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UNITED STATES DISTRICT COURT
DISTRICT OF MINNESOTA

IN RE CATTLE AND BEEF ANTITRUST LITIGATION This Document Relates to: *CONSUMER INDIRECT PURCHASER PLAINTIFFS' ACTION*	No. 0:22-md-03031 (JRT/JFD)

EXPERT REPLY REPORT
of
Russell W. Mangum III, Ph.D.
Regarding Class Certification

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I. Introduction

I.A. My Education, Experience, and Qualifications

1. I am the Executive Vice President at Cirque Analytics. Cirque Analytics is an economic consulting firm that provides economic, financial, and statistical research and analysis to private and public sector clients.

2. I hold a Ph.D. and a M.A. in economics from the University of Southern California, and a B.A. in economics, with honors, from California State University, Fullerton. I have been using econometrics and economic analysis to evaluate and model the effects of anticompetitive behavior for over 25 years. From 1995 to 1998, I served as a staff economist at the United States Federal Trade Commission, in the Antitrust Division of the Bureau of Economics. While at the FTC, I conducted economic investigations into proposed mergers and other business practices with potentially anticompetitive effects, including investigations into coordinated interactions, boycotts, and price fixing. From 1998 through 2001, I was an economist at Nathan Associates (my first tenure at Nathan Associates), where I served as a consulting and testifying economist on various litigation assignments, including antitrust and class action antitrust matters. From 2001 through 2007, I was an economist at PricewaterhouseCoopers and Analysis Group Inc., where I acted as a consulting and testifying economist on various litigation matters, including those related to commercial disputes, alleged intellectual property infringement, and alleged antitrust violations. From 2007 to 2021, I served as an economist and firm officer at Nathan Associates (my second tenure at Nathan Associates), where I acted as a consulting and testifying economist on various litigation matters, including those related to alleged antitrust violations, commercial disputes, and alleged intellectual property infringement. In April 2021, I moved into my current role at Cirque Analytics.

3. I am or have been a member of several professional associations, including the American Economic Association, the Intellectual Property Law Association, and the American Bar Association, and I have served as the Chair of the Orange County chapter of the Licensing Executives Society. I have taught courses in undergraduate and graduate economics and econometrics at Johns Hopkins University, the University of Southern California, Pepperdine

University, and recently retired as a Professor in the School of Business and Economics at Concordia University Irvine.

4. My experience in economic analysis includes evaluation of the potentially anticompetitive effects of business conduct, including the measurement of the effects of such conduct. In conducting such analyses, I have often analyzed numerous relevant markets (both geographic and product markets), barriers to entry, market power, and monopolization. My experience in such matters includes numerous class actions, in which I have analyzed and assessed commonality of impact and whether methods I have used databases related to purchase ˙transactions, invoices, and company financial performance to specify econometric models of pricing and costs, and to estimate the impacts of business combinations and cartel behavior on both direct and indirect purchasers.

5. Cirque Analytics currently charges its usual and customary rate of $870 per hour for my work in this matter. Professional staff members employed by Cirque Analytics also assist me. Neither my compensation nor that of Cirque Analytics is contingent upon the outcome of this case.

6. My curriculum vitae is attached to this declaration as **Appendix A**. Also included in **Appendix A** is a list of the matters in which I have testified in the past four years, along with a list of my publications for at least the past ten years.

I.B. Assignment

7. On September 25, 2024 counsel for the Consumer IPP class submitted my expert report ("Mangum Class Report"[1]) in this case regarding my analysis related to the Consumer IPPs' motion for class certification. On January 2, 2025, I gave deposition testimony regarding my opinions in the Mangum Class Report.[2] On January 24, 2025, Dr. Lauren Stiroh, Ph.D. submitted a report ("Stiroh Report") on behalf of Defendants.[3] In her report, Dr. Stiroh responds to certain of the opinions and analyses contained in the Mangum Class Report, as well as certain of the opinions and analyses contained in the expert reports of Dr. Michael Williams, Ph.D. and Dr. David Sunding, Ph.D. Dr. Sunding and Dr. Williams had previously

[1] Corrected Expert Report of Russell W. Mangum, Ph.D., Sept. 25, 2024 ("Mangum Class Report").
[2] Deposition of Russell W. Mangum, Ph.D., Jan. 2, 2025.
[3] Expert Report of Lauren J. Stiroh, Ph.D., Jan. 24, 2025 ("Stiroh Report").

submitted reports on behalf of different classes of plaintiffs in this matter.[4] Dr. Stiroh subsequently gave her deposition testimony in this matter.[5] Counsel for Consumer IPPs has asked me to review the Stiroh Report and Dr. Stiroh's testimony, and, where appropriate, respond to her opinions and analysis.

8. Dr. Stiroh has responded to my report, alongside those of Dr. Williams (representing the commercial indirect purchaser class), and Dr. Sunding (representing the direct purchaser class) simultaneously. At times, Dr. Stiroh does not clearly indicate to which report she is responding, instead making arguments about "Plaintiffs' experts" generally. I respond to the arguments addressed to my report, and those which arguably touch upon my report. But to the extent that any statements or exhibits in Dr. Stiroh's report are not explicitly addressed in this reply report, it should not be construed as my agreeing with Dr. Stiroh's arguments.

I.C. Information and Materials Relied Upon

9. In preparing the Mangum Class Report, I described the various types and sources of information that I (and/or those working under my direction) relied on. To prepare this reply report, I again rely on those same materials and sources of information, but I also rely on information contained or referenced in the Stiroh Report, as well as the deposition testimony of Dr. Stiroh. Further, since preparing my original report, I have received additional data related to Defendants' and third parties' sales of beef, which was produced before January 7, 2025. A full list of the materials I considered and relied upon and considered in forming my opinions is provided in **Appendix B** and/or referenced elsewhere in this report. The conclusions reached in this report are based on the information and data that have been reviewed to date. In the event that additional data or information becomes available, whether through discovery or other sources, I reserve the right to update my analysis and conclusions appropriately.

[4] Expert Report of David L. Sunding, Ph.D., Sep. 25, 2024; Corrected Expert Report of Michael A. Williams, Ph.D., Sep. 25, 2024.

[5] Deposition of Dr. Lauren Stiroh, Mar. 19, 2025 ("Stiroh Deposition").

I.D. Case Background

10. In the Mangum Class Report, I discussed the Class Definition, including a description of the relevant beef products for purposes of my analysis.[6] I also provided a brief discussion of the parties to this case, including the class representatives and Defendants.[7] While I do not repeat those sections again in this report, I incorporate them by reference.

11. I also previously described my understanding of the alleged conspiracy in this matter and cited or discussed voluminous documents and materials that relate to those allegations.[8] I also incorporate those same materials by reference in this report.

I.E. Summary of Conclusions

12. In the Mangum Class Report, I concluded that the facts, evidence, methodologies, and analyses used to determine 1) whether Consumer IPP Class members paid artificially inflated prices for class beef products sold by Defendants and 2) the amount by which the prices paid by Consumer IPP Class members exceeded those that would have prevailed in the absence of the alleged cartel, are common to the Class.[9] Specifically, I concluded that:

I.E.1. The Structure of The Beef Processing Industry Was Conducive to the Formation and Success of the Alleged Conspiracy

13. First, Defendants dominated the market for beef. Defendants controlled the vast majority of beef processing capacity during the relevant time period. As such, Defendants represent both the demand curve for fed cattle and the supply curve for beef. Therefore, the Defendants collectively have market power in both the upstream and downstream markets.

14. Second, there are significant barriers to entry for new competitors, which facilitates the formation and maintenance of the alleged conspiracy. Due to the extremely high capital/financial, technological, and regulatory costs associated with operating a successful beef processing company, Defendants have been largely shielded from meaningful competition. High barriers to entry allow a cartel to earn supracompetitive prices for a

[6] Mangum Class Report, ¶¶ 8–10.
[7] Mangum Class Report, ¶¶ 11–16.
[8] Mangum Class Report, ¶¶ 17–19.
[9] Mangum Class Report, ¶ 24.

sustained period of time with less (or no) risk that new entrants will emerge and compete profits away. New entry is made more difficult due to the high degree of market share controlled by Defendants combined with the cattle life cycle, which makes obtaining a meaningful supply of cattle difficult and time-consuming.

15. Third, Defendants had ample opportunities to form an anticompetitive scheme. For example, Defendants are members of numerous trade organizations and industry groups that hold annual (or more frequent) meetings. Defendant executives often hold leadership positions in these organizations. Record evidence shows that Defendants frequently communicate with each other for a variety of reasons, including (potentially) legitimate business purposes, social engagements, and other events. These frequent events and interactions present ongoing opportunities to coordinate, monitor, and enforce an agreement. Evidence also shows that Defendants frequently communicated through industry analysts, commentators, and other third-parties, which would further enable them to signal, communicate, monitor, and enforce an agreement.

16. Fourth, Defendants possessed the ability to monitor and enforce an anticompetitive scheme. Through the combination of information sharing through third parties and public reporting of slaughter and pricing data through the USDA, Defendants were able to quickly and reliably monitor each other's output decisions. The transparency of each Defendant's output decisions through these sources enables each Defendant to closely monitor each other and enforce the alleged conspiracy. The Defendants also possessed the ability to punish each other for cheating on the alleged agreement by expanding output, aggressively bidding for cattle, or reducing prices for beef products. The Defendants' ability to monitor, enforce, and reach an anticompetitive agreement is enhanced by the commodity-like nature of beef products.

I.E.2. The Alleged Conspiracy in This Case Would Affect All or Nearly All Purchasers of Beef

17. First, due to their commodity-like nature, beef products are priced through common mechanisms such as USDA cutout values, which both enables Defendants to observe changes in prices that affect all customers, and also to monitor and punish cheating through the direct link between output expansion and pricing.

18. Second, the widespread use of USDA cutout values for pricing wholesale beef products means that prices respond quickly to changes in supply. Even when prices are not formulaically linked to specific USDA cutout values, they are nevertheless negotiated in reference to them, because these prices represent the market price of beef. Thus, beef sold under true fixed-price contracts or negotiated separately in the spot market would still be affected by the alleged conspiracy.

19. Third, consumers of beef would be largely limited to Defendants' products, because Defendants dominate the market and there are barriers to entry that prevent new competitors from materially disciplining Defendants.

20. Fourth, customers' limited ability to switch to non-Defendant beef would not shield them from the effects of the alleged conspiracy. This reality flows from the fact that beef products are commodity-like in nature and are priced through industry-wide mechanisms like the USDA cutout, which depends on total industry supply (which is dominated by Defendants). As such, the prices charged by non-Defendant beef packers would still be artificially inflated due to the alleged collusion, and consumers therefore cannot avoid beef impacted by the alleged conspiracy.

21. Fifth, beef exhibits relatively low demand elasticity, which means that consumers are relatively less likely to respond to higher prices for beef by switching to other foods. Accordingly, low demand elasticity makes charging supracompetitive prices for beef more feasible.

22. Sixth, statistical analysis of pricing demonstrates that prices paid for Defendants' beef products are closely related to each other. These relationships persist across Defendants, customers, and geographic regions. These findings further support the conclusion that consumers would not be able to avoid supracompetitive prices charged by Defendants, and therefore that all consumers would be affected by the alleged conspiracy.

I.E.3. My Econometric Regression Model Allows for A Rigorous and Reliable Analysis of Whether Prices for Beef Were Artificially Elevated During the Class Period as a Result of the Alleged Conspiracy

23. Multiple regression analysis is routinely used and accepted as a method for estimating impact and damages in antitrust cases like this matter. Data produced by Defendants, as well as publicly available data to control for lawful factors of supply and demand, allow for the

specification of an appropriate econometric model that can be applied on a class-wide basis to determine impact and calculate damages.

24. My econometric analysis demonstrates that prices paid by direct purchasers for beef products were artificially inflated during the Class Period as a result of the alleged conduct. I showed that this finding is robust over time, robust to an analysis of only those direct purchasers who sell into channels relevant to the Consumer IPP Class, and is robust to the use of industry-wide USDA data.

I.E.4. Overcharges Resulting from The Alleged Conspiracy and Paid by Direct Purchasers Would Be Passed Through to Consumer IPP Class Members.

25. Economic theory, literature, and case-specific evidence suggest that any overcharges paid by direct purchasers in this case would be passed through to consumers at the retail level. It is possible to utilize empirical methods, including regression analysis, to measure the degree, if any, of pass-through in this case. Using appropriate third-party sales and purchase data produced in this case, I specified an econometric regression model to demonstrate that direct purchasers consistently passed on cost increases, including the overcharge, to indirect purchasers, including Consumer IPP Class members.

I.E.5. My Econometric Models Produce Overcharge and Pass-Through Estimates That Can Be Utilized to Calculate Damages on a Class-Wide Basis.

26. The results of my econometric analysis demonstrate that direct purchasers paid an overcharge of [redacted] percent on purchases of beef during the Class Period. Similarly, my econometric analysis demonstrates that direct purchasers [redacted] passed on this overcharge to consumers downstream. That is, the effective pass-through rate was [redacted] For purposes of my analysis, I have capped the pass-through rate at 100 percent.

27. After calculating the relevant commerce, the estimated overcharge of [redacted] percent and a pass-through rate of 100 percent lead to total damages for the Consumer IPP Class of [redacted]

I.E.6. Dr. Stiroh's Arguments Do Not Alter My Conclusions Concerning Class Certification

28. After reviewing the Stiroh Report, I affirm the opinions and analysis contained in the Mangum Class Report. Additionally, I have reached the following conclusions based on the opinions, analysis, and evidence presented by Dr. Stiroh.

- None of the analyses in Dr. Stiroh's report contradict my finding that all or nearly all direct purchasers in the Consumer IPP supply chain were impacted.
    - Dr. Stiroh largely ignores the structural characteristics of the beef industry that make it conducive to a successful conspiracy and generally prevent individual customers from avoiding impact.
    - In general, Dr. Stiroh's opinions and analysis represent an attempt to shift the question of common impact into one of "identical impact." In doing so, Dr. Stiroh manufactures standards and tests that are not economically necessary, reasonable, or relevant.
    - Dr. Stiroh's opinions about pricing in the market for beef, including individualized negotiations and different pricing mechanisms, do not refute my conclusions about common impact, and, in fact, broadly support them.
        - Dr. Stiroh misleadingly attempts to portray routine market interactions between buyers and sellers as indications that some customers could avoid anticompetitive impact, when this is not the case.
        - Dr. Stiroh's arguments concerning individualized pricing are without merit. First, she fundamentally misunderstands and downplays the prevalence and importance of market pricing. Second, the specific examples she cites as evidence of extraordinary pricing do not support her opinion, as they are all exemplary of routine market index-based pricing.
- Dr. Stiroh's criticism of my direct overcharge regression model are limited and without merit. My overcharge model is correctly specified and includes the appropriate explanatory variables that control for changes in price that would have occurred absent the alleged conspiracy. Dr. Stiroh does not present any alternative models to control for any other factors which could explain the overcharges I find.

- Dr. Stiroh's econometric "testing" using customer subregressions is fundamentally and indisputably plagued by small-sample estimation issues. Her finding of "no impact" across a subset of individual customers is not a conclusion based on reliable and unbiased results, but rather a *predetermined* outcome due to her flawed modelling structure.
    - Dr. Stiroh's testing methodology is structurally incapable of detecting most overcharges of the magnitude that I have estimated. This is a reason why Dr. Stiroh purportedly finds more "unimpacted" customers based on my model, compared to Drs. Sunding and Williams (who both find higher overcharges). The implication of these fundamental flaws in her model is that Dr. Stiroh's customer subregressions based on my model will often find "no impact," *regardless of the existence of impact*.
    - Dr. Stiroh's customer subregressions suffer from low statistical power, which means that her results are unreliable and cannot be interpreted as a finding of non-impact.
    - Customers for whom Dr. Stiroh finds "no impact" do show impact when grouped together, demonstrating that Dr. Stiroh's customer subregression results are driven by small sample size, and not the existence of impact.
    - Dr. Stiroh's opinion that "untested" customers are likely to be uninjured rests on a superficial, irrelevant, and fundamentally misleading comparison of prices. More appropriate empirical analysis of these customers demonstrates that they are likely to be impacted in the same manner as other customers.
- Using evidence provided by my overcharge regression, Dr. Stiroh's own testing methodology can be used to demonstrate common impact.
- Economically reasonable "testing" of my regression model demonstrates the improper and irrelevant nature of Dr. Stiroh's customer-level overcharge estimation approach.
    - My overcharge estimates are robust across time, product type, customer type/sales channel, purchase type, supplier, and geography.

