

Responses to the GIPSA and External Reviews of the Report

*Econometric Analysis of Fed Cattle Procurement in the Texas Panhandle\**

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We appreciate the time and effort the GIPSA economists and external peers have taken to review the above report. We found most of their comments and suggestions to be thoughtful and helpful in the revision process. In what follows, we first give a general reaction to the reviews. Second, we present the major points raised by each individual reviewer, respond to each point, and indicate if and where it was addressed in the report. We should note, however, that because the revised version of the report includes additional analyses, and the econometric models have undergone some major changes in specification, estimation, and interpretation, some of the comments and suggestions based on the earlier version are no longer pertinent to the current one and, as a consequence, will not be discussed in any great detail.

In general, the comments and suggestions by reviewers revolve around issues of economic theory, econometric estimation and interpretation, institutional detail, and scope of study. With regard to economic theory, some reviewers suggested alternative formal theories to explain the mechanism behind the relationship between the use of non-cash procurement methods and spot market prices. Other reviewers offered less formal ones. Doing justice to all would be quite a tall order. And, even if we were able to test all the theories suggested by the reviewers, there are other competing ones in the academic literature that deserve equal attention. There are spatial and non-spatial theories of backward integration. Some treat the firm as a production function, others as a governance structure. Some emphasize factor-market distortions

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\*The first draft of this report was titled "Captive Supplies and Spot Market Prices for Fed Cattle in the Texas Panhandle."

as an incentive for backward integration, others emphasize transaction costs. Moreover, most of the models to which the reviewers refer are highly stylized and, therefore, their propositions are somewhat difficult to translate to real-world fed cattle markets.

With regard to econometric estimation and interpretation, there were also several suggestions. Unlike issues dealing with competing economic theories, however, most of the suggestions dealing with estimation are manageable since no new data are required for implementation. Those dealing with interpretation are and will always be difficult to resolve as different individuals have in their minds different models (formal and informal) of how markets work.

While economic models must be tractable, they must also incorporate some institutional detail if they are to have any correspondence to reality. Deciding how much detail to consider hinges on what facets of the cattle business a researcher sees as relevant to the problem at hand. This could be a source of contention between practitioners, who do not see enough cattle business in the economic models, and analysts who strive to keep models manageable. Our approach was to distill as much institutional detail as we could from the information provided by GIPSA, and either incorporate such detail directly in the models or use it to guide interpretation of the econometric results.

Regarding scope of analysis, practically every reviewer offered a menu of things that GIPSA should have done or should do, and a series of suggestions about the kind of data it should have collected or should collect. Some reviewers have also suggested other analyses that are either not feasible with available GIPSA data or, in all honesty, are not relevant to the questions asked by GIPSA. We believe GIPSA is in a better position to judge the merits of alternative proposals for enlarging the scope of the analysis. Therefore, the primary focus of the individual responses that follow will be economic theory and econometric estimation and interpretation.

Professor Bailey's first major point deals with our reliance on the "aggressiveness" of bidding to explain the observed negative relationship between the use of non-cash procurement methods and spot prices at the plant level. He believes the whole discussion about the level of aggressiveness of bidding is just a bit fuzzy from the point of view of economic theory. In section VII (pp. 31-33) of the revised report, we provide an economic model underlying the analysis at the plant level. The model explains, in simple graphical terms, how relatively "high" volumes of pre-committed deliveries of non-cash cattle for a given decision period will lead a profit-maximizing packer to demand relatively few spot market cattle for that period. Assuming the packer possesses at least some degree of market power in the spot market, this will translate into relatively "low" prices being paid, on average, for spot market purchases.

We argued that our results from the plant-level analysis implied merely that plants with a "high" value for RATIO (the proportion of slaughter attributable to non-cash purchases) tend to pay spot market prices that are "low" relative to a given distribution of transaction prices. Professor Bailey, and other reviewers, appeared to be uncomfortable with our interpretation of

these results as having no implications about the impact that an across-the-board increase in RATIO would have on the overall position (the mean, say) of the spot price distribution. To more sharply focus the plant-level study on causes and effects expressed in relative terms, we have re-done the analysis in the following manner. Rather than taking the dependent variable to be the FOB feedyard price of a spot market lot, and including AMSPRICE (a regional average price for the day of purchase) as an independent variable, we have specified the dependent variable, RPRICE, in "relative" terms as the lot's FOB feedyard price minus AMSPRICE. (This follows a specific recommendation made by Professor Love.) Defined in this way, RPRICE can be interpreted as the departure of the lot's price from a representative average price for the day of purchase. Also, the key independent variable, RATIO, has been replaced by "RRATIO," the purchasing plant's value of RATIO expressed as a deviation from the average value of RATIO for the four Texas plants on the day of purchase.

