation (5.3). Here, for example, retail price has a coefficient of 1.67 in an equation for saleyard price. That is, a 1% increase in retail price corresponds to a 1.67% increase in saleyard price. For this to be feasible, retailers or wholesalers would have

to be absorbing increased costs through the saleyard price, only passing on two-thirds of this cost. Such a pricing strategy is unlikely in the long run. The coefficient on domestic production has the right sign, and if the equation is interpreted as an inverse demand curve, it would correspond to a realistic demand elasticity of around -1.

This cointegrating relationship was then tested for stability with a structural change test. In the absence of an import volume variable, any evidence of structural instability in the long run relationship at times corresponding to changes in the import market would suggest that imports have had important effects on domestic prices. The structural stability test showed up possible instability around 1989-1990, although tests which do not impose a date for the change showed this to not be a statistically significant break. This date is important as it represents the beginnings of the opening up of the market to imports. However, the weakness of the possible structural change result suggests there is no effect here to be found.

I would consider these structural stability test outcomes as reasonably neutral in the question at hand. The arrival of imports into the domestic market was a gradual thing, beginning at very low levels, and growing quite slowly. Thus any possible structural effects on the equilibrium equation would be gradual, and possibly not picked up by a "Chow"-type test designed for a one-off discrete structural change, even when the change point is not imposed. I am aware that such tests do have asymptotic power against even gradual structural changes (Andrews, *Econometrica* 1993), but there is no doubt they would lack power, and thus likely not pick up such a slow change over a long period.

The report then goes on to a potentially more fruitful and direct means of analysing the effects of imports on the domestic market. Canadian Import volumes is included in the VAR system, and the cointegrating relationship is re-estimated by maximum likelihood and by fully modified least squares. The results suggest very strongly that imports are not significant in the cointegrating relationship, whichever estimation procedure or sample period is used. This is the crucial result which drives the conclusions of the report: it suggests that there is no significant evidence that imports have affected domestic prices in the long run. Estimation of the corresponding error correction model, where the cointegrating relationship is imposed, also suggests no role for imports in driving even short run fluctuations in domestic prices.

Once again, there is no methodological problem with the approach taken here. The evidence seems compelling that there is no impact of imports to be found. However, there are unsettling aspects of the results. First, the estimated cointegrating vector once import volume is included is even more unusual. For example, the fully modified OLS results in Table 5.10 suggest that a 1% rise in retail price of bacon corresponds to a 2.3% rise in the pig saleyard price (similar results for the Johansen estimates). One would have expected a fairly stable margin between these two price variables, indicating a coefficient of near unity. Secondly, the finding of cointegration is again marginal, as in the case where import volume was omitted. The other surprising result here is that imports have no measurable effect. In theory, imported pigmeat represents an increase in supply to the domestic market. If the product was treated the same as domestic production, an equation for a domestic price variable ought to show a similar coefficient on domestic production and on volume of imports. Increases in either of these variables represent an increase in supply (holding other factors constant), and hence ought to drive the price down. These results suggest that an increase in supply via domestic production will drive price down, but that an increase in supply via imports will have no effect at all.

The report authors seem aware of this puzzling finding in their final discussion on page 66 and 72. Their discussion makes sense, but is, however, not consistent with the empirical findings in their report. They state (last line of p.66) that in determining saleyard pig prices, "the only variable which appears to be of importance is quantity or the supply of pigs". They then go on to say (top of p.72) that "if imports are to have an effect it will be important only to the extent that those imports represent a more significant proportion of total pigmeat production. ... to the extent that they (higher levels of pigmeat imports) would represent a more substantial increase in supply they would on the basis of our results depress saleyard prices." The first statement quoted is, however, not correct. The variable which is significant in the model is domestic production. The supply of pigmeat includes imported volume which was not significant. Thus the results in this report suggest that pigmeat imports increase, they will have NO EFFECT on domestic prices.

In defence of these results, the models are estimated over a period where import volumes are relatively low for most of the period. There may thus not yet be adequate data to measure the effect of imports. The authors may well be correct in the statement quoted above - higher imports will increase supply and may well drive saleyard prices down. However, this statement has no supporting evidence in the empirical results presented in this report.