- Dr. Stiroh does not dispute the existence of pass through in the beef supply chain and does not criticize my pass through regression analysis in any meaningful way. I have updated my pass through analysis to include additional third-party data, and the results are consistent with those that I estimated in the Mangum Class Report.

II. Dr. Stiroh Does Not Dispute or Rebut Key Elements of My Report

29. In the Mangum Class Report, I discussed common economic evidence that is consistent with the alleged conspiracy. Specifically, I discussed the importance of the structural characteristics of the beef market, and how those characteristics would lead to widespread (*i.e.*, common) impact from the alleged conspiracy. I also discussed extensive documentary evidence depicting actions from Defendants that are consistent with the alleged conspiracy and presented certain empirical analyses that are consistent with plaintiffs' allegations and a finding of common impact. As I discuss below, Dr. Stiroh either ignores or misunderstands these elements of my report, and this causes her to reach flawed and economically nonsensical conclusions.

II.A. Dr. Stiroh Does Not Dispute that the Beef Industry is Characterized by Factors that Facilitate the Successful Formation and Maintenance of a Collusive Scheme

30. In the Mangum Class Report, I discussed the structure of the beef market and explained that it is characterized by factors that facilitate the successful formation and maintenance of a collusive scheme like that alleged in this case. First, I discussed the fact that Defendants dominate the market for beef in the United States.[10] This has implications for common impact, because it means that beef consumers are unlikely to be able to avoid purchasing beef produced by Defendants. In her report, Dr. Stiroh does not respond to, or rebut, this conclusion.

31. Second, I discussed the implications of the fact that beef is commodity-like in nature. Because Defendants collectively dominate the beef industry, customers cannot easily switch to non-Defendants in response to higher prices. Importantly, even if customers could switch, the commodity-like nature of the beef products at issue means that market prices depend on total industry supply. Because Defendants control the vast majority of market share, the prices charged by non-Defendants would also be artificially inflated due to the alleged collusion.

[10] Mangum Class Report, ¶¶ 102–106.

Likewise, due to their collective dominance of the existing market, as well as the barriers to entry, significant new entrants into the market are virtually non-existent, so customers could not avoid impact by turning to new competitors. Dr. Stiroh does not respond to, or rebut, this conclusion.

32. Third, I cited evidence showing that beef products generally exhibit a low elasticity of demand; this is also important for a finding of common impact because it means that purchasers are less likely to switch away from beef products entirely in response to higher prices due to the alleged conspiracy. In her report, Dr. Stiroh does not respond to, or rebut, this conclusion.

33. Fourth, I concluded that there are significant barriers to entry in the market for beef.[11] This also has relevance to the conclusion of common impact, because it means that supracompetitive profits earned through a collusive scheme would not be easily competed away by new entrants into the marketplace. In her report, Dr. Stiroh does not respond to, or rebut, this conclusion. In her deposition, Dr. Stiroh opined that my analysis of barriers to entry was limited to "Greenfield entry" and that barriers to such entry is less important than barriers to expansion.[12] Dr. Stiroh's testimony that what "you care about is whether there are barriers to expansion or some ability of somebody outside of the alleged cartel to compete on price. And that doesn't -- isn't always a brand new entrant."[13] I disagree with Dr. Stiroh's testimony and characterization of my opinions in this regard. My discussion of barriers to entry is not limited to greenfield entry or otherwise "brand new" entrants. While opportunities for such are, in fact, limited by the significant barriers to entry, opportunities for expansion among existing fringe competitors were also limited. As I have explained in the Mangum Class Report, Defendants dominate the beef industry and possess the vast majority of slaughter capacity for fed cattle. These facts grant Defendants significant scale economies that cannot be easily matched by smaller entrants. The cattle cycle's long duration further complicates entry or expansion, because it will take an extended period of time for a hypothetical entrant (or expanded competitor) to achieve scale and become cost efficient with Defendants. Additionally, evidence indicates that Defendants utilized deed restrictions to ensure that

[11] Mangum Class Report, ¶¶ 107–112.
[12] Stiroh Deposition, pp. 300:16–302:18.
[13] Stiroh Deposition, p. 302:9–15.

would-be competitors (either new or expanded) could not utilize existing facilities.[14] In short, evidence exists that the barriers Dr. Stiroh opines are important do exist, and that Defendants sought to maintain them during the Class Period.

34. Fifth, I discussed the fact that Defendants had ample opportunities to form and maintain an anticompetitive scheme. These opportunities came through trade organizations and events, personal relationships, and other industry interactions.[15] Dr. Stiroh does not respond to, or rebut, this conclusion.

35. Finally, I discussed the fact that Defendants possessed the ability to monitor and enforce and anticompetitive scheme like that alleged in this case.[16] This evidence is important to the determination that a conspiracy would have impact on purchasers, because it decreases the likelihood of cheating. Dr. Stiroh does not respond to, or rebut, this conclusion.

36. Dr. Stiroh's failure to address or engage with any of these conclusions is noteworthy, because they form the basis for my finding of common impact. Understanding the structure of the industry and what it means for customers is important when forming hypotheses about statistical tests, as well as for evaluating the plausibility of the results of econometric analysis. The realities of the beef industry and how products are priced mean that the alleged conspiracy would be expected to affect all or virtually all purchasers. This is a straightforward implication of the fact that Defendants—the alleged conspirators—sell the vast majority of beef in the marketplace, and it would be very difficult, if not impossible, for any beef purchasers to avoid impact throughout the Class Period. Due to how beef is priced, a conspiracy that affects the supply of beef would be expected to affect all beef prices. Indeed, it is unclear as a matter of economics how any customers could conceivably avoid being affected by an industry-wide scheme such as that alleged in this case. As such, Dr. Stiroh's opinion that many customers were not affected lacks any logical or economic rationale.

II.B. Dr. Stiroh's Criticisms of My Overcharge Model are Limited and Unsupported

37. In her report, Dr. Stiroh makes several criticisms of my overcharge regression model, as well as those put forward by the other plaintiffs' experts in this case (Dr. Sunding and Dr. Williams).

[14] Mangum Class Report, ¶¶ 191–218.
[15] Mangum Class Report, ¶¶ 331–362.
[16] Mangum Class Report, ¶¶ 186–188.

In the Mangum Class Report, I specified an econometric model which measures the effect of the alleged conspiracy using an indicator variable that spans the duration of the Class Period. This is a well-accepted approach that I have utilized many times in other matters, and is consistent with best practices for measurement of conspiratorial effects.[17] In her report, virtually all of the criticisms Dr. Stiroh levels at my overcharge model relate to one particular aspect: the overcharge variable itself. Dr. Stiroh argues that, due to her characterization of the alleged conspiracy and the beef industry, it is necessary to split the overcharge variable in several ways in order to measure any potential differences over time, across customers, and across products.[18] I disagree with Dr. Stiroh's characterizations, and I address these specific criticisms in various parts of this report. However, outside of these criticisms specific to the overcharge variable itself, Dr. Stiroh says very little about the rest of my overcharge model. For example, Dr. Stiroh does not criticize my use of 2-stage least squares (2SLS), and even appears to endorse it.[19] Dr. Stiroh does not appear to dispute any of the specific control variables in my model. The only potential exception is Dr. Stiroh's complaint that I did not control for the type of purchase (or pricing mechanism) for each transaction.[20] However, while

[17] *See, e.g.,* Jonathan Baker and Daniel Rubinfeld, "Empirical Methods in Antitrust Litigation: Review and Critique," *American Law and Economics Review* 1, no. 1 (1999): 386–435 at 392 ("Reduced form equations are perhaps the most commonly employed in price-fixing cases… The price effect of the alleged conspiracy is measured by the coefficient on a dummy variable that takes on the value of one during the period (or in the markets) in which the conspiracy is in operation."); ABA Section of Antitrust Law, *Proof of Conspiracy Under Federal Antitrust Laws*, 224–230, (American Bar Association, 2010), pp. 224–230 at 225 ("When the necessary data are available, formal statistical models can be a valuable supplement to other types of economic analysis. Regression methods allow the economist rigorously to control for the effects of other factors and isolate the effect of the variable of interest… For instance, an economist may model price as a function of determinants of demand, costs, market structure, government regulation, and a dummy variable for the possible existence of conspiracy."); Theon van Dijk and Frank Verboven, "Quantification of Damages," chap. 93 in *3 Issues in Competition Law and Policy* (ABA Section of Antitrust Law, 2008) pp. 2331–2348 at 2335 ("To the extent that cartel prices differ in a statistically significant way from the pre- and post-cartel prices, it may be possible to attribute the difference to collusion. The before-and-after approach is usually implemented within a multiple regression framework in which one estimates the price over the entire period (conspiracy and benchmark period) and includes an indictor (or "dummy") variable that is equal to one during the conspiracy period and zero otherwise. The estimated coefficient associated with this dummy variable then measures the amount of the price overcharge.").

[18] *See* Stiroh Report, Section II.B.

[19] In her report, Dr. Stiroh imposes the structure of my regression model on Dr. Sunding's and Dr. Williams' variables, along with a cattle price variable I used, in an attempt to rebut their regression models. In contrast, Dr. Stiroh does not reverse this procedure and impose the structure utilized by Dr. Williams or Dr. Sunding on my chosen explanatory variables. *See* Stiroh Report, ¶¶ 59–64.

[20] In her report, Dr. Stiroh argues that she has provided evidence (through various charts of predicted prices) that "Plaintiffs' experts' models are unable to capture real-world pricing dynamics and patterns, such as the different pricing mechanisms that customers negotiate with Defendants and underlying contractual relationships," and that the "data on which Plaintiffs' experts' models rely do not include contract details for the types of contracts adopted

Dr. Stiroh complains that I did not control for this particular aspect of pricing, she did not take any steps to demonstrate that doing so would have any material impact on the model or affect results in any way.

38. I highlight this fact, because in her deposition, Dr. Stiroh made references to an "unidentified flaw" and "demonstrably omitted variables" in the econometric models put forward by plaintiffs' experts.[21] However, Dr. Stiroh does not make any claims in her report that my direct overcharge model suffers from omitted variable bias. This matters because, when Dr. Stiroh's own analyses failed basic econometric tests or logic, she attempted to explain these results away by saying that the models were otherwise flawed due to "demonstrably omitted variables" or an otherwise "unidentified flaw."[22] Dr. Stiroh has not actually identified any such omitted variables with respect to my direct overcharge model.[23]

39. Dr. Stiroh is playing a game of "Heads I win, Tails you lose," wherein her application of my model in her econometric tests is valid and reliable when it shows "no impact," but when her econometric tests do show impact, she can retreat to vague criticisms ("*you get weird results in a weird model*"[24]) about supposed unidentified flaws in the underlying model, which she has not substantiated or supported.

II.C. Dr. Stiroh Does Not Address Evidence Related to Defendants' Actions that Are Consistent with the Alleged Conspiracy

40. In the Mangum Class Report, I cited extensive evidence of Defendants' actions that are consistent with the alleged conspiracy. That discussion included evidence related to the widespread sharing of competitively sensitive information, including slaughter intentions, as well as evidence that Defendants routinely monitored each other and understood the threat of

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by different direct purchasers." See Stiroh Report, ¶ 73. Dr. Stiroh is incorrect regarding the availability of information on types of contracts and pricing mechanisms for three of the four Defendants. As I show later in this report, including such information in my overcharge model does not affect the results.

[21] Stiroh Deposition, pp. 216:13–219:24.

[22] Stiroh Deposition, pp. 216:16–217:19 ("I think that calls into question is there an unidentified flaw in the model…When you see something weird like that, that tells you there's something is in the model that's -- that's hidden that we don't yet know what it is….you get weird results in a weird model.").

[23] Given that the questions in her deposition were primarily related to Dr. Sunding's model, it is possible she did not intend to extend her "weird results" opinion to my model and analysis. However, the criticisms of her results that prompted her "weird results" testimony are equally applicable to my analysis, as I show later in this report.

[24] Stiroh Deposition, p. 217:18–19.

punishment for overproduction of beef.[25] While it is my understanding that some of this evidence may be important for merits-related questions, it is also relevant to the question of impact. The ability to monitor and enforce each other is important because it facilitates the continuing viability of the alleged conspiracy, which serves to ensure that, ultimately, all customers that purchased beef during the Class Period would be affected.

41. Dr. Stiroh does not address any of this evidence at all in her report, even when it has direct bearing on opinions she is citing. For example, Dr. Stiroh argues that the alleged conspiracy could not have any effect for several years due to the nature of the supply chain in the beef industry. However, in the Mangum Class Report, I cited evidence that Defendants took actions early in the Class Period that significantly affected their profit margins. As explained by JBS's Al Byers, [redacted][26] This is a direct contradiction of much of Dr. Stiroh's report and opinions about Defendants' inability to affect prices, the predetermined cattle slaughter decisions, and the possibility of impact earlier than 3 years into the Class Period.

42. In her deposition, Dr. Stiroh was unclear about whether or not she had even reviewed the documents cited in my report (or the report of other plaintiffs' experts).[27] This is problematic because the specification of an econometric model—as well as criticism of one—should be grounded in a clear understanding of the facts and evidence that motivate it. In this case, Dr. Stiroh's refusal to address this evidence leads to a scenario in which she claims that Defendants could not do things—affect cattle slaughter in the short run—that Defendants themselves were claiming they could do.

43. Dr. Stiroh's opinion also forces Defendants into repeatedly making economically irrational decisions. If output is truly fixed in the short run (as Dr. Stiroh opines), then whenever a Defendant reduced its own slaughter, another packer must immediately increase its production to offset the reduction. But if that was the case, it would make no economic sense for the

[25] Mangum Class Report, ¶¶ 295–372.

[26] JBS-0000167217 (Byers Exhibit 1378).

[27] *See* Stiroh Deposition, pp. 326:5–328:17.

Defendants to reduce slaughter in the first place—prices for beef will not be affected, because output remains constant. The fact that Defendants so frequently did make reductions to slaughter demonstrates that Dr. Stiroh's opinion about short-run fixed slaughter is incorrect.

44. Defendants also engaged in actions to restrain and reduce capacity during the Class Period. As I explained in the Mangum Class Report, Defendants not only reduced significant slaughter capacity during the Class Period, they also utilized deed restrictions and refusals to sell to keep previously idled and existing capacity away from competitors or would-be entrants. Dr. Stiroh does not address this topic in her report.

III. Empirical Analyses Are Consistent with Consumer IPPs' Allegations

45. In the Mangum Class Report I explained that empirical evidence, including high profit margins and my overcharge regression model showing artificially high beef prices, is consistent with the alleged conspiracy. My direct overcharge model estimates the artificial inflation of beef prices during the Class Period to direct purchasers. Due to the fundamental relationship between supply and demand, my finding that beef prices during the Class Period were inflated above levels that can be explained by market factors also demonstrates that the supply of beef products was artificially restrained.[28]

46. Dr. Stiroh claims that Defendants collectively expanded the amount of fed cattle they slaughtered over the Class Period, implying that an increase in production is inconsistent with Consumer IPPs' supply restriction allegations.[29] Dr. Stiroh's emphasis on absolute decreases or increases (see Stiroh Figure 2.2a and 2.2b) is misleading, because it focuses on an irrelevant question. Increasing levels of production is not evidence against a supply restraint or price-fixing scheme. Stated simply, whether output grows is not the relevant question—the question is whether it grows at a slower rate than it would have but for the Defendants' allegedly

[28] The relationship between price and quantity is not unique to beef, or agricultural proteins—it is a fundamental relationship explained in the early chapters of typical economics textbooks. *See* Robert S. Pindyck and Daniel L. Rubinfeld, *Microeconomics*, 9th ed. (Pearson, 2018) ("Pindyck and Rubinfeld 2018"), pp. 48–49 (the authors illustrate the relationship between a change in the supply curve and prices for a good with an upward-sloping supply curve and a downward sloping demand curve). *See also* Steven A. Greenlaw and David Shapiro, "Demand and Supply," chap. 3 in *Principles of Economics* (OpenStax Rice University, 2018) ("Greenlaw and Shapiro 2018"), p. 58. (the authors explain that "[decreased] supply means that at every given price, the quantity supplied is lower…Increased supply means that at every given price, the quantity supplied is higher.").