The idea behind these changes, as we explain in more detail in section VII.1, p. 33, is that the aggressiveness of a plant's spot market bidding ought to be related to its degree of reliance on non-cash cattle *relative to its rivals' current degrees of reliance on cattle from non-cash sources*. To illustrate, consider the following scenario. In week 1, all plants have 20% of their near-term slaughter needs pre-committed in the form of non-cash cattle. In week 2, all plants have 30% of their near-term slaughter needs pre-committed in the form of non-cash cattle. All plants have a higher value of RATIO in week 2 than in week 1. All plants will probably seek to purchase fewer spot market cattle in week 2 than in week 1, but one would not necessarily expect them to be able to do so with less aggressive bidding and at lower prices. The reason is that spot market supply has been commensurately reduced. Now consider an alternative scenario. In week 1 all plants have 20% of their near-term slaughter needs pre-committed in the form of non-cash cattle. In week 2, plant A has 30% pre-committed while the other plants have 20%. Now one would expect plant A to be in an advantageous position in the sense that it should be able to acquire the spot market cattle it needs to fulfill its desired slaughter volume with less aggressive bidding at prices that are "low" relative to the current average of prices paid by all plants. It is not simply that plant A's value of RATIO is higher in week 2 than in week 1. It is that plant A's value of RATIO is higher *relative to rivals' RATIOS*.

Another major point has to do with our hypothesis that the mechanism behind the negative relationship between spot prices and non-cash deliveries is that of packers and non-cash cattle feeders intertemporally shifting non-cash cattle deliveries in response to price expectations. Professor Bailey sees the plausibility of the mechanism but asks whether such an explanation makes sense since feeders who sell on the spot market can also use the same publicly available information to decide whether to market or withhold their cattle. This is the same point brought up by Professor Richard Sexton, who further argues that intertemporal arbitrage opportunities available to spot market sellers will drive current and expected future price to equality, at least up to arbitrage cost. Price in this case is a martingale process: "Today's" best guess of the value of "tomorrow's" price is simply the value of today's price. So, according to this argument, no incentives for intertemporal arbitrage remain for marketing agreement feeders and forward contract buyers. Thus there is no reason to expect a relationship between non-cash cattle delivery volumes and price expectations.

We disagree. To begin, as we explain in more detail in section VIII.2, pp. 50-52, cattle lots are heterogeneous. Some have high arbitrage costs; some have low. When intertemporal arbitrage opportunities arise, lots with low arbitrage costs will be arbitrated first so that, in equilibrium, the surviving expected price gap would represent marginal arbitrage cost. Moreover, the distributions of arbitrage costs within the population of spot market lots does not move in lockstep with the distribution of arbitrage costs within the populations of lots subject to marketing agreements or forward contracts. A large expected price difference means that arbitrage costs among spot market lots are high on average (otherwise more of the expected price difference would have been arbitrated away through intertemporal re-allocations of lots). But because arbitrage costs among non-spot lots are not necessarily correspondingly high, more often than not, this will leave room for numerous marketing agreement or forward contract lots to be profitably arbitrated. So we should expect to see the predicted relationship between the volume of non-cash deliveries and price expectations.

The rest of Professor Bailey's points are either minor and deal with reporting additional information from the data set, or are supportive and help to reinforce our conclusions. We found his knowledge of the cattle business helpful and thank him for his constructive comments.

Regarding GIPSA's review comments on section VI of the original draft, most of their major concerns have been addressed by the new specification of the model detailed in section VII of the revised report. In particular, the new graphical model underlying hypothesis 1 is now consistent with the view that the spot market serves as a "residual" supply of cattle, planning horizons 4 and 5 have been deleted from the analysis, the potential exogeneity problem with AMSPRICE has been resolved by the new specification of the dependent variable (see above), and the confusion surrounding the interpretation of RATIO is alleviated through resort to the new "RRATIO" variable which expresses a plant's non-cash cattle usage relative to other plants' degrees of reliance on non-cash supplies.