3.3 Overall Assessment

Is there anything wrong with the empirical work undertaken in the Curtin University report? Basically, No. The work is competent, uses good, up-to-date techniques, and is quite thorough.

Does the empirical work get to the bottom of what drives the domestic pigmeat market and hence allow a fair assessment of the possible impact of imports? In my view, No. A number of questions still hang over the empirical results which lead me to strongly suspect that there are other important factors to take into account in the model. The most compelling concern is best seen from Figure 5.1 - this "cointegrating" error term appears to be non-stationary (an observation consistent with the formal hypothesis tests). The estimates of the parameters of the cointegrating vector and the subsequent tests on the impact of imports are all thus going to be affected by a possible "spurious regressions" problem, which biases results and distorts findings of test procedures. There are enough counter-intuitive aspects of the cointegration parameter estimates and test results to suggest that this problem is real with the results presented in this report.

In terms of the question of the impact of imports on the domestic market, the findings of this report indicate strongly that there is no causal link from import volume to domestic prices. I believe, however, that there are enough concerns about the results to suggest that we should treat this "negative" result with a great deal of caution.

4. Review of *The impact of trade liberalisation and increasing imports on the Australian pig industry,* by T. Purcell and S. Harrison (20 September 1998).

I will give some very brief comments on this submission, focusing on methodology and the validity of the conclusions drawn. Such brief comments cannot do the report justice, but I am confident that I have highlighted the main issues. The outline of my comments will follow the chapters of their report.

4.1 Stationarity tests (Chapter 6)

The authors conduct a number of tests for unit roots, including taking account of seasonality and possible structural breaks. The techniques used here seem fine, and I do not have any problems with the analysis. My only general comment is that one must always bear in mind the potential unreliability of such tests with small samples. When tests throw up results which seem important but counterintuitive, one must treat the results with caution. For example, the finding of seasonal unit roots in import volume and price but not in any other variable is a bit of a puzzle. Why would we expect these variables to display such different seasonal behaviour? Subsequent analysis really ought to bear in mind the uncertainty surrounding this outcome.

4.2 Imports and Producer Prices (Chapter 7)

The authors first consider a possible structural break in an equation for saleyard pig price corresponding to the introduction of imports in 1990. They find evidence in favour of a break. However, subsequent analysis (section 7.3) shows that the model needs several lags in order to be well specified. These structural break results are thus based on a misspecified model, and the results suspect. The results themselves seem quite unsatisfactory - only the time trend (why is that there?) and (in one case) retail price is significant in Table 7.1. No significant quantity effect is found. Results in Table 7.3 are also unimpressive. The results are based on a mix of first differenced and levels variables (based on the unit root tests of the previous chapter). As pointed out in section 2 in my discussion of the G. Griffith report, there are serious misspecification problems with simply differencing data to achieve stationarity. There are other problems of possible endogeneity on the right hand side, which I discuss in the next paragraph.

The authors then move to consider a distributed lag model of saleyard price of pigmeat. The model includes saleyard price of beef, domestic pigmeat production, retail price of pork, volume of imports from Canada, and price of Canadian imports, as well as lags of these variables and quarterly seasonal dummy variables. This model reveals a most striking result. Working from the last paragraph of page 68 of the report, a 1000 tonne per quarter increase in imports will drive the saleyard price down by 20 cents/kg. In contrast, a 1000 tonne increase in domestic production leads to a 2.6 cent drop in price. This result seems remarkable, and extremely important to the question at hand. One might expect an increase in imports to have a similar effect to increasing domestic production - increased supply, regardless of source, drives the price down. However, to find that its effect is an order of magnitude stronger is to reveal that imports must be having some other impact on the domestic market apart from simply increasing supply.