[29] Stiroh Report ¶¶ 13, 17–18, Figure 2.1.

wrongful conduct. In this case, Defendants' aggregate production of beef grew during the Class Period. This is hardly surprising, given that the Class Period started shortly after the trough of the cattle cycle.

47. Dr. Stiroh also claims that I have not established that the price overcharge I estimate was caused by a supply reduction.[30] Dr. Stiroh's primary criticism is that I have not separately modelled the level of but-for supply.[31] First, modelling the level of but-for supply is not a necessary modelling step in order to establish the price effects of an alleged conspiracy. The essential question is whether Class members sustained impact and injury from the alleged conspiracy and my overcharge model directly answers that. I explain below that I do present analyses based on evidence common to the class that is consistent with artificially reduced beef supply.

48. Second, as I explained above, the artificial inflation in price estimated by my direct overcharge model necessarily implies deflated levels of beef supply. Since output and price are two sides of the same coin, my finding that there were widespread price increases that cannot be explained by supply and demand factors during the Class Period is evidence consistent with the alleged collusion artificially restricting the supply of beef products.[32]

49. Third, as I explained in the Mangum Class Report, the alleged conspiracy involved an agreement to reduce competition for market share and instead emphasize "margin over market share." This necessarily involved decisions on supply, and in particular limitations on slaughter to existing slaughter capacity shares. Outside of the context of a conspiracy, each individual Defendant would be incentivized to seek as much market share as possible at the expense of other Defendants. This would lead to each Defendant seeking to increase slaughter, because failing to do so would lead to gains for competitors, at their own expense. Therefore, coordinating on supply restraint was inherent to the alleged agreement. I understand that Defendants are alleged to have enacted this agreement in July 2014, and my overcharge model measures the impact of this alleged agreement starting in August 2014 through the end of the Class Period.

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[30] Stiroh Report, Section VII.A.

[31] Stiroh Report, ¶ 120.

[32] *See* Pindyck and Rubinfeld 2018, pp. 48-49. *See also* Greenlaw and Shapiro 2018, p. 57.

50. Dr. Stiroh further claims that I "have not evaluated whether it was in each Defendant's unilateral best interest to expand production."[33] In a commoditized market like beef, no producer has the ability to unilaterally charge higher prices. Without a conspiracy, it is in each individual firm's self-interest to pursue market share, because that would be the only path to growth. Only within the confines of a conspiracy does it make sense to abandon market share in exchange for margin, because without such an agreement, ignoring capacity and market share growth would not lead to margin gains—other packers would capture that share and earn those profits instead.

51. On a more fundamental level, however, Dr. Stiroh's insinuation that it may not be in each Defendant's unilateral best interest to expand production ignores the fact that Defendants' margins during the Class Period were historically high. As Tim Klein testified in his deposition, anytime the Defendants could earn more profit, it would be advantageous to expand production.[34] However, when he was communicating internally, [redacted].[35]

52. Lastly, Dr. Stiroh consolidates the claims from all 5 separate Plaintiff classes and insists that the but-for world is "inconsistent with economic logic and unsupported by the results of [Plaintiffs' experts'] own analyses."[36] To do this, Dr. Stiroh combines various empirical results from downstream and upstream class experts and purports to quantify the total economic impact of these findings. First, I disagree with Dr. Stiroh's basic approach of attempting to impose but-for world pricing and output changes on the Defendants' actual-world profit and loss statements. In a but-for world without the alleged wrongdoing, it is plausible that each firm would make different decisions that could lead to different financial outcomes. For example, increased production could lead to increase scale economies or efficiencies in production facilities, which would increase overall profitability. Second, it is my understanding that Dr. Stiroh's analysis—combining the results from multiple distinct litigations—is not

[33] Stiroh Report, Section VII.B.

[34] Deposition of Timothy Klein, Aug. 7, 2024, pp. 30:13–31:20.

[35] *See* NationalBeef-00002095–96 at 95 [redacted]

[36] Stiroh Report, Section VII.C.

relevant to the question of common impact for the Consumer IPP class, and that the damages estimated in this case are the only relevant damages for consideration. Accordingly, I have adjusted Dr. Stiroh's Figure 7.2 to incorporate only the overcharge estimated by my econometric model in this case.[37] The losses that Dr. Stiroh stresses Defendants would have sustained no longer exist – each Defendant would have maintained positive profits in aggregate.

[redacted]

IV. Dr. Stiroh's False Standard of Common Impact & the "Theory of Many Zeroes"

53. In the Mangum Class Report I concluded that all or nearly all direct purchasers in the Consumer IPP distribution chain were impacted by the alleged conspiracy.[39] This conclusion was based on (i) my overcharge regression analysis; (ii) the presence of structural characteristics that tend to prevent customers from avoiding overcharge; (iii) the widespread use of market based indices in setting prices; (iv) my finding that other purchasing methods such as fixed-price contracts or spot market purchases were still affected by market prices; and (v) correlation analysis which showed that prices across Defendants, customers and geographic locations were affected by common factors.

54. Dr. Stiroh makes various arguments regarding common impact at the direct purchaser level that I respond to in this section. In particular, I explain that the ability of some customers to negotiate is typical of most industries and has no bearing on common impact. Likewise, I

[37] I also adjusted the timeframe to end in December 2019, coinciding with the Consumer IPP Class Period.

[38] See backup production.

[39] Mangum Class Report, Section IV.

explain that the particular contracting mechanism is irrelevant. I also explain that nothing in Dr. Stiroh's discussion of pricing negotiations or fixed price contracts refute the evidence presented in the Mangum Class Report that prices of individual customers are tied to market prices. I demonstrate how Dr. Stiroh's purported findings of "uninjured" or "unimpacted" customers are the result of a fundamentally flawed methodology. I also respond to Dr. Stiroh's criticisms of my correlation analysis and her finding that there is price dispersion across individual products or customers.

55. At the root of Dr. Stiroh's criticism is a false standard of uniform pricing and impact. In her report, Dr. Stiroh repeatedly points to variation in prices across customers, and concludes that overcharges *could* therefore also vary—"by class member, product, and year."[40] First, Dr. Stiroh has merely speculated that this could be the case—she has not demonstrated it to be the case or even that it would be economically logical. Further, Dr. Stiroh does not explain how, even if that were true, it would be inconsistent with a finding of common impact. While there is price dispersion within and across beef products, the existence of price dispersion does not mean some purchasers were affected while others were not. There would have been similar dispersion among prices across customers or products before and after the onset of the conspiracy. Throughout this report, I explain how Dr. Stiroh has inappropriately applied a false standard of uniform pricing and identical impact when she criticizes my finding of common impact.

56. Throughout the remainder of this report, I refer to the "Theory of Many Zeroes" to highlight a flawed and misleading criticism that is central to all of Dr. Stiroh's opinions and analysis. According to the Theory of Many Zeroes, statistical estimates based on more than one customer's data ("pooled" estimates) are misleading and unreliable, not because they purportedly "mask" variation, but because they mask variation that would fully contradict the pooled estimate. Through the Theory of Many Zeroes, when Dr. Stiroh opines that overcharge estimates would "vary by class member, product, and year," her underlying opinion is that the variation includes "many zeroes."[41] In this way, a pooled overcharge of [redacted] percent is merely an "average" that includes "many zeroes" (*i.e.*, unimpacted customers). In this report, I

[40] Stiroh Report, Section II.A.

[41] Stiroh Report, Section II.A.

demonstrate that the Theory of Many Zeroes is implausible, lacks evidentiary or empirical support, and leads Dr. Stiroh to adopt misleading and rigged methods that give her favorable outcomes.

IV.A. Dr. Stiroh's Analysis of Market Pricing Mechanisms is Flawed and Misleading

57. In the Mangum Class Report, I detailed the commodity-like nature of the beef products at issue and explained how, in a commodity industry like this one, the market price that each individual supplier receives is contingent upon the collective supply decisions of all industry participants rather than solely on those of that specific supplier. Dr. Stiroh cites evidence that Defendants sold beef using a variety pricing mechanisms, that customers may negotiate with multiple suppliers, and that pricing decisions often depended on factors like inventory reserves and forecasts.[42] Yet, Dr. Stiroh ignores the commodity-like nature of beef and disregards overwhelming evidence of beef prices being a function of the market value.

58. While each individual supplier (*e.g.*, the Defendants) may utilize various pricing mechanisms, they are all fundamentally linked to the underlying value of beef in the market.[43] Consequently, over time, all price indices and contracts will eventually converge towards the market price. As a result, regardless of the pricing mechanism, the prices paid for beef reflect the prevailing market price. As the market price fluctuates up or down, so will individual prices charged by Defendants.

59. One component of Dr. Stiroh's self-invented definition or standard for showing common impact is a so-called "rigid pricing structure." Specifically, Dr. Stiroh argues against common impact by asserting that beef prices "do not follow a rigid or common price structure," because of the "presence of individualized pricing" for "thousands of products to more than 4,300 customers."[44] In her report, Dr. Stiroh concluded that, as a result of negotiations, prices for beef "vary over time, across customers, and across products."[45] Taken at face value, Dr. Stiroh's assertions would mean that any industry in which customers and suppliers negotiate

[42] Stiroh Report, ¶ 24.

[43] The truth of this can be seen from assuming that it is not true. If it were not true, it must be the case that a meaningful amount of beef is bought and sold in the marketplace with complete disregard to the actual market value of that beef. This is obviously economically and logically nonsensical, but that is effectively what Dr. Stiroh is arguing.

[44] Stiroh Report, ¶¶ 23–24.

[45] Stiroh Report, ¶ 24.

over prices for multiple products would be immune to widespread anticompetitive impact. This is obviously incorrect—numerous classes have been certified by the courts in industries which also feature customer-supplier price negotiations and multiple products. Indeed, both Dr. Stiroh and I have been retained in several such matters in recent years.[46]

60. Ignoring the fact that Dr. Stiroh's conclusion that common impact is inconsistent with customer-supplier pricing negotiations, Dr. Stiroh's testimony on the concept of a "rigid pricing structure" highlights both her self-invented standards and her own confusion about how prices are set in the beef industry. Specifically, Dr. Stiroh testified that she was unaware of any commonly accepted method for testing her own requirement of whether industry follows a "rigid or common price structure."[47] However, she then speculated that it would mean that "if there's a price increase for one customer then there is a structure by which you could determine an increase for another customer through that structure," and affirmed her opinion that this "does not exist in this industry."[48]

61. Dr. Stiroh's assertion that there is no mechanism for distributing price increases (or decreases) in the beef industry across customers is simply incorrect. As I explained in the Mangum Class Report, prices for beef are set through a variety of different contracting or purchasing mechanisms, such as fixed-price contracts, spot negotiations, and formula pricing.[49] However, despite these different mechanisms, prices in the beef industry are still effectively based on market-wide indices—primarily the USDA cutout values. These values represent the "market prices," and it is economically nonsensical to expect that Defendants and buyers would simply ignore such prices when negotiating a new purchase of beef.[50] Yet, Dr. Stiroh devotes a large

[46] *See* Stiroh Report, Exhibit 1 and Mangum Class Report, Appendix A.

[47] Stiroh Deposition, pp. 236:20–240:8.

[48] Stiroh Deposition, pp. 227:24–228:9.

[49] Mangum Class Report, ¶ 57.

[50] That is, if the current market price of beef is lower than a supplier is asking, that means that beef is selling at lower prices elsewhere, and the buyer would rationally demand a lower price or purchase from a different supplier. But because both the supplier and the buyer know this, it would be irrational for the supplier to ask a higher price in the first place. The same situation holds true if the buyer is requesting a price lower than the market will justify—the supplier would rationally sell its beef to other customers at the higher prevailing market price. There will obviously be exceptions to this scenario caused by idiosyncratic shortages or excesses in inventory for different suppliers, or for idiosyncratic promotional events held by a buyer (*e.g.*, sales). Thus, the reliance on the market price does not dictate uniformity, but it does define the sandbox in which negotiations take place.

section of her report to an incorrect and highly misleading argument that customer-supplier negotiations are not related to these market reference values.

62. Specifically, Dr. Stiroh argues that the variation in pricing across customers comes (at least in part) from purported differences in the ways and methods through which prices for beef are set or determined.[51] Central to Dr. Stiroh's argument is her assertion that "a substantial share of Defendants' sales of beef products were negotiated and prices on an individualized basis, and were not tied to a formula based on industry-wide indices."[52] Here, Dr. Stiroh is taking an extremely narrow—and meaningless—view of what it means to have prices based on industry-wide indices like the USDA cutout values. In doing so, she ignores how the industry viewed the USDA cutout as the price of beef,[53] and the constant focus on USDA cutout prices.[54] Although I addressed this topic in the Mangum Class Report, in light of Dr. Stiroh's comments, I revisit it below.

IV.A.1. Formulaic Pricing Is Related to Market-Wide Indices and Is Affected by the Alleged Conspiracy

63. Record evidence demonstrates that many beef products are sold at prices that directly and formulaically fluctuate in accordance with the USDA cutout values. A contract between [redacted]) illustrates this type of pricing. As shown in **Figure 2**, the contract specifies, for each product, an appropriate "USDA Index" cutout reference value, and an "overage" (*i.e.*, the cost above and beyond the reference value).

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[51] Stiroh Report, ¶ 24.

[52] Stiroh Report, ¶ 24.

[53] Deposition of Donald Kieffer, Mar. 21, 2024, p. 47 [redacted]"); p. 192 [redacted]).

[54] *See, e.g.*, CARGILL001341910–911 at 910 [redacted] NationalBeef-00558908–909 at 908 ("[redacted]; TYSONBEEF00479028 [redacted] TYSONBEEF00414270 [redacted] TYSONBEEF00779844–47 at 45 [redacted] JBS-0000087380–381 at 380 ("[redacted]; JBS-0001302439 [redacted]"); CARGILL001366712 [redacted]

Thus, a price for each of the products below would be USDA index price plus one cent per pound.

Figure 2. Products and Pricing Table from 2018 Supply Agreement between JBS and [redacted][55]

[redacted]

64. This type of pricing is widespread in the beef industry. Data produced by Defendants indicates that they utilized "contract" pricing for a majority of their beef sales.[56] Any products that are prices in this directly formulaic fashion will follow, by definition, the exact type of price structure that Dr. Stiroh (incorrectly) claims does not exist in the beef industry. Contracts with formulaic pricing do not necessarily lead to uniform pricing, however. Individual customers may negotiate different overage/underage rates, volume or distribution benefits, or other considerations that lead to differences in the net prices paid. Additionally, as seen in the figure above, the "overage" amount is reduced to zero upon reaching certain volume thresholds.[57] Further, the contract between JBS and [redacted] includes various benefits and incentives for

[55] Supply Agreement between [redacted], and JBS, June 20, 2018 (JBS-0000391846–59 at 53).

[56] According to Defendants' data, which includes some details as to the purchase type for transactions, there were only 207 customers who only bought beef through "negotiated" pricing (*i.e.*, non-formulaic pricing) during the Class Period out of 1,305. See backup production. That is not to say that these 207 customers, who only purchased through the spot market, were unimpacted (as I discuss below), but rather to point out how widespread formulaic pricing really is.

[57] JBS-0000391846–859 at 853.

volume growth and marketing funds.[58] Even if different customers negotiated identical terms, they may not all earn the maximum benefits, and thus would have some variation in their ultimate net pricing. This is all consistent with a finding of common impact and is also accounted for in a benchmark regression model like the one I have specified. These prices are all tied to USDA cutout values as a starting point. Thus, allegations which would affect these reference values would, by construction, affect all prices paid by customers who utilize this type of pricing.

65. Record evidence shows [redacted] had negotiated a similar supply agreement with National Beef in 2015 and 2016. The agreement stated that pricing for the beef products would be the current USDA cutout reference price plus the overage (per pound).[59]

[58] JBS-0000391846–859 at 856.

[59] NationalBeef-00955792–798 at 796. *See also* NationalBeef-00955791.

Figure 3. Products and Pricing Table from 2016 Supply Agreement between National Beef and [redacted][60]

[redacted]

66. [redacted] corporate representative testified that during the Class Period, [redacted] followed a pricing formula based on USDA for its beef products.[61] An example from [redacted] from 2014 links each product to the USDA reference price:[62]

[60] NationalBeef-00955792–798 at 796.

