REVIEW OF GIPSA LIVESTOCK AND MEAT MARKETING STUDY
VOLUME 2: DATA COLLECTION METHODS AND RESULTS; FIRST DRAFT FINAL REPORT
PREPARED BY RTI INTERNATIONAL IN NOVEMBER 2006
REVIEW SUBMITTED 22 DECEMBER 2006

Volume 2 discusses data collection procedures used to survey cattle, hog, and sheep producers; beef, pork, and lamb packers and processors; and downstream market participants, including wholesalers, retailers, food service operators, and exporters. Specifically, sampling frames for each segment are outlined and survey pre-test procedures are explained. The survey was designed to solicit information about purchase and sales methods (particularly spot market versus alternative marketing agreements) for livestock and meat products. Results generally indicated respondents most heavily relied on spot market transactions for both sales and purchases, citing they provide the level of independence, control, and flexibility needed for their operations. Volume 2 also outlined procedures and sample selection for the transaction data collection for meat packers and processors.

In general, Volume 2 is thorough and well-organized. It contains a significant amount of detail from survey responses, yet the text itself is relatively concise. The researchers appear to have employed appropriate methods in most cases. The quality of writing is excellent throughout the volume. The following comments are offered and intended to make additional improvements and/or suggest other possible methods, etc. They are grouped as "Major Comments" (more substantive in nature, revision should be considered if appropriate) and "Minor Comments" (primarily editorial corrections).

<u>**Major Comments**</u>**:**

1. **Section 2.1.1. By using the Dun & Bradstreet database to identify cattle producers by SIC code, the researchers significantly limited the population of cattle producers to survey. For example, the researchers report 14,166 feedlots in the "universe size" to survey (Table 2-1). The February 2006 *Cattle on Feed Report* estimates 86,000 feedlots with less than 1,000 head capacity and 2,199 feedlots with more than 1,000 head capacity, for a total feedlot number of 88,199 (USDA National Agricultural Statistics Service). NASS also reports in the 2002 Census of Agriculture 1.018 million farms with cattle and calves; thus, the researchers' estimate of the "universe size" of "Ranching and farming" at 35,442 significantly limited the population size (Table 2-1), and likely eliminated smallsized operations. The use of the D&B database, rather than the NASS database, likely eliminated from the population several small-sized operations. Further, by excluding establishments without reported revenue or employees, the researchers eliminated additional small-sized operations. The researchers point out that using the NASS database would have required NASS to obtain informed consent from the survey participants. An alternative way to have done this would be to have NASS mail the surveys rather than GIPSA or the researchers (so NASS would not have to release names and addresses). Additionally, the survey respondents should have been informed of their rights as a human survey subject (informed consent). Was the survey externally**

reviewed to be sure that it would be minimally invasive for respondents, and were they informed that their participation or lack thereof would not affect their relationship with the researchers, GIPSA, or USDA?

RTI response: As described in Section 2.1.1, we evaluated the feasibility of working with NASS to draw the sample for livestock producers and feeders. In a teleconference with NASS, we discussed the alternative you proposed (i.e., having NASS, instead of RTI, mail the survey to respondents); however, in consultation with our statistician we decided against this option because of the desire to maintain consistency in survey procedures across all industry segments and because of time constraints for completion of the study. Although using the D&B database instead of NASS data to develop the sampling frame for producers yielded a smaller universe size, we still surveyed many small operations. Seventy-two percent of operations have total gross sales of less than $100,000, and about 65% of operations have less than 50 head of beef calves in inventory.

In regard to your comment about the exclusion of establishments without reported revenue or employees, our experience suggests that these are operations that are no longer in business or fail to qualify for the survey. Thus, it increased the cost-effectiveness of survey administration to exclude such establishments from the sampling frame, the number of which was very small in comparison with the included establishments in each segment.

In regard to your comment about obtaining informed consent, our survey procedures were reviewed by RTI's Institutional Review Board (IRB). As required by RTI's IRB, we provided information on informed consent in the instructions to the survey questionnaire and in the information brochure. In the instructions to the survey questionnaire, we state that "Your response to the survey will not be used as the basis of an investigation or enforcement action against your plant."

2. **Section 2.1.1. Despite the survey population potentially eliminating many small-sized operations, the researchers determined that an insufficient number of larger sized operations were included. Therefore, they included or substituted several sample operations with operations on National Cattlemen's Beef Association (NCBA) lists. Presumably, this would include only NCBA members. This inherently biases the sample by excluding non-NCBA members. Quite likely, cattle producers would have different views on marketing and contracting according to their affiliation with various industry associations.**

    RTI response: We contacted NCBA about the list we used to supplement the sampling frame for cattle producers. NCBA stated that this list includes operations that are NCBA members, as well as operations that are not NCBA members. We added a footnote in Section 2.1.1 to clarify this.

3. **Section 2.1.2. The researchers excluded from the meat packer sample all state inspected plants and those that slaughter fewer than 50 head. This again reduces the number of small plants potentially surveyed. The researchers point out that many of the smallest plants would not likely use alternative marketing agreements (AMAs), which is likely correct. However, this is a good opportunity to discover *why* they don't use AMAs and to probe deeper into the reasons they are not a good fit for small-sized operations.**

   RTI response: Although state-inspected plants and plants that slaughter fewer than 50 head were excluded from the sampling frames for meat packers, small plants are still represented in the survey. About 66% of the beef packers surveyed purchased fewer than 1,000 head of steers and heifers during the past year, about 44% of the pork packers surveyed purchased fewer than 1,000 market hogs during the past year, and 86% of the lamb packers surveyed purchased fewer than 10,000 lambs during the past year.

4. **Section 2.1. The researchers state that they use annual slaughter volume as the size criterion for packers. Why not use capacity (number of head) for the size criterion for the livestock producers as well? Typically, livestock operations are sized by number of head, rather than gross sales.**

   RTI response: We agree that livestock operations are generally sized by number of head, rather than by gross sales; however, information on number of head for producers was not available in the D&B database. Because number of head and gross sales are highly correlated, we believe that the size criterion for producers allowed us to differentiate small and large operations.

5. **Section 2.2. The assumed response rate of 60% or greater is very high. This would be an unusually high response rate for any survey, but a complicated, 20-plus page survey will not generate such a response rate, particularly from livestock producers. The survey respondents were not given any incentive to participate (other than knowing they were able to express their opinions), which may have helped improve the survey response. Given the length and complexity of the survey (for livestock producers in particular), a higher response rate might have been generated with a phone survey or some other method.**

   RTI response: In other industry surveys conducted for USDA with a questionnaire similar in length and complexity to the questionnaire used for this study, we achieved response rates of 65% to 78% among meat and poultry slaughter and processing plants. Despite the procedures we implemented to help increase the response rate, our response rates for the GIPSA surveys were lower than anticipated.

   The survey instrument and procedures for the study had to be approved by the Office of Management and Budget (OMB). We did not offer monetary incentives because OMB generally does not approve of the use of incentives without first conducting an experiment to evaluate the effectiveness of incentives, which was not feasible given the time constraints for completion of the study.

Our survey procedures included a telephone call to the operation to screen for eligibility and to identify the person to complete the survey. The survey was then mailed to this person. We found that it was very difficult to contact some operations by telephone, and ultimately we sent the survey to some operations without completing this initial call. Thus, we think that the response rates likely would have been lower if we had conducted a telephone survey instead of a mail survey. Also, given the complexity of the survey and the need for the survey to be completed by more than one person (particularly for larger operations), it would have been difficult to administer the survey by telephone.

6. **Section 3.2.1 -3.2.2. Additional specifics on the Question Appraisal System would be helpful, if possible. Also, what are the "probing techniques" used to evaluate respondent comprehension in the pre-test interviews?**

   RTI response: As suggested, we revised the description of the Question Appraisal System to provide additional information. Also, we added an example of a probing technique (e.g., explain what you mean by your response).

7. **Section 3.3. Nonrespondents to the mail survey were contacted to remind them to complete the survey. How were nonrespondents identified? Were respondents asked to provide their name/address on their surveys (or leave the label on the front page of the survey)? If so, that would likely decrease the response rate to the survey substantially.**

   RTI response: To correctly develop the survey weights, it was necessary to keep track of whether each selected establishment was a respondent or nonrespondent. Therefore, each survey questionnaire included a label with the survey ID number. This unique identifier allowed us to map the questionnaire to the survey respondent, as well as to maintain confidentiality of survey responses. Using labels with the survey ID number may have reduced the response rate to the survey, but we do not have any way to assess the impact; however, we had to use the labels with the survey ID number to develop the survey weights.

8. **Section 4. In addition to the lower than expected response rate, the actual eligibility rates were lower than the researchers anticipated. This caused the researchers to use the "reserve sample" for some segments of the survey. Please provide additional explanation regarding the source of this reserve sample.**

   RTI response: As a contingency plan for compensating for lower than expected eligibility and response rates, a somewhat larger sample was selected for each segment during the initial sample selection stage. The resulting reserve samples were then portioned into random replicates and released as such so that at any time during the data collection the released portion would constitute a random sample. Section 2.2 describes the selection of the reserve sample.

9. **Section 6.1. Most of the beef producer respondents were cow-calf producers, with relatively few feedlot operators amongst the respondents. Feedlot operations would be more likely to use AMAs because they routinely purchase and sell cattle throughout the year. By having a small portion of feedlots in the beef producer group, the results may show less use of AMAs than actually exists. Further, this results in having a greater proportion of smaller-sized operations in the respondent group that are less likely to use AMAs and focuses the attention on the feeder cattle market rather than the fed cattle market (where more AMAs are likely to exist). This also seems inconsistent with the researchers' goals in terms of wanting to select businesses that are likely to use a variety of AMAs (as they did for the transaction data collection, see Section 10). It would be helpful to have the cow-calf and feedlot operator responses separated. Alternatively, the results could be weighted by the number of head, rather than weighting all producers equally. Doing so would alleviate the challenge that the results indicate that few producers use AMAs, but a large number of livestock may be traded by those that do use AMAs.**

    RTI response: We stratified the sample for beef producers by type of operation and size and computed survey weights based on the number of eligible operations to reflect the sample design. We could have reported results by type of operation (cow-calf versus feedlot) but did not because it would have generated a very large number of tables. We considered weighting the results by number of head, but no reliable external data source was available on the number of fed animals sold by state. The analysis of the transactions data in the cattle/beef volume provides additional information on sales by feedlots to packers.

10. **Section 6. The problem in #9 would not be as concerning, however, had the respondents been asked to provide *in depth* reasoning into their non-use of AMAs (this applies to all survey segments, not just beef producers). Certainly, the respondents identified, almost consistently, that they prefer spot market transactions because it gives them "independence, complete control, and flexibility". However, the researchers did not appear to probe deeper into the reason the respondents felt there was greater independence and control for spot market transactions. Specifically, how did that benefit their operation? The sensitivity of these results is further illustrated by the high ranking respondents that used and did not use AMAs placed on receiving a higher sale price or lower purchase price. It appears that many respondents felt that they were obtaining more favorable prices (regardless of whether they used AMAs), but why? The responses to questions 4.1 and 4.2 in Table 6-1 (and analogous questions for other segments) provide very little useful insight as to why producers, packers, processors, and others use spot market transactions or AMAs. It does not appear that the respondents were asked how spot market transactions gave them greater control, or how AMAs managed supply chains better. These specifics would be helpful in better understanding the issues surrounding AMAs. Further, it would enrich the discussion of the results in the text and allow the researchers to discuss potential implications of the results, rather than simply reporting the numbers that are contained in the tables.**

RTI response: Unfortunately, one of the limitations of survey research is that most questions in the survey questionnaire need to be closed ended (i.e., choosing one or more items from a list). Although using open-ended questions would have provided a more in-depth understanding of respondents' reasons for using different purchase and sales methods, it likely would have increased the amount of time to complete the survey and thus negatively affected the response rate. Also, it would have been very difficult and time consuming to analyze responses to numerous open-ended questions. We did, however, conduct a number of industry interviews to address some of the issues you raise above. The findings from these interviews are reported in the species-specific volumes of the final report and in the interim report produced for the study.

11. **Table 6-1. Table 6-1 (and analogous tables throughout Sections 6 and 7) are very long. In fact, Table 6-1 is 21 pages long. It would make the volume much easier to read if the tables were broken into smaller sections. One possibility is to organize the responses according to demographics, procurement, and sales. Regardless, the results in the tables should be presented in the order in which they are discussed in the text. Also, many of the results in the tables are based on a very small number of observations. This limitation could be more thoroughly discussed in the text, or perhaps the information should not be provided in the tables.**

    RTI response: In our opinion, the way in which the tables are numbered is of secondary importance. We believe it is more important to structure the tables to follow the order of the questions in the survey questionnaires. The summaries of the survey results are written to provide a narrative in a logical fashion and, for that reason, the summaries do not always follow the same ordering as the tables. To aid readers, the summaries provide the table and question numbers that correspond to the text. Based on these reasons, we did not revise the structure of the tables.

    Regarding the small number of respondents for some questions, we fully disclose the number of respondents to the survey in the header of each table and also provide the number of responses for each question item. Also, in the summaries of the survey results, we note when the number of responses is too small to discuss the findings. This is further reflected in the width of confidence intervals, whereby estimates based on smaller sample sizes are associated with confidence intervals with longer widths. We believe that the small sample sizes are already adequately addressed in the report, so we did not make any revisions.

12. **Sections 6 and 7. In several instances throughout the results in Section 6 and 7, comparisons are made between small and large respondents or changes between three years ago, the current year, and the next three years. Were any statistical tests examining the difference of means for each group conducted? Please report the results of such tests. Further, there are cases where the same difference (in magnitude) between two groups is interpreted in the text as large and small (or significant or insignificant) depending upon which segment is being discussed.**

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RTI response: The comparisons between small and large respondents and changes between 3 years ago, the past year, and the next 3 years are based on the magnitude of the point estimates and not on statistical testing. However, the reader can use the constructed confidence intervals to make comparisons between survey estimates. That is, overlapping confidence intervals suggest that the difference between the corresponding point estimates is not statistically significant. We added a description of how to make such comparisons in Section 5.4. Also, at the beginning of Sections 6 through 9, we added a paragraph indicating that the comparisons described in the summaries are not based on statistical testing, and we explain how to use the confidence intervals to make comparisons between survey estimates. Additionally, we reviewed the survey summaries and made corrections, as necessary, to ensure that we were consistent in our interpretation of results across industry segments.

13. **Sections 6 and 7. It would be helpful to have more comparison to other relevant research results to calibrate the results presented in Sections 6 and 7. Conclusions such as "…the majority of pork producers relied primarily on spot market transactions for selling pigs and hogs…" seems counter to actual market practices for the majority of hog sales in the U.S. (Section 6.2.5), as does the "most common valuation methods for market hogs purchased in the last year were liveweight" (Sections 7.2.2 and 7.2.4). The response that "less than 1% of the fed cattle purchased for slaughter were imported from Canada" during 2002 may be low based on Canadian cattle import numbers (Section 7.1.1). Are the respondents and results typical of actual livestock industry practices in the U.S.?**

    RTI response: In the species-specific volumes of the report, we describe the results of analyses conducted to compare the survey responses with other data sources. For example, in the hog/pork volume of the report, we present the results of a comprehensive comparison of the survey data with other data sources, such as MPR data. Also, in the cattle/beef volume of the report, we present the results of a comparison of the survey data with the transactions data (which represents the majority of transactions). Furthermore, throughout the survey volume, we provide comparisons of the survey data with other data sources. For example, we compare the survey responses for the proportion of cow-calf operations versus feedlots with NASS data and we compare survey results for pork packers' use of valuation methods with industry reporting.

    In regard to your comment about the estimate of fed cattle imported from Canada, according to the *Red Meat Yearbook*, 5% of the cattle slaughtered in 2002 were imported from Canada, indicating this percentage is very small.

    To provide additional information comparing the survey data with other data sources, we tried comparing the survey data on packers' use of purchase methods with MPR data. Because MPR uses a different classification system to describe types of purchase and pricing methods than the classification system used in the survey, it was difficult to conduct a meaningful comparison. Also, MPR data are only for the largest plants. However, based on the analysis we conducted, it appears that the survey responses for large plants are generally consistent with MPR data.

At the beginning of Section 7 and other places, as appropriate, we note that some of the survey estimates differ substantially from those obtained from other sources. Such differences could be due to small sample sizes, sampling frame error, differences in how the information was collected, and nonresponse bias, even though survey weights were calculated to compensate for some of the incurred bias.

14. **Section 9.1.2. About two-thirds of the wholesalers report purchasing or receiving meat products that had some type of certification. Yet, results from meat packers indicated that they sold very little certified product. An explanation for this discontinuity is needed.**

    RTI response: As indicated in the footnote, we believe that wholesalers may be over-reporting their use of process verified certification programs because they are confusing it with USDA inspection. For other types of certifications, the responses from wholesalers and packers are fairly similar; thus, we believe no additional explanation is necessary.

15. **Section 11.3.1. The authors indicate not using data because it was provided in hard copy format or in an incompatible electronic format. These are not particularly strong reasons to discard otherwise useful data. These are problems that should be easy to overcome.**

    RTI response: Because of the time constraints of conducting the study, it was not possible to hand key large volumes of hard copy data provided in many different formats by some respondents (often provided in numerous boxes). All decisions regarding use of data had to be made in a very short time period to meet the deadline for delivery of the final report. We reviewed the hard copy data provided by respondents and only hand keyed data that we determined could be used for the analyses required for the study. Likewise, for data provided in an incompatible electronic format, we reviewed the data and only converted data that we determined would be a useful contribution to the analyses required for the study.

    In our opinion, we believe that it was more valuable to spend project time and resources working with the data that were provided in a usable format. For nearly all plants, we had to contact the plant or company to request clarification on their data, or in many cases, obtain entirely new data sets. This demonstrates the complexity of obtaining the data required for conducting the study's analyses.

16. **Section 11.3.1. The sample sizes for beef and pork packers started at 60 and were ultimately reduced to 25 and 18, respectively, that provided useable profit and loss data. This is a significant reduction, and it is not clear why this occurred. Did the process in the 18-20 page direction book fatigue the respondents?**

    RTI response: GIPSA made the decision to reduce the initial starting sample from 60 plants to 37 beef plants and 39 pork plants to reduce response burden among smaller plants. All of the 37 beef plants and 37 of the 39 pork plants provided P&L statement data. We ultimately used data from 25 beef plants and 18 pork plants in the analysis of

the P&L statement data. Based on the needs and time constraints for completion of the study, we were able to conduct analyses with a subset of the P&L data from packing plants. We used electronic data sets and hand keyed hard copy data from a representative subset of the plants. Data from these plants allowed us to capture the majority of sales in the beef and pork industries.