Given the crucial nature of this finding, one must look closely at how well it stands up to critical inquiry. There are a few concerning aspects of this result. First, whilst the coefficient of import volume is large, it is only marginally significant - a t-statistic on the long run coefficient of 1.95 (Table 7.5). This can make one cautious that a small change in data period or model specification may cause the result to disappear. Second, note that when the model is estimated in log form (Table 7.4), none of the variables were significant, and the impact of imports was very small. It is surprising that such a small change in functional form should have such an effect. Third, no diagnostics are reported with the equation, nor do we get to see how robust the coefficients are to slight variations in lag lengths chosen, etc. Such concerns are particularly important when the reported result is only marginally significant. Fourth, some concern must be expressed about the endogeneity of various regressors in the equation. Whilst it may be possible to argue for the exogeneity of Beef price and import price and volume, it is unlikely that domestic production is exogenous. Domestic production will surely respond to price level (even if with a lag), and hence there will be feedback which will bias the coefficients of all variables in the regression. My hunch is that this factor alone is not likely to create serious biases in coefficients on imports - the lag in feedback helps the situation, and there are reasonable arguments to suggest that imports are exogenous, so the only bias here is what is inherited from the biased estimate on domestic production. However, the problem could be serious, and together with the other problems I highlight here, must at least create some uncertainty about the reliability of the conclusions.

4.3 Imports and Domestic Production (Chapter 8)

The authors then undertake a virtually identical analysis to their chapter 7, with domestic production as the dependent variable, and saleyard price as one of the explanatory variables.

The results show an even stronger structural change in the model, around 1990, which could be attributed to imports. However, all the comments made above in relation to the price equation apply equally. The model is misspecified due to lack of dynamics, virtually none of the variables in the models are significant (Tables 8.2 and 8.3), and the differencing of the data leads to further misspecification. I would thus pay little notice of these structural change results.

A distributed lag model of domestic production was then estimated, with the following explanatory variables: saleyard price of beef, saleyard price of baconers, retail price of pork, volume of imports from Canada, and price of Canadian imports, as well as lags of these variables and quarterly seasonal dummy variables. A dummy variable for a structural break in 1990 was also included. Unfortunately the results here turn out somewhat less appealing than those in chapter 7. The key driver, saleyard price for pigs, ends up with a negative sign, indicating that higher prices lower production. This is inconsistent with theory, if one interprets this equation as primarily a supply equation. There is a hint of a small impact of import volume - higher imports lower domestic production, but this variable is barely significant.

The concerns expressed above (in section 4.2) apply even more strongly to this equation. One would not like to place much credence at all on the implications of these models. The endogeneity problem is even more severe when production is regressed on price, and may well explain the seemingly counter-intuitive sign on price. The only story that one could tell about these results that makes statistical sense is that domestic supply is relatively price-insensitive

(even in the long run), and that the equation here is primarily a demand equation, in which case the price coefficient is plausible. Whether this is an accurate description of market behaviour is another question.

Chapter 8 finishes with an estimation of an error correction model (ECM) for domestic production, which is a rearrangement of the distributed lag model. The ECM term in the model is crucial. It is where the information about possible long term effects of imports (or any other variable) will be found. Unfortunately the results presented do not include the coefficients in the ECM, so there is nothing interesting here to interpret. There is also no explanation of how the model is estimated (eg is this the Engle-Granger 2-step procedure? or a rearrangement of the estimates in earlier ADL tables?), so again it is not possible to make any meaningful comments on the results.

4.4 Imports and Wholesale Prices (Chapter 9)

The analysis of chapter 7 is then reproduced with wholesale price as the dependent variable. The results indicate no effect of import volume or price on wholesale prices. These results suffer from the same criticisms as those for chapters 7 and 8. They are, on the whole, disappointing.

4.5 Imports and Retail Prices (Chapter 10)

The analysis of chapter 7 is then reproduced with retail price for pork as the dependent variable. The results indicate no effect of import volume or price. An ECM analysis like that reported in chapter 8 was also undertaken. Again, nothing can be learned from this ECM analysis. Overall the results suffer from the same criticisms as those for chapters 7 and 8.

4.6 Granger Causality Tests (Chapter 11)

The authors conduct tests for bivariate Granger causality on the various variables. It seems this is done on differenced variables. Little detail is given on lag lengths, etc. These results suggest some causality of imports to production, and through this on prices. However, the use of differences is flawed, as pointed out in section 2 of this report. It is probably wise to not seek to draw any meaningful conclusions from this finding.