[61] *See* [redacted].

[62] [redacted]

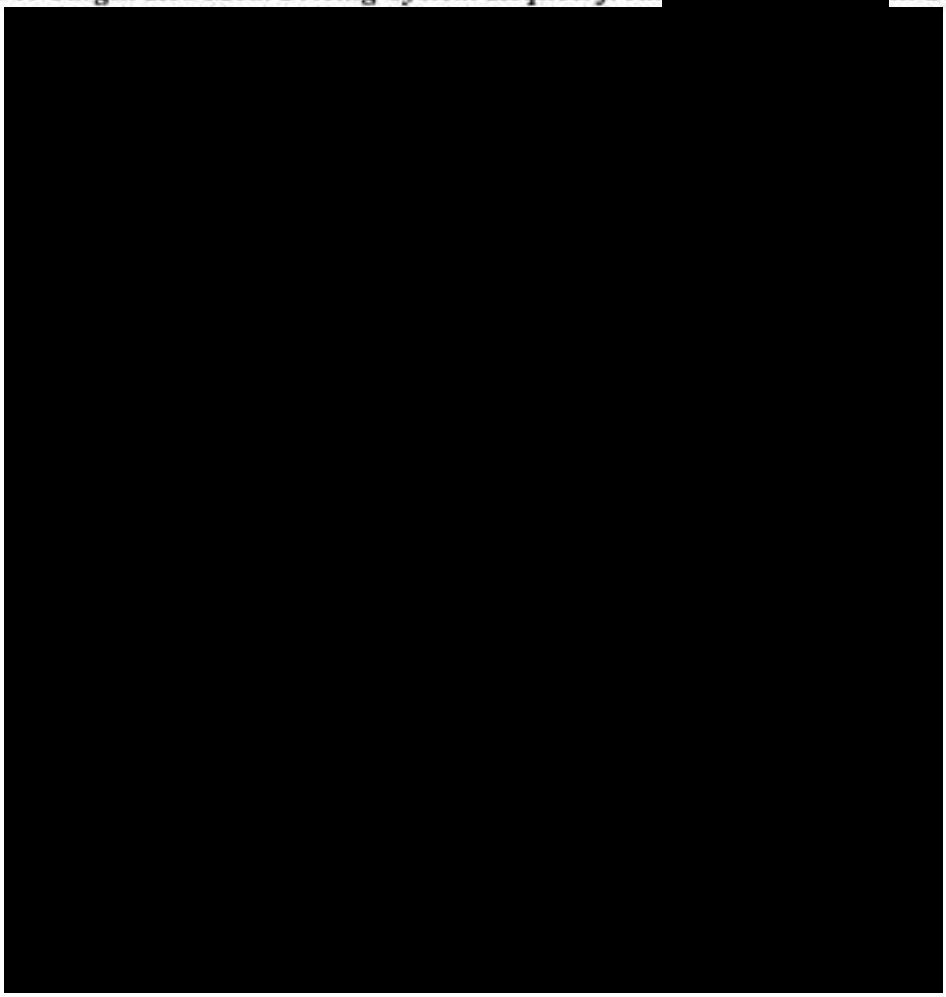
Figure 4. [redacted] ***Beef Pricing Formula from 2014***

[redacted]

67. A contract from [redacted] dated November 15, 2018, which Dr. Stiroh cites in her report, shows pricing for two products that were expressly tied to USDA market indices.[63] The price for 81072, Cargill's product code for Premium Signature Angus blade meat (a chuck primal product) is shown as USDA index price plus 2 cents. The price for 11020, which is a USDA cutout reference value for Cap and Wedge Meat (a rib primal product) is shown as being the USDA index price.

[63] Stiroh Report footnote 54; CARGILL001158755–756. *See also* CARGILL001158761–762.

Figure 5. Cargill Red Meat Pricing System Request from [redacted] in 2018 [64]



68. [redacted] negotiations with Cargill in January and February 2019, which Dr. Stiroh cites in her report, show that pricing for Denuded Knuckles (a product made of loin primal) was negotiated in reference to the market price and specifically tied to USDA cutout reference values.[65] The price offered was either as Option 1: being a percentage of the USDA market price (110%) or Option 2: a set overage of 30 cents above the USDA market price.[66] Cargill's account manager provided market trend charts of the USDA cutout reference value with the two options. Peoples Sausage Company ultimately chose Option 1.

[64] CARGILL001158755–756 at 756; CARGILL000280348 (listing product codes).

[65] Stiroh Report, footnote 54; CARGILL001158757–760.

[66] CARGILL001158757–760 at 758–759.

Figure 6. Email Showing Negotiations Between Cargill and [redacted] in 2019

[redacted]

69. In another example, Dr. Stiroh discusses [redacted] two pricing strategies used during the Class Period.[67] Record evidence indicates both of the strategies were expressly tied to USDA cutout values or referenced USDA cutout values as a starting point. Before 2017, [redacted] used a strategy called "bid-and-quibble" where [redacted] would solicit price lists from multiple packers which contained beef products listed with their corresponding USDA cutout reference values.[68]

[67] Stiroh Report, ¶ 27.

[68] [redacted]

Pricing was agreed 6-7 weeks before delivery "over a 'conference call' process to determine market conditions and [redacted] volume need."[69] After 2017, it employed a strategy called Joint Value Management or JVM where two times during [redacted] fiscal year, [redacted] would commit to a certain volume in pounds from a vendor. [70] The JVM model tied all [redacted] commodity beef prices to a "tiered pre-committed and variable pricing formula (based on USDA market)."[71] [redacted] also required the vendors participating in JVM to provide USDA market forecasts on a monthly basis throughout the contract term.[72] Dr. Stiroh also references an instance in 2019 when she says [redacted] "[redacted] [redacted][73] This event occurred in the immediate aftermath of the Holcomb fire, and the then-prevailing market prices were elevated due to the supply shock, were not consistent with market prices National Beef had initially forecasted and provided to [redacted][74] My econometric model accounts for the effect that the Holcomb fire had on beef prices.

Figure 7. Excerpt of Item Bids Tab, from [redacted] Request for Proposal from April 2019[75]

[redacted]

[69] [redacted]

[70] [redacted]

[71] [redacted]

[72] [redacted]

[73] Stiroh Report, ¶ 27.

[74] [redacted]

[75] Deposition of Nichloas Birkhofer, Feb. 7, 2024, pp. 183:4–185:8 and Birkhofer Defense Exhibit 1A (CARGILL000798826).

70. Dr. Stiroh also provides an example of testimony from [redacted] regarding its purchasing product volume on "ads."[76] A Request for Proposal that [redacted] drafted to [redacted] in 2018 indicates that "[redacted][77] The Request for Proposal lists products with a "Relevant USDA Index" reference price.[78]

[redacted]

[76] Stiroh Report, ¶ 27.

[77] TYSONBEEF03280287. *See also* TYSONBEEF03280285.

[78] TYSONBEEF03280286.

71. A weekly pricing sheet Tyson sent to [redacted] in 2015 in order to initiate negotiations or purchasing expressly linked products to USDA cutout values.[79]

[redacted]

72. National Beef's August 25, 2016 proposal for Branded Beef Marketing and Supply Agreement for [redacted] also shows beef turn pricing that is tied to the USDA market prices.

[79] TOPCO_0001170097. *See also* TOPCO_0001170096

Figure 10. Pricing Proposal for Turn Business from National Beef to [redacted] in 2016[80]

[redacted]

IV.A.2. Negotiated & Spot Pricing Is Related to Market-Wide Indices and Is Affected by the Alleged Conspiracy

73. Dr. Stiroh's attempt to downplay the prevalence of formulaic pricing is misleading, as is her attempt to argue that beef prices negotiated in the spot market are somehow divorced from baseline values like the USDA cutout prices. Dr. Stiroh is fundamentally incorrect; beef sold outside of formulaic pricing contracts is still sold in the context of a market price, and is thus inherently linked to industry-wide prices like the USDA's cutout values.[81] The misleading nature of Dr. Stiroh's discussion of spot/negotiated prices becomes clear with some additional information about what these transactions actually involve.

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[80] NationalBeef-00298725–727, at 726.

[81] The impact of the fire at Tyson's Holcomb slaughter plant is an example of the impact of overall industry supply on market prices and thus the existence of a baseline price. *See, e.g.*, CARGILL001949678–682 at 680 (Cargill's Randy Carlgren noting that the "[redacted] NationalBeef-00616780–87 at 80 (National Beef's Bill McLaurin noting in the days after the fire that [redacted]

74. Defendants report on their beef sales as required by the Livestock Mandatory Reporting program.[82] Class products are reported on various USDA indices that form the starting point, or reference point for negotiations.[83] The twice daily and weekly spot market prices the USDA reports show a then-current 2-day weighted average price for each beef item.[84] USDA reports on the spot market daily through reports such as the National Daily Boxed Beef Cutout and Boxed Beef Cuts report (LM_XB403) and weekly through the National Weekly Boxed Beef Cutout and Boxed Beef Cuts report (LM_XB459) as well as the National Boxed Beef Weekly Item Summary (AMS_2699).[85]

75. For a given cut of beef, the USDA cutout value is simply the weighted average price of all wholesale-levels sales of that cut of beef over a specified period of time.[86] Rationally, no customer wants to pay above market prices, and no supplier wants to sell at below market prices.[87] Thus, for both buyers and sellers of beef, reference values like the USDA cutout are

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[82] *See* Agricultural Marketing Service, "Livestock Mandatory Price Reporting," U.S. Department of Agriculture, accessed Apr. 2, 2025, https://www.ams.usda.gov/rules-regulations/mmr/lmr.

[83] *See* USDA Market News, "National Weekly Boxed Beef Cutout and Boxed Beef Cuts- Negotiated Sales" (LM_XB459), Feb. 1, 2019, U.S. Department of Agriculture, *available at* https://downloads.usda.library.cornell.edu/usda-esmis/files/br86b359j/1831cr32r/jw827j37m/LM_XB459.TXT

[84] AMS publishes two daily reports (LM_XB402 and LM_XB403). The morning report (LM XB402) "covers market activity from 1:30 p.m. of the prior business day through 9:30 a.m. of the current business day" and the afternoon report (LM XB403) is a cumulative report for the entire day. *See* Agricultural Marketing Service, "User's Guide to USDA's Boxed Beef Cutouts," U.S. Department of Agriculture, Jan. 2023, *available at* https://www.ams.usda.gov/sites/default/files/media/LMRBoxedBeefCutoutsUserGuide.pdf, pp. 2–3.

[85] *See* U.S. Department of Agriculture, "National Weekly Boxed Beef Individual Item Summary - Overview," *available at* https://www.ams.usda.gov/sites/default/files/media/National%20Boxed%20Beef%20Weekly%20Item%20Summary%20Overview%20PDF_1.pdf (USDA notes that the National Boxed Beef Weekly Item Summary "has two major sections, the spot market and comprehensive market, with both listed side by side for comparison purposes. It should be noted however, that the spot market section is also a component within the comprehensive market section."); U.S. Department of Agriculture, "National Weekly Boxed Beef Individual Item Summary - Spot and Comprehensive sales of fed steer/heifer beef," Mar. 31, 2025, *available at* https://www.ams.usda.gov/mnreports/ams_2699.pdf; Agricultural Marketing Service, "National Boxed Beef Weekly Item Summary (PDF) (LSWBBIS)," U.S. Department of Agriculture, accessed Apr. 3, 2025, https://mymarketnews.ams.usda.gov/viewReport/2699 (noting that the weekly LM_XB459 is derived from the daily LM_XB463 market report.)

[86] The various USDA boxed beef cutouts cover different periods of time, but many processors rely on the USDA National Weekly Boxed Beef Cutout and Boxed Beef Cuts – Negotiated Sales Report (LM_XB459). This report is a published weekly report which is based on boxed beef cuts delivered within 0-21 days. *See* Economics, Statistics and Market Information System, "National Weekly Boxed Beef Cutout & Boxed Beef Cuts – Negotiated Sales," U.S. Department of Agriculture, accessed Apr. 2, 2025, https://usda.library.cornell.edu/concern/publications/br86b359j?locale=en.

[87] *See* Agricultural Marketing Service, "User's Guide to USDA's Boxed Beef Cutouts," U.S. Department of Agriculture, Jan. 2023, *available at* https://www.ams.usda.gov/sites/default/files/media/LMRBoxedBeefCutoutsUserGuide.pdf, p. 2 ("The cutout

a critical form of price discovery—it is the "market price" and acts as the benchmark each market participant will utilize to negotiate their own prices and determine if they are getting a "good" deal. Because supply and demand vary over time, the USDA cutout prices will also vary over time. Contracts (such as the one discussed above between JBS and [redacted]) will typically use an average of recent cutout values (*e.g.*, "The Index will move based on the applicable USDA five (5) day weighted average."[88]) rather than constantly updating prices each time a new sale is made. Thus, while all wholesale prices ultimately affect the cutout value, how quickly those changes flow through to the prices of any particular customer's prices may vary.

76. Because of contractually-defined averages, spot market or negotiated purchases are an important part of the USDA cutout price determination. Because they are not beholden to such averages, spot market purchases are more exposed to changes in supply and demand. While this could (at least conceptually) lead to some price variation (compared to contractually formulaic pricing), it wholly contradicts Dr. Stiroh's conclusion that such transactions would somehow be shield or immune to the alleged conspiracy. If anything, they could be more exposed to Defendants' anticompetitive conduct. Further, and more importantly, Dr. Stiroh has ignored the fundamental fact that negotiated and spot sales are part of the actual basis for the industry-wide indices in the first place.[89]

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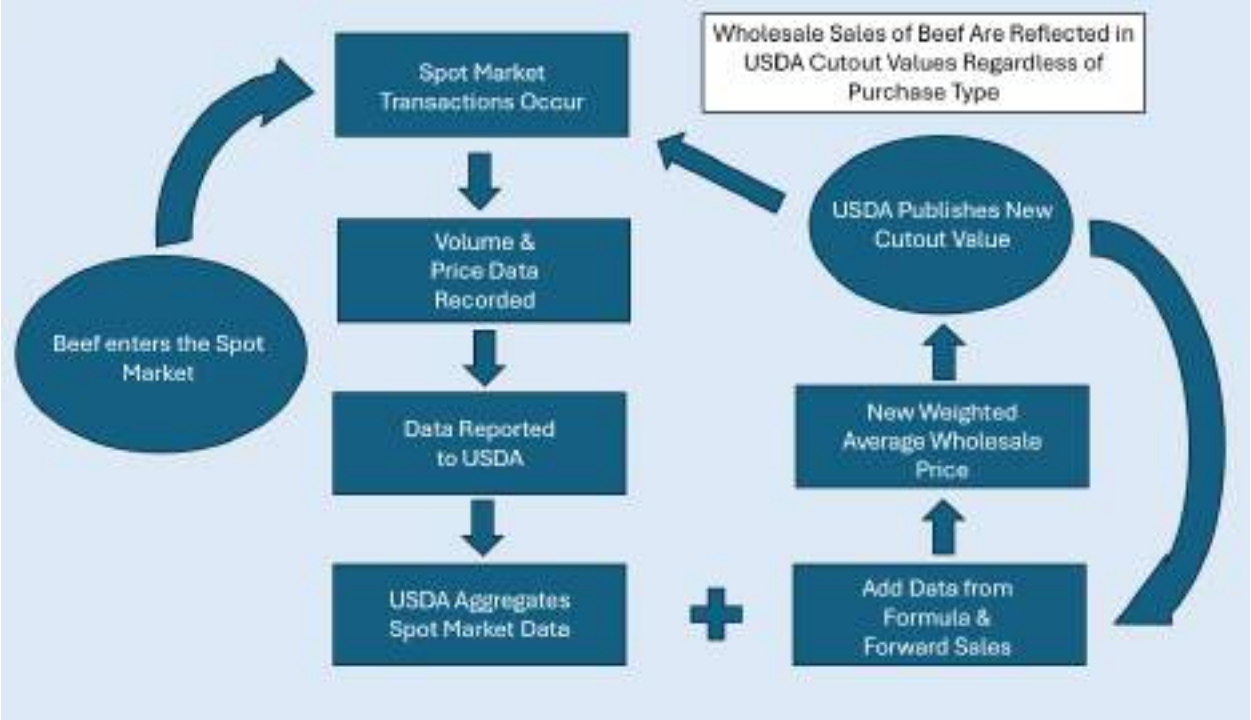
reports are used by the cattle and beef industry in various ways. For example, some use the reports as a price barometer for trade negotiations with other parties, while other segments of the industry use the reports as an impartial starting point to base formula and contract agreements, and the CME Group utilizes the cutout values in calculating its Boxed Beef Index. As packers and producers continue to look for ways to contract cattle, many look to the cutout to provide a pricing basis. Therefore, the role of the beef cutout may become even more essential to the industry as time goes on.").