Minor Comments:

1. **Page 7-6. The citation at the end of the second paragraph should be "See Table 7-1, Questions 5.4 through 5.7."**

   RTI response: We made this correction as suggested.

2. **Page 7-80, Table 7-8, Question 9.1. Option 2 generated 37 responses and Option 4 generated 38 responses, but the Option 2 percentage response was 53.6 and the Option 4 percentage response was 30.5.**

   RTI response: There is a difference in the percentage responses because the results are weighted by number of establishments. Small and large establishments have different weights, thus the difference in the estimated percentages.

3. **Page 8-5. The citation at the end of the first (incomplete) paragraph should be "See Table 8-1, Questions 5.1 through 5.3."**

   RTI response: We made this correction as suggested.

4. **Page 9-31. The citation at the end of the first (incomplete) paragraph should be "See Table 9-1, Questions 3.2 through 3.5."**

   RTI response: We made this correction as suggested.

5. **Page 9-43. In the second paragraph (Section 9.4.1), "…and 4% owned 100 or more establishments" should be "…and 3% owned 100 or more establishments" to be consistent with Table 9-4.**

   RTI response: We made this correction as suggested.

6. **Page 9-45. The citation at the end of the first (incomplete) paragraph should be "See Table 9-4, Questions 2.6 through 2.8."**

   RTI response: We made this correction as suggested.

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VOLUME 2: DATA COLLECTION METHODS AND RESULTS; FIRST DRAFT FINAL REPORT
PREPARED BY RTI INTERNATIONAL IN NOVEMBER 2006
REVIEW SUBMITTED 8 DECEMBER 2006

This reviewer was charged with the task of reviewing the first draft final report of Volume 2: Data Collection Methods and Results of the GIPSA Livestock and Meat Marketing Study prepared by RTI International. Reviewed was the text of Volume 2 in its entirety, the tables contained within the body of Volume 2 (some with more and others with less scrutiny), and some sections of the appendices. Two of the survey instruments contained in the appendices were reviewed in detail. The remainder of the appendices were used only for reference during the review.

I have twice previously provided comments on the data collection plans (reviews submitted on October 20, 2004 and December 16, 2004) and provided a review of a document entitled 'Spot and Alternative Marketing Arrangements in the Livestock and Meat Industries: Parts A and B: First Draft Report (June 10th, 2005).

At this time, I was not asked to, and did not, review the other Volumes of the draft final report. Therefore some of my comments and suggestions may be addressed elsewhere in the report and I personally would not be aware of this.

Overall, the report is very well written and the written portion well summarizes the pertinent information from the tables. The data collection methods were reasonable and well suited to the proposed analysis. The data collection process was clearly defined and issues associated with such (e.g., lower than expected response rate) were clearly disclosed and addressed. The initial results presented contain meaningful information. Finally, methods used in the transactions data collection process were well presented and appear appropriate for the information desired.

RTI Response: We appreciate the positive feedback.

Primary concerns are the clarity of the tables, interpretation of the data (e.g., verbiage used sometimes indicates conclusions that cannot be drawn from the data because of how the questionnaire was designed), and the general idea that the respondent pool is not representative of the industry. First, the text seems to, at times, conflict with what I believe to be presented in the tables or I am not clear on what is presented in the tables. For example, in Table 6-10 on page 6-93 (and elsewhere), 4 respondents is 38.9% in two cases, and 30.5% in another. Is the percentage the number of animals handled, but the number is the number of enterprises? If so, please make this more apparent so the tables are not misinterpreted. Second, data must be interpreted only as far as the survey design allows. Finally, additional information about how the respondent pool differs from known industry values and what this might mean would be useful.

RTI Response: We provide responses to these comments in response to your specific comments below.

I begin my more detailed comments by reviewing comments / suggestions I have made in previous reviews of your data collection plans. Next, I address items associated with the sample design, design and administration, response rates, and data set preparation for the industry survey. This is followed by my comments regarding survey results for livestock producers and feeders, meat packers, meat processors, and downstream market participants. I conclude with comments regarding sample design, study design and procedures, and data set preparation for the transactions and profit and loss statement data collection.

PREVIOUS COMMENTS. Here I address only those comments made in previous reviews that are germane to Volume 2 (i.e., are likely handled in Volume 2 rather than another volume of the report). Additional details about these comments are provided in the previous reviews.

1. *Response rate*. In both of my previous reviews of data collection plans I indicate that the anticipated 50% response rate for surveys seemed very high given the extent of information you were looking to obtain (i.e., given the length and detail of the questionnaires). In both instances, I encouraged a follow-up contingency plan. You in fact went into the data collection process with a contingency plan (a reserve population to receive surveys) and you followed a rigorous plan to maximize response rate. Although it was not enough to gain the number of responses you hoped for, the efforts of RTI are notable, and I am encouraged to see this.

RTI Response: Comment acknowledged.

2. *Survey covers limited time frame in cyclical industry.* In previous reviews I noted the importance of considering the length of livestock cycles, seasonality of marketing (practices), and unique recent changes affecting supply or demand (e.g., BSE, Atkins diet). Although data over a longer period is of course preferred over that from a shorter period, your extension of the length of consideration for surveyed participants to three years prior and three years forth helps ensure your information is of value as more than just a snapshot of the industries today. Thanks.

RTI Response: Comment acknowledged. In the species-specific volumes of the report, we discuss the effects of the stage of the livestock cycle on the results of the analysis.

3. *Not clear how the characteristics of transactions and product will be carried through the marketing channel.* The information presented in the transactions data collection section indicates that survey respondents in the marketing channel that are linked (e.g., a retailer buying from a particular packer) will be linked in the analysis by tying them together with unique (non-disclosing) numeric identifiers. It is still not clear what information will be used to identify the effect of marketing arrangement on quality of the product, cost, etc., but I assume this is dealt with in another volume of the report.

RTI Response: As you note, we address analyses of the effect of marketing arrangements on quality of product, cost, etc., in the species-specific volumes of the report. We also examine links between upstream and downstream market participants in Volume 6 of the report.

4. *The list of reasons for employing alternative marketing arrangements seems relatively complete but the "check three" method may leave out some important considerations, and may not allow for meaningful interpretation.* **The final survey instrument used the "check three" approach, although additional reasons were added after the pretest. In a perfect world, one would employ other methods (e.g., focus groups, personal interviews, ranking) to more carefully elicit the reasons behind the selection of marketing alternatives. These methods may also allow for identification of reasons a respondent might otherwise not identify (e.g., unethical, questionable business practices). That said, the selected method seems appropriate given the more general nature of the study.**

RTI Response: As described in the species-specific volumes of the report, we conducted a number of industry interviews to collect information on reasons for using alternative marketing arrangements (AMAs). We considered using ranking questions in the questionnaire but decided against it. Based on previous experience conducting surveys, respondents find it very difficult to answer ranking questions and often answer ranking questions incorrectly in such a way that it makes analysis difficult (e.g., give several responses a number one ranking); thus, we used the "check three" approach.

5. *Quarterly transactions data will not allow for researchers to capture seasonality.* **The transactions data collection procedures indicate weekly (or monthly) data was collected.**

RTI Response: We collected *daily* transactions data on purchases and sales of livestock and meat. We requested that companies provide *weekly* P&L statements because companies do not generally produce P&L statements on a daily basis. However, if weekly P&L statements were not available, it was acceptable for companies to provide monthly P&L statements.

6. Did you check self-reported survey data against survey data?

RTI Response: We assume you are asking whether we checked the self-reported survey data against the transactions data (not survey data). As suggested, for each beef packer that provided both survey data and transactions data, we compared their survey responses with their aggregated transactions data (i.e., the analysis was conducted as the plant level). This comparison was conducted for the following variables: purchase method, type of pricing method for purchases, formula base (if formula pricing was used for purchases), valuation method for fed cattle purchases, sales method, type of pricing method for sales, and formula base (if formula pricing was used for sales). For the purchase data, we found that, with a few exceptions, the survey data and transactions data were very consistent, with some comparisons being exactly the same. For the sales data, we found that, for most respondents, the survey data and transactions data were generally consistent. We revised the survey volume to present the findings of the comparison (see Section 5.3).

SAMPLE DESIGN, DESIGN AND ADMINISTRATION, RESPONSE RATES, AND DATA SET PREPARATION FOR THE INDUSTRY SURVEY (Sections 1 through 5)

7. Your use of industry publications and USMEF to update lists of top producers, packers, and exporters was an excellent idea and certainly contributed to your ability to capture those in the large category. Nice work!

RTI Response: We appreciate the positive feedback.

8. The footnote on page 2-4 (and elsewhere throughout the report) notes that, because revenue is a categorical variable, it was necessary to select (e.g., more than 50 establishments). It is not clear why that would need to be the case (i.e., please provide some additional explanation, at least in the first footnote indicating such).

RTI Response: We revised the first footnote to provide an example, thus clarifying why it was necessary to select more than 50 establishments.

9. You note that exporters did not include those who are also qualified as packers (p. 2-8). Do these packers handle a considerable volume (percentage) of exports? If so, how does this affect your ability to generalize results about exporters?

RTI Response: We did not include packers that also export in the sample of exporters in order to help minimize respondent burden. In Section 9.2, we added a footnote to remind the reader that the sample for exporters did not include packers that also export. Based on the survey responses, it does not appear that many of the packers surveyed also export. Less than 1% of beef product sales were to foreign buyers (Table 7-1, Question 5.1), 3% of pork product sales were to foreign buyers (Table 7-8, Question 6.1), and none of the lamb packers surveyed sold to foreign buyers (Table 7-15, Question 5.1).

10. Methods for pre-testing and of interviewing were well suited to the current study.

RTI Response: We appreciate the positive feedback.

11. Perhaps add a comment regarding the surprisingly large percentage of ineligible establishments (p. 4-4). That is, perhaps add some insight into why these establishments were on your list but not eligible (e.g., outdated lists?)

RTI Response: We used the most current data available to develop the sampling frames for the survey. We used the most current version of the D&B database, which is updated quarterly, to develop the sampling frames for producers and the downstream market participants. We used the most current version of USDA, FSIS' Enhanced Facilities Database, updated in 2005, to develop the sampling frames for packers and processors. The large percentage of ineligible establishments is due partially to the fact that databases used for sample selection are subject to compilation error. In Section 4, we added text to describe why selected establishments were ineligible, by industry segment. For the producer industry segment, most of the establishments that were classified as ineligible do not produce the selected livestock species; this could be due

partly to misclassification error in the sampling frame. For the packer industry segment, most of the establishments that were classified as ineligible only conduct custom slaughter, so they were not eligible for the survey. For processors, most of the establishments that were classified as ineligible do not conduct meat processing activities; this could be due partly to compilation error. For the downstream market segments, most of the companies that were classified as ineligible do not conduct the type of business activity for which the company was listed in the sampling frame or do not buy meat (e.g., only purchase poultry or seafood).

12. Methods used to weight responses seem appropriate, although it is not perfectly clear whether you weighted within size categories or you weighted size categories to reflect industry data about firm sizes. Please provide additional detail regarding these methods and perhaps consider adding this information as a footnote to tables. For example, it is not clear when you review a table if the average is an average of the responses by establishment (i.e., a simple sum of averages / numbers by establishment divided by the number of establishments) or if it is weighed by the volume of animals or meat handled by the establishment. The text generally seems to imply the latter but I suspect the former may be the case. For example, the information on p. 7-9 seems to indicate the table values are weighted by product volume for each firm. Is this the case?

RTI Response: All survey results are weighted by the number of eligible business units (not by volume of animals or meat) obtained from their corresponding sampling frames. To help clarify this for the reader, we made the following revisions. In Section 5.1, we revised the first sentence to clarify that the survey weights were used to weight the survey results by number of eligible business units. In the introductory paragraph to the results sections (Sections 6 through 9), we added several paragraphs to explain how to interpret the tables. Also, on the first table for each industry segment, we added a box that provides a legend for the notation used in the table headers.

For estimates of means, we calculated such values using the survey weights to reflect the number of eligible business units. However, it should be noted that respondents have answered the questions in terms of volume of animals or meat. For the first mean value reported in Sections 6 through 9, we added a footnote to help to clarify this for the reader. For example, for the sentence "More than 80% of the calves and feeder cattle received were owned solely by the operation, 13% were not purchased, but delivered to the operation for custom feeding," we added the following footnote, "These values were computed as the mean percentage of head weighted by the number of eligible operations. Other reported means were computed similarly (i.e., weighted by the number of eligible operations).

SURVEY RESULTS FOR LIVESTOCK PRODUCERS AND FEEDERS, MEAT PACKERS, MEAT PROCESSORS, AND DOWNSTREAM MARKET PARTICIPANTS (**Sections 6 through 9**)

13. The report notes inconsistent beliefs by channel participants (e.g., on pp. 6-5 and 6-6, that packers use spot markets to obtain a low price while feeders use them to obtain a high price). This is truly interesting! If not included elsewhere, it would be useful to have some insight into which is more likely the case and why the beliefs seem to conflict.

RTI Response: We agree it is an interesting insight. We analyze the relationship between transactions prices and different types of marketing arrangements in the species-specific volumes of the report.

14. Were any differences by region considered (especially for producers, but also elsewhere)?

RTI Response: We analyzed regional differences in the species-specific volumes of the report, rather than getting into that much detail in the survey volume.

15. Throughout the results section, it may be useful to add percentage of volume handled rather than simply number of packers in each category. The size division (small versus large) somewhat addresses this, but your numbers show considerable variability within size category. For example, on page 7-52, the report indicates that a relatively large percentage (40%) of pork packing plants did not use any measures to assess the quality of slaughtered pork. What percentage of total volume handled is this?

RTI Response: We agree that it would be informative to report certain survey results by volume of animals or meat. We considered weighting the survey data by volume of animals or meat, but no reliable external data source was available for each industry segment; thus, we weighted the survey data by the number of eligible business units. However, many of these issues you raise above are analyzed in-depth and detailed statistics are provided in the species-specific volume.

16. Again, I applaud your disclosure of the lower than expected response rate, and that you compared USDA and other data to your findings throughout the report to help assess respresentativeness of your sample population. It would be useful to have some information about how far off and in what direction your population is in each case.

RTI Response: In the species-specific volumes of the report, we describe the results of analyses conducted to compare the survey responses with other data sources. In the hog/pork volume of the report, we present the results of a comprehensive comparison of the survey data with other data sources such as MPR data. Also, in the cattle/beef volume of the report, we present the results of a comparison of the survey data with the transactions data (which represents the vast majority of transactions). In addition, throughout the survey volume, we provide comparisons of the survey data with other data sources. For example, we compare the survey responses for the proportion of cow-calf operations versus feedlots with NASS data and we compare the survey responses for valuation method for market hogs with industry data.

To provide additional information comparing the survey data with other data sources, we tried comparing the survey data on packers' use of purchase methods with MPR data. Because MPR uses a different classification system to describe types of purchase and pricing methods than the classification system used in the survey, it was difficult to conduct a meaningful comparison. Also, MPR data is only for the largest plants. However, based on the analysis we conducted, it appears that the survey responses for large plants are generally consistent with MPR data.

17. On page 7-2, "nearly 60%" is different from "most".

RTI Response: We changed "most" to "many."

18. Your analysis discovers that the most common method used to purchase beef cattle was carcass weight without grid pricing. Is this because the quality of the animals is in general known (e.g., past records of the quality of the cattleman's cattle) or more simply because averages are easier for the packer?

RTI Response: The actual reason behind the decision to sell on an average price cannot be determined from the survey data, but simplicity for the buyer and seller are possible reasons. Another reason may be that sellers do not want to stand the grading risk of selling on a grid. During part of the transactions data collection period, short cattle supplies gave feedlots more market leverage to negotiate an average price and let the packer stand the grading risk.

19. You regularly disclose what percentage of market participants cover transportation costs. I am not sure of the relevance of this. I assume if a packer pays transportation cost of the animals, the bid is lower for those particular animals than if the producer brought them to the plant. Because you don't discuss relative price (e.g., of delivered versus not) animals, providing this information may even be a bit misleading. Can you provide a rationale for its inclusion?

RTI Response: We included questions on payment of transportation costs in the survey, based on suggestions in the Performance Work Statement (PWS) for the study. Also, information on payment of transportation costs provides a better understanding of common marketing prices, which are not reflected in USDA price reports, and thus aids in correctly interpreting the transactions data. We agree that who pays transportation costs affects the price paid, but we did not collect information on prices in the survey.

20. The summaries are well written and well cover the main points.

RTI Response: We appreciate the positive feedback.

21. It is useful that you include differences (e.g., of pricing method) depending on who is selling or buying the product.

RTI Response: Comment acknowledged.

22. Authors disclose that those who responded among pork packers is a different population than the universe of pork packers (i.e., that they are different than in other published sources). This is unavoidable, but indicates a need to attempt to correct for non-response bias. Was this attempted?

RTI Response: The survey respondents to the pork packing survey were selected from the universe of pork packers. As indicated in the report, we found that some of the survey results are different from other published sources. These differences could be due to small sample sizes, sampling frame error, differences in how the information was collected, and nonresponse bias,

even though survey weights were calculated to compensate for some of the incurred bias. Section 5.1.3 describes the adjustment we made for nonresponse. These adjustments, implemented with the computation and application of adjustment factors in each weighting class (in this case, size of establishment), can help reduce nonresponse to the extent that weighting classes are homogeneous.

23. The use of verbiage could sometimes be misleading. For example, on page 7-53 (second full paragraph), the report indicates that the most common valuation method for market hogs purchased in the last year was live weight. It indicates that 79% of plants use this method. However, the survey simply asks that the respondent check methods used. So, the statement 79% of plants use this method can be supported, but not that it is the most common valuation method used.

RTI Response: We revised the sentence to read, "The most frequently cited methods for valuation of market hogs were liveweight (79% of plants) and carcass weight dependent on merit (31%)." We reviewed the survey summaries and made similar changes elsewhere in the text, as appropriate.

24. This is otherwise true as well. For example, respondents were not asked to rank responses, only to indicate their use (e.g., top three reasons for using a particular market). Thus, the conclusion that the reason indicated by the largest percent of respondents is "most important" is not well supported.

RTI Response: We agree with your comment. We reviewed the survey summaries and revised the text to eliminate the use of verbiage referring to "most important."