4.7 Multivariate VAR Analysis (Chapter 12)

The authors then seek to model a system comprising of logs of saleyard pig price, retail pork price, saleyard beef price, and domestic pig production, all of which had been found earlier to be nonstationary. No cointegrating relationship was found, and so these variables were differenced. Import volume and price were added to the model, not in first difference form. Variance decompositions suggest a significant role for import volume in influencing domestic price changes. Interpretation of this model is difficult, though, since the import variables are included in levels, and domestic variables have been differenced. While this may be consistent with the results of unit root tests, it makes little sense economically.

Multivariate Granger Causality tests were performed on the basis of this VAR model. Imports were found to have a significant causal effect on domestic prices. Once again, the inclusion of

imports in levels and domestic prices in difference form is problematic; it is hard to know what to make of significant causal relationships in this case.

4.8 Multivariate VAR Analysis (Chapter 13)

The authors repeat the VAR analysis of chapter 12 with the same variables, but this time variables are not logged. Surprisingly, this different transformation now yields two cointegrating vectors, where the unlogged data showed none. These cointegrating vectors are then included in error correction model regressions for saleyard and retail prices, and domestic production. Import volume and import price were also included: volume was significant in the saleyard price equation and almost significant in the production equation. The authors quantify the effect of imports on saleyard price: an extra 1000 tonnes of imports will reduce price by around 10 cents/kg. This result is not quite correct, as it omits the multiplier effect from the presence of saleyard price in the ECM terms. This inclusion would show the effect to be slightly larger.

The authors should probably have noted the magnitude of the effect of changes in domestic production on saleyard price: from Table 13.2, the coefficient from the first cointegrating vector indicates that a 1000 tonne increase in production will reduce price by around 37 cents/kg. This is a far stronger effect than changes in imports. It also seems somewhat unrealistic - such a change in production represents only a 1% increase, and yet it purports to drive price down by nearly 20%!

The results here are indicative of a possibly important role of import volume in the domestic market. Certainly, this approach justifies further investigation. However, there are too many unanswered questions about this work to be confident about the conclusions - why do the results differ so much between the logged and levels functional form? How do we make sense of some of the unrealistic values in the cointegrating vector? How sensitive are the ECM results to lag length, choice of variable, etc? What do the diagnostics of these models look like?

The state space modeling reported in this chapter is discussed in section 5.

4.9 Other VAR/ECM modeling (Chapters 14 and 15)

The authors consider various other VAR models. Chapter 14 considers a model with monthly data from 1993 to 1998, which includes import volume and price. The important result is the cointegrating vectors, presented in Table 14.4. No tests on these vectors are performed, so it is unclear whether imports have a significant effect. The significance of change in imports in the ECM models is less important, as this only represents short run effects.

Chapter 15 looks at a different set of variables, with a focus on the retail meat sector - variables include prices for pigmeat, beef, lamb and chicken. The model seems poorly specified, as the authors concede, and even if we accept these results, no light is shed on the role of pigmeat imports.

4.10 Summary

The submission contains a vast array of results using all sorts of techniques. Many of the results are, for various reasons, misleading or unhelpful to the questions at hand. However, some results - especially chapter 13 - suggest potential effects of import volume on the domestic market, primarily prices. However, the work would need to be undertaken more thoroughly and many of the unanswered questions resolved before one could state any firm conclusions from these results.

5. Review of *Endogenising structural change and the effect of imports on the Australian pig industry*, by T. Purcell and R. Beard (9 October 1998).

Again, my comments on this work will be brief. In this report the authors seek to focus in on possible structural breaks in the behaviour of pig producer prices in late 1997, early 1998. The authors argue that this price drop could reflect substantial increase in imports at that time.

One of the difficulties in analysis this question is that the period of primary interest is right at the end of the sample, and there are only a few observations in which to observe any effects. The authors try to deal with this by use of Kalman Filter estimation techniques for a State Space formulation of the model.

The multivariate state space model was fitted to a system of 7 variables, 5 of which were first differenced - the domestic pig (saleyard and retail) and beef (saleyard) prices, domestic production and exports, and two of which were fourth differenced (import volume and import price. These differencing operators were applied after finding unit roots in all data, with the imports series having seasonal unit roots. The model also included a constant, time trend, and quarterly seasonal dummies.