[88] JBS-0000391846–859 at 846.

[89] Agricultural Marketing Service, "User's Guide to USDA's Boxed Beef Cutouts," U.S. Department of Agriculture, Jan. 2023,), *available at* https://www.ams.usda.gov/sites/default/files/media/LMRBoxedBeefCutoutsUserGuide.pdf; Deposition of Todd Nogelmeier, Jun. 28, 2024, p. 214 ("Q. And you mentioned the USDA weekly weighted average is basically a reflection of the numbers that's been reported from the market? A. From the negotiated sales, yes. Those are negotiated sales that drive the formula pricing."); p. 212 ("And formula business kind of takes all that negotiation away because it's just based on an average of what the industry sells on any particular boxed beef cut or trim item each and every week. And it's – it's a weighted basis, so, you know, it -- one day that price is up, the next day that price is down, depending on what the trades were. And it's also -- the volume is also reflected in the reporting by the USDA. So if 300,000 pounds of ribeyes trade on a given week or a given day, it goes in that day, and then you see a weighted average at the end of the week. And that's what a lot of our formula-based pricing is on, and it basically keeps the customers and us on the market. You know, there's no negotiations involved. That's the market for that week, and it's kind of an easier way for both our customers and us to price beef.").

Figure 11. Illustration of Determination of USDA Cutout Prices



77. As I have depicted in the flow-chart above, when beef enters the spot market, existing cutout values (as well as new information on supply and demand) affect the prices that spot market sales transact at, which then feed (with prices from formulaic and forward sales) into new cutout values, which contribute to future spot market transactions, and so on. Thus, Dr. Stiroh's arguments that customers' use of the spot market to purchase beef is meaningless as it pertains to anticompetitive impact.

IV.A.3. "Fixed" Pricing Is Related to Market-Wide Indices and Is Affected by the Alleged Conspiracy

78. Dr. Stiroh also mentions that Defendants relied on fixed-price contracts for some portion of their beef sales.[90] This is true, but once again (as I explained in the Mangum Class Report[91]) this contracting mechanism is still related to the USDA cutout prices and would still be affected by the alleged conspiracy. Dr. Stiroh's recitation of examples and evidence all demonstrate the underlying ties to the "market price" for beef. That is, they are priced according to the market value of beef at the time of the negotiation or purchase. This is also the case for fixed-price

[90] Stiroh Report, ¶¶ 24–26.

[91] *See* Mangum Class Report, Section IV.B.

contracts. The prices included in fixed-price contracts are obviously not chosen at random. They are negotiated in the same fashion as all other prices for beef: in the context of the prevailing market price. The only difference is that both the supplier and the customer choose to eliminate any price risk for the specified products for the specified duration of the contract. Thus, any contract entered into during the Class Period would necessarily be based on prices that are affected by the alleged conspiracy.

79. As I explained in the Mangum Class Report, a customer could avoid impact from the alleged conspiracy through fixed-price contracting *if and only if* it entered into the contract prior to the conspiracy's onset and never purchased any beef outside of that contract.[92] This is because, upon expiry, the customer would either purchase beef in the spot market, via a formula, or through a new fixed-price contract that would reflect terms consistent with then-current (and conspiratorial) market conditions (*e.g.*, the USDA cutout values). While it is possible that a customer with such a contrived and limited purchasing history exists, it would be so rare as to be irrelevant to the question of common impact. I note that Dr. Stiroh did not attempt to identify any such customers in her report.

80. According to Defendants' data, fixed-price transactions are relatively uncommon. National Beef does not include a transaction type for "fixed-price" at all. However, to the extent they are utilized at all, they would be at most a portion of the "CONTRACT" type transactions, which account for less than 1 percent of the Class Period data after removing missing values.[93] JBS's entries include a very small number of purchases labeled "FIXED PRICE – WITH CONTRACT" and "FIXED PRICE – NO CONTRACT"[94] but these transactions similarly combine to account for only about 2 percent of transactions after removing missing values.[95] Like National Beef, Tyson does not include a transaction type that appears to correspond to fixed-priced contracts. However, given the descriptions of the other transaction types, it would

[92] Mangum Class Report, ¶¶ 396–397.

[93] See backup production.

[94] The label "FIXED PRICE – NO CONTRACT" seems contradictory, in the sense that, without a contract, a fixed price purchase would logically only involve a single purchase, at which point it is no longer "fixed." Additionally, a handful of transactions with "SYSCO FIXED PRICING – REGIONAL" contract types are found in the data for JBS.

[95] See backup production.

be reasonable to assume that fixed-priced transactions, if they exist, would be a subset of the non-classified transactions, which are a small minority of total transactions.[96]

81. The contract between JBS and [redacted] includes an example of the potential use for fixed pricing, but it also serves to demonstrate how its use is limited. While regular purchases of beef follow the contractually-defined USDA indices, the agreement also includes "ad pricing" for irregular, short-term, limited purchases of beef for specific promotional events.[97] For such short-term and limited purchases of beef, the contract offers [redacted] the option of utilizing USDA indices, an auction, or negotiating a fixed price.[98] This makes sense—due to market fluctuations in beef prices over time, neither party may want to agree to fixed pricing for a particularly long period. Fixed prices are not inherently "favorable" or "unfavorable;" if the market turns lower, fixed prices will lead to customers paying higher prices than they would otherwise pay. Thus, fixed-pricing is a risk-shifting mechanism—not a mechanism to avoid high prices.

IV.A.4. Defendants Emphasized the Use of USDA Pricing in Negotiations

82. Dr. Stiroh includes numerous examples in her report of Defendants engaging in pricing discussions and negotiations with individual customers.[99] In using these examples, Dr. Stiroh either ignores explicit references to a market price, or she draws conclusions that are inconsistent with the evidence. As I show below, the documents cited by Dr. Stiroh support Defendants' use of USDA cutout pricing (*i.e.*, market prices).

83. Dr. Stiroh begins by discussing JBS and tries to downplay the fact that more of JBS's business is formula-based than spot or fixed.[100] Dr. Stiroh's emphasis on spot and fixed-price sales is bizarre, because they are both equally affected by the alleged conspiracy, as discussed above. Regardless, the documents Dr. Stiroh cites do not even support her claim. The first JBS document Dr. Stiroh cites shows that formula contracts account for approximately 50 percent of JBS's business.[101] Another JBS document Dr. Stiroh cites plainly undermines her argument

[96] See backup production.
[97] JBS-0000391846–859 at 855.
[98] JBS-0000391846–859 at 855.
[99] Stiroh Report, ¶¶ 24–28, including footnotes.
[100] Stiroh Report, ¶ 24.
[101] Stiroh Report, ¶ 24 footnote 41; JBS-0000409333–95 at 49.

about a lack of "a rigid or common price structure" and demonstrates that all three contract types used by JBS are expressly tied to industry-wide market prices.[102] The document states that JBS has three types of pricing: Spot Pricing, Formula Pricing, and Forward Pricing/Fixed Pricing.[103] "[redacted]"[104] Formula prices are also "[redacted]."[105] And prices for forward/fixed contracts "[redacted]"[106] which Steve Cohron, then-Head of Pricing and Sales at JBS USA Fed Beef, testified [redacted][107] Thus, regardless of the pricing mechanism, all JBS prices mainly move with the market price for beef.

84. Dr. Stiroh then turns to Cargill and cites testimony from executive Hal Sankey, whose titles were Vice President of Beef Pricing and then Beef Pricing Strategist, that [a]ll pricing or even formula negotiations are basically "[redacted]" and that it involves "[redacted]"[108] But Dr. Stiroh fails to mention critical information: Cargill's prices were tied to USDA pricing or were negotiated in the context of the prevailing market price, meaning the market price was a factor in any "bid and quibble." For instance, in discussing a 2018 pricing report, Mr. Sankey testified that Cargill had a target of 20 percent for spot sales, which were based on the prevailing market price.[109] Mr. Sankey also testified that Cargill's formula sales (target of [redacted] percent) were tied to reported USDA cutout prices and Cargill's formula prices would update automatically with the release of the USDA LM_XB459 report.[110] Mr. Sankey further testified that ceiling sales (target of 3 percent) were "[redacted]"[111] agreed that negotiated-out-front sales

[102] Stiroh Report, ¶ 24 footnote 44; JBS-0000806473–78 at 74–75.
[103] JBS-0000806473–78 at 74–75.
[104] JBS-0000806473–78 at 74.
[105] JBS-0000806473–78 at 75.
[106] JBS-0000806473–78 at 75.
[107] Deposition of Steven Cohron, Jan. 7, 2025, pp. 24:23–26:25.
[108] Stiroh Report, ¶ 24.
[109] Deposition of Hal Sankey, Nov. 6, 2024, pp. 54:11–55:1; CARGILL000836278–288 at 281.
[110] Deposition of Hal Sankey, Nov. 6, 2024, pp. 55:2–56:4; CARGILL000836278–288 at 281.
[111] Deposition of Hal Sankey, Nov. 6, 2024, p. 57:20–24; CARGILL000836278–288 at 281.

(target of 20 percent) "[redacted]"[112] and explained that plan-pricing-program sales (target of 2 percent) similarly were based on the market price at the time and may have even used historical USDA prices to forecast a future price.[113] Nicholas Birkhofer, Cargill's Pricing Manager for Beef from 2018 to 2022, testified that "[redacted]"[114] The first "[redacted]"[115] The second "[redacted]"[116] For spot negotiations, "[redacted]" Mr. Birkhofer said he and his team would "[redacted]" as one of "[redacted]"[117]

85. Dr. Stiroh next cites testimony from National Beef executives that formula contracts "[redacted]"[118] and argues that "[redacted]"[119] And then in the context of those individually negotiated transactions, she includes a quote from Bill McLaurin, National Beef's Vice President of Pricing, and concedes that "[redacted]"[120] In fact, when asked about the process for setting beef prices, Mr. McLaurin testified, "[redacted]."[121] Dr. Stiroh ignores this critical testimony showing that *all National Beef prices used the USDA cutout prices as a baseline*

[112] Deposition of Hal Sankey, Nov. 6, 2024, pp. 57:17–59:9; CARGILL000836278–288 at 281.
[113] Deposition of Hal Sankey, Nov. 6, 2024, pp. 59:16–61:23; CARGILL000836278–288 at 281.
[114] Deposition of Nicholas Birkhofer, Dec. 12, 2024, p. 163:10–11.
[115] Deposition of Nicholas Birkhofer, Dec. 12, 2024, p. 163:10–16.
[116] Deposition of Nicholas Birkhofer, Dec. 12, 2024, p.163:22–23.
[117] Deposition of Nicholas Birkhofer, Dec. 12, 2024, pp. 164:23–165:1.
[118] Stiroh Report, ¶ 24, footnote 48.
[119] Stiroh Report, ¶ 24.
[120] Stiroh Report, ¶ 24.
[121] Deposition of William McLaurin, Jun. 25, 2024, pp. 35:24–36:9.

price. When asked about National Beef's price lists that were sent to its customers, Mr. McLaurin explained again that [redacted][122] So, Dr. Stiroh again misses the larger point that "individually negotiated transactions" are still based on market prices (*i.e.*, USDA pricing), which are a function of the alleged conspiracy. Dr. Stiroh also cites National Beef's testimony that "inventory levels" are an important factor in determining price,[123] but she again fails to recognize that "inventory" is just part of "supply." In other words, the supply of beef—which is a function of the alleged conspiracy—is an important factor in driving beef prices.

86. Finally, for Tyson, Dr. Stiroh points to Tyson's use of "[redacted]"[124] Again, the specific share of sales Tyson made through the spot market is not relevant, because such negotiated prices are still based on market prices. Regardless, even the two documents she cites indicate that the share of spot sales was declining. The first document she cites shows that [redacted] percent of sales were formula and [redacted] percent of sales were negotiated.[125] She also ignores the large yellow box at the bottom of the slide stating Tyson's goal to increase formula sales to [redacted] percent in 2014.[126] The second document she relies on shows that by 2020, formula sales had increased to [redacted] percent of all sales, while negotiated sales decreased to [redacted] percent.[127] This is consistent with the increase in the percentage of business tied formulaically to USDA prices.[128] And it suggests that throughout the Class Period, the majority of Tyson's sales were formulaically tied to USDA pricing.[129] Dr. Stiroh also tries to argue, relying on the testimony of Todd Nogelmeier, that even sales tied to market prices "[redacted]

[122] Deposition of William McLaurin, Jun. 25, 2024, p. 332:15–21 [redacted]

[123] Stiroh Report, ¶ 24.

[124] Stiroh Report, ¶ 24.

[125] Tyson, "Boxed Beef Pricing" (presentation, Dec. 2013), slide 7 (TYSONBEEF02156223).

[126] Tyson, "Boxed Beef Pricing" (presentation, Dec. 2013), slide 7 (TYSONBEEF02156223).

[127] Tyson, "YTD F2020 – Boxed Beef Pricing" (presentation, Feb. 2020), slide 1 (TYSONBEEF02438065).

[128] *See* Deposition of Todd Nogelmeier, Jun. 28, 2024, 210:8-212:24; Tyson, "Boxed Beef Pricing" (presentation, Dec. 2013), slide 7 (TYSONBEEF02156223); Tyson, "YTD F2020 – Boxed Beef Pricing" (presentation, Feb. 2020), slide 1 (TYSONBEEF02438065).

[129] Deposition of Todd Nogelmeier, Jun. 28, 2024, p. 217:10–16 [redacted]

[redacted][130] But she ignores that in the very testimony she cites, Mr. Nogelmeier explains how the prices would nevertheless be linked to USDA pricing.[131] Not mentioned by Dr. Stiroh is that Don Kieffer, Tyson's Vice President of Pricing, testified he considers published USDA pricing when setting Tyson's beef prices, regardless of pricing mechanism.[132] These examples all demonstrate that Tyson's prices were tied to the "market price" for beef.

87. Dr. Stiroh concludes that "there is no pricing mechanism that would necessitate that the purported price increase be spread to all sales and to all customers."[133] This is simply incorrect: *all of the pricing mechanisms* Dr. Stiroh mentions would spread a price increase to all customers, because sales are all fundamentally based on the market supply of beef, which is affected by the alleged conspiracy.[134]

IV.A.5. Dr. Stiroh's Suggestion that Buyer Power Increased is Inconsistent with Market Outcomes

88. Dr. Stiroh also argues that the decisions of direct purchasers would lead to the avoidance of anticompetitive impact. For example, Dr. Stiroh mentions sales and promotions run periodically by downstream customers, as well as opportunistic purchases on the spot market made by "smaller customers."[135] Dr. Stiroh's recitation of the strategies from [redacted] and other such purchasers is meaningless, because such actions and strategies would exist in both the actual and but-for world. Smaller

[130] Stiroh Report, ¶ 24, footnote 52.

[131] Deposition of Todd Nogelmeier, Jun. 28, 2024, p. 217:17–21 ("[redacted], 221:14–16 ("[redacted]").

[132] *See* Deposition of Donald Kieffer, Mar. 21, 2024, p. 143 [redacted]), p. 243 ("[redacted]

[133] Stiroh Report, ¶ 24.

[134] It is important to note that a) not all customers buy beef in identical quantities and at identical times, and b) reference values like the USDA cutout prices go up and down over time, based on changes in supply and demand. Accordingly, it is possible that a customer who buys relatively more during a period of rising prices will pay, on average, more than a customer who buys relatively less during the same period. This reality makes precise, customer-by-customer comparisons of prices particularly challenging. The presence of some fixed-price contracts further complicates this. Ultimately, the point is, changes in the USDA cutout prices will affect all customers at the time they purchase beef.

[135] Stiroh Report, ¶ 26.

(or any sized) customers are always likely to make opportunistic purchases on the spot market to "stay competitive" in the marketplace, because that is how every business operates and tries to earn a profit. But for Dr. Stiroh's argument to have any merit, it must be the case that all of these customers somehow increased their bargaining power during the alleged conspiracy.