25. The report fully discloses the low response rate among lamb packers. It is disappointing, but could not be reasonably overcome. Again, the population is noted as different in makeup / practices than values from other sources (e.g., USDA, MPR). It would be useful to identify how the population / practices differ and, in doing so, to report some of the "other data". I suspect adjustments are later made in the analysis to represent this (in other volumes), but since the reader's curiosity is peaked here, it would be nice to have some information.

RTI Response: As described in our response to Question 16, we were unable to conduct a meaningful comparison of the survey data with MPR data because of differences in definitions used. However, in the lamb volume, analyses of the MPR data are conducted, and the practices of lamb packers are analyzed in detail.

26. For lamb carcasses, what are "other than" Yield Grade 1 to 5 carcasses?

RTI Response: The survey questionnaire was designed to ask about other yield grade or no yield grade as a combined response option (see Question 1.6 of the survey questionnaire). It is likely that respondents who answered for this response option were referring to lambs without a yield grade.

27. It is noted that wholesalers reported buying 38% of meat from other wholesalers but only sold 4% to them. Perhaps add some comment about what this likely means for the representativeness of your respondent pool if appropriate (other than that it is not representative, e.g., likely smaller enterprises than the average in the overall population).

RTI Response: We agree that the survey responses for wholesalers likely are skewed toward smaller companies. We added an explanation in Section 9.1.3 to reflect this.

28. Add some overall comment about responses not included in the aided responses, but not likely to have "been checked" even if included. I suspect some of the smaller enterprises do not "profit maximize" but rather satisfice. That is, they may be doing business in a particular way because of history (that is how they have always done it), or for some other reason (including unethical or illegal reasons). It is not likely that this information would be gained, regardless of the detail of the survey or the method of attempting to elicit it, but it is important to note.

RTI Response: We believe that the concern you are referring to is the limitation of self-reported survey data; however, we were unable to determine an appropriate location in the text to respond to your concern.

29. In section 9.2.1., I suggest you change the report order for the exporting companies by size so that there is a logical flow.

RTI Response: As suggested, we revised the report order for the exporting companies by size so that this flows more logically.

30. In footnote 2 on page 9-16 (and elsewhere) it indicates that respondents may have confused USDA process verified with USDA inspection because the numbers do not match. Please report what percentage industry-wide are actually process verified.

RTI Response: We contacted the Agricultural Marketing Service and were informed that they do not track volume of products produced under the USDA process-verified program. We revised this footnote (and the corresponding footnotes for each downstream market segment) to reflect that we were unable to compare the self-reported survey data with other data sources.

31. In the retail survey, it would have been nice to see "health claim" as a choice for retailers to indicate why consumers purchase particular products.

RTI Response: We agree; however, we did not include "health claim" as a response option. Respondents could, however, write it in as a response, although none did so.

32. It was nice to have information about type of pricing participants use to buy and sell products, why they use particular markets / marketing arrangements, and so on by category (e.g., the former by type of customer). Nice touch!

RTI Response: We appreciate the positive feedback.

33. In this section (and elsewhere), it would have been useful to have 10+ years and evergreen contracts as separate options. An evergreen contract, by definition, is not binding in that it can be terminated by either side. A ten plus year contract is binding.

RTI Response: Comment acknowledged.

SAMPLE DESIGN, STUDY DESIGN AND PROCEDURES, AND DATA SET PREPARATION FOR THE TRANSACTIONS AND PROFIT AND LOSS STATEMENT DATA COLLECTION (**Sections 10 through 12**)

34. The report indicates that data was reviewed for internal consistency and that profit and loss statements were compared to transactions data. Were any spot checks done internally such as through a visit to the plant or by sending the information back to the plant in summary form for them to verify?

RTI Response: Although it would have been desirable to do so, we were unable to visit plants or send summary data back to plants for verification because of the time constraints of conducting the study and the additional burden it would have placed on plants.

35. The process to ensure confidentiality was very strong!

RTI Response: We appreciate the positive feedback.

**REVIEW OF GIPSA LIVESTOCK AND MEAT MARKETING STUDY
VOLUME 3: FED CATTLE AND BEEF INDUSTRIES; FIRST DRAFT FINAL REPORT
PREPARED BY RTI INTERNATIONAL IN NOVEMBER 2006
REVIEW SUBMITTED 19 DECEMBER 2006**

The study presents a careful description of the data. This is very useful in assessing the value of the results. The information reported in the report based on these data is more comprehensive than any previous analysis of this industry.

RTI Response: We appreciate the positive feedback.

Some credit should be given in the report to past studies that, even with limited publicly available data, arrived at basically the same conclusions (i.e., small amounts of market power have been generated by increasing market concentration, AMAs are used primarily as inventory control, AMAs have not greatly affected pricing methods, etc.). I believe this confirmation of results from past studies speaks highly for the profession and methods that have been developed and used in the past. I was especially struck by the average cost graph for meat packing plants that confirms closely some of the work done by Clem Ward years ago (p. 3-17).

RTI Response: We have added additional references throughout the report, including mention of the work by Clem Ward and others in Section 3. We also note that the interim report that we completed in July 2005 contains a more extensive review of the literature on the effects of AMAs.

The results demonstrate that AMAs are used primarily as a means of inventory control (strongest result) with weaker evidence that AMAs contribute to quality. The marginal contribution to quality is likely a result of packers establishing relationships with cattle feeders who have a reputation for quality. Thus, it is unclear to me how AMAs increase overall quality for beef. Consequently, I believe that the authors need to be careful about drawing conclusions about AMAs and their relationship to quality. The results basically confirm that beef packing continues to be dominated by commodity market thinking. The fact that average quality actually declined during the study period provides further evidence that quantity considerations due to economies of scale in packing far outweigh quality considerations.

RTI Response: We agree that the marginal contribution of AMAs to improving beef quality is the result of packers using such agreements to secure cattle supplies from cattle feeders who produce high quality cattle. Reducing the use of AMAs could, therefore, break this important linkage and reduce the ability of packers and feeders to coordinate the production of higher quality cattle. We also agree that the beef industry is dominated by a commodity system, primarily because the most successful modern packing plants (those built since the 1960s) were those that followed a low-cost business strategy. However, most of these plants also have branded beef or strategic alliances designed to provide higher quality to those willing to pay for it. However, much of the "sorting" of quality is done postharvest because of preharvest signal extraction problems related to beef quality. We estimated additional models of the quality effects of AMAs using the transactions data and believe that AMAs are associated with better quality

compared with direct trade. However, the relatively small percentage of cattle sold through auction barns was associated with the highest quality over the time period of the data.

I believe it would have been good to examine the relationship between pricing methods and quality. For example, we have been told for years that grid pricing is designed to increase beef quality. Is there really any evidence that this is the case? This was not tested in this study.

RTI Response: We estimated an additional model to examine the effect of valuation method (liveweight, carcass weight without grid, or carcass weight with grid) on quality using a quality index constructed from the transactions data. We found the effect you noted—carcass weight with grid pricing is associated with higher fed cattle quality (and also reduced variability of quality). A discussion of this model and the results of estimation have been added to the report.

In general, I am skeptical about the inferences in the study about beef quality. Another example of what I would consider to be potential problems related to discussions about beef quality in the study is the regression analysis about beef demand presented on p. 4-21. The authors suggest that retail beef price rise as a result of increased quality. This should be no surprise given that increases in the percentage of choice and prime carcasses in the market should raise average wholesale prices and, as a result, retail prices. The use of a percentage rather than a quantity number may give a misleading result that the demand curve shifts outward as quality increases. I don't believe that the percentage or actually the index verifies that this is the case. The calculation of the quality index also appears to be somewhat arbitrary and needs further explanation.

RTI Response: A description of how the quality variable was constructed has been added to the report in Section 4.3. We use average quality grade as a quality indicator of slaughter cattle and quantify the relationship (if any) between this variable, other exogenous factors, and procurement methods. Quality grades are reported as categorical data in MPR data. Our modeling strategy requires that numerical values for quality be developed for use as dependent variables in the regression analyses. In addition, an increase in the value of this dependent variable should reflect increased quality that would be manifest in increased retail demand.

Thus, our constructed quality variable is based on the numbers of cattle slaughtered in each quality grade by month and not on percentages of each grade produced. Therefore, when evaluating the effect of beef quality on retail beef price (renumbered Eq. [4.13]), the marginal contribution of the quality variable (QG) is evaluated under *ceterus paribus* conditions. That is, it does not measure changes along the inverse retail demand function. As you note, this would be the case if QG was based on percentages of Choice and Prime grade carcasses. Increases in these percentages would, as you note, increase retail prices. Rather, because we control for beef production (QB) and wholesale beef prices (PBX) in (renumbered) Eq. (4.13), our estimated quality impacts on retail prices represent a shift in the retail demand function. These changes reflect consumer preferences with respect to changing meat quality.

The results confirm much of the research completed in the past and suggest that the beef industry, as currently structured, is cost efficient but slow (unable?) to effectively solve many of the quality issues continuing to plague the industry. Producers will find little

evidence in this report to support their continuing claim that thin cash markets are hurting cattle prices. The greater concern, in my opinion, is that AMAs are likely not going to accomplish much as far as actually improving beef quality.

RTI Response: As noted above, our research indicates that AMAs have made a positive, albeit small, contribution to improved beef quality. Nonetheless, the growth of alliances, branded beef programs, rancher-owned cooperatives (U.S. Premium Beef), and increasing evidence that seedstock producers are being rewarded for bulls that have high EPD intramuscular fat and ribeye area scores provide evidence that the beef sector is working on improving quality. To the extent that AMAs improve the transference of price and quality signals from consumers to producers, they appear to be an important aspect of quality improvement.

The report is thorough in most cases and should be given the unparalleled access to data the researchers were provided. The P&L information about packers was especially informative in that it confirms much of the past research dealing with captive supplies, market power, and the question of returns to size in general in the beef packing industry.

RTI Response: Comment acknowledged.

I know less about equilibrium displacement models, such as described in the last part of the report, but it follows closely past work by Brester. Consequently, I reserve judgment on the results generated therewith. The idea though that eliminating AMAs would have small effects on cattle prices would be completely consistent with past research and is likely the case.

RTI Response: Comment acknowledged.

While I am certain the study will receive its share of criticism, I believe that it will represent a significant step toward confirming past research relating to market power and captive supplies in the cattle and beef markets. For me, the most discouraging part of the report is the fact that commodity market considerations will continue to dominate the beef market.

RTI Response: Comment acknowledged. However, as discussed above, we believe quality considerations will also affect the use of AMAs in the beef market in the future.

REVIEW OF GIPSA LIVESTOCK AND MEAT MARKETING STUDY
VOLUME 3: FED CATTLE AND BEEF INDUSTRIES; FIRST DRAFT FINAL REPORT
PREPARED BY RTI INTERNATIONAL IN NOVEMBER 2006
REVIEW (BF-2) SUBMITTED 29 DECEMBER 2006

GENERAL COMMENTS:

Overall, the *Fed Cattle and Beef Industries* study is well written and successfully analyzes underlying issues integral to the economics of transaction relationships in the beef industry.

RTI Response: We appreciate the positive feedback.

More specific points follow below segmented, by section, in order of presentation in the manuscript. These comments include an assortment of editorial, contextual, and methodological issues.

EXECUTIVE SUMMARY

1. I suggest including a brief definition of AMAs in the opening paragraph. This executive summary may be the first and/or only portion reviewed by some readers.

RTI Response: We agree and have added this definition.

2. It is important to maintain the discussion regarding the unique time period (Oct. 2002 Mar. 2005) during which this analysis was conducted. This point needs to be firmly established throughout this volume and any summary documents referencing the *Fed Cattle and Beef Industries* section.

RTI Response: We added additional mention of this in several places in the report, particularly in Sections 2, 4, and 7, and the summaries at the end of each section.

3. In the second paragraph of page ES-6, clarification is needed regarding the "Profit per head is estimated to decrease …." sentence. I suggest changing this to "Slaughter and processing profit per head is estimated to decrease …" to avoid confusion or misuse of the sentence.

RTI Response: We agree that this could cause confusion and have changed the wording to make it clearer.

4. The first bolded point on page ES-7 as currently phrased could easily be misunderstood or taken out of context. I suggest changing this to "Hypothetical reductions in AMAs are found to negatively affect producer and consumer welfare." As the sentence currently reads, one may concluded that AMAs have been reduced and the statement is reflecting on an observed result.

RTI Response: We agree and have made the suggested change.

5. The second bolded point on page ES-7 could currently lead a reader to believe oligopsony power is found by the study. The extent of market power is debatable in the research and not further investigated by this study. Rather, this work uses a range as suggested by prior work. As such, I suggest modifying the sentence "In the model simulations, even if the complete elimination of AMAs would eliminate market power, …" to something such as "In the model simulations, even if the complete elimination of AMAs would eliminate market power that might currently exist, …" This adjustment is more consistent with the analysis.

RTI Response: We agree and have made the suggested change.

6. An additional statement could be added to page ES-7. In particular, an expansion of the last point could be made such as: "Collectively, this suggests that reducing use of AMAs would result in an economic loss for consumers and the beef industry."

RTI Response: We agree and have added the suggested sentence.

7. The use and implications of AMAs varies significantly across livestock species. I presume, in a volume not presently available to me, that this study will incorporate some summary comments across livestock species comparing the implications of AMAs. However, within this beef volume, I encourage the authors to make note of how restrictions in AMAs may hurt the relative competitiveness of beef with alternative meats. In particular, AMAs may be more important mechanisms of information transfer, quality control, etc. in the beef industry than in more consolidated and integrated meat industries. This is casually mentioned in later sections, but not in the executive summary.

RTI Response: A discussion of this point is contained in Section 7.2, but we also added a comment to the executive summary.

SECTION 1. INTRODUCTION AND BACKGROUND

1. This section appears to be in good standing. I would suggest incorporating some information on the demographic make-up of producers at different stages of the beef production process. It is important to note the existence of many small (sometimes referred to as hobby farms) operations in the beef industry. The existence of these smaller operations, higher dependency of off-farm income, etc. influence the viability of auction or direct transaction methods. As such, they influence the value and importance of AMAs to the industry. In a sense "concentration" in the packing sector is noted but comparable numbers for other stages in the production process are not provided.

RTI Response: A discussion of the size of cattle operations in the country and the proportion of production coming from different-sized operations, based on NASS data, was added to Section 1.1.2. This was supplemented with a discussion of household income sources for small and large producers based on ARMS data.

SECTION 2. VOLUME DIFFERENCES, PRICE DIFFERENCES, AND SHORT-RUN SPOT MARKET PRICE EFFECTS ASSOCIATED WITH ALTERNATIVE MARKETING ARRANGEMENTS

1. There is a typo in the last sentence of the second paragraph on page 2-4. The word "depend" needs changed to "dependent."

RTI Response: Correction made.

2. The last part of page 2-11 compares the purchasing methods of packers by geographic regions. Further, statements suggest packers in the High Plains use auction barns significantly less than those in the Cornbelt/Northeast and West regions. Can the authors comment on why this may be? For instance, is there evidence to provide that the size of producers varies across region suggesting that viability, and hence use of, auction barns varies across these geographic regions?

RTI Response: The structure of the feedlot industry differs significantly across the regions and has an impact on marketing practices. In the Cornbelt/Northeast states where USDA reports the number of feedlots and cattle on feed, the average annual marketing per feedlot in 2005 was 45 head from feedlots with less than 1,000-head capacity. These feedlots represent 74% of the marketing from these states. Auction markets and brokers and dealers that assemble small lots into truckload lots are an important market outlet for these small feedlots. The under-1,000-head capacity feedlots in Washington and Idaho represented 22% and 8% of the 2005 inventory, respectively, and they too may use auctions, brokers, and dealers to market cattle. However, feedlots with over 1,000-head capacity accounted for nearly 100% of 2005 inventories in the High Plains and some Western states. The average annual marketings from these feedlots were over 20,000 head in the High Plains and over 30,000 head in the Western states. Feedlots of this size typically deliver directly to packers. We revised the paragraph to make note of the reason for the difference in use of auctions.

3. The last paragraph of page 2-14 notes that approximately 40% of beef products sold by packers have unidentified (at least to the authors of this report) sales methods and pricing methods. This is a large number that requires further attention. Do the authors truly believe packers (both large and small as shown in tables 2-11 and 2-12) don't know this information? Alternatively, do the authors believe this information could not be revealed for some reason? Frequency of the *Other or Missing* category in tables 2-11 and 2-12 dominates frequency of all other responses. This need to be clearly addressed and elaborated upon by the authors to add confidence to the research. This issue can arise in all survey work and warrants further attention.

RTI Response: Based on our interactions with packers during the data collection process, we believe that packers do not keep track of this information because they generally do not have a need to maintain it for their business operations. They keep track of information on their buyers but not necessarily the method they use for transacting with each buyer. However, we think that information on the type of sales method used is of secondary importance to the goals of the study. We revised the sentence to indicate the reason why we believe this information is not available from some packers.

4. Table 2-13 suggests that over 1/4th of beef product sales by large packers are based upon an *Other Market Price*. Can the authors include a sentence discussing what these alternative sources likely are or could be?

RTI Response: We believe they are likely unique pricing methods that involve combinations of market prices, but, because of the way data were collected for the study, we do not have specific information about these.

5. Page 2-28 notes the range in average prices across alternative marketing arrangements. This may be a spot to clearly note that several AMA prices would have been established prior to the prices realized at the auction barn. That is, comparing average prices is not a solid way to analyze price differences given that the timing of price determination varies across marketing methods. When analyzing factors during a trending market (as in this upward trend), this point is even more important. All else equal, if the price trend during this time period of analysis was downward, the opposite "average price relationships" may have possibly been observed. These points must be firmly established and duly noted in this section given the short and unique time period of the analysis.

RTI Response: We agree that comparing average prices has its limitations and that the timing of price determination varies across marketing methods. However, we found that the difference in dates of price determination is typically much less than 2 weeks, based on data from companies that provided purchase, pricing, and slaughter dates. For more than half of the transactions in the data set, we did not receive reliable pricing and purchase dates. Thus, for the analysis, we relied on slaughter dates, which are reported for all transactions and are generally reliable. We have added an explanation about the differences in the dates in the report (see last paragraph of Section 2.2.2).

6. The previous comment on trend impacts and time period sensitivity can be extended to each point made on page 2-29.

RTI Response: We agree and have added a comment to the end of Section 2.2.1.