The graphical results suggest that the model fits well within sample. However, the fit and out-of-sample forecasts tend to go "off track" towards the end of the data period - late in 1997, early in 1998. The authors attribute this to possible structural change in the market induced by rapidly increasing imports. When the model is augmented with a structural break dummy variable for this period, the problem of poor fit and forecasts is alleviated, providing support for the hypothesis of a structural break.

I think initially it is important to point out that the use of a state space formulation and Kalman filter is in essence not qualitatively different to using a standard VAR analysis. The state space equations in the report - equations (2.1) and (2.2) - can easily be rearranged to give a VAR representation, where the only difference is in the treatment of the deterministic / exogenous terms - constant, time, seasonal dummies. We thus do not expect any qualitatively different results.

There are a number of serious concerns with the estimation of the model. First, the use of differencing to make all variables stationary is not a good approach. I have discussed this earlier, in most detail in section 2. The problem is accentuated here by using first differences for some variables, and fourth differences for others. The misspecification problems will be nontrivial. Secondly, the state space model seems to have been estimated in such a way where only one lag of the variables enters - equivalent to a VAR(1). Earlier work suggests that a higher order VAR is necessary to capture all the dynamics. This underspecification will certainly corrupt the results. Third, I note from the footnote on page 10 that estimation of the

model was problematic, and the seasonal dummies and trend had to be omitted. Omission of the trend is probably a good thing, as there seems to be no justification for it anyway. However, the seasonal dummies would be important to the analysis - there is clearly a seasonal pattern to the market. Failure to account for this may well lead to spurious results. Fourthly, there are no diagnostics with the results. All we see are graphs of model fit. Whilst this looks okay, would, for example, the residuals pass simple tests for autocorrelation? Without such information, we cannot really judge the adequacy of the model.

Turning to the results and the implication of them, the authors do make much of the good fit in the models. However, my impression is that the fit is nothing special - without some benchmark to compare against. Further, with the first differenced data, fit of the levels (which is what the graphs represent) will always be pretty reasonable within sample and one step ahead. Differenced models are kept "on track" in levels, as they are simply a generalisation of a random walk, where next period's prediction is simply today's value of the series. One is never likely to be too far wrong with such an estimate.

In terms of the finding of a structural change, the authors make much of the ability of the model to capture this when a change dummy is included. They may well be right, but I consider their conclusion to be far too strong, given that we have very little data after the alleged structural break. Only time will tell whether the break is permanent or temporary.

Overall then, there are several serious questions to be asked about this work. Whilst the raw data suggests that there does indeed seem to be a hint of some significant change in the market in late 1997 / early 1998, there are too many questions surrounding this study to say that it reliably sheds light on the statistical significance or causes of any such possible change.

6. Summary

Overall, this review of the reports exposes a number of methodological problems with much of the work. The different approaches seem to lead to a wide variety of conclusions. Some of the reported work has serious deficiencies which can only suggest that the conclusions drawn from them cannot be relied on. On the other hand, most of the results reviewed can be viewed as giving clues as to the role of imports in the domestic pig market. No particular set of results is definitive.

If a judgment had to be made, one would have to say that it is unlikely that imports have played a dominant role in affecting domestic prices or production. Many results fail to find any significant effect, and only a few questionable results show a particularly strong effect. On the other hand, the evidence also suggests that imports are not completely irrelevant to the domestic market. They do show up as significant a number of times - too often for this result to be entirely spurious.

It is clear that much of the ambiguity in the results is due to conflicting and at times inappropriate choice of methodology. However, even if one focused only on those results which use "acceptable" methodology, the outcome is not clearcut. Two possible reasons exist for this: first, that there simply has not yet been enough data for the effect of imports to be adequately measured. I believe this is a valid observation - whilst we do have five years or more of data where imports have been entering the market, they have comprised a relatively small share of the market, and thus their impact would be hard to measure. The second reason would be that the econometric methodologies have not yet been executed to their full potential. By this I mean that with more investigation, some results could be developed further, and I believe a clearer picture could emerge. It is apparent from even the "best" of the results reviewed that an adequate quantitative model of the workings of the pigmeat market has not been achieved. Only in the context of such a model can we measure with some confidence the role imports have played in shaping this market.