89. This proposition is highly implausible; I am unaware of any evidence indicating a sudden change in bargaining power coinciding precisely with the alleged conspiracy took place for any single customer, let alone multiple, many, or all customers. Yet, in her report, Dr. Stiroh appears to speculate that this may have actually happened.[136] Specifically, she cites a figure in her report (reproduced below) notes that the average price of beef "was trending up in the years prior to the start of the Class Periods and began to decline in mid-2015," and that this "is consistent with individual customers being able to negotiate better pricing during the alleged Class Period than they had negotiated prior to the Class Period."[137]

Figure 12. Dr. Stiroh's Figure 2.3: Average Beef Price Per-Pound, for Class Products Sold to Direct Purchasers

90. In other words, it appears that Dr. Stiroh is attributing the decline in average beef prices from mid-2015 until early 2017 to a widespread change in customer bargaining power. First, I note that a simple line chart of prices, without any control for changes in demand, costs, or the cattle supply, is meaningless in this context. Regardless, even taking Dr. Stiroh's interpretation of

[136] Stiroh Report, ¶¶ 26–29.
[137] Stiroh Report, ¶ 23.

the decline in average prices during 2015 and 2016 at face value, she does not explain or offer any economic logic or justification for where this sudden increase in bargaining power came from, nor does she explain how or why they all suddenly lost that increased bargaining power in 2017, when beef prices started going up again. In reality, there was no drastic change in customer bargaining power. If it were the case that customers' bargaining power was growing during this period, this would manifest itself in the decreased margins earned by Defendants. However, as I showed in the Mangum Class Report, that is not what happened. Defendants' costs began decreasing at a faster rate than their prices were falling, leading to steady increases in their margins from 2014 to 2016—right when Dr. Stiroh suggests they were (supposedly) losing ground to customers.

Figure 13. Growth in Defendants' Profits During the Class Period [138]

[138] See backup production; Mangum Class Report, Figure 27.

IV.A.6. Dr. Stiroh's Examples of Routine Customer-Supplier Interactions Are Not Evidence of Avoided Impact

91. Dr. Stiroh includes several pages of text in her report dedicated to highlighting instances where various customers engaged in negotiations with Defendants to try and obtain better pricing.[139] This is exactly what virtually all companies do all the time, including the beef industry prior to the allegations in this case. This market reality is in no way a fact that inoculates customers from underlying price changes from a conspiracy. She highlights "bid and quibble" methods, pitting different suppliers against each other, utilizing spot markets to augment regular or formulaic purchases, and other interactions between Defendants and their customers. None of Dr. Stiroh's discussion of any of these topics has any relevance to the question of overcharges, the alleged conspiracy, or common impact. Bidding, negotiating, quibbling, and pitting suppliers against each other are common practices in any industry or marketplace with more than one supplier, and exist independent of the alleged conspiracy. In her deposition, Dr. Stiroh conceded that such pricing negotiations have existed in numerous cases she has worked on, and in which the court has certified the classes, despite her opposition.[140] In my experience as an economist, I have consulted on scores of matters, in antitrust and other areas of law, and pricing negotiations exists in virtually all of them. This is because firms rationally seek to maximize profits by paying as little as possible for inputs and charging prices as high as the market will bear. The market for beef is no different, in this regard. What matters, however, is that in the beef market, there is widespread transparency—due to USDA reporting—about the market value of beef products. Defendants themselves have touted the transparency of the beef market due to USDA reporting and pricing.[141] JBS's Al Byers bragged, "[ ]

[139] Stiroh Report, ¶ 24.

[140] Stiroh Deposition, pp. 223:25–224:19, 233:10–235:20 (referencing cases that were certified involving pork, turkey, and HDDs). Dr. Stiroh testified that she was uncertain whether customers and suppliers negotiated prices for chicken (pp. 223-233). Given that I also consulted and testified in the *Broilers* matter, I can confirm that there was extensive evidence of price negotiations between purchasers and suppliers for chicken, and that the court certified direct, commercial indirect, and consumer indirect classes.

[141] *See, e.g.*, Deposition of Jerry Holbrook, Mar. 13, 2024, p. 242 [ ]").

[redacted]."[142]

92. As I have previously explained, this means that customers may negotiate prices, but the prices they are negotiating are nevertheless in reference—either explicitly or implicitly—to the market value of beef (*i.e.*, the USDA cutout values), which are subject to the forces of supply and demand, as well as the anticompetitive forces of conspiracy.[143] If the alleged conspiracy has the effect of decreasing supply, it will lead to higher cutout and reference prices, regardless of individual customers' negotiating methods or the specific pricing mechanisms employed by Defendants. I discuss the specific examples Dr. Stiroh highlights in the section below.

IV.A.7. Dr. Stiroh's Hand-Picked Examples of Individualized Pricing Are Still Consistent with Common Impact

93. Even setting aside the lack of any evidence that any (let alone many, most, or all) specific customers exhibited changes in their bargaining power consistent with Dr. Stiroh's "theory," the specific examples she cites do not even provide any support for her opinion against common impact. That is, Dr. Stiroh's quotes related to direct purchasers like [redacted] (among others) merely provide evidence that these customers utilized the spot market.[144] But it is unclear why this would matter, because (as I have explained) the spot market is not immune to the alleged conspiracy—it is in fact a source of the market pricing (*i.e.*, the cutout) through which the conspiracy spreads to other purchasers.[145]

94. Dr. Stiroh cites several other documents which either contradict her or are not relevant. For example, Dr. Stiroh discusses differences between "[redacted]" and "[redacted]," and argues that the latter is "[redacted]

[142] Deposition of Al Byers, Aug. 5, 2024, pp. 225:20–226:2.

[143] Mangum Class Report, ¶ 86.

[144] Stiroh Report, ¶ 26.

[145] Even if the spot market was shielded from the conspiracy (which it certainly was not), none of Dr. Stiroh's quotes remotely indicate that these customers purchased beef *solely* through opportunistic buys on the spot market. Indeed, the quotes all appear to indicate that these customers simply augmented their regular beef purchases with opportunistic buys during periods of favorable pricing. The fact that some (or many, most, or even all) customers took advantage of periodic sales or other seasonal "lower markets" could not shield them from the impact of a market-wide conspiracy occurring over five-and-a-half years.

[redacted]"[146] But the testimony from [redacted] that Dr. Stiroh cites clearly indicates that these negotiations—even the ad business—were centered around market measures for beef products, namely USDA cutout values, which were impacted by the Defendants' alleged conspiracy.[147] Some of Dr. Stiroh's examples of price negotiation fall outside of the relevant period[148] or are outside of the scope of Class Products. For example, Dr. Stiroh cites [redacted] as an example of a customer that relied on "[redacted]."[149] While the email she cites does say this, 1) the email is outside of the Class Period, 2) appears to relate to shortages created by the COVID-19 pandemic's onset, and 3) still does not provide any evidence that spot purchases would be immune or even partially shielded from the effects of the alleged conspiracy on market-based prices in the beef industry.[150] Dr. Stiroh also cites [redacted] purchases of ground beef patties as an example of ad buys, but these are not Class Products.[151]

95. Finally, examination of Dr. Stiroh's individual overcharge table shows that virtually all of the customers she calls out as potential examples of strong negotiators who were willing to switch suppliers, shop around, or otherwise push for favorable pricing, have positive and statistically significant overcharges according to Dr. Stiroh's own misspecified tests of my model.[152] In

[146] Stiroh Report, ¶ 25, footnote 55.

[147] [redacted]

[148] [redacted]

[149] Stiroh Report, ¶ 26.

[150] [redacted]

[151] [redacted]

[152] Stiroh Report, Exhibit 7C.

this section of her report, Dr. Stiroh specifically mentions [redacted][153] All but one of these customers show positive and statistically significant overcharges in Dr. Stiroh's tests using individual customer subregressions, her customer interaction regression, or both.[154]

96. In summary, Dr. Stiroh has done nothing more than provide examples of routine business negotiations between Defendants and beef purchasers. Such negotiations took place during the benchmark period and Class Period, and these negotiations—regardless of the particular pricing mechanism—were subject to the same market conditions and reference prices (*i.e.*, USDA cutout values), and therefore were not shielded or immune to the effects of the alleged conspiracy. To the contrary, they were fully subject to the effects that Defendants themselves intended: higher cutout values than would have prevailed in the but-for world. Further, Dr. Stiroh's hand-picked examples of such negotiations fail to provide any material evidence of ability to avoid anticompetitive impact. Indeed, her own (flawed) analysis shows that these customers were virtually all affected.

IV.A.8. Dr. Stiroh's Analysis of Price Dispersion is Meaningless

97. Dr. Stiroh claims that there is not uniform pricing either across customers or products and incorrectly concludes that an economic model that does not account for all of the price dispersion that she finds, cannot assess antitrust impact.[155] I discussed above that the existence of price dispersion does not mean some purchasers were affected while others were not.

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[153] Stiroh Report, ¶¶ 24–28, including footnotes. Dr. Stiroh also mentions [redacted] but these entities are excluded from her analysis because they had no purchases in the benchmark period. It is my understanding that both of these companies have purchased beef through [redacted] which does show a positive and statistically significant overcharge in Dr. Stiroh's subregression analysis.

[154] [redacted] shows a positive overcharge estimate in both of Dr. Stiroh's subregressions, but neither is statistically significant. [redacted] does show a positive and statistically significant overcharge in Dr. Stiroh's subregressions based on models presented by Dr. Sunding and Dr. Williams (Stiroh Exhibit 7A and Exhibit 7B). All other customers except [redacted] have positive and statistically significant overcharges in both of Dr. Stiroh's subregressions. [redacted] shows a positive and statistically significant overcharge only in Dr. Stiroh's customer-interaction subregression. [redacted] does not appear in Stiroh Exhibit 7C, because its purchases during the Class Period were made through [redacted] However, I note that [redacted] shows a positive and statistically significant overcharge in Dr. Stiroh's analysis.

[155] Stiroh Report, ¶ 24.

98. Further, Dr. Stiroh exaggerates the degree of price dispersion in her report based on her selection of customers and products. To support her claim of "substantial variation in pricing" among customers that "purchased the same product from a specific Defendant," Dr. Stiroh presents an example of monthly average prices for Beef Center Cut Back Rib products sold by National Beef to ten customers.[156] It is unclear how Dr. Stiroh selects the product that she plots in her Figure 2.4 and Exhibit 4A, which does not make the top 500 products sold by defendants.[157] In fact none of the 6 products Dr. Stiroh plots to demonstrate "price dispersion" rank in the top 100 products. [158] **Figure 14** below shows the prices paid by customers for the top beef products sold. The pricing patterns for the remaining top 100 products are included in **Appendix C**.[159] These plots show how closely prices generally tracked for individual products, and that Dr. Stiroh's cherry-picked examples do not reflect the general pricing behavior.

[156] Stiroh Report, ¶ 29 and Figure 2.4. Dr. Stiroh does not explain why she selected the products she plots in Figure 2.4 or Exhibit 4A. However, I note that all of these customers included in Dr. Stiroh's individual overcharge regression analysis are shown to have paid an overcharge in at least one of Dr. Stiroh's 6 individual customer subregressions. [redacted] is not included in that analysis, as it did not have any purchases during the Class Period. However, [redacted] which purchased beef on behalf of [redacted] during the Class Period, does show a positive and statistically significant overcharge in Dr. Stiroh's analysis. See Stiroh Exhibit 7C and backup materials.

[157] The product she plots in Figure 2.4 is ranked 507 based on pounds sold.

[158] The 6 products are each ranked 125, 150, 239, 271, 507, and 582 by pounds sold. See backup production.

[159] See backup production.

Figure 14. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products[160]

99. Further, Dr. Stiroh relies on data for customers who purchased during different timeframes, with almost no overlapping time periods between them, and proceeds to draw conclusions about their relative "pricing dynamics" *over time*.[161] If Dr. Stiroh instead examined the prices for the same product across different customers *over the same time periods*, she would have seen that prices exhibit similar patterns (see **Figure 15** below)

[160] See backup production.

[161] Stiroh Report, ¶ 29 and Figure 2.4. Dr. Stiroh's Figure 2.4 includes sales to [redacted] that are recorded for 2010 – early 2013; sales to [redacted] that are recorded from mid-2013 through mid-2016, and sales to [redacted] that are mostly recorded from early 2015 through 2020.

Figure 15. Modified Stiroh Figure 2.4: National Beef Average Price for Beef Center Cut Back Rib Products, Limited to Overlapping Timeframes ([redacted])

100. While some degree of dispersion in beef prices does exist, Dr. Stiroh exaggerates it through her chosen presentation format and combination of customers. To support her claims of "substantial variation in pricing" across the customers, Dr. Stiroh presented Figure 2.4 and five more charts in her Exhibit 4A, thus limiting her review to only six products and a total of 44 customers.[162] Beyond those price charts, she did not perform any statistical analyses to support her claims of price variation. In the Mangum Class Report, however, I performed correlation analyses across different customers, for which I analyzed top 50 customers (ranked by sales quantity), with the top 100 products for each such customer.[163] I demonstrated that price indices for different customers buying the same primal follow very similar trends over time, which is supported by correlation coefficients over 0.9 for the majority of the top 50 customers.[164]

[162] Stiroh Report ¶ 29 and backup to Stiroh Figure 2.4. 31 customers appear in this review with only one product, 10 customers – with two products each, and three customers are reviewed with three products each.

[163] Mangum Class Report, Figures 42–49.

[164] Mangum Class Report, Figures 42–49.

101. Dr. Stiroh further claims that "different price patterns [were] also evident" across different Defendants as she attempted examining groups of similar products.[165] While she claimed that "[t]hese pricing dynamics and the individualized nature of these transactions lead to the possibility of unharmed customers", she misrepresented the fact that the pricing patterns she presented in her Figure 2.5 and Exhibit 4B were in fact product-specific price disparities.[166] Dr. Stiroh plots different products that she identifies as falling under the product categories of "Strip Loin" (Figure 2.5) Chuck Tender, Tenderloin, Back Ribs, Bottom Round, Chuck Eye, and Chuck Short Rib (Exhibit 4B). It is unclear how she selects the products, aside from to simply handpicking products that include potentially relevant terms in the description. This is significant because 1) she is not purporting to identify representative or top selling products and 2) she is not identifying the parameters of the "product" she's comparing (or why it is an appropriate comparison). For instance, she appears to include both fresh and frozen products and highlights price variation when they have different pricing patterns. The two frozen Cargill products ("[redacted]" and "[redacted]") show relatively stable pricing patterns compared to the other products (see also Dr. Stiroh's Exhibit 5 where these product price series are highlighted). This also results in Dr. Stiroh introducing price dispersion where none really exists. In her Appendix Exhibit 4B for the "Chuck Tender Products," Dr. Stiroh mixes the "chuck tender" subprimal cut with smaller cuts originating from this subprimal ("chuck tender *steak*" and "chuck tender *roast*"[167]) and further adds "*petite* tender," which introduces even more volatility to the chart. Limiting the chart to the *similar* products, in **Figure 16** below, produces very similar pricing patterns across Defendants.

[165] Stiroh Report, ¶ 30.

[166] Stiroh Report, ¶ 30.

[167] *See* "Chuck Tender," Cattlemen's Beef Board and National Cattlemen's Beef Association, accessed Apr. 3, 2025,https://www.beefitswhatsfordinner.com/cuts/cut/2753/chuck-tender.

Figure 16. Modification to Stiroh Exhibit 4B: Prices for Comparable Chuck Tender Products, by Defendant

102. Instead of cherry-picking a handful of products for cursory review as Dr. Stiroh did, I performed a correlation analysis of the Defendants' price indices for the four beef primal categories. Figures 32-33 in the Mangum Class Report indicate high degree of price correlation across Defendants.

IV.B. Dr. Stiroh's Claims that the "Nature" of the Alleged Conspiracy Precludes Common Impact Are Unsupported and False

103. Dr. Stiroh claims that the nature of the alleged conspiracy indicates that not all class members would be impacted, and that any impact would vary across class member, product and year.[168] Dr. Stiroh argues that "differences in slaughter patterns would be expected to have different effects on customers purchasing beef products from different Defendants or at different times and are unlikely to trigger a common price impact for all Defendants' products and

[168] Stiroh Report, Section II.A.

purchasers."[169] As support, Dr. Stiroh references textbooks and articles that discuss the nature of cartels and the incentive members have to cheat) and concludes that "if *all* cartel members cheat on the cartel agreement, prices and output in the industry revert to their competitive levels."[170] Dr. Stiroh's reasoning is flawed and misleading. As an initial matter, Dr. Stiroh has incorrectly framed an overcharge that has varying impact over time, products, or customers, as being inconsistent with common impact. This is simply incorrect; it is my understanding that common impact requires that all (or virtually all) customers be affected by the alleged conduct. It is not my understanding that all customers must be affected in identical magnitudes at all points in time.