Regarding the comments on section VII.2 (now section VIII.2), the GIPSA reviewers pointed out that the evidence we cite in Appendix B indicates that the volume of marketing agreement deliveries is actually determined two weeks in advance, rather than one week in advance as we assumed in the original report. So we modified the analysis to reflect this (pp. 48-50). Before we were looking for correlations between week  $t + 1$  deliveries, on the one hand, and week  $t$ 's price and week  $t$ 's expectation of week  $t + 1$ 's price, on the other. Now we are looking for correlations between week  $t + 2$  deliveries and the expectations, formed in week  $t$ , of prices in week  $t + 1$  and in week  $t + 2$ . Generally speaking, this and other changes to the analysis in the current section VIII.2 have strengthened the econometric results with respect to marketing agreement cattle.

For forward contract cattle, the length of the representative interval between scheduling and delivery is less clear. For these lots, the "purchase date" in the data should be the scheduling date (if available), so the difference between "purchase date" and kill date should reveal the length of the scheduling - delivery lag. The data indicated an average interval of between one and two weeks, so we redid the analysis for both assumptions: Deliveries of forward contract

cattle are scheduled one week in advance, and two weeks in advance. The GIPSA reviewers also suggested that the connection might be between delivery volumes and the expected *change* in price, rather than the expectations of the two price levels separately. We test that relationship and the results are discussed in the revised version of the report (pp. 57-60).

Questions relating to cattle quality were raised by several reviewers, so we have included a new section (section VI in the revised draft) in which we undertake investigations of two questions concerning differences among cattle procured by different methods: Are there cattle quality differences across procurement methods? Are there quality-adjusted price differences across procurement methods? We tackle the first question using a product-characteristic price model. We had expected to find marketing agreement cattle to be superior in quality to spot market cattle, which would be consistent with the conjecture of the GIPSA reviewers and what we understand to be the conventional wisdom. What we found was relatively little evidence of quality differences across procurement methods. To address the quality-adjusted price difference issue, we ran a price (delivered hot cost) regression with explanatory variables including quality variables, other relevant lot characteristics, and dummy variables identifying procurement methods on a plant-by-plant basis. We found evidence that marketing agreement cattle are paid more than spot cattle of equal quality and, for three of the four plants, evidence that contract cattle are paid more than comparable-quality spot cattle. For the case of contract cattle, some rough preliminary calculations suggest that the apparent " premia " may simply be due to futures market performance which, over the period of investigation, happened to favor basis forward contract sellers relative to buyers.

Dr. Ronald S. Fecso's main points revolve around bringing the analysis to the levels of packer versus packer bidding behavior, and packer-feeder negotiations. Regarding bidding behavior, he suggests in his review that it may be better to examine price impacts from one bidder relative to purchases made by his rivals during the same time period rather than relative to regional average price. We do not disagree but the approach would require observing and recording all bids and the identities of all bidders, and not just the final bid on a particular feedlot. That information is not available. Regarding the packer-feeder relationship, he suggests modeling the roles packers and feeders play in the negotiation process. A case study approach would be an excellent tool for studying such interactions, but that is beyond the scope of the current report. The final major point in Dr. Fecso's review is the sensitivity of the analysis to the assumption of one week, versus two weeks, advance notice in the scheduling of deliveries. This is the same point raised by the GIPSA reviewers and addressed in the report's section VIII.2.

Professor Alan Love's review calls for 1.) "broadening the analysis to consider additional theoretical literature relating to vertical integration," 2.) considering additional empirical analyses and resolving econometric issues that may strengthen the results, and 3.) considering a broader interpretation of the empirical results.

With respect to consideration of additional theoretical models, the alternative models to which Professor Love refers are selections from a much broader literature on incentives for vertical integration. Those he mentions belong to the class of models in which a firm is viewed

as a production function and incentives for vertical integration are driven by price distortions in factor markets. But there are several alternative explanations of why packers in a regional market may choose different forms of vertical integration/coordination, including the incentive to economize on the transactions costs of using the spot market. More importantly, the class of models to which Professor Love refers are "long-run models." The GIPSA data are well-suited to analysis of "short-run" issues (How do spot prices respond to week-to-week fluctuations in non-cash cattle delivery volumes?), but not particularly well-suited to the analysis of "long-run" issues (What would happen to the spot market price if the overall degree of reliance on non-cash procurement methods were to change; perhaps because of a new law restricting non-cash methods?).