7. Given the large observation set available, was equation 2.1 also estimated separately for beef and dairy animals (as compared to including a dummy variable as specified in 2.1)? While the current specification can be claimed to "control for breed," it also imposes the assumption that relative price impacts of other factors are the same across breeds. For instance, as currently specified in equation 2.1, price premiums/discounts of AMAs are assumed equal for beef and dairy transactions. This may not be the case and warrants further investigation using alternative model specifications. Instead of estimating two separate equations, one could also replace the *d_beefcattle* variable with a set of interaction variables interacting *d_beefcattle* with *D_AMA* and possibly the other variables currently in equation 2.1. In short, the focus of this section is price differences, these likely differ for beef and dairy breeds, and this needs to be addressed.

RTI Response: We agree and thus reestimated Eqs. (2.1) and (2.4), including interaction terms between cattle type (fed beef or fed dairy cattle) and AMA type. We now discuss price differences for fed beef cattle and fed dairy cattle separately.

8. Specification of equation 2.1 fails to capture the fact that price determination can vary across AMAs. For instance, price realized at time *t* for auction cattle and forward contracted cattle may have been determined at different times. The price at time *t* realized at an auction barn is determined at time *t*. Conversely, the price of a forward contract transaction may have been determined at a period prior to time *t*. Equation 2.1 fails to recognize this fact. This is particularly important given the upward trend in the market during the period of analysis. A similar comment would apply to equation 2.4.

RTI Response: We agree that the dates of price determination can vary across AMAs. As described above, we found that the difference in dates of price determination is typically much less than 2 weeks, based on the data we received from some companies. However, we describe the possible effect of the differences in the dates of price determination at the end of Section 2.2.2.

9. The first sentence of paragraph 3 on page 2-43 needs to be modified. A logical replacement would be "An econometric analysis was conducted of the relationship …"

RTI Response: Correction made.

10. I was a little surprised that this section did not include commentary relating to traceability. Intuitively, as the beef industry continues to improve information transfer throughout the supply chain, incentives such as improved traceability (e.g., ability to track beef &/or cattle back to previous owners and locations) would become increasingly important for packers to note in making purchasing decisions. This in turn may affect the economic value of AMAs. While this discussion may be better fit for Section 4, I suggest the authors incorporate such a discussion to better recognize a growing incentive for AMA use.

RTI Response: We included traceability as a reason to use AMAs in both producer and packer surveys to estimate the importance of this issue in the industry. Less than 1% of producers and 4% of packers indicated that traceability was an important reason to use AMAs. Based on these responses, we were not able to draw the conclusion that traceability is an important motivation for use of AMAs. However, we note in Section 7.2 that the incentives for using AMAs may increase if country of origin labeling or a national animal identification system is implemented or if there is increased demand for certification.

SECTION 3. ECONOMIES OF SCALE, COSTS DIFFERENCES, AND EFFICIENCY DIFFERENCES ASSOCIATED WITH ALTERNATIVE MARKETING ARRANGEMENTS

1. The fourth bulleted point on page 3-2 states "reduced costs of production of $1.25 to $10.00 per head (or 17% to 22% of costs for some producers)." The dollar amounts and percentages don't make sense. For instance, this statement implies that AMAs reduce costs (relative to cash markets) by $1.25/head which is 17% of at least one producer's costs. This in turn implies the producer's costs of production are only $7.35/head, which is hardly believable. I assume there is a mistake in one of these figures that can be corrected.

RTI Response: We have revised the text to eliminate the confusion. Different producers provided each type of response. That is, some answered on a per head basis and others on a percentage basis, and thus the responses are not directly comparable.

2. There is a typo in the third paragraph of page 3-3. The sentence beginning as "Because many smaller beef packers ..." needs to have the word "and" removed.

RTI Response: Correction made.

3. In the second paragraph of page 3-4 is another typo. The sentence beginning "Thus, only data from plants the provided ..." needs to be changed to "Thus, only data from plants that provided ..."

RTI Response: Correction made.

4. In equation 3.1 on page 3-5, the x_t variable does not contain a j subscript, yet the α_j parameter does. This inconsistency needs to be corrected. The text (fourth paragraph) suggests that input price variables were removed. However, no mention is made of removing the trend variable. Skipping ahead to the estimated results in table 3-3 (page 3-16) a trend coefficient is not presented. This needs cleared up in the text of page 3-5 and/or table 3-3.

RTI Response: The reviewer is correct, and we added the subscript. The trend variables were kept in all the models but are not presented because they are not used in the simulations and are discussed in the text. Furthermore, there are a variety of variables in some of the models for some of the plants and some of the firms, but they are not in all of the models. These variations are all discussed, but the table presents only the variables used in the simulations.

5. The AMA variables of equation 3.1 are clearly defined to enter in percentage terms. However, definitions in equation 3.2 are less clear. If the AMA variables enter equation 3.2 as quantities as I expect, than note so on page 3-7.

RTI Response: We agree this was not clear and therefore modified the sentence containing Eq. (3.2) to make it clearer.

6. Equations 3.5 and 3.6, like equation 3.1, include an x_t variable without a j subscript, yet the parameter (α_j)does contain a j subscript. This needs to be clarified. Furthermore, the first and last paragraphs of page 3-9 notes that variables "represented by x_t ... were found to be important." However, table 3-3 does not present coefficients on factors such as the trend and farm-to-wholesale price spread variables.

RTI Response: The reviewer is correct and thus we added the subscript. Also, as with the previous comment and Eq. (3.1), there are variables in the AGM models that are not presented in the table, for simplicity. The variables are important in the model and are reflected in the R^2, but they are not used in the simulation and thus are not presented. There are also variables unique to individual plants that help capture outliers and accounting differences that are not reported. We include discussion of these variables in the text.

7. The last paragraph of page 3-10 says the block of estimated equations "also contains other equations specific to each packer. For example, labor costs, plant costs, …" More explanation on exactly how these "other equations" were specified is needed to fully understand the econometric analysis presented in table 3-3.

RTI Response: We included the recommended additional discussion.

8. In section 3.4.2 of page 3-13, were the volume models (equation 3.2 presented in table 3-2) estimated with OLS? If so state that on page 3-13.

RTI Response: We included the recommended addition.

9. The first sentence (and table 3-3) of section 3.4.3. on page 3-15 needs to be updated to reflect the equations actually estimated. The SUR estimation was of equations 3.1, 3.5, and 3.6.

RTI Response: Correction made.

10. The discussion on page 3-17 regarding the effect of AMA volume on ATC can be improved. Table 3-3 is presenting weighted averages of the estimated model coefficients and standard errors. As such, table 3-3 suggests that the impact of higher volumes transacted through AMAs (holding total volume constant) is not significant. That is, the weighted model coefficients could be deemed not different from zero. This is implicitly noted in the text of page 3-17 but could be further highlighted. I would suggest, in addition to presenting volume weighted coefficient and standard errors, to explicitly note (at least in the text) the percentage of firms estimated to have significantly negative or positive coefficients in individually estimated models being aggregated in table 3-3.

RTI Response: We included the recommended addition. Approximately 49% of the coefficients on the AMA variables are negative and 51% are positive. Negative signs were expected prior to estimation. Of the negative coefficients, 33% are statistically significant, and of the positive coefficients, 9% are statistically significant.

11. The last sentence of page 3-17 suggests a 1% increase in the percentage of cattle procured through marketing agreements is associated with a $0.12/head decrease in slaughter and processing costs. My interpretation of table 3-3 would suggest a $12.15/head decrease in costs. If your $0.12/head interpretation is correct, then an adjustment is needed in how the *P_FC, P_MA,* and *P_PO* variables are defined in the text in preceding pages. A similar point applies to the discussion on page 3-18 interpreting the forward contract and packer-owned impacts on ATC.

RTI Response: The interpretation in the noted sentences is correct. We added a footnote to Table 2-3 to explain that the estimated coefficients represent cents per head.

12. The second paragraph of page 3-18 claims that "… the percentage of cattle procured through marketing agreements has the largest significant effect on ATCs, …" This statement needs further clarification. Table 3-3 would suggests that this effect is not significant. I presume the statement stems from an observation that *of the significant*

coefficients in individually estimated models **the impact of marketing agreements is largest. If this is the case, clearly note so as it is not observable from table 3-3.**

RTI Response: You are correct that the statement stems from the noted observation. We revised the text to note this.

13. The third paragraph of page 3-20 needs to properly reflect estimation of equations 3.5 and 3.6, rather than 3.4 and 3.5 as currently stated. Furthermore, the second sentence of this paragraph is incorrect. Larger volumes of cattle are associated with lower ATC, not higher ATC as stated.

RTI Response: We revised the sentence to be more accurate.

14. The last sentence of page 3-20 refers to the importance of the farm-to-wholesale price spread in the AGM and PPH models. This, in conjunction with prior comments, should be sufficient to demand presentation of these coefficients (and trend) in table 3-3.

RTI Response: As explained in response to a previous comment, there are variables in the models that are not presented in the table, for simplicity. The variables are important in the model and are reflected in the R^2, but they are not used in the simulation and thus are not presented. There are also variables unique to individual plants that help capture outliers and accounting differences that are not reported.

15. Consistent with my ATC model comments, I suggest noting (even in the text of page 3-21) the portion of firms with significant coefficients in the AGM and PPH models aggregated to obtain table 3-3.

RTI Response: We included the recommended addition.

16. I also suggest modifying the discussion of page 3-21 to directly note that changes in each AMA implicitly imply offsetting adjustments in cash/direct trade volumes. That is, be clear with non-economists what the "all else equal" statement means in this context. The discussion currently notes that volume is held constant, but fails to note that the alternative AMAs and variables not presented (e.g., trend and farm-to-wholesale price spread) are also being held constant in the example interpretations provided. There are important implications of clearly making this distinction.

RTI Response: We agree that it is important to note that the model results control for the effects of other variables included in the model. We have added this qualifier at the beginning of Section 3.4.4, where we discuss the estimates used for the simulations in Section 6. Because the cash/direct trade variable is omitted from the regression, the estimated coefficients on the AMA variables are relative to the cash/direct trade volumes. Thus, as you note, changes in each AMA volume imply offsetting adjustments in cash/direct trade volumes.

SECTION 4. QUALITY DIFFERENCES ASSOCIATED WITH ALTERNATIVE MARKETING ARRANGEMENTS

1. The third paragraph of page 4-2 could use further clarification. In particular, premiums of 1-1.5%, $1/cwt liveweight, and $15-$17/head are not consistent numbers. For instance, a $15-$17 premium per head and a $1/cwt liveweight premium correspond to animals with live weights between 1,500 and 1,700 lbs. This is obviously not the case and corresponding adjustments/clarifications need to be made.

RTI Response: These estimates were obtained from different respondents. Some provided estimates on a percentage basis, while others provided responses on a dollar basis. We clarified the text.

2. Equations 4.1 – 4.4 were estimated individually. It seems highly likely that the errors of equation 4.1 and 4.2 (likewise, 4.3 and 4.4) would be highly correlated. An argument consistent with your use of SUR methods in section 3 could be extended to this analysis. Was a simultaneous system also estimated and not reported/discussed for some reason? If a system is not being used, was a series of Tobit models rather than the OLS models considered? All four dependent variables are bound between 0 & 100 making a Tobit model at least worth investigating.

RTI Response: We agree that the errors of Eqs. (4.1) and (4.2), and likewise, Eqs. (4.3) and (4.4), would be highly correlated. However, because the regressors of each equation are identical, OLS and SUR regression results would be identical. (See Wooldridge, J.M. (2001). *Econometric Analysis of Cross Section and Panel Data*, page 164, Theorem 7.6). We agree that Tobit would be more appropriate than OLS for the reason you note and, therefore, we reestimated the models using Tobit.

3. The beef quality grade index (depicted in figure 4-3) was developed from simple average premiums and discounts reported by USDA during the sample period. Hence, only 1 set of weights was used. Alternatively, the weights could have been allowed to vary over time to more accurately reflect market demands. Why were variable weights not utilized? If this was evaluated and found to be insignificant than note so in the text.

RTI Response: Nelson (*AJAE*, 1991) notes that the use of fixed weights is appropriate when considering composite products that include quality changes. Nelson references Theil (*Rev. Econ. Stud.*, 1952-53) regarding this issue, and argues that quality change within a composite category is captured entirely by an index that has as weights fixed relative prices of the individual components. We have added this reference to the report.

SECTION 5. RISK SHIFTING ASSOCIATED WITH ALTERNATIVE MARKETING ARRANGEMENTS

1. Page 5-13 states that the results of estimating equation 2.4 are presented in table 2-2 (see page 2-7). This is not correct. I believe the estimates are presented in the final column of table 2-20. If this is correct, make corresponding adjustments.

RTI Response: Correction made.

2. Page 5-15, second paragraph, states that auction transactions are associated with the highest average price and volatility. A comment regarding time sensitivity of the analysis should be added here. In particular, if the price trend was downward (rather than upward) these conclusions may be tempered.

RTI Response: We revised the text to clarify this point. Average prices do not account for the fact that prices are trending upward or downward. If prices are trending upward, and auction prices are set later than for the other AMA types, auction prices will have a higher average. However, the differences between pricing date and slaughter date are relatively similar across AMAs, and thus we do not think this has a large effect on the averages.

3. The entire discussion of risk (in particular on page 5-14) needs to better note that *risk* in this context encompasses both downside *and* upside risk. This evaluation of volatility is bi-directional. This is not a problem; the text simply needs to better reflect the evaluation procedure utilized.

RTI Response: We revised the wording to mention explicitly that our discussion of price risk includes both upside and downside risk.

4. As previously mentioned, I would suggest incorporating (maybe in summary points) a short discussion on current and future differences in volatility based incentives for using AMAs. In particular, future demands for enhanced information transfer leading to long-term improvements in quality may necessitate long-term AMA relationships. As such, the volatility of price in these long-term contractual environments may be quite different than currently observed.

RTI Response: We agree and have revised the text at the end of Section 5. If the use of traceability increases for the purposes of reducing variation in quality levels, and AMAs help facilitate traceability programs, then we would expect to see reduced price volatility in the future because prices are based on quality levels.

SECTION 6. MEASUREMENT OF WELFARE EFFECTS OF RESTRICTING ALTERNATIVE MARKETING ARRANGEMENTS

1. I would suggest changing the heading in table 6-2 (page 6-12) from *Standard Deviation* to *Short-Run Standard Deviation* to avoid misuse of the presented statistics.

RTI Response: We made the suggested change.

2. Page 6-29 (section 6.5.1) states that the estimates of interests are presented in table 6-2. These "measures of interests" do not include expenditure elasticities. Given the meat expenditure (denoted *Me*) is included in the model, it seems prudent to include corresponding elasticity measures in table 6-2.

RTI Response: Only the own-price supply and demand elasticities are presented in Table 6-2 because they are inputs into the EDM model. Other elasticities (e.g., expenditure elasticities) are not used in our EDM model specification. However, each of these is presented in Tables 6-3,

6-4, and 6-5, because the equations were estimated in double-log form. Hence, the parameter estimates are also estimates for each of the elasticities.

3. Proper specification of equation 6.71 (page 6-46) is extremely important given the multiple uses of implied values for μ_t in subsequent analysis. What specification tests were conducted in deriving equation 6.71?

RTI Response: We agree that the beef market power equation (Eq. [6.71]) is critical in terms of quantifying a measure of potential beef market power (MBF_t) used in Eq. (6.72). The maintained hypothesis was carefully constructed so that the residuals of Eq. (6.71) would be net of processing costs, technological change, retail beef demand, and beef production. Thus, several statistical tests of the properties of Eq. (6.71) were conducted to increase our confidence that the residuals of the equation represent market power given that a direct measure of market power is not observable. Specifically, the equation was specified as a dynamic function with a Koyck term and distributed lags on the independent variables. Nonetheless, the inclusion of dynamics did not improve any statistical properties. In addition, an alternative dependent variable in Eq. (6.71) was considered. The model was estimated using the live cutout beef price spread. However, the statistical properties were again inferior to those presented in this paper. The Augmented Dickey-Fuller unit root test of the residuals of Eq. (6.73) could not reject the null hypothesis of no unit roots. Thus, Eq. (6.73) was cointegrated, which allowed for its estimation and resulting residuals to be based on data in level form.

4. In section 6.9.1 I think additional clarity is needed that a "high" four firm concentration ratio does not necessarily imply the existence of market power. Arguably, higher four-firm concentration ratios make market power more feasible. However, if the four firms are themselves competing fairly rigorously, then market power is not being exercised. This subtle but important point needs to be noted.

RTI Response: We agree and have made the appropriate changes.

5. The definition of *MBF* in table 6-13 needs to accurately reflect the use of equation 6.71's residuals rather than equation 2.3.

RTI Response: Correction made.

6. Throughout the discussion of estimated equation 6.74 (e.g., page 6-49 second paragraph), wording should be *possible market power* or *potential market power* rather than market power. That is, the study has not established the existence of or quantified actual market power. Rather, estimation of equation 6.74 reveals factors impacting *possible market power*. The discussion should more clearly note prior comments made on page 6-46 to this end.

RTI Response: We agree and have made the appropriate changes throughout the section.

7. I suggest elaborating on the last point made on page 6-53. It is likely that "instigators" in trying to remove/reduce AMA use would be in the sector modeled here as the *feeder cattle* sector (e.g., backgrounders and cow-calf). Given the likely audience of this report, highlighting this result is important. Maybe a couple corresponding sentences can be added to the summary points of this section.

RTI Response: We added additional explanation of the implications of this result in the summary for Section 6. Specifically, in every simulation, the feeder cattle production sector loses the most producer surplus relative to all other sectors in both absolute and percentage terms. Feeder cattle producers collectively lose the most producer surplus from restricting the use of AMAs, because the derived demand and primary supply elasticity estimates at this level are more inelastic relative to other levels. That is, feeder cattle producers have less ability to make short-run supply adjustments to price changes relative to other sectors. In addition, feeder cattle producers have the most to lose from decreases in the demand for beef, because the own-price elasticity of derived demand for feeder cattle is also more inelastic relative to other sectors. Therefore, to the extent that a reduction in AMAs reduces processing cost efficiencies and beef quality by more than the gains obtained from reductions in potential market power, the feeder cattle sector is harmed more than any other cattle/beef production sector.

SECTION 7. IMPLICATIONS OF ALTERNATIVE MARKETING ARRANGEMENTS

1. Page 7-8 notes the difficulty in empirically evaluating the link between upstream and downstream use of AMAs. Furthermore, note is made on page 7-5 of how COOL or a national identification system might change incentives for AMA use. However, no mention of food safety is noted here. In short, it is intuitive that some packers may view certain AMAs as strategies to reduce risk from potential food safety issues. That is, a possible incentive for using AMAs to some packers would be to reduce potential cost of lost markets and/or legal suits. There is no mention in this volume of this incentive which likely varies across packers. It seems prudent to incorporate some sort of discussion on this point as it is likely to be increasingly important in the future.