104. Dr. Stiroh's arguments about cheating lack merit. The conceptual incentive to cheat exists in virtually every cartel, and yet cartels do exist, and conspiratorial effects have been found and measured, both in academic and legal settings.[171] Thus, the mere fact that an incentive to cheat exists does not mean conspiracy members will always act on that incentive, much less that they will always act on it to a degree that fully unwinds the conspiracy's effect on prices. To this point, Dr. Stiroh does not provide any support for her proposition that all Defendants may have cheated on the alleged conspiracy at the same time, identify any such periods of time, or that such purported cheating was effective. Regardless, as I discussed in the Mangum Class Report, Defendants had the ability to effectively monitor and discipline each other, which facilitates the maintenance and success of a cartel.[172] Dr. Stiroh wholly ignored all of this evidence in her report and did not respond or engage with it in any way. I also note that the articles Dr. Stiroh cites as support for the risk of cheating or cartel instability also state that these concerns are diminished when there is a high degree of concentration, few conspirators, strong monitoring capabilities, and a high degree of familiarity or trust, as is the case in this matter.[173]

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[169] Stiroh Report, ¶ 19.

[170] Stiroh Report, footnote 35, emphasis added.

[171] *See* Margaret C. Levenstein and Valerie Y. Suslow, "What Determines Cartel Success?" *Journal of Economic Literature* 44, no. 1 (2006): 43–95 (for a review of numerous cartels).

[172] Mangum Class Report, ¶¶ 115–179.

[173] Dr. Stiroh cites Christopher R. Leslie, "Trust, Distrust, and Antitrust," *Texas Law Review* 82, no. 3 (2004): 524–528. However, later in the article (p. 564–576), the author writes that "Firms in concentrated industries are more likely to attempt cartelization and are more likely to be successful than firms in less concentrated markets." The author further notes that "[t]ransparency facilitates trust" in the cartel, and that "use of a centralized reporting mechanism" helps cartels. Additionally, "[a]greements to share transaction prices increase transparency and thus

IV.B.1. Dr. Stiroh's Portrayal of "Episodic Impact" is Flawed and Misleading

105. In her report, Dr. Stiroh makes repeated references to what she claims are "episodic" conspiracy allegations. Dr. Stiroh asserts that Plaintiffs had pled an "episodic" conspiracy and concludes that such an episodic conspiracy would lead to periods of competitive pricing, and therefore a lack of common impact. In her own words:

> *Further, I understand that Plaintiffs in this matter allege the existence of a conspiracy involving periodic coordinated slaughter reductions. Such a conspiracy would reasonably be expected to affect beef prices only when beef sales were purportedly below the competitive level. [...] Even if Defendants engaged in conspiratorial conduct throughout the rest of the Class Period, periodic slaughter reductions imply that the impact of the alleged conspiracy, if any, would not be uniform throughout the duration of the Class Period. Moreover, if cattle slaughter returned to the competitive level between the periods of coordinated slaughter reduction, beef prices would similarly have returned to the competitive levels for at least part of the Class Period. Any putative Class Members who purchased beef products during periods when cattle slaughter was at the competitive level would not have been injured even if the conspiracy is true as alleged.*[174]

106. There are several major flaws in Dr. Stiroh's reasoning here. First, this is not a correct description of Consumer IPPs' allegations. While Consumer IPPs' allegations in this case do include episodes of coordinated slaughter reduction, those actions are levers of the broader commitment to "margin over market share" and an agreement not to exceed their target share of weekly fed cattle slaughter that existed throughout the Class Period. Second, Dr. Stiroh incorrectly assumes that prices following coordinated cuts to production would necessarily return to competitive levels before further cuts would be enacted. She does not provide any support for this conclusion. Third, and most importantly, Dr. Stiroh's claim that customers purchasing during periods of what she calls competitive pricing "would not have been injured" is fundamentally incorrect and relies on wholly unrealistic assumptions. I discuss each of these flaws in Dr. Stiroh's reasoning below.

'help solve the detection problem.'" … "Second, product standardization creates transparency," and "Research has long shown that cartels are more stable in homogeneous product markets." The presence and adoption of USDA pricing are precisely the type of reporting and transparency mechanisms that facilitate stable and successful cartels.

[174] Stiroh Report, ¶ 79.

IV.B.2. The Alleged Conspiracy is Not Episodic in Nature

107. Throughout her report, Dr. Stiroh redefines the alleged conspiracy as "episodic" in nature, and uses this characterization as part of the basis for her opinion against common impact.[175] In the Mangum Class Report, I discussed documents and evidence consistent with the allegations in this case. This evidence included numerous documents indicating that in order to maintain the alleged market share agreement, Defendants routinely restrained slaughter in a variety of ways throughout the Class Period, including pulled shifts (especially Saturday shifts),[176] changes to chain speed,[177] and the use of unplanned maintenance days.[178] I also reviewed evidence showing Defendants would punish those who ran too aggressively.[179] While the evidence shows that Defendants engaged in this type of behavior frequently throughout the Class Period, it is not my understanding that Defendants made production cuts every day or every week. As such, Dr. Stiroh argues that prices outside of such "episodes" would revert to competitive levels, enabling customers to make purchasers without being impacted by the alleged conspiracy. Dr. Stiroh's characterization of the alleged conspiracy as purely episodic in nature is incorrect, and thus her conclusion is also incorrect.

108. In addition to the documents and evidence I discussed in the Mangum Class Report related to slaughter restraints, I also highlighted the Defendants' alleged adoption of a "margin over market share" approach to their production and sales of beef.[180] As part of this, I understand (and I discussed in the Mangum Class Report) that Defendants are alleged to have agreed to stop competing with each other for market share, and instead target a certain percentage of

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[175] Stiroh Report ¶ 13.

[176] Mangum Class Report, Section III.B.5.b. (¶¶ 307–314, referencing JBS-0002516077 (Byers Exhibit 1371); Deposition of Al Byers, Jun. 26, 2024, pp. 180–181; TYSONBEEF01825774; TYSONBEEF00426665–666; NationalBeef-01695856; JBS-0002460801 (Rose Exhibit 1824); Deposition of Larry Rose, Jul. 23, 2024, pp. 208:2–209:6; JBS-0002461510–511; CARGILL000171373–375; CARGILL001386937–938).

[177] Mangum Class Report, Section III.B.5.c. (¶¶ 315–319, referencing JBS-0002176713; TYSONBEEF00406610; TYSONBEEF01811823–825 at 824; NationalBeef-00279460–464 at 462; TYSONBEEF02069072; TYSONBEEF02101786–787).

[178] Mangum Class Report, Section III.B.5.d. (¶¶ 320–324, referencing TYSONBEEF02158474; NationalBeef-00160611–616 at 613; NationalBeef-01109397; HALES270152; NationalBeef-00598747; HALES270330–331; HALES270580–581).

[179] *See* NationalBeef-00004142–143 at 142 (Klein Exhibit 2041) ([redacted]"); HALES204856 (JBS aggressively bidding on cattle in December 2015, "[redacted]"); TYSONBEEF01642792 [redacted]" after Cargill ran its Dodge plant on a Saturday). *See also* Mangum Class Report, ¶¶ 370–372.

[180] Mangum Class Report, Section III.B.3 (¶¶ 237–270).

weekly fed cattle slaughter.[181] Indeed, the starting point for my measurement of the impact of the alleged conspiracy is when National Beef permanently reduced its target market share to 12.5 percent (after previously informing its investors that it would be targeting a higher share of 13.25 percent).[182] This reduction came on the heels of [redacted][183] **Figure 17** below depicts each Defendant's actual and target market shares over time. As shown, Tyson's target market and actual market share declined over the course of the Class Period, while the targets and actual shares of the other Defendants remained largely unchanged. While JBS temporarily increased its target market share in 2015-2016, it significantly underperformed this target in most periods until ultimately reducing its target share in 2017 back to its prior level. Taken together, these charts indicate that Tyson was continually reducing its target market share, but that the other three Defendants were not trying to capture that reduction. This is consistent with the allegation that Defendants had agreed not to compete on market share in furtherance of the alleged conspiracy.

[181] Mangum Class Report, ¶ 294.
[182] Mangum Class Report, ¶ 278.
[183] Mangum Class Report, ¶¶ 277–278.

Figure 17. Defendant Target and Actual Market Shares[184]

109. Defendants' alleged agreement to restrain beef production by eliminating competition for market share is not episodic in nature, and would be expected to have an ongoing impact on the production—and therefore the price—of beef in the United States, regardless of what additional incremental or periodic production cuts were made by Defendants throughout the remainder of the Class Period. Moreover, the effect of such an agreement would also have an immediate effect on the output—and therefore the prices—of beef produced by Defendants.[185]

[184] See backup production.

[185] This is important, because Dr. Stiroh erroneously opines that the alleged conspiracy, even if true, would not have an immediate impact on beef prices (see Stiroh Report ¶ 78). Beef processors influence cattle supply, and thus beef supply, by virtue of their cattle purchasing decisions. Even if it could take some time for the long-term cattle supply chain to *fully* adjust to Defendants' decreased demand for cattle, voluminous evidence indicates that Defendants were capable of restraining beef production in order to exert influence on beef prices over the short run. In other words, Dr. Stiroh has not offered any explanation of why Defendants changed slaughter plans with the purpose of affecting prices, and claimed that they were effective to affecting prices, if they actually had no ability to affect prices.

110. Additionally, as discussed in the Mangum Class Report, the allegations in this case include Defendants' artificially restraining production capacity through keeping plants closed and restrictive covenants to prevent new competition. The removal of industry capacity—and the prevention of additional capacity from replacing it—would also have an ongoing, non-episodic impact on the production—and therefore prices—of beef in the United States.

IV.B.3. Dr. Stiroh's Assumption Regarding Periods of Non-Impact Is Wholly Implausible

111. Dr. Stiroh's theory of non-impact involves a three-step logic chain: (1) the alleged conspiracy was episodic in nature; (2) if the conspiracy is episodic in nature, there must be periods of time around the anticompetitive episodes, during which beef would be sold at competitive prices; and (3) therefore, customers who purchased during these purported competitive windows would not be impacted by the alleged conspiracy. As explained above, the first step in Dr. Stiroh's theory—reliance on a purely episodic theory of the alleged conspiracy—is flawed and contradicted by the facts of this case. Thus her conclusion—that customers purchasing "between" episodes of anticompetitive conduct would not be injured—is flawed from the outset. However, even if one were to wholly embrace Dr. Stiroh's inaccurate portrayal of the allegations and pricing in the beef industry, her conclusion of non-impact still does not follow logically.

112. First, Dr. Stiroh has assumed that prices would return to competitive levels following anticompetitive action. But in the context of a conspiracy, that is illogical—prices returning to levels unacceptable to the Defendants would be met with additional action. Further, many of the specifically identified restraints were not short-term, but were indefinite in nature.[186] For example, National Beef's decision to reduce its target slaughter was permanent—it remained in place throughout the Class Period.[187] Similarly, reductions in capacity like the closure of Tyson's Denison plant—and ensuring that previously idled capacity does not come back—would have an ongoing impact throughout the Class Period.

[186] *See* NationalBeef-01697714 (Lowe Ex. 1763) ([redacted]).

[187] National Beef's target slaughter increased proportionately in early 2019 when it purchased Iowa Premium's plant in Tama, Iowa in early 2019.

113. Second, and more importantly, for Dr. Stiroh's conclusion of non-impact to hold, it would not be sufficient for customers to purchase beef during the purported periods of competitive pricing. Rather, such customers must ***only*** purchase during such periods, and never purchase during periods of anticompetitive pricing. Unless customers are somehow able to only make purchases during periods of purported competitive pricing, they will still be impacted. Moreover, because customers are not informed by conspirators about episodes of anticompetitive conduct (much less the existence of the conspiracy in the first place), there is no basis to believe that customers would ever be able to time their purchases to always and exclusively coincide with periods of purported competitive pricing. It is extremely unlikely that even *one* customer could, through sheer luck or insider information, perfectly time their purchases in such a way; it is even more implausible to imagine that multiple, many, or most customers would be able to do so. Accordingly, this has no bearing whatsoever on the question of common impact.

114. The nature of the products and customers in this case ensure that this extreme hypothetical is virtually impossible to exist. The products at issue are cuts of fresh and frozen beef, and the direct purchasers are distributors, wholesalers, retailers, restaurants, and processors. Price changes that flow from conspiratorial conduct may impact the quantity purchased in a given period, but it is economically absurd to think that they would reduce purchases to zero.[188] As such, I would expect that most direct purchasers purchase beef during most time periods. Examination of the transaction sales data in this case reveals that this is correct.

115. Of the 254 direct purchasers in the grocery retail channel that bought beef products during the Class Period, over half (62.6 percent) bought during *every* month of the Class Period.[189] Of all direct purchasers, 83.5 percent made a purchase in at least half of the months during the Class Period and 93.3 percent made purchases in at least 10 different months during the Class Period.[190]

[188] That is, even if a purchaser knew about anticompetitive conduct, that would not guarantee that the purchaser would limit its purchases to periods of purported competitive pricing. The purchaser will buy more beef when its inventory is depleted and it needs more beef to meet its own customers' needs.

[189] See backup production.

[190] See backup production.

Figure 18. Share of Direct Retail Customers by Purchasing Frequency [191]

116. At the indirect level, retail customers also make frequent purchases of beef, greatly diminishing the likelihood that a consumer, *even if* they avoided impact on *one* purchase, would avoid impact across many, multiple or all purchases of beef products throughout the Class Period. Industry estimates show that retail consumers eat beef (excluding ground beef) one to four times a month, on average.[192] These estimates imply 65 to 260 purchases over the Class Period. Consumer IPPs would need to have avoided impact on *every* purchase to have avoided being harmed during the Class Period. Dr. Stiroh has not provided any support for there being periods of competitive pricing during the Class Period, but *even if* there was, the likelihood that any consumer, let alone multiple, many, or most consumers, would have bought beef *only* during

[191] See backup production.

[192] The average household makes four meals per week with meat and poultry, or 208 meals per year (*see* TYSONBEEF01241512–518, at 513). Beef is roughly half of retail meat sales (*see* HYEBEEF0000013595–629, at 598.) Ground beef is roughly half of beef retail sales (*see* Lee Schulz, "Ground beef demand remains strong," *Ag Decision Maker*, Mar. 2021, 6–8, *available at* https://www.extension.iastate.edu/agdm/articles/schulz/schmar21.html). *See also* TROYER_0000588391–396 at 392 "Sirloin and top round steaks are eaten 2-3 times a month… Tenderloin and ribeye are eaten only about once a month"; TYSONBEEF00690786–902, at 866; HYEBEEF0000013595–629, at 598.

these speculative and unsubstantiated periods, and therefore avoided impact throughout the Class Period, is miniscule.

117. Record evidence and public data I reviewed also supports these findings. For example, a 2023 survey that asked participants about the "frequency of fresh (not frozen) steak purchases" reported that 2.9 percent of consumers purchase fresh steak daily, 5.4 percent purchase fresh steak 4–5 times per week, 17.5 percent purchase fresh steak 2–3 times per week, 31.1 percent purchase fresh steak 1 time per week, 34.3 percent purchase fresh steak 1-2 times per month, and 8.9 percent purchase fresh steak less than 1-2 times per month.[193] A 2019 study showed similar results, with 5.37 percent of participants purchasing steak more than once a week, 16.35 percent of participants purchasing steak about once a week, 22.89 percent of participants purchasing steak about once every two weeks, 23.48 percent of participants purchasing steak about once a month, and 22.74 percent of participants purchasing steak less than once a month.[194] Moreover, a 2000 paper, which reported the findings from a survey of 631 respondents representing a wide range of demographics from all 48 continental U.S. states, shows participants consumed steak an average of three times per month.[195] A 2022 report published by the National Cattlemen's Beef Association, on behalf of the Beef Checkoff program and using data from the Consumer Beef Tracker, details the percentage of respondents who ate steak at home during the prior week.[196] In August 2022, 38 percent of respondents reported eating steak at home the prior week.[197] Over an eight-month period, the percentage of respondents who ate steak at home during the prior week ranged from 26 to 38 percent, with

[193] Annika J. Thies et al., "Consumer willingness to pay (WTP) for beef based on color and price discounts," *Meat Sci. 109597*, no. 217 (2024), at 2, Table 3, https://doi.org/10.1016/j.meatsci.2024.109597 .