However, we did consider the paper by Love and Burton because, in our judgement, it and the paper by Zhang and Sexton (see below) represent the latest theorizing about the impacts of vertical integration on prices received by independent producers. What we learned is that Love and Burton's model yields no unambiguous conclusions about the effects of the use of non-cash supply sources on spot market price. Without additional assumptions about the elasticity of supply by independent producers, the model could be consistent with a spot market price that increased, decreased, or remained unchanged with decreases in the proportion of input supplies procured on the spot market. However, one implication of their analysis is that the price paid to the input suppliers that are under the processor's vertical control (feeders with marketing agreements, for example) will, however, be higher than the price paid to independents in the spot market. Our analysis in section VI found evidence consistent with this prediction.

The additional empirical analysis Professor Love suggests includes changes in the regressions at the plant level and at the regional level. At the plant level, since AMSPRICE reflects all transaction prices in the region on a particular day, including it as an explanatory variable for transaction prices on a lot-by-lot basis may introduce correlation between a regressor and the error term and, consequently, bias the results. The remedy he suggests is to express the dependent variable as a deviation of each lot-specific transaction price from AMSPRICE, thus dropping AMSPRICE as an explanatory variable. He also recommended we test for the endogeneity of non-cash deliveries both in the plant-level and the regional-level model. As we noted earlier in our responses to Professor Bailey's comments, we followed Professor Love's suggestion and "moved" AMSPRICE to the left-hand-side of the plant-level regression. Having redefined, not only the dependent variable, but also the key independent variable (RRATIO) as deviations from mean values, we feel that the regressors are now more-arguably predetermined, thus avoiding the simultaneity bias problem. As for the endogeneity of the non-cash delivery volume variable in the regional model, we see this as less of a priority. Our objective in that analysis is not to determine whether the 2SLS estimates should be "favored" over the OLS estimates, or vice-versa. Our objective was simply to investigate whether the negative correlation that others have found between the use of non-cash supply sources and spot market prices at the regional level is present in our data too and, if so, whether the correlation is robust with respect to alternative estimation methods. For this reason, we did not carry out exogeneity tests on the regional-level model.

In matters of interpretation of the plant-level regression results, we argue that the negative estimate of RATIO's coefficient does not mean that market prices would increase, on average, if the use of non-cash supplies were restricted. Professor Love has some doubts. He argues that our conclusion is correct only if excluding purchases made by packers with high levels of RATIO would not affect AMSPRICE, our proxy for the mean of the distribution of transaction prices. Since the revised model specifies "both sides" of the relationship in the plant-level analysis (both the dependent variable and the key independent variable, RRATIO) as deviations from the mean, the discussion now is more explicitly framed in terms of changes relative to means, not shifts of the means.

He also argues that, short of the equilibrium analysis we mentioned in footnote 29 of the original draft, our story about the role of price expectations in the scheduling of non-cash cattle deliveries is not tenable as an explanation for the negative correlation between price and non-cash cattle usage. Our answer is simply that a full equilibrium analysis is beyond the scope of the report. However, we now make clear in section VIII, p. 61, the assumptions that implicitly underpin our "partial" approach: "(S)pot market transaction volume and price are jointly determined in each period; the resulting price (or, rather, the expectations thereof), in turn, influence non-cash delivery schedules; which then have only negligible feedback into spot market price determination." We feel that this simplifying assumption is justified in view of the fact that the cash market remains the dominant source of fed cattle (accounting for 71% of the fed cattle represented in the GIPSA data).

The bulk of the comments and suggestions by Mr. Dayton Lehman revolved around the investigative processes utilized by GIPSA and the information sought in its investigation. We believe that GIPSA is in a better position to respond to these comments than we are. Other comments and suggestions dealt with the price/non-cash-supply regression model described in section VI of the original draft. In particular, Mr. Lehman suggests, as did Professor Love, that we express the dependent variable in the regression model as a deviation from AMSPRICE.