RTI Response: Although the survey and interview respondents did not mention food safety as a reason for using AMAs, we agree that food safety could be an important reason in the future. We added text to indicate this in Section 7.2.

REVIEW OF GIPSA LIVESTOCK AND MEAT MARKETING STUDY
VOLUME 4: HOG AND PORK INDUSTRIES; FIRST DRAFT FINAL REPORT
PREPARED BY RTI INTERNATIONAL IN NOVEMBER 2006
REVIEW (PK-1) SUBMITTED 29 DECEMBER 2006

GENERAL COMMENTS:

The authors are to be commended for their ambitious and rigorous analysis of the impacts of alternative marketing arrangements (AMAs) in the hog and pork sector. A diverse set of econometric tools are used for hypothesis testing and simulation. The report improves understanding of how AMAs might affect the behavior of hog and pork markets, and also how changes in policy regarding their use might prospectively impact the industry. In general it appears that methods used are appropriate to the analysis and many of the results are robust given the assumptions and data used.

RTI Response: We appreciate the positive feedback.

The report was intended to address three issues:
  1) **Determine the extent of use and analyze price differences, and analyze short run price effects of AMAs.**
  2) **Measure and compare costs and benefits associated with spot and AMAs.**
  3) **Analyze the implications of AMAs for the livestock and meat marketing system.**

With regard to item one, the report included information on the impact of AMAs on spot markets and provided information on price differences from both transactions data and mandatory price reporting data. With regard to item two, the report considered the impacts of AMAs on quality issues and created a simulation model to address the expected consequences of restrictions on the use of AMAs which shows their costs and benefits to the industry as well as to producer, packer and consumer segments of the industry. The simulations concluded that, on net, the reduced costs of using AMAs and the improved quality of pork resulting from the use of AMAs offset any costs through market power induced by captive supplies. Regarding item three, the study examined the impacts of AMAs on market power and concluded that indeed there is evidence of market power which the authors characterized as modest. The authors also created a simulation model to examine the impacts of reducing the use of AMAs and showed that the net costs imposed on hog producers and pork consumers offset any minor benefits to packers.

It is my evaluation that the report successfully achieved the objectives requested in its commission by GIPSA.

RTI Response: Comment acknowledged.

However, there are a few key issues which affect the overall value of the report. First, it appears that while much of the effort was spent on modeling, greater effort could have been taken in identifying and collecting improved data on AMAs. Most of the key analyses (e.g., analysis of the impact of captive supplies on prices and modeling the impacts of market power) relied on secondary data such as USDA Mandatory Price Reports and monthly farm price and wholesale price series. In defense of the study, obtaining more

refined data clearly would require cooperation of packers and producers in sharing highly confidential information. The current report could be improved upon with a thorough discussion of the challenges posed by the data and the limitations in drawing conclusions based on data availability. In fact, data limitations (not modeling capabilities) are frequently the weakest link in empirically evaluating industrial organization issues such as this and the report made only limited contributions in this area.

RTI Response: We agree that the challenges posed by the data limitations have been enormous. We have added a more precise description of the problems associated with analyzing the data (survey responses, individual transactions data, and P&L data) that has been supplied by the packers in Section 2.1. We also noted in the executive summary that decisions had to be made with regard to use of the data in order to complete the study on schedule.

Second, the study implicitly assumes the hog market is a national market which is likely true in aggregate and over a long run time frame. However, given observed regional differences in ownership structure, the use of AMAs, and concentration it would be insightful to include some analysis of regional differences that exist. Although previous studies (e.g., GIPSA's 1996 study) tested for regional price differences and in particular cointegration of prices (and found weak cointegration in some cases), this study did not do so and had a unique opportunity given the transactions data available. The mean values of the transactions data showed dramatic differences in price levels which piqued interest for further information, but which the report did not provide.

RTI Response: We agree that regional differences in organizational structure of the upstream segment of the hog and pork industry are important. However, the fact that the downstream segment constitutes a fully functional national market mitigates the significant differences in prices for live animals that may exist in the long run. The reviewer is correct in citing previous literature finding that regional live hog markets are in fact cointegrated. More important than regional differences in prices are the differences in prices across various AMAs, which is the central focus of our analyses. The asymmetric distribution of AMAs across regions (packer ownership in the East and spot markets and marketing contracts in the Midwest) explains what appear to be the regional differences. More important than regional differences are the differences in the same-day, same-place prices across individual transactions. As Section 2.5 indicates, we spent a great deal of time and effort trying to understand what appears to be in direct conflict with the "law of one price."

A final conclusion of the authors is they expect the pork industries use of AMA to remain relatively unchanged in the future and suggest that the pork industry will not follow the poultry industry. However, again regional consideration might provide a different perspective. The eastern region is clearly more similar to the poultry industry in ownership structure and is consolidating further with recent acquisitions. Meanwhile the cornbelt region can be hypothesized to exist as it does due to regional packer ownership and corporate farm ownership statutes. These are being aggressively challenged by several players in the hog industry as evidence that perhaps the industry would prefer to move to a more integrated ownership structure. The study ignores these on-the-ground facts by maintaining the ten thousand foot view of the national level. Again, a defense for this is the usual lack of data for more refined analysis.

RTI Response: Our conclusions about the future developments in industrialization of the hog and pork sector largely rest upon the conducted interviews and surveys of the industry participants and much less on rigorous theoretical modeling. The fact that some states have already adopted certain laws and regulations against the rapid industrialization of agriculture in general, and the livestock sectors in particular, could have played a significant role in framing the situation that we currently observe. However, the issues of state and regional regulatory impediments toward the industry's industrialization and concentration have not been studied in this project because they are outside the scope of work.

Following are specific comments related to specific sections of the report. Some are technical, others are more general. All are intended in the sincerest interest of improving the quality of this report for use by the industry as well as policymakers. Given the short time allotted for review and the technical complexity of many of the sections, questions are frequently posed which, if answered, may help clarify the results. It was not possible to vet all estimation procedures and results.

SECTION 1: INTRODUCTION AND BACKGROUND

This is a standard overview of the structure of the hog and pork sector including location, size, concentration, trade, etc of firms in the industry. Seems to adequately address overview for general audience.

It does ignore the role of the feed sector in structure of the industry. Along with multisite production for health management, the specialization of feed manufacturing and the associated economies of scale with a feed mill hub supplying multiple farms revolutionized the structure of the industry. Murphy Family Farms was built on this model and has been replicated throughout the U.S., including the traditional corn belt. This drives economies of scale to a new level centering on the feed manufacturing facilities.

RTI Response: We agree and revised the wording to indicate the role of the feed sector in the structure of the hog industry in Section 1.

SECTION 2: VOLUME DIFFERENCES, PRICE DIFFERENCES, AND SHORT-RUN SPOT MARKET PRICE EFFECTS ASSOCIATED WITH ALTERNATIVE MARKETING ARRANGEMENTS.

Section 2.1.1. "The number of respondents to the surveys is 229 pork producers and 88 pork packers."

Were packer respondents individual plants? If so, multiple plants for a single firm must be included. Is this addressed in the survey results? Please clarify the nature of these respondents. On page 2-3 there were 29 processing plants owned by 15 different companies. But is it possible to expound on that even further? What was the share of plants owned by largest firm (e.g., did one firm own 16 plants and the other 14 firms own one plant each)? How many plants were there to begin with?

Note: The reviewer does not have access to reports such as "Volume 2, Appendix D" referenced in footnote on page 2-2, hence, some issues may be addressed in other volumes and are unknown to the reviewer.

RTI Response: Volume 2 of the report does provide the details on the survey procedures and responses. The packer respondents were individual plants because the survey questions were designed for plant-level responses. We did obtain multiple plant responses for some packers. Because of the confidentiality concerns with the study, we do not provide the details on the number of plant responses received from each company that responded. The initial survey sample for hog packers included 309 plants (the entire population of federally inspected plants), but some of these plants were not eligible for the survey because they only slaughter spent sows and boars or only custom slaughter. We did not obtain the response rate that we had hoped for, but we did obtain a sufficient number of responses to provide some generalizations about industry practices. Also, the survey responses included smaller packers that were not included in the transactions data collection.

Section 2.1.2. **Is it possible for the authors to summarize the impact on the data that the data cleaning procedures described in this section had on the variables? The lost observations represent over 16% of the total sample, it would be helpful to be able to assess any impact this had.**

RTI Response: As it turns out, the implemented data preparation procedures caused considerably smaller losses of information in terms of the actual number of hogs transacted then appears to be the case when one looks at the eliminated number of transactions (lots). Specifically, the cleaning procedures eliminated 20.3% of all transactions (lots) but only 6.7% of transacted market hogs. The resulting effects on the subsequently performed analyses are likely to be relatively insignificant.

Section 2.1.4. **As the authors suggest it is surprising that the implied live-weight yield is approximately 0.88. The number of observations is not given for each price. Large scale packers almost exclusively buy on a carcass weight basis – is it possible that the live prices were over-sampled from smaller plants; contributing some to the price difference? Is it possible that the live purchases are 'fill hogs' which are purchased mainly to fill a day's kill at the large plants and hence have a higher price? I suspect it is a sampling effect because most packer formula price live/carcass weight prices using a standard yield so there must be some explanatory anomaly to this.**

The sentence: "These results suggest packers consistently paid more for live hogs by over 10% in excess of these hogs' physical yields." should have some caveats. Otherwise it leads to the impression everyone ought to sell on a liveweight basis which I doubt is true.

RTI Response: The puzzle surrounding the abnormal implied liveweight yields definitely deserves further attention and explication. We accepted your suggestions for why we observe this anomaly and along those lines performed a variety of different additional analyses, which are now presented in Section 2.1.4. We now have a slightly better understanding of what may be going on in the data, but we are still unable to solve this puzzle. One possible explanation may be data entry error for some of the records (e.g., liveweight pricing was indicated instead of carcass weight pricing).

Page 2-32. **Top of page it is stated that: "All the estimates for individual coefficients are significant at the 1% level except for the dummy for the Midwest Region (R3)." This is incorrect. The insignificant dummy is R3, but that is identified as the "Other" region. Also, why not conduct a test for endogeneity to determine whether it exists?**

RTI Response: The model with results represented in Table 2-16 has been reformulated and reestimated, correcting for endogeneity of the AMA portfolio choices using 2SLS. The entire procedure is explained in the new version of the report.

Section 2.3.3. **Tables 2-9 through 2-11 show that the price of hogs in the eastern region is much lower than other regions. However, no explanation is provided of this discrepancy and this is an issue that is ignored throughout much the report. The East region includes (page 2-3) NC, SC, GA, VA, PA, and MD. This region has highly concentrated processing. This region gets to the primary issue of the potential for market power and the impact of ownership – and the results of transactions prices provide some interesting prospects. However, it is lost in the rest of the report as nearly all subsequent results are based on national level secondary data. Authors will likely argue that the hog market is a national market. This may be true for weaned pigs, but is not necessarily true for market hogs which are expensive to deliver over longer distances. Further, the hogs in the east are largely packer owned or contracted and therefore are not able to easily change locations due to market forces. Perhaps the very low price in the East is indicative of this unique industry structure, but the report provides few insights.**

RTI Response: We added additional explanation for the observed differences in prices across regions. As we mentioned in our response to one of the earlier comments, the main reason for the regional difference in prices between the East and the rest of the country is the composition of AMAs used by packers to procure their hogs. As discussed in Section 2, the mean price of packer-owned hogs in the East is substantially less than the national average price for packer-owned hogs. Packer-owned hogs account for more than half of all packer purchases in this region. Because the price of packer-owned hogs is an internal transfer price, and as such may not represent an arms-length transaction, it would be very hard to use this information to conjecture anything about the packer's market power in this region. In fact, when one looks at the cash/spot segment of the market, the dealer or broker price in the East is actually higher than the national average, whereas the direct trade price is still lower than the national average.

In the regression of Table 2-16, it would be interesting to include a variable that represents capacity indexed by number of plants comprising that capacity since this appears to be a regional value.

RTI Response: We agree. The model with results represented in Table 2-16 has been reformulated and reestimated, and the text has been revised.

Section 2.5. **In table 2.27, the sign in 'distance' changes and no explanation is given. Also, why isn't the binary variable coefficient included in the results? Shouldn't the coefficient of competition index cut both ways? That is, shouldn't there be one relative to the number of buyers (packers) in the region too? Perhaps that would explain some of the price dispersion?**

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RTI Response: We do not have a very good explanation for why the sign of the "distance" variable changes. However, the estimated coefficient is close to 0, so it has virtually no impact on price dispersion. The binary variable coefficients are not reported because there are too many of them. We use three-digit zip codes and therefore there are about 30 zip-code binary variable coefficients to report. The reviewer is correct when saying that the competition index cuts both ways. This is precisely what the zip-code binary variables do; they measure the competition intensity in a particular geographical area (zip code), and this competition is simultaneously determined by the number of sellers and the number of buyers.

SECTION 3: ECONOMIES OF SCALE, COST DIFFERENCES, AND EFFICIENCY DIFFERENCES ASSOCIATED WITH ALTERNATIVE MARKETING ARRANGEMENTS.

Section 3.1.1. **The interpretation of the estimation is a bit confusing. The dependent variable is defined as the wholesale price (P) divided by the farm price (W_1). Then, the last sentence on the page states: "Theory suggests that this effect should be negative if there is market power in the market for hogs." This statement seems to be saying that as the share of captive supplies goes up, the ratio of wholesale price to farm price (P/W_1) should decrease in other words the farm price would need to rise relative to the wholesale price (the spread would narrow). Isn't this counter to what one would expect to happen with increased market power? Shouldn't increased market power result in farm prices falling relative to the wholesale price? In other words, the spread widens? It's later stated on page 3-4 that the positive sign on the regression estimate of the *capshare* coefficient in equation 3.2 on page 3-3: ".. has a positive and significant effect on market power…" Something in this section seems to be amiss since the statement of theoretical expectation (negative) is different from the statement of empirical results (positive).**

Probably simply a phrasing issue.

RTI Response: This is a misstatement. We have removed the sentence and reference from the text. Thank you for pointing out this error.

Section 3.1.1, page 3-4, first full paragraph, last sentence. **Can the authors expand on how the degree of market power is determined to be 'modest'? No market power evidence would obviously result from a failure to reject the hypothesis. What would be considered severe? Can this be made more objective to the reader? As I interpret it the mark-up is almost 11% (1.10814). Or am I interpreting this incorrectly? Perhaps a term of 1.1084% was meant?**

RTI Response: The estimated degree of market power is modest in the sense that many researchers often estimate markups of 20% or more for other industries (see, for example, Bhuyan, S. and R.A. Lopez (1997). "Oligopoly Power in the Food and Tobacco Industries." *American Journal of Agricultural Economics* 79:1035-1043.). We added the reference to the above work in the footnote.

Section 3.1.1, Conclusion. **The conclusion from this section is that market power is outweighed by reduced costs of AMAS and in Appendix B, Attachment 2, it is given as a – 1.47% improvement. This conclusion is central to the welfare effects. How does one allocate the net affect to producer price and wholesale price which is a proportion in the estimation? In other words, who gets the cost savings from captive supplies and who captures the market power gain? The analysis does not appear to be an allocation of this 'net benefit' to AMAS. It would seem logical that the processor captures all of the market power value. So, what remains is how is the value of the marginal cost savings from AMAS allocated? The analysis does not seem to address this allocation of returns.**

RTI Response: This estimate is strictly the added net costs from restricting use of AMAs. What this means is that packers' costs of processing would increase if they were forced to restrict use of AMAs in hog procurement. How this increase in costs would be distributed between consumers and producers depends on relative supply and demand elasticities. Given the overall relative inelasticity of supply, a large proportion of the costs would be borne by producers in the short run but less in the long run, as producers are able to adjust to the regulation. The section on simulation analysis (Section 6) answers this question more fully.

I am somewhat concerned about the use of USDA monthly farm/wholesale margins for this analysis. I recognize the difficulty in getting a good wholesale price series. However, I strongly suggest the authors provide a thorough and critical assessment of the monthly data set used and particularly the wholesale prices. Issues to include might be the thinly traded carlot reports, the use of standard yields on primals to calculate the wholesale pork cutout value, etc. Each of these fundamental data characteristics can affect the results.

RTI Response: The monthly farm/wholesale margin data are the only widely consistent data available for this type of analysis. The traded carlot reports you refer to actually are a scientific sample of industry sales. These data have been the industry standard for decades, and we are not aware of any major concerns about the reliability of these data. The price spread data are carefully put together and seem to track changes in the industry, at least based on the statistical analyses we did and others that have been done in the past. Questions have been raised about the reliability of the retail data in margins for meats, but ERS now tracks prices with scanner data to capture sales and other features the BLS series did not capture. However, the analysis here is based on farm–wholesale price spread data, which come from reliable sources.

Finally, back to an earlier comment about the differences in regional transactions prices. As a hypothetical question, what to you think the results of this analysis would be if regional transaction prices were used in the dependent variable – do you think the Eastern region might show much greater market power? Subsequent results refute this outcome and it's highly important to assist readers with understanding the implications of using alternative forms and sources of data.

RTI Response: In a separate study, state-level data published by NASS (16 states and 9 years) were used to test for market integration in the swine market (http://www.cals.ncsu.edu/ waste_mgt/smithfield_projects/phase2report05/phase2report.htm [Appendix B.2]). The null hypothesis of market integration is not rejected, indicating that the law of one price holds for the

U.S. hog market. Based on this analysis, we believe it is reasonable to treat the market for hogs as a national market.

***Section 3.1.2, Packers' Individual Transactions Data Approach.* The authors recognize the results of Table 3-4 conflict with those using the industry data. The discussion should be expanded to describe what the implications are rather than simply saying it's 'very interesting'. It strikes me that perhaps market power is significant and that the value of captive supplies (the off-setting benefit) may be non-existent according to transactions data which in my opinion is probably a more accurate evaluation. In particular, wouldn't the subsequent welfare simulations be impacted by this result?**

RTI Response: We believe the results are actually not that much different. Both approaches find the presence of market power in the market for live hogs but they disagree on the sources of that market power. The aggregate data approach finds that the origin of market power is linked to the use of AMAs, but in the individual transactions data approach this parameter is statistically insignificant, allowing for possible alternative explanations for the existence of market power (for example, the traditional oligopsony story). In the revised version of the report, we stated that the obtained result would require further investigation into possible sources of market power, but this topic is beyond the scope of the project. The possible existence of market power is incorporated in the simulation model (via a built-in wedge between price and marginal revenue product of hogs), so the subsequent simulation analysis is not affected by these results.

***Section 3.2.2, page 3-16.* In the last paragraph, about mid-paragraph it is stated that "…economies of scale diminish as firm size increases." The estimation is done on plant size so firm should be plant. However, this does raise an interesting issue not addressed. Are there economies of scale associated with multiple plants? This is particularly relevant in terms of recent acquisitions. What exactly are the benefits of the mergers of large scale firms – it must not be at the plant level according to your results, but are there efficiencies to be gained in extending the firm?**

RTI Response: This is an extremely interesting question to which there is no answer with the currently available data. The problem stems from the fact that all P&L data are plant level, so we have no information about possible efficiency gains that may have occurred at the company level through, say, mergers and acquisitions. To answer the above question, one would need to have access to the company level P&L data, provided that their accounting systems are actually set up to produce this kind of information, and then one would need to analyze how the firm size affects the cost structure, especially line items such as overhead, research, and marketing. We changed "firm" into "plant" everywhere in the text to avoid confusion.

***Section 3.3.* This is an interesting contribution, and I find the concept intriguing. The finding of a lack of evidence of complementarity or substitution are still useful for demonstrating that any regulations proposed to limit use of contracts can be best analyzed by focusing on the direct effects of the AMAs.**

RTI Response: We appreciate the positive feedback and agree with your conclusion. Notice also that the entire section of testing for complementarity has been redone. We discovered that the small sample size (18 firms) renders the methodology presented in the draft report volume

inadequate and thus we had to resort to a substantially simpler procedure. However, the new results turned out to be qualitatively the same as the old results.

SECTION 4: QUALITY DIFFERENCES ASSOCIATED WITH ALTERNATIVE MARKETING ARRANGEMENTS

This section is a relatively straightforward assessment of quality impacts of alternative contract arrangements based on Mandatory Price Report data. The results are straightforward and support the conventional wisdom and results from surveys that suggest one of the primary reasons for AMAs is quality control and assurance.

RTI Response: Comment acknowledged.

My only suggestion is that the authors might refer to the provisions of AMAs. Most have quality specifications (carcass weight, and some form of lean %, LEA, etc) right in the contract so one would expect them to be higher quality by definition – pre-sorting essentially occurs. Secondly, several AMAs have provisions that require quality relative to plant averages which provides incentives for continuous improvement so that those producers with 'captive demand' don't become complacent in keeping up with the industry.

RTI Response: Based on our knowledge of the industry, we concur. The problem is that we did not have access to a representative sample of the actual marketing or production contracts that the industry is using, so we cannot refer to the actual contract provisions in formulating testable predictions about the quality differences across various AMAs. Instead, we let the data tell us the story.

SECTION 5: RISK SHIFTING ASSOCIATED WITH ALTERNATIVE MARKETING ARRANGEMENTS

Section 5.1. **Are the variances and covariances reported somewhere? It would be useful to see those values here in addition to the Wald test in Table 5-1; or provide a reference to elsewhere in the report where they can be found.**

RTI Response: The variance-covariance matrix has not been reported anywhere in the report. We added the variance-covariance matrix in the revised version of the report in Table 5-1. The original table with the Wald Test Statistics is now Table 5-2.

Section 5.2. **Regarding the conclusion, is the information available to report the return and compare that to the baseline as well?**

RTI Response: Although we agree that this would be of interest, we are not able to calculate the return on capital (investment) because it requires data on the size of the investment required in contract production versus the size of investment required in independent production. We do not have access to this type of data.

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Section 5.3. **This section is predicated on the assumption that the selection of a contract is based on the producer's characteristics (farm type, location, acreage etc.) and risk aversion. However, this seems to ignore the endogeneity of the availability of open markets v. production contracts. Going back to the transaction data that showed market prices in the Eastern Region to be $10 per cwt. lower than in the Midwest region, this could have an effect on the choice of open market versus production contracts. On page 5-12 this may come through in the results for disutility of contracts based on east location (I realize these are completely different datasets, ARMS instead of transactions data). The disutility in the East from open market may be due to lower market prices observed in that region in contrast to the Midwest results, but the authors don't explore this issue further.**

This concern then becomes manifested in section 5.3.4 where the authors simulate what would happen if production contracts were not offered and producers with contracts were forced into a higher risk option such as the cash market. It shows that there would be a welfare loss by these growers. What if the farmers are actually forced (i.e., take-it-or leave-it offers) into a lower return all-be-it less risky contract because of a lack of availability of cash market contracts through market power which was shown to exist in previous sections of the report? In other words, what if the contract selection is not purely a choice variable? Then the value of risk aversion is over-estimated based on the contract selection.

RTI Response: This point is well taken. It essentially says that farmers did not self-select themselves into contract production because of the lower risk associated with this type of market arrangement, but because the expected cash price is lower that the expected contract payment. However, this reasoning does not appear to be supported by the empirical evidence. As we argued before, the main driver of the difference between the price in the East and the rest of the country is the price of packer-owned hogs, which cannot be considered as the true market price because it represents the internal company transfers. When one considers the cash/spot segment of the market, the dealer or broker price in the East is actually higher than the national average, whereas the direct trade price is still lower than the national average.

SECTION 6: MEASUREMENT OF WELFARE EFFECTS OF RESTRICTING ALTERNATIVE MARKETING ARRANGEMENTS.

Appendix B: Welfare Effects of Restricting AMAs in the Hog and Pork Industries

The stand-alone econometric model seems to be well specified, including accounting for imperfect competition as supported by previous section results. I am curious as to why the Rotterdam demand specification excludes other meats? Even with pork cuts, I would expect that these are not separable in a two stage budgeting process. Inclusion of other meats (beef and poultry) would limit the ability of pork processors to pass on price effects to consumers as they would substitute beef and poultry for pork. Therefore, I would expect pork consumers to be harmed less by the scenarios than indicated here. However, producers may be harmed more with the inclusion of beef and poultry so I'm not sure it impacts the general outcomes of the simulations.

RTI Response: We appreciate the positive feedback on the econometric model. The demand estimates do in fact account for substitution between pork and other meats. The Rotterdam

Model (RM) is used to estimate substitution among individual pork products. On page B-22 in Appendix B there is a discussion of how, using two-stage budgeting, the conditional elasticities from the RM are used with elasticities from the first-stage demand estimation (allocation of total consumer expenditures among individual meats [beef, pork, poultry], and all other goods) to develop unconditional demand elasticities that account for all sources of consumer substitution. Tables B-11a and B-11b show the resulting compensated and uncompensated elasticities.

It would be helpful to report standard errors of elasticities in the tables in Appendix B. Given time restrictions of review, it's not possible to go through the complete specification of the model. However, frequently any issues embedded in the specification and coefficients are ultimately manifested in the simulation results, so my comments are focused there.

RTI Response: Given the schedule for completion of the study, we were not able to modify all of the tables to present the standard errors for the elasticities. However, it is possible to infer which elasticities are significant based on the econometric models, because they are linear models. Indeed, in the case of the packer behavioral equations (Tables B-9 and B-10), the elasticities will have exactly the same t-values as the parameters in the corresponding structural equations. For the wholesale demand elasticities, it is more difficult because these are actually synthesized estimates, but again, one can look at the RM model to get some idea of the precision of the elasticities, because the conditional demand estimates from the RM dominate the elasticities. (Recall that the conditional compensated elasticities are obtained by dividing parameter estimates of the RM model by expenditure shares, which are regarded as nonstochastic.)

In Table 6-2, could the authors explain more clearly why some primal quantities increase? In particular, hams for example increase by 7% while loins decrease by 6.2%. The authors comment only briefly on this on page 6-7, but provide no insight into how this might occur. Seems odd in a market where the production side should have relatively constant proportions as often assumed in other parts of the report.

RTI Response: The reason we see different changes in quantities of primal cuts from the simulations is because relative prices change, causing consumers to substitute among the wholesale primals. No a priori restrictions were placed on substitutability in consumption and production in estimation. Table B-1 (Appendix B) shows quite clearly that there is considerable variation in the quantities of primals sold. Indeed, the coefficients of variation (CV) in quantities tend to be larger than the quantities of AMAs produced (Table B-2). It is not clear that the fixed proportions assumption you allude to is valid. In Section 3, analysis of the price spread was conducted *as if* the fixed proportions assumption is valid, not whether in fact it *actually is* valid. A footnote is included in that section to indicate that, in fact, the degree of market power likely is overestimated because of the assumption of fixed proportions in interpretation of the results.

When accounting for processor's net revenue, how were the company owned pigs accounted for? It appears that all that value is included only as a producer surplus issue, but couldn't it be argued that a restriction would result in a loss of that revenue to packers? If so, perhaps packers actually do suffer a loss. If you consider packers' existing financial reports, they tend to treat the hog production segment as the profit center. This accounting difference would perhaps affect packer net revenue making that negative too.

RTI Response: Loss in net revenue from forcing the sale of packer-owned hogs is in fact included in the change in processor's net revenue (Appendix B, Eq. [B.19c]).

SECTION 7: IMPLICATIONS OF ALTERNATIVE MARKETING AGREEMENTS

Section 7.1. **Given the very low sample size of producers (eight) and packers, this doesn't seem to add much other than to provide limited perspective from industry – particularly as regards the results of the simulation model.**

RTI Response: This section is included to provide descriptive information that might be of interest to some readers of the report. By indicating that the summary is based on a low sample size, readers will have an understanding of the representativeness of the information.

Section 7.2. **The statement that there is no expected change in the use of AMAs raises the issue that the report does not address the fact that state restrictions already exist on the ownership of hogs – primarily in the Midwest. States which do not have statutory or constitutional restriction on ownership (e.g., North Carolina, Oklahoma) have a higher degree of packer ownership and AMAs and also higher packer and producer concentration ratios – more in line with what is observed in the poultry sector. This results in the Midwest (and particular the Western Cornbelt) being the primary price discovery region with mandatory price reporting as it has the greatest share of open market hogs procured. Were it not for these state ownership restrictions, my intuition is that the industry would in fact look much more like the poultry industry. Failure to allow for incorporating regional heterogeneity (most starkly identified in regional price differences in transactions data) limits the ability to make broad generalizations.**

RTI Response: As we argued earlier, our conclusions about the future developments in industrialization of the hog and pork sector largely rest on the conducted interviews and surveys of the industry participants and much less on a rigorous theoretical modeling. The fact that some states have already adopted certain laws and regulations against the rapid industrialization of agriculture in general, and the livestock sectors in particular, could have played a significant role in determining the situation that we currently observe. However, the issues of state and regional regulatory impediments towards the industry's industrialization and concentration are outside the scope of this project.

REVIEW OF GIPSA LIVESTOCK AND MEAT MARKETING STUDY
VOLUME 4: HOG AND PORK INDUSTRIES; FIRST DRAFT FINAL REPORT
PREPARED BY RTI INTERNATIONAL IN NOVEMBER 2006
REVIEW (PK-2) SUBMITTED 28 DECEMBER 2006

COMMENTS

1. Page ES-2, last sentence. This sentence is confusing. Can it be simplified or can more explanation be given. Why is this finding important?

RTI Response: We agree that this sentence is confusing and have deleted it.

2. Page ES-3, 1st bullet.
   a. **Should the 1st sentence read ". . .increased market power, *but* a second model . . ."?**
   b. **It would help to have some indication as to whether the 1st or 2nd model is a preferred specification for testing market power. My guess is that it would be the model fit to company-level data.**
   c. **The sentence in bold suggests market power is found in the first model but not the second, but then later in the bullet the text indicates "Both approaches found a statistically significant presence of market power . . ." Please clarify**

RTI Response: The results from the two models are actually not that much different. Both approaches find the presence of market power on the market for live hogs but they disagree on the sources of that market power. The aggregate data approach finds that the origin of market power is linked to the existence of packer-owned hogs. However, in the individual transactions data approach, this parameter is statistically insignificant, allowing for possible alternative explanations for the existence of market power (for example, the traditional oligopsony story). As previously written, the entire bullet was confusing and was rewritten.

   d. **Although there may be *statistically* significant findings of market power, there needs to be some statement about the *economic* significance. How large is the effect relative to say the cost efficiencies of larger plants?**

RTI Response: In the report we state that the magnitude of market power is modest in the sense that many researchers often estimate markups of 20% or more (see, for example, Bhuyan, S. and R. A. Lopez (1997). "Oligopoly Power in the Food and Tobacco Industries." *American Journal of Agricultural Economics* 79: 1035-1043.). On page 3-5 in the report, we indicate that a 1% increase in packer ownership increases market power by 0.735% but decreases marginal costs by 4.99%. This might suggest $0.735 / (0.735 + 4.99) = 0.128\%$ of the change is due to market power.

3. Page ES-3, 2nd bullet. I suggest deleting the redundant sentence that begins, "As the plant size decreases, negative economies of scale . . ."

RTI Response: We agree and have deleted this sentence.

4. Page ES-5. It is indicated that the reason consumers lose in the welfare analysis is because of lost efficiency. But the analysis in the report also indicates that quality would decline if there were restrictions on AMAs. Would it be fair to say that there would be further consumer welfare losses resulting from quality reductions beyond those losses resulting from a price increase?

RTI Response: There may be further consumer surplus losses resulting from quality reductions, but these would stem from changes we could not anticipate (e.g., not offering certain products for sale that were offered before). One reason for formulating and estimating such a disaggregated model, at the level of individual primal cuts and different sources of AMAs, was to account for quality in the simulation analysis. So the consumer surplus losses do account for surplus losses due to changes in composition of pork consumed. As indicated in that section, using the Theil measure of quality change, there are (slight) reductions in the quality of pork in the different simulations.

5. Page 1-15. Figure 1-11. Is this figure necessary? It is very similar to the previous figure. Would this figure not fit better in volume 6 devoted to the downstream industries?

RTI Response: For completeness and consistency across the report volumes for each species, we have left this figure in the report volume. This allows us to also describe sales methods used by the packers. Although it has similarities to the figure on purchase methods, there are several differences.

6. Page 2-2. I was somewhat surprised by the relatively small number of producers (204) in the pork producers survey. If this is the sample size, what is the size of the population? What degree of sampling error results from this sample size? If a stratified random sample was used were the strata weights used to make the sample match the population? If this is the population (i.e., there are only 204 pork producers in the US), some comment might be made about the size of this segment in relation to welfare effects of AMAs.

RTI Response: As explained in Volume 2 of the report, we used standard survey sampling and weighting procedures for the industry survey. The sample was stratified based on size. Information on the survey population and response rates, and confidence intervals for all questions included in the survey, are included in Volume 2. Although we obtained a lower response rate from pork producers than we had desired, we obtained enough responses to make inferences to the population. The measures of economic surplus associated with AMAs are estimated based on MPR data, and thus the number of producers that responded to the industry survey does not affect the economic surplus results.

7. Page 2-7. It would be helpful to provide some discussion on *why* packers pay more for live hogs vs. carcasses.

RTI Response: This issue has been addressed and some additional analyses were performed (see Section 2.1.4). A part of the explanation may have to do with the timing of the purchases (i.e., "live" pricing could be frequently used to smooth out slaughterhouse scheduling problems) and therefore, these prices may be unreasonably high. However, the effect that we found is not large enough to explain this anomaly.

8. Page 2-16. Beginning of section 2.2.2. Why classify packers by number of head? Why not report correlations between size and %spot?

RTI Response: We classify packers by number of head because plant capacity is typically expressed in number of head. A measure of size based on revenue would vary over time because of changes in prices, but the number of hogs that can be slaughtered is fixed based on the line speed and operating hours of the installed equipment. In this part of the study, we generally do not report the correlation coefficients, because we are working with cross-sectional data and the number of observations in certain subcategories is too small.

9. Page 2-16. Beginning of section 2.2.2. It is important to note in regard to the results in table 2-5 that these are not ceteris paribus effects and the results could be due to regional differences or other factors.

RTI Response: The results presented in Tables 2-5a and 2-5b are simple tabulations of the survey results describing purchases and sales of live hogs according to the size of producer or packer. We agree that differences could be due to regional differences of other factors, but we believe that it is not necessary to note that the presentation of the summary data have not accounted for these other factors.

10. Page 2-25, table 2-9. Why report the statistics for 2004 separately?

RTI Response: Prices for 2004 were reported separately to provide a basis for comparison with other publicly available sources, which usually present their data on an annual basis.

11. Page 2-29. It is indicated that "higher prices are clearly associated with higher loin-eye depth." But the highest prices are for the "missing" category. Why might this be?

RTI Response: The content of the "missing" category is unknown to us, so it was not very meaningful to report this result to begin with. Hence, the data in this row have not been reported in the revised version.

12. Page 2-29 and 2-30. It would be helpful to provides some theoretical discussion as to what is being measured by this model—or is the model completely ad-hoc—measuring simply associations in the data—if so this needs to be explicitly stated.

RTI Response: This model has been reformulated and reestimated. The model does not have firm theoretical foundations. Its purpose is to explain the most important factors driving the price differences across AMAs. However, it is similar in spirit to the "performance approach" used in complementarity testing later in the same section.

13. Pages 2-31, 2-32, table 2-16 and the associated discussion.

    a. **The significance values (p-values) are almost meaningless given the large sample size. What is needed is more discussion on economic significance.**

    RTI Response: We added more discussion related to the interpretation of the coefficients.

b. **The magnitude of some of the coefficients is small because the magnitude of the independent variables are large (i.e., slaughter capacity). As such, have you considered reporting standardized regression coefficients?**

RTI Response: Yes, we have rescaled some variables, such as capacity, to obtain more meaningful magnitudes of parameter estimates.

c. **Should there be a negative sign in front of the coefficient on weekly slaughter capacity in table 2-16?**

RTI Response: Correct. However, the results changed with the revised estimation approach we used.

d. **In the discussion on page 2-32 comparing tables 2-9 to 2-16, it should be mentioned that the latter report ceteris paribus effects whereas the former does not.**

RTI Response: Yes, this is correct.

e. **Middle of page 2-32. If larger plants have more packer owned hogs, then could this explain the lower prices of companies with more than 5 plants (see table 2-15) not market power?**

RTI Response: This interpretation is certainly possible, although not very probable, because as indicated in Table 2.5b, the large plants procure only 12% of their hogs through packer ownership. The majority of their hogs come from marketing contracts (65%) and cash spot markets (23%).