Professor Richard Sexton evaluated the economic arguments we provided to explain the observed econometric relationship and shared with us an alternative model that suggests that packers strategically use non-cash procurement methods to depress cash prices. He also suggested that, because our story is not based on an explicit optimization model, there is need to examine its robustness to alternative specifications. However, since other studies have found the same relationships for different regions and using different data sets, Professor Sexton believes the data supports the plant-level model and the regional model.

Professor Sexton believes that we made a serious error in interpreting the results of the regional-level analysis as a reflection of the intertemporal arbitrage in which packers and feeders engage when they reschedule non-cash cattle deliveries in view of spot market price expectations. The error, Professor Sexton notes, has to do with our failure to consider that feeders who sell cattle on the cash market have similar incentives for intertemporal arbitrage. When one takes into account this feature of the market, Sexton adds, the implication is that the spot market price should be a martingale, leaving no profitable arbitrage opportunities for the

market's "fringe" of non-cash sellers and no reason to expect the relationship between price expectations and non-cash delivery volumes that we predict. One rebuttal to this criticism was provided earlier in our response to Professor Bailey who raised a similar point. Another way to respond to this criticism is to point out that, as an empirical matter, the series of spot market prices does not appear to be a martingale. If it were, a regression of price in period  $t + 1$  on variables in the period  $t$  information set would return a statistically significant estimate of the coefficient of only one explanatory variable: period  $t$ 's price. Our "one-week-ahead price forecasting equation" is just such a regression. The estimation results, reported in Table VIII.2.1 include statistically significant estimates of coefficients for a number of right-hand-side variables other than  $p_t$ .

In the alternative economic model Sexton proposes to provide a rationale for the use of non-cash procurement methods, packers divide their market areas into spot market and long-term contract regions. By offering long-term contracts to cattle feeders near the boundaries of the market areas, two packers can create a geographic buffer between them, thus enabling a heightened degree of monopsonistic exploitation of the remaining independent cattle producers. The role of the buffer region is to make it unprofitable for a packer to jump the buffer and compete directly within a rival packer's spot market territory. If this story were at the heart of packers' incentives for the use of non-cash procurement methods, we should observe two stylized facts in the data: Cattle procured by non-cash means tend to be shipped farther, on average, than cattle purchased in the spot market. And packing plants rarely purchase spot market lots from feeders that are located closer to a rival plant than to the purchasing plant. As we detail in the report (p. 14), neither of these predictions is consistent with the data.

Professor Suslow raises several points. One, which is similar to that raised by Love, is the problem of including AMSPRICE on the right-hand-side of the price equation in the plant-level analysis. Suslow also questions our approach of presenting results from a variety of regressions, with different specifications for key variables, rather than the results of only one. She labels the approach "data mining." We disagree. In our view, we would be guilty of "data mining" had we reported only those regressions that best supported our hypotheses. In fact, our reason for estimating several specifications of a single regression was to provide the reader with an indication of the robustness of the analysis to alternative specifications. As noted earlier, some of her suggestions for changes to the plant-level model have been incorporated in the revised draft.

Professor Wohlgenant's review suggests that we have missed the point by focusing on the relationship between non-cash procurement methods and the spot price. The focus should be on monopsonistic market power as reflected in the relationship between the cash price and the number of cash cattle marketed. Professor Wohlgenant's point is well taken, but testing monopsonistic power was not among the objectives of the project. More importantly, a complete model of monopsonistic power would also have to take into consideration the influence of pre-committed non-cash cattle on the ability of packers to exercise monopsonistic power (see Azzam, 1998). A number of econometric improvements suggested by Professor Wohlgenant and others were addressed in the revised report. In particular, the forecasting equations in section VIII.2

were tested and corrected (where needed) for autocorrelation. We estimate the counterparts to equation (2) of the old report by OLS, as before, but now we base the t-tests on standard errors that are robust with respect to heteroscedasticity and autocorrelation. These changes produce econometric results that are stronger than before.

In sum, the external peer and GIPSA reviews of our report's earlier draft produced a wealth of good comments and suggestions, and we again would like to express our gratitude for the time and effort invested in those reviews. In fact, as we discovered, the reviews produced far more worthwhile comments and suggestions than could be exhaustively treated in our revision. The revised draft of the report, and this document, reflect our best efforts to address the points raised by the reviewers within the time and resource limitations of the revision process.