14. Page 2-33, beginning of section 2.3.4, first sentence. Does the analysis that follows really test whether "observed average prices . . . vary with market conditions . . . because of changes in feed costs or shifts in consumer preferences?" Wouldn't this sort of test involve testing the stability of the OLS dummy variables associated with the AMAs? This sub-section (2.3.4) is rather detailed and tangential to the primary purpose of the report; can it be relegated to an appendix?

RTI Response: We agree with your statement that Subsection 2.3.4 is rather detailed and tangential to the primary purpose of the report. We accepted your advice and removed that section from the report.

15. Page 2-36. Start of section 2.4. What is MPR data? Is this new data or some of the data previously discussed? I believe this is data from mandatory price reporting, but some discussion of this data source is needed.

RTI Response: MPR data is Mandatory Price Reporting data. It has been previously introduced in Section 2.1.4 and then in detail in Section 4.1 and Section 5.1. In the final version of the report, the presentation of all data sources has been moved to Section 2.1.

16. Section 2.4. First sentence. How does this analysis, in terms of its purpose, differ from what was done in the regression in the previous section (e.g., table 2-16)? Which analysis is the more appropriate one?

RTI Response: The two sections address two different issues. Table 2-16 explains the causes for differences in prices across AMAs, whereas Section 2.4 explains the determination of the negotiated price.

17. Section 2.4. Again, it would be helpful to have some theoretical discussion here. It was not clear where this was all going until I hit equation 2.3. But even then, I question whether this is an appropriate way to conceptualize the problem. Do packers really demand contract types (or different marketing arrangements) or do they demand hogs? A marketing arrangement is a means to an end—not what is actually demanded. Why would a packer have demand a particular *method* of purchasing? I think there be some good answers to this question, but they are not presently in the text. It would be helpful to have some discussion indicating that this is the proper way to conceptualize packer derived demand.

RTI Response: Packers may actually differentiate based on characteristics of the carcasses purchased but, for the problem at hand, there doesn't seem to be any loss in generality by assuming hogs are available from different sources. To the extent that differences in qualities of carcasses are reflected in the source of hogs procured (i.e., AMAs), then the classification of inputs into the AMA categories serves the purpose of the investigation into the effect of the possible regulations on the mix of AMAs at the market level. The acid test is the econometric results, which are quite consistent with theory, showing high but not perfect substitutability among hogs distinguished by AMA.

18. Page 2-40. The discussion on the estimation procedure either needs to be expanded, removed, or simply refer readers to an appendix—at present it is simply confusing.

RTI Response: Appendix B is referenced in the text to provide more details on the estimation, so the reader can pursue those details if so desired. These paragraphs only give a brief overview of how the model was estimated so that this section can focus on the results.

19. Page 2-41. Is it customary to find cross-price effects larger than the own-price effect? What does this imply about the nature of the demand system?

RTI Response: Actually, cross-price effects are not larger than own-effects—see Appendix B, Table B-6 (note that the a_{ij} are absolute values). Recall that both symmetry and negative-semidefiniteness are imposed so the estimated inverse demand functions are theoretically consistent. Also, note that negative-semidefiniteness relates to the slopes not the elasticities; therefore, it is possible to have cross-price flexibilities that are larger than own-price flexibilities, even though the cross-price effects are smaller than own-effects.

20. Figures 2-4 and 2-5. The labeling of the horizontal axis doesn't make any sense.

RTI Response: We fixed the horizontal axes in the graphs such that they now indicate calendar dates.

21. Section 2.5. Just a thought. One explanation for the price dispersion not mentioned in the discussion is asymmetric information regarding quality. Many of the quality measures are only observed after slaughter. Presumably packers have more data and experience predicting carcass quality pre-slaughter than do individual producers.

RTI Response: We agree that this could be an explanation. However, we do not have the data to control for asymmetric information and therefore this proposition cannot be formally tested.

22. Page 3-10. It is a bit unclear as to what data is being used in the estimation. It is indicated that "monthly data with a total of 30 observations per firm" is used. The other parts of this volume seem to imply many more transactions per firm. Also, I assume the downstream inverse demand function is estimated using data other than the transaction data, but what data is used?

RTI Response: This model has been estimated with individual transactions data (millions of transactions) aggregated into monthly observations. Therefore, there are 30 observations (2.5 years) per firm. As noted, all other data used in the estimation come from the Bureau of Labor Statistics.

23. Page 4-6, there appears to be a—sign missing from equation 4.1.

RTI Response: Correct, it has been added.

24. Section 3.1.2 vs. 3.2. Some discussion seems warranted regarding the apparently contradictory findings regarding economies of scale with the 2 different cost functions being estimated in the two different approaches in sections 3.1.2 and 3.2.

RTI Response: In fact, there is no contradiction but a mistake on our part. The model presented in Section 3.1.2 cannot be used to calculate the economies of scale measure, as given by Eq. (3.21) because the constant term (fixed costs) in that cost function cannot be separately estimated. Therefore, this model is silent about the existence of economies of scale, and that paragraph on page 3-12 has been deleted.

25. Section 5. It would be useful to relate the findings and discussion in this section to the following studies:

   a. **Pennings, J.M.E. and A. Smidts (2003), "The Shape of Utility Functions & Organizational Behavior," *Management Science* 49 (9/September): 1251-1263.**

   b. **Pennings, J.M.E. and B. Wansink (2004), "Channel Contract Behavior: the Role of Risk Attitudes, Risk Perceptions, and Channel Members' Market Structures," *Journal of Business* 77 4(October): 697-723.**

   c. **These studies show a complex interaction between risk and organizational behavior (i.e., operating in a spot market or using production contracts) and suggest that there may be some endogeneity in measured risk preferences across alternative marketing arrangements.**

RTI Response: Thank you for bringing this literature to our attention. We related our findings to this literature; see footnote 6 in Section 5.

26. Section 6. This section presents welfare results from a ban on AMAs given a simple supply and demand model. It would be useful to tie the statements of welfare changes in with the findings from the previous analyses. For example, a preceding section informed us that consumers are likely to lose if AMAs were banned because quality would decline. Further, another section indicated producer welfare would decline because they would face higher price risk. Are the welfare losses reported in these previous sections in addition to those reported in this section?

RTI Response: The simulation analysis section accounts for changes in quality resulting from changes in the composition of pork produced and consumed (see response to Question 4). The effects of increased risk are not included in that section. However, all these effects are brought together in Section 7 and the Executive Summary for the report volume.

27. Page 7.6. The sentence in bold at the end of the page is long and confusing. A final "overall" comment. This volume reports numerous econometric models estimated from several different data sources, but often the underlying conceptual model is the same. For example, several sections report estimates of a cost function. As another example, several sections report estimates of derived demand or supply for pork. Further, there are several "ad-hoc" models where price is the dependent variable. Asking for a fully integrative analysis is perhaps unrealistic given the variety of questions being asked. However, it seems there might be some room for consolidating some of the repetitive modeling efforts—fitting the underlying conceptual model using the best available data. At a minimum it would be useful to, perhaps in an appendix, compare all the econometric estimates (or elasticities) that pertain to the same underlying conceptual model, but where the estimates are obtained from different data sources or different functional forms.

RTI Response: We agree that the report contains a substantial number of modeling approaches. These multiple approaches are needed to address the requirements of the study. We also used multiple approaches for some questions in order to compare results using different data sources, because each data source has its limitations. Because of the time constraints on the study, we were not able to prepare an additional appendix that compares all of the models. However, we do integrate the results of the analyses in Section 7 and the Executive Summary of the report volume.

REVIEW OF GIPSA LIVESTOCK AND MEAT MARKETING STUDY
VOLUME 5: LAMB AND LAMB MEAT INDUSTRIES; FIRST DRAFT FINAL REPORT
PREPARED BY RTI INTERNATIONAL IN NOVEMBER 2006
REVIEW SUBMITTED 21 DECEMBER 2006

Overall, I found "Volume 5: Lamb and Lamb Meat Industries" to be very complete, accurate, and well written. The authors did a very good job, especially considering the lack of data.

RTI Response: We appreciate the positive feedback.

I do have a few comments/questions regarding the study.

Page 1-8, second paragraph: Have packers increased the amount of meat produced per animal slaughtered or have sheep producers contributed. If liveweight increased 9 pounds and carcass weights increased 4 pounds, with a dressing percentage of 60-70%, much of the increase comes from producers.

RTI Response: We agree. The sentence has been rewritten. Most of the increase in the amount of meat produced per animal slaughtered has been the result of genetic changes, feed management, and health improvements. Some also may have come from improved processing technologies.

Page 2-3: It would be interesting to include an increased reporting of the use of AMAs by the packer size.

RTI Response: We agree. However, MPR data do not include information about the use of AMAs by packer size. In addition, the lamb packer survey did not include enough responses to allow for the disclosure of this issue by packer size. Nonetheless, it does appear that larger packers tended to use AMAs more heavily than smaller packers.

Page 2-8: Any thoughts as to why the opposite trends with formula and auction procurements as reported by the MPR data?

RTI Response: There are three possible explanations. First, it could be that a variety of market factors (e.g., consolidation, changing demands, logistical issues) have caused this to occur. That is, the relative benefits versus costs of using AMAs were altered during the sample period. Second, it could be that packers used proprietary information when designing contractual arrangements. Once this information became transparent, packers may have lost their individual competitive advantages in lamb procurement through the use of AMAs. Hence, they reduced the amount of this activity. Third, it could be that packers were manipulating prices through the use of AMAs, and that increased price transparency caused them to change their practices and return to using auctions.

Our research efforts are unable to distinguish among these possible alternatives. However, the survey data indicated that packers were not likely to radically alter their procurement methods in the future. Hence, it could be that packers were adjusting their procurement methods toward an equilibrium during the sample period.

We have addressed this issue in the revision at the end of Section 2.1.2.

Table 2-10: The maximum for the Cash Procurement column should be 59.78 not 29.78.

RTI Response: We have corrected this typographical error in Table 2-3.

Page 2-17, 18: It seems very inconsistent to drop the second quarter seasonality dummy variable (S_2) here when the other variables in the model that were insignificant were not dropped, and S_2 remained in several of the other equations throughout the study when it also was not significant.

RTI Response: We agree, but were forced to selectively parse the seasonality variables because of sample size issues. That is, our relatively small sample size contributed to coefficient estimates that were sometimes unstable (in addition to occasionally not meeting a priori sign expectations) when seasonal variables were included. Nonetheless, we know that seasonality is a factor that should be considered in such research. Also, the lack of degrees of freedom caused us to use quarterly rather than monthly seasonal binary variables. Regression results were more sensitive to the inclusion/exclusion of the second quarterly binary variable than to any of the others. Perhaps this is because of the large seasonal effect on the lamb market of the Easter and Passover seasons.

Our final decision on whether to include/exclude the second quarterly binary variable when it was not statistically significant generally hinged upon the sample size and equation dynamics. For example, the estimated coefficients for the lamb slaughter price difference model reported in Table 2-6 were much more stable when the second quarterly binary variable was excluded from the specification.

In every case, those seasonal binary variables that were not significantly different from zero were retained in final specifications, as long as their inclusion did not generate parameter instability.

Section 2.4.1, first paragraph: Should "lag orders may not be equal" read "lag orders need not be equal"?

RTI Response: We agree and have made this change.

Equation 2.7: Should seasonality variables be included in this "purely stochastic" specification? My guess would be that the high adjusted R-square obtained for this equation was largely made up of the variation explained by the seasonality variables, which have an economic interpretation.

RTI Response: The purpose was to measure a purely stochastic process through the AR (lagged dependent variable) and MA (systematic error) processes "net" of seasonal influences in the lamb market. If seasonality were excluded from the ARMA process, resulting parameter estimates may include seasonal factors and influence the stability of the model. In addition, the idea was to compare a stochastic model with the economic model previously estimated. The economic model included seasonal binary variables; hence, it seems appropriate to include seasonal effects in the stochastic model for comparative purposes. Nonetheless, we reestimated the stochastic model after excluding the seasonal binary variables to ascertain their contribution

to how the model fit the data. The adjusted R-squared statistic in this case was 0.633, which is only slightly smaller than the adjusted R-squared statistic of 0.643 that was obtained with the seasonal variables included in the specification. Thus, the majority of the explanatory power of the stochastic model was embedded in the AR and MA terms.

Section 2.5, first paragraph: Should "We estimated" read "We estimate", to be consistent with the other verb tenses in this paragraph?

RTI Response: We agree and have made this change.

Page 2-26: Because of the lack of data, the authors had to use many proxies in place of the theorized variables whose data were unavailable. A good case-in-point was using volume shares of each marketing alternative as proxies for procurement costs associated with each AMA. There is little discussion regarding the justification of the many substitutions throughout the study; how the equations should be interpreted in light of these proxies, any cautions or limitations of the results because of these proxies. Because so much of the data had to be proxied, the study would be stronger if the authors spend more time on this. At least, I would like the authors to discuss some of these limitations in Chapter 7, where the results are summarized with little mention of the data limitations.

Another example is the use of residuals to represent the maximum market power…in Section 2.5.6, the authors could add to their concluding remark "However, the economic effect of this buying power in all cases was quite small" ", especially given that the measure used to represent market power likely contained information beyond that attributable to market power effects."

RTI Response: The use of proxies for unmeasured variables is not unusual in empirical econometric work. Excluding such variables can result in specification bias and, hence, biased and inconsistent parameter estimates. However, the use of proxies requires caution because measurement error bias can be generated. In principle, proxies should have a close functional relationship to unmeasured variables so that measurement error bias is minimized and consistent estimates can be obtained. Our use of proxies was necessitated by data limitations. Although data limitations exist to some degree in all empirical work, these limitations were particularly acute for the lamb industry. In all cases, however, proxies were carefully chosen based on both pragmatic and theoretical reasoning. The following presents our use of proxies in each empirical model of the lamb study.

SECTION 2.3. MONTHLY LAMB PRICE DIFFERENCE MODEL

The independent variable, *FCRP* in the lamb price difference model (Eq. [2.1]), represents the real formula carcass lamb price minus the real spot lamb carcass price. Ideally, the spot price should include negotiated carcass prices. However, data for negotiated lamb carcass prices are not available. Therefore, the spot (cash) price for lamb carcasses was used in the place of negotiated carcass lamb prices in the equation, as reported in Table 2-6. As a measure of appropriateness, we note that spot and negotiated live lamb prices are highly correlated. Therefore, one might expect similar correlations between spot and negotiated lamb carcass prices.

SECTION 2.5.1. MONTHLY STRUCTURAL LAMB AND SHEEP PRICE MODEL

AMA procurement cost data for formula procurement (*pf*), packer ownership (*po*), and cash procurement (*pc*) were not available. Procurement costs are necessary to estimate the impacts of changes in AMA usage on wholesale lamb demand and, subsequently, on derived slaughter lamb demand. Therefore, reported monthly MPR AMA volume shares were used as a proxy for procurement costs associated with each AMA. The empirical results tend to support the use of this proxy. That is, Tables 2-9 and 2-10 show that an increase in formula procurement volume share (*pf*) (i.e., a decrease in formula procurement costs) shifts wholesale lamb supply to the right and reduces wholesale lamb price. Conversely, a decrease in formula procurement volume share (i.e., an increase in formula procurement costs) shifts the derived slaughter lamb demand to the left and increases slaughter lamb price. Thus, the empirical results support the use of AMA volume shares as a proxy for AMA procurement costs.

Food marketing costs (*mc*) in Eqs. (2.9) and (2.12) were not reported on a monthly basis. Therefore, monthly lamb processing costs were used as proxies for this variable. For the entire red meat industry, the food marketing cost index reported by USDA is highly correlated with red meat processing costs. Hence, we expect a similar relationship exists in the lamb industry.

Data for boxed mutton price in Eq. (2.20) were not available. Therefore, the price of boxed lamb was used as a proxy. In the beef sector, the price of boxed beef obtained from fed steers and heifers is highly correlated with the price of boxed beef obtained from slaughter cows. We expect a similar relationship exists between boxed lamb and boxed mutton prices.

We use the "residuals" from Eq. (2.34) as an estimate of the degree of oligopsony power in the slaughter lamb sector. Both structure/conduct/performance models and New Empirical Industrial Organization (NEIO) models have been used to estimate oligopsony market power in the beef industry. These models have not been applied to the lamb industry. Once again, data limitations prohibited us from using such models to estimate the potential degree of oligopsony power. The residuals from Eq. (2.34) are used as the dependent variable (m_k) in Eq. (2.33). This measure represents the largest effect of market power possible, because data are not available for a variety of factors that could reduce the size of this residual in Eq. (2.33). Nonetheless, our estimates of the economic effects of market power in Table 2-11 are quite small.

SECTION 3.1. PROCUREMENT COST MODEL

The specification in Eq. (3.1) is a substitute for estimating a formal cost function for the lamb packing industry. Generally, a dual cost function would be estimated using plant-level data. Again, we do not have access to such data. However, our more aggregate approach is related to the theoretical components of a formal cost function. Our dependent variable in Eq. (3.1) was not directly observable. Therefore, we calculated a proxy by subtracting lamb slaughter costs from the farm–wholesale marketing margin and explain that this represents an upper bound on lamb procurement costs (*C*), as noted on page 3-2. Consequently, this proxy implies that the parameter estimates in Eq. (3.3) also represent upper bounds. Any errors associated with the calculation of *C* are manifest in the error term associated with the estimated equation, such that our parameter estimates are unbiased.

SECTIONS 4.1–4.3. LAMB QUALITY MODEL

The lamb quality model specified in Eq. (4.1) quantifies the relationship between fed slaughter lamb quality and AMA procurement methods. We use a weighted average yield grade variable to measure slaughter lamb quality. A consistent series of lamb quality grade data does not exist. However, yield grade is related to quality, based on yield price grids. We have included this discussion in the revision.

Measurement errors associated with our constructed lamb yield grade variable will be manifest in the error term of Eq. (4.1). We model this error term as an AR(2) process. Almost all measures of technological change represent some type of proxy. In general, trend is often used as a proxy. In initial regressions, we found that trend was highly collinear with a number of our independent variables. Hence, we used lagged average liveweight in Eqs. (4.1) and (4.2) as a proxy for technological change (*Tech*) in the lamb production sector. (In the original manuscript, this variable was incorrectly identified as a linear time trend.) Changes in genetics, animal health, animal nutrition, management, and marketing technologies are likely manifest in larger live animal weights.

SECTION 6.2.4. ANNUAL LAMB ECONOMETRIC MODEL

An annual econometric model of the lamb industry was estimated to obtain demand and supply elasticity estimates for the A matrix in the equilibrium displacement model (Eq. [6.41]). The model was estimated using annual data from 1970 to 2003. Some of the variables had missing observations for some years. Other variables were not observable, and proxies were used in their stead. Some of the data were incomplete. Specifically, most missing observations were associated with domestic and imported retail lamb prices and imported wholesale lamb prices. These issues are explained on pages 6-31 and 6-32. Also, proxies were used for slaughter lamb by-product values, lamb cut-out values, and lamb technology.

Slaughter lamb by-product values (P_{bp}) are specified in the domestic wholesale lamb supply and domestic slaughter lamb demand (Eqs. [6.55] and [6.62], respectively). The price of shorn wool was used as a proxy for by-product value because it is one of the critical by-products of lamb production.

Sporadic observations on lamb cut-out values were available. These observations were highly correlated with carcass lamb prices. Hence, lamb carcass prices were used a proxy for lamb cut-out values in the domestic retail lamb supply Eq. (6.47), the domestic wholesale lamb demand and supply Eqs. (6.54 and 6.55), and the domestic slaughter lamb demand Eq. (6.62).

In all production sectors of the vertical lamb marketing chain, trend was used as a proxy for technological change. This variable should capture technological change that trends over time (e.g., genetic improvements, feed efficiency, processing technology) relevant to production dynamics in the farm-to-retail lamb sectors. Trend is often used as a proxy for technological change in econometric models of supply response.

Chapter 3, first sentence: I think something is missing from this sentence. It is not understandable in its present form.

RTI Response: The acronym "AMAs" should have been included between "that" and "help packers." This has been corrected in this revision.

Page 4-4: In the explanation of the variables included in equation 4.1, the authors state that Technology is measured by a one-period lag on average liveweight of lamb. Later, on Page 4-6, they state that technology was measured by a time trend in the empirical model. I believe a time trend (year) would be the best proxy in this case. Throughout the study, the authors state that a time trend was used, without specifically stating what that trend was. I assumed it was just a yearly time trend, but perhaps my assumption was not justified? Do the authors need to further explain what each time trend consisted of?

RTI Response: Eq. (4.1) uses the lagged average liveweight of slaughter lambs ($Tech_t$) as a proxy for technological change. The first sentence of the fourth paragraph on page 4-6 was not correctly stated and has been changed to read "…, as measured by lagged lamb average liveweight…." In preliminary models, a linear trend variable was used as a proxy for technological change. However, its use caused a high degree of multicollinearity in this particular model. Therefore, we used lagged average liveweight as a proxy in that model.

However, a linear time trend was used as a proxy for technological change throughout the annual econometric model. The use of a time trend as a proxy for technological change is common in empirical analyses of vertically related marketing chains in agriculture, and its use did not cause associated collinearity problems in the annual model.

A linear time trend was also used as proxy for technological change in the market power model (Eq. [2.34]).

Table 6-1. Was the hay variable 'all hay', 'grass hay' or 'alfalfa hay'?

RTI Response: The hay variable is actually "all hay." The change has been made in this revision.

My biggest concern regarding this study is the use of Yield Grade as a proxy for Quality at the retail level. As the author's state, there is a general inverse relationship between the two. To state that restrictions on the use of AMAs result in lamb meat quality declining with a resulting reduction in demand of domestic lamb is a stretch based upon this proxy. Most lambs (85%) grade choice right off their mothers. An increase in yield grade does not reduce quality, especially given that most eastern U.S. markets prefer the fatter lambs. At the most, the impact is that packers have to trim fat off. This assumption needs to be flushed out more and highlighted in Chapter 7.

RTI Response: As you note, most lamb has a quality grade of Choice. In fact, the MPR data does not report any lamb quality grade information. However, yield grades are reported. The primary concern of the reviewer is whether yield grade is a proxy for quality. We believe that an increase in yield grade number is an indication of a reduction in lamb quality for a number of reasons. First, the Mountain States Lamb Cooperative (which markets about 25% of domestic

lamb sales) uses a quality grid based upon yield grade. Yield #5 and Yield #4 receive $0.30/lb and $0.08/lb discounts, respectively. Yield #2 and Yield #3 both receive $0.08/lb premiums. However, Yield #1 receives no premiums or discounts. Hence, a Yield #1 appears to be of inferior quality with respect to Yields #2 and #3, but superior to Yields #4 and #5. So, in general, we believe that our use of yields as a proxy for lamb quality is reasonable. Our premise that lower yield grade numbers are consistent with higher lamb quality seems appropriate for Yield Grades #2 through #5. However, Yield Grade #1 is not of superior quality to Yield Grades #2 and #3 (although it is of superior quality compared with Yield Grades #4 and #5). Therefore, we may be biasing our measure of quality upward somewhat. However, our data indicate that Yield Grade #1 lambs represented only about 5% of total lamb slaughter in 2004. Hence, the upward bias inherent in our procedure should be quite small.

We believe that this issue is very important. Therefore, it has been addressed in the first paragraph of Section 4.1.

**REVIEW OF GIPSA LIVESTOCK AND MEAT MARKETING STUDY
VOLUME 6: MEAT DISTRIBUTION AND SALES; FIRST DRAFT FINAL REPORT
PREPARED BY RTI INTERNATIONAL IN NOVEMBER 2006
REVIEW SUBMITTED 28 DECEMBER 2006**

"MAJOR" COMMENTS

1. The authors of the report are to be congratulated for getting thus far in what is an immense undertaking. The type of information collected is unique and will be insightful for those interested in marketing arrangements in the meat industries. What follows are a number of comments to help improve the final report.

RTI Response: We appreciate the positive feedback.

2. The first part of this volume that I read (and the first and only part that may be read by many) is the Executive Summary. Unfortunately, it was very difficult to get through and it is far from a "stand alone" document as one might hope it to be. Much of my concern with the executive summary is symptomatic of my overall concerns thus, some of my comments are somewhat repetitious. What follows are some "difficulties" of getting through the executive summary:

　　a.　**the sentence leading into the bullets reads (pg. ES-1), "Primary conclusions for this final report as they relate to the fed cattle and beef industries are as follow:" However, the summary goes on to say little about fed cattle and a lot about pork in addition to beef.**

RTI Response: Correction made.

　　b.　**The 1st bullet seems to undermine the entire report by casting doubt on the reliability of the data sources; at a minimum some explanation or qualifiers need to be placed here to describe differences in survey and transaction data**

RTI Response: We reworded the bullet to explain that the difference in results is due to the differences in the sample for the survey, which represents a broad range of plant sizes, and the transactions data collection, which represents large plants.

　　c.　**More on this latter—but what is a processor? Is it a packer? Is it only a "stand alone" processor?**

RTI Response: A processor is a company or establishment that processes meat products but does not slaughter animals. We clarified this in the text.

　　d.　**Third and fourth bullets: are these figures % of pounds or % of transactions?**

RTI Response: We have clarified in the text which figures are percentage of pounds and which are percentage of transactions.

e. **2nd bullet on page ES-3: it is stated that meat processors play an important distribution(al?) role in the meat value chain, but what does this mean? What *share* of meat passes through a processor?**

RTI Response: We added words to explain why meat processors play an important distribution role in the meat value chain. Specifically, meat processors buy relatively few large lots from a few packers and sell relatively more small lots to many buyers. From the beef packer sales data, we know that approximately 15% of beef packer sales pounds are to meat processors, and from the pork packer sales data, we know that approximately 21% of pork packer sales pounds are to meat processors. We revised the text to indicate this.

f. **3rd bullet on page ES-3: discussion abruptly shifts from purchases and sales of *meat* to purchases of *cattle*.**

RTI Response: We revised the text to make it clear that the last three bullets are further analyses for processing and distribution of beef products. We conducted this additional analysis on beef because the beef industry offers a measure of quality (USDA Quality Grades) that is used in both cattle purchases and beef sales. Pork industry value hog purchases by lean percentage and carcass weight, which have little bearing on consumer purchases (most product is closely trimmed). Furthermore, the beef industry primarily uses USDA Quality Grades to differentiate cattle purchases and branded beef products sold (i.e., Certified Angus Beef). The beef industry also has more variety (spot and AMA) in purchases and sales than does the pork industry. We also added two additional bullets on pork and meat as a whole to balance the summary.

3. A substantive confusion I had while reading this volume was the precise definition of a processor and whether the data reported in this volume derives from both packers *and* stand-along processors; sometimes it seems one way and other times it seems another. Figure 1-1 is a very useful schematic, but we don't have any idea what the prominent chain is for most meat. Does most meat travel from a packer to a processor or does most move from the packer-processor (bypassing the "stand alone" processor) to the wholesaler or retail? It would be helpful to have some differentiation on this matter. I suppose this is somewhat captured by the "internal transfers" data, but still it is hard to know whether the data being reported is representative of most meat sales (more on this in the next point). Would it be possible to indicate the # of plants or # of transactions that are packer-owned or that are the packer itself?

RTI Response: We apologize for the confusion regarding processors versus packers. As mentioned above, processors process meat products but do not slaughter animals; we have clarified this in the text. However, some of the analyses presented in this volume are based on data from beef packers so that we could further examine the possible relationships between output from the packers and the purchases of livestock. We have added to the report the estimated percentages of sales transactions from beef packers and pork packers to meat processors or food manufacturers, based on calculations from the transactions data. Internal transfers are not a good indicator of transfers from a packer to a processor because these transactions could represent transfers to retail or food service if also owned by the same

company that owns that packer. Thus, we do not include information on packer owned sales transactions within the report.

4. Throughout I kept wondering about the representativeness of the sample of surveyed firms and of transactions. It appears some sampling plan was used. As such, it should be straightforward to report statistics associated with sampling error—i.e., we can be 95 confident the true mean is with +/- units of the reported mean. This is somewhat problematic given the apparent drastic differences in survey and transaction data. Can the data be weighted such that it matches some known population statistics? Is sample-selection bias an issue? If so, can we place any trust in the data being reported? Which data is the most reliable indicator of the population, the survey or the transaction data? Why are both reported? Are both included to test for robustness of results? Fortunately, some of these issues were discussed on page 2-15, but I suggest moving some of this discussion earlier. For example, the reader should be given some clue as to why both types of data are important, which are expected to be more reliable, etc. One would typically expect a "methods" section before the results describing some of this—i.e., # of firms surveyed, advantages/disadvantages of differing data collection methods, etc.—such that the results can be read in context.

RTI Response: The details of the sampling plan and the 95% confidence intervals for all of the survey results are presented in *Volume 2. Data Collection Methods and Results*, and thus we did not repeat them in this volume. The results are all weighted using standard survey weighting procedures. The issue that arises between the two sources of data is that the survey represents a broader range of company sizes, including many smaller operations, but the transactions data, for practical purposes, were only collected from large plants. Although the transactions data are obtained from only the large plants, the volume represented from these plants represents a very high (but unknown) percentage of all sales in the industry. In a sense, the survey data provide a better representation of what is done across the range of company sizes, while the transactions data provide a better representation of what is done for the vast majority of product volume. We have added additional explanation in the text regarding this distinction.

5. It is unclear why the authors report in several places that the analysis and statistics reported in this volume are "qualitative" (e.g., page 1-2). Virtually everything reported is *quantitative* and the types of inferences being drawn and statistics being reported are what one would expect of a *quantitative* study. One widely accepted description of qualitative research is as follows: "Unlike quantitative research, qualitative research relies on reasons behind various aspects of behaviour. Simply put, it investigates the why and how of decision making, as compared to what, where, and when of quantitative research." This volume does very little in terms of answering *why* but tries to provide a lot of answers to what, where, and when. Please clarify.

RTI Response: We agree that the terminology was incorrect. We changed "qualitative" to "descriptive" throughout.

6. The units of reporting are inconsistent throughout the volume. Sometimes the statistics are reported as the % of plants, sometimes the % of pounds, sometimes the % of sales, sometime the % of transactions, sometimes some combination of these, sometimes units are not indicated. What is the most relevant statistic? Which statistic is most relevant in answering which questions?

RTI Response: Each of these types of percentages may be relevant for some purposes or of interest to some readers, so we included percentages in terms of plants and pounds for the survey questions and percentages in terms of transactions and pounds for the transactions data. We reviewed the text and made changes to ensure that it is always noted how the percentages were calculated. In general, many of the survey questions ask about plant practices and thus percentages relative to the number of plants were most relevant. However, we also asked respondents to estimate percentages of pounds in some cases. For the transactions data, the detailed data allowed us to provide results at a more disaggregate level.

7. On page 2-1 the lead paragraph mentioned that the volume will report on "volume, quality, and *price* differences . . ." (emphasis added). Unfortunately, there is no analysis of price differences in this volume. I do not know if this is outside the purview of project, but some discussion of price differences would seem warranted.

RTI Response: We revised the text to remove price differences from the lead paragraph of this section. We conducted detailed analyses of prices using packer purchase data in the other report volumes. We did not conduct analyses of price differences using the packer sales data or the meat processor data because of the heterogeneous products sold and because this analysis would not directly address the questions we were asked to analyze as part of the study. Upon examining the data, we found that prices differed primarily by the type of product being sold and that it would have been difficult to disentangle the effect of the product attributes versus the type of marketing arrangement used when considering the results of the analysis.

8. Many of the tables (starting with table 2.3) have a large (often the largest) category of "other and missing" data. Would it be possible to separately report statistics for "other" from "missing?" The difference could be important.

RTI Response: We agree that the difference could potentially have been important. However, we did not separate the data by "other" and "missing" because we believe many of the missing data are essentially the same response as "other." When respondents could not classify the transactions based on the categories listed, they may either have specifically indicated "other" or have left the field blank. Thus, providing separate summaries for "other" and "missing" might have been misleading.

MINOR COMMENTS

1. page 1-4. Is there really an inverse relationship between price and quantity over the entire period? Both seem to roughly trend downward over the time period. What is the correlation coefficient?

RTI Response: Although both prices and quantities have trended downward over the time period, the inverse relationship does still exist. In other words, the demand curve is still downward sloping. Section 6 of *Volume 3. Fed Cattle and Beef Industries* provides a more detailed analysis of demand for beef products included the estimated coefficients of the demand curve. We have revised the text on page 1-4 to make the explanation clearer.

2. page 1-4. The "box" on the left hand side of the page is confusing as a stand-alone statement—why is this statement so noteworthy that it deserves a "box?"

RTI Response: We agree and have removed the box.

3. page 1-5. Have pork consumption levels really been "much less variable?" The units on the graphs differ so it is hard to say? How do the standard deviations compare?

RTI Response: We agree that this conclusion is not apparent based on the graphs and have deleted the sentence.

4. Page 1-6. Same comment for pork as 1 above for beef. Even if there is an "inverse" relationship why does it matter? Wouldn't be more prudent to say something about supply and demand? For example, is it simply that supply is relatively inelastic in the short run, so most of what is being observed in the price swings are movements along the demand curve?

RTI Response: We agree that this was confusing and revised the sentence.

5. page 1-10. The "2004 National Meat Case Study" is not cited in the reference list.

RTI Response: We added the reference.

6. pages 1-4 through 1-13. What is the purpose of this information? Most of the information in these sub-sections is easily obtained from USDA-ERS publications available on the web. What one would expect to find in this volume is a brief synthesis of this information and a discussion of *how it relates to alternative marketing arrangements*.

RTI Response: The intent of this section was to provide background useful to the reader prior to discussing the analysis of the effect of AMAs. We agree that much of this information is available in other publications, but we provide it here as a convenience to the reader. We added a sentence at the beginning of Section 1.2 to help ease the transition.

7. Pages 1-16 and 1-17. Apparently most of the meat processor data (in terms of pounds) was pork, but most the meat processor sales data (in terms of records) was beef. Is this due to differences in reporting (pounds vs. records) or something else (e.g., pork is more heavily processed with other non-meat ingredients)? Some discussion here is warranted.

RTI Response: As you indicate, the difference is due to differences in reporting. Plants were not asked to provide data for products that were less than 50% meat. We added an explanation in the text. Fewer plants that process pork products sold products that were at least 50% meat, and thus we obtained more data on sales of beef products.

8. Page 2-2. Is all of the information from the survey in terms of the % of plants? Can anything be said about the % of pounds?

RTI Response: Many of the survey responses were about practices at the plant and thus the data are summarized with respect to the percentage of plants. We did not ask the respondents to provide percentage of pounds for all of the survey questions because of burden on the respondent and because we also would be obtaining detailed transactions data that are in terms of pounds.

9. Page 2-2 and 2-3. What is "flat pricing?"

RTI Response: Under flat pricing, buyers and sellers agree to a specific dollar per pound for a specified time period. We added this definition to the report. It also appears in the glossary that was prepared as a separate document for the entire report.

10. Page 2-3. What is "floor-and-ceiling" pricing?

RTI Response: Under floor-and-ceiling pricing, agreed upon purchase (sales) price increases and decreases with market prices, but the price has a lower limit and an upper limit for a specified time period. We added this definition to the report. It also appears in the glossary that was prepared as a separate document for the entire report.

11. Page 2-17. Section 2.2. This section seems to imply all processors are packers as we shift discussion to the purchases of cattle rather than meat. Some explanation here is needed in term of the where this data is coming from and the % of the sample being used in this analysis.

RTI Response: We apologize for the confusion. In addition to the analysis of meat processor data (covering both beef and pork), we conducted more specific analyses using beef packer data to examine the relationship between sales transactions characteristics and livestock purchases. We revised the text to make this clearer.

12. Page 2-17. Why categorize plants into two broad categories (0-20% branded and 21-40% branded)? Why not simply run a regression or calculate the correlation coefficient between % spot and % branded?

RTI Response: The categorization is needed because it allows for a way to compare sales data with purchase data. Each is a separate data set, and thus we had to determine a method of matching up the two different data sets. The regression you suggest could have been conducted if

the data indicated which sales transactions were associated with each purchase transaction, but the data are not maintained this way in the industry.

13. Page 2-20. Little is said about *why* one does not observe the expected relationship between rating of branding and use of AMAs. Is it simply because the packers are so large that they can just sort cattle to get enough into branded programs without having to make use of AMAs?

RTI Response: We add two paragraphs of possible explanation for the weak relationship between cattle purchases and beef sales programs using AMAs. We agree that the size of branded beef programs relative to the total is one possible explanation. We also note that most branded programs use USDA Quality Grades and other visual measures as specifications. Until credence attributes become more prominent in branded programs, AMAs are not a necessary condition for branding. While not apparent in the aggregate cross-tab analysis, individual firms may use AMAs to help manage the distribution of cattle they purchase.