[194] Ryan Feuz, F. Bailey Norwood & Ranjith Ramanathan, "The Spillover Effect of Marketing Discolored Beef on Consumer Preferences for Nondiscolored Beef," *Journal of Agricultural and Applied Economics,* no. 52 (2020),at 173, Table A1, https://doi.org/10.1017/aae.2019.39.

[195] Jayson L. Lusk and John A. Fox, "Consumer Valuation of Beef Ribeye Steak Attributes," Presented at the Am. Agric. Econ. Association Annual Meeting, Tampa, Fla., Aug. 2000, at Table 2, http://dx.doi.org/10.22004/ag.econ.21793.

[196] National Cattlemen's Beef Association, "Are Consumers Still Satisfied with Steak? An Update from the Steak Satisfaction Survey," Sept. 2022, at Figure 2, https://www.beefresearch.org/resources/market-research-planning/white-papers/are-consumers-still-satisfied-with-steak#:~:text=Consumer%20satisfaction%20remains%20the%20highest,home%20and%20at%20a%20restaurant.

[197] National Cattlemen's Beef Association, "Are Consumers Still Satisfied with Steak? An Update from the Steak Satisfaction Survey," Sept. 2022, at Figure 2, https://www.beefresearch.org/resources/market-research-planning/white-papers/are-consumers-still-satisfied-with-steak#:~:text=Consumer%20satisfaction%20remains%20the%20highest,home%20and%20at%20a%20restaurant.

32 percent or more of respondents eating steak the prior week in every month but one.[198] Two years later, in 2024, the data was similar, with 38 percent of consumers eating steak at home in the past week according to data from Consumer Beef Tracker.[199]

118. I explained in the Mangum Class Report that Defendants comprised 80 percent of market for fed cattle.[200] Counsel has asked me to calculate the likelihood that a Consumer IPP Class Member purchased *only* non-Defendant beef products.[201] Assuming each retail purchase of beef has a 20 percent chance of not being a Defendant's product based on Defendants' production of 80 percent of market for fed cattle, then the likelihood that a consumer who purchases beef once per month on average, buys *only* non-Defendant beef products throughout the Class Period is effectively 0.0 percent.[202]

119. Counsel has also asked me to evaluate the likelihood that a Consumer IPP class member purchased a Class Product that was made from meat other than fed cattle as was defined in my Class Report. [203] Essentially all of the raw or frozen beef primal products sold at grocery stores originate from fed cattle.[204] Meat other than fed cattle, such as that from culled cows, is generally tougher or leaner, so it is considered to have a "lower quality in a sense of tasting."[205]

120. While animals other than fed cattle do produce primals, those are mainly exported, made into jerky or sometimes ground beef, or served in marinated products.[206]

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[198] National Cattlemen's Beef Association, "Are Consumers Still Satisfied with Steak? An Update from the Steak Satisfaction Survey," Sep. 2022, at Figure 2, https://www.beefresearch.org/resources/market-research-planning/white-papers/are-consumers-still-satisfied-with-steak#:~:text=Consumer%20satisfaction%20remains%20the%20highest,home%20and%20at%20a%20restaurant.

[199] Beef Checkoff, "Trust, Taste, Trends," Feb. 3, 2024, https://www.beefboard.org/2025/02/03/trust-taste-trends/.

[200] Mangum Class Report, ¶ 104 and Figure 21.

[201] I note that, due to Defendants' influence in the market, purchasing non-defendant beef products would not eliminate impact. Because non-defendant beef is priced through the same market mechanisms (*i.e.*, USDA cutout values) that Defendants' alleged coordinated action affected, non-defendant beef would be overcharged by a similar magnitude.

[202] The likelihood of buying non-defendant beef for one purchase would be 20 percent. The likelihood of buying nondefendant beef for *every* purchase during the Class Period would be 0.2 ^ 65 = 0.0 percent. Evening assuming a beef customer buys once a year (instead of once a month), the likelihood would be 0.2 ^ 5.4 = 0.017 percent.

[203] Mangum Report, ¶ 28.

[204] National Cattlemen's Beef Association, "Understanding the Different Kinds of Beef in the Marketplace," accessed Apr. 4, 2025, https://www.beefresearch.org/resources/product-quality/fact-sheets/understanding-the-different-kinds-of-beef-in-the-marketplace (Some research shows that about 99 percent of beef in major retail supermarkets is from conventionally raised grain-fed cattle.); Deposition of Karl Skold, May 9, 2024 ("Skold Deposition") at 116:13–117:23.

[205] Deposition of Karl Skold, May 9, 2024, pp. 116:13–117:23.

[206] JBS-0000333516–765 at 618–620.

IV.B.4. My Overcharge Model Is Robust to Dr. Stiroh's Claims of "Episodic" Impact

121. Finally, in her discussions about the purported episodic nature of the alleged conspiracy, Dr. Stiroh fails to acknowledge the fact that I have already demonstrated how my model finds impact over time. As I showed in the Mangum Class Report, my regression model finds positive, statistically significant overcharges during the Class Period, even when measured using annual overcharge variables.[207] In the Mangum Class Report, I showed that the overcharge over the entire Class Period was approximately [redacted] percent, but I also showed, as a test of the robustness of my overcharge model, the results of examining the overcharge on a year-by-year basis.[208] The results of that robustness test show a positive and statistically significant overcharge in every period, from a high of [redacted] percent (in 2014 and 2015) to a low of [redacted] percent (in 2019).[209] The fact that the conspiracy-long estimate of [redacted] percent is slightly higher than these annual values is not surprising—it illustrates how regression estimates can become less precise when they are based on smaller samples. The shorter time frames over which the annual overcharges are estimated makes the estimates more sensitive to temporary shocks to the idiosyncratic error term[210] as well as the other explanatory variables.[211] Over a longer period of time, a regression model is able to more accurately disentangle the effects of the alleged conspiracy from the effects of changes in macroeconomic and other control variables. When the model is forced to effectively treat each year as a distinct set of observations, it conflates some year-specific variation in other control variables with the overcharge, leading to less precise estimates.

122. While it is my opinion that a single overcharge variable provides a superior estimate of the true overcharge (by virtue of having a larger sample of data over which to measure it), these annual

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[207] Mangum Class Report, ¶ 434.

[208] Mangum Class Report, ¶ 432 and Figure 50.

[209] Mangum Class Report, ¶ 434.

[210] The "idiosyncratic error term" is any random variation that impacts the dependent variable that is not explicitly modeled by the explanatory variables. It can be thought of as the influence of small and independent events.

[211] During my deposition, opposing counsel asked me several questions about the fact that the annual estimates were lower than the class-wide estimate (Mangum Deposition, pp. 186:5–195:24). The assertion that the annual overcharges should equal the Class Period overcharge on average completely ignores the fact of correlation between variables. When correlation exists between the annual dummy variables and other explanatory variables - in this case it would mean explanatory variables have different average values in different years - shocks to the other explanatory variables during a given year will impact the overcharge estimate for that year. For this reason, one should not expect the Class Period overcharge to be any sort of average of the annual overcharges.

results contradict Dr. Stiroh's claims that customers could have avoided impact due to her characterization of the alleged conspiracy. These results also contradict Dr. Stiroh's opinion that there could be no anticompetitive impact early in the Class Period due to the long-term nature of the cattle supply chain.

IV.C. Correlation Analysis Demonstrates That Prices Move Together Across Defendants, Geographic Locations and Customers

123. In the Mangum Class Report I conducted a range of correlation analyses in order to evaluate how prices move within an individual primal across customers, defendants and geographic regions.[212] I explained that the high correlation across these categories provided further evidence that no individual direct purchaser could have avoided the impact of the alleged collusion.[213]

124. Dr. Stiroh claims that my correlation analyses fail to establish class-wide impact because 1) correlation analysis is not, on its own, capable of establishing common impact and 2) my correlation analysis is flawed in that it masks variation in beef prices.[214] I explain below that correlation analysis is informative in analyzing common impact and that my price indices do not "mask" any meaningful variation, but rather *enable* a more reliable and informative analysis based appropriately on the factors that influence the price of beef.

IV.C.1. Correlation Analysis is Informative in Evaluating Common Impact

125. Dr. Stiroh agrees that "Beef product prices are influenced by similar sets of demand and supply factors; therefore, it is expected that the average prices or price indices of beef products would tend to move together."[215] However, she claims that correlation analysis itself is not capable of establishing common impact and that a high correlation in price says nothing about whether all, or substantially all, class members would have been impacted by the alleged conduct. [216]

126. As an initial point, I explain thoroughly in the Mangum Class Report and in this report that my correlation analysis is but one component of the economic evidence that supports my opinion

[212] Mangum Class Report, Section IV.C.
[213] Mangum Class Report, ¶ 399.
[214] Stiroh Report, Section VI.A.
[215] Stiroh Report, ¶ 87.
[216] Stiroh Report, ¶¶ 87–88.

of common impact and it does not (and does not need to) serve as standalone proof.[217] Dr. Stiroh's claim that correlation analysis alone is "insufficient" is entirely misguided since, in addition to my correlation analyses, I also base my finding of common impact on (at least) 1) my analysis of the market-based pricing that is typical for direct purchasers; 2) evidence that individual negotiations and contracts would also be impacted; 3) the market structure characteristics that made it difficult for customers to avoid price increases; 4) my direct overcharge model establishing and estimating the impact direct purchasers incurred on purchases of beef products; 5) the industry analysis inherent in the structure of my well-specified direct overcharge regression, which appropriately accounts for the relevant economic factors impacting beef prices and does not omit any variables, such as customer-specific impact measures; 6) robustness checks of my direct overcharge regression, including annual and industry-wide variations; 7) overcharge sensitivities discussed above that support the finding of impact across a range of potentially relevant economic delineations, including by primal, customer type, and region; 8) my pass through analysis including well accepted economic theory of pass through and qualitative evidence of the behavior of distributors, redistributors, and grocery wholesalers; and 9) my pass through regression analyses showing high levels of pass through across 35 separate third parties which collectively account for approximately 70 percent of direct purchases in the relevant distribution channels.[218]

127. Second, correlation analysis is in fact informative in evaluating whether impact may be class wide. The high correlation of prices across Defendants indicates that individual customers would be unable to avoid impact from the alleged conspiracy by switching to other Defendants.[219] Likewise, the high correlation across geographic locations indicates that the effects of the alleged conspiracy would be nationwide, rather than limited to any particular geography.[220] Finally, the high correlation across individual customers indicates that there is a structure to prices, such that individual customers would not have avoided impact from the

[217] *See* Mangum Class Report, ¶ 407.

[218] Mangum Class Report, ¶¶ 483–491. In the Mangum Class Report, I analyzed data from 32 third parties which accounted for 64 percent of commerce in the relevant distribution channels. In this report, I include data and analysis for an additional 3 third parties, which brings the total to 70 percent of relevant commerce. See backup production.

[219] Mangum Class Report, ¶ 404.

[220] Mangum Class Report, ¶ 405.

alleged conspiracy, even in the presence of price dispersion.[221] This empirical analysis is not intended to isolate the damages resulting from the alleged conspiracy, but to demonstrate that, to the extent damages exist (which I separately determine using my direct overcharge models), they would be widespread and common across the direct purchasers.

128. Dr. Stiroh appears to insist that because beef prices are affected by common major economic factors, such as feed costs, the prices of substitute proteins, and economic recessions, that price correlation analysis cannot be informative when evaluating responses to another common factor, such as a conspiracy. I see no logical or economic basis for this assertion. Additionally, Dr. Stiroh does not explain why she would expect beef prices to respond in a uniform manner to a supply restriction resulting from a demand shock, but nonetheless vary wildly in response to a supply restriction resulting from a conspiracy. High correlation establishes that prices move together across meaningful dimensions such as Defendant, geography, and customer, showing that, while the price changes may differ depending on the cause (cost increase, supply restriction, economic recession), customers who purchased different beef products from different Defendants, or across different states, are unlikely to have avoided impact. The value and use of correlation analysis is to investigate whether two data series (such as prices paid by two different customers) move together. This type of analysis is relevant in a case where questions of common impact are important. Dr. Stiroh seems to be questioning the well-established value of correlation analysis.

129. Dr. Stiroh constructs a hypothetical scenario where all products increase in price by 2 percent overtime due to market factors, but some products are also subject to an additional 10 percent overcharge.[222] Dr. Stiroh explains that both groups of products would still exhibit a high degree of correlation. Dr. Stiroh's hypothetical example is constructed to have high correlations yet not have common impact. If two data series are highly correlated, adding a constant value or a fixed percentage to one of those data series will not change the resulting correlation coefficient. This outcome is a mathematical reality, based on the formula for the computation of a correlation. The reason that high correlations are relevant for a finding of common impact is that it indicates that customers' prices are determined by the same, or highly similar, market

[221] Mangum Class Report, ¶ 406.
[222] Stiroh Report, ¶ 88.

forces. Therefore, it is not likely that individual customers would have been able to avoid the force of the higher market prices due to the alleged supply restriction. Dr. Stiroh *agrees* that "[b]eef product prices are influenced by similar sets of demand and supply factors."[223] Her finding that it is mathematically possible for correlations to be high without common impact in her constructed hypothetical is uninformative because it ignores whether such hypothetically fabricated data series are realistic.

IV.C.2. My Correlation Analysis Does Not Mask Price Variation

130. Dr. Stiroh's second criticism of my correlation analysis is her claim that my methodology masks variation in beef prices and is therefore "not informative about whether prices for all products and customers changes in a similar manner."[224] She presents four charts, based on Dr. Sunding's data, that compare the price index for a primal to all the transaction prices for individual products within that primal category. Dr. Stiroh claims that "there is substantially more variation in actual prices than is captured in the calculated price indices" and extends this criticism to my and Dr. William's correlation analysis as well.[225]

131. The monthly price index that I use in my correlation analysis does contain *less* variation than individual transactional prices, but Dr. Stiroh's argument that it "masks" variation is misleading. The construction of price indices and some level of aggregation is necessary for the correlation analysis to be functional and reliable. It would not be more informative to compare price patterns using completely disaggregated data due to gaps in the data arising from customer purchasing patterns, discontinued products, or changes in geographic availability; all of which would reduce the number of pair-wise observations and likely preclude data sufficiency. The price indices I create enable me to *better* identify and evaluate the underlying trends and relationships, controlling for changes in product mix and other relevant factors. Dr. Stiroh simply pointing to the existence of price variation (especially when the variation is due to factors, like product mix, for which I control) does not indicate a weakness in my correlation analysis.

[223] Stiroh Report, ¶ 87.
[224] Stiroh Report, ¶¶ 90–91.
[225] Stiroh Report, ¶ 91.

132. Further, Dr. Stiroh does not propose any meaningful dimension or economic factor that is not controlled for in my analyses. For my customer correlations, for instance, I look at prices by month, product, grade, primal, Defendant, and individual customer, and run separate regressions for each top customer using product, grade, primal and Defendant-specific fixed effects. This means that for each customer, I generate a monthly price index which controls for variations in price based on differences in products and Defendants. I then run correlations between the individual series for each customer. Dr. Stiroh does not mention any additional dimension (aside from product, primal, grade, customer and Defendant, which I include) that she claims influences price, let alone provide any support for further disaggregation. She simply points to the variation of product prices within the same primal as evidence that transaction-level variation exists. Differences in price due to product differences (even if it's within the same primal) do not constitute unexplained price variation since my correlation index controls for product mix. Dr. Stiroh's claim that I am "grouping and averaging prices" or that my price indices "masks" price variation is therefore entirely misguided.[226]

V. My Direct Overcharge Regression Model is Properly Specified and Provides a Reliable Estimate of Class-Wide Overcharges

133. In the Mangum Class Report, I discussed the use of econometric techniques to estimate overcharges in class action antitrust matter like this case. After explaining the design and specification of my regression model for this matter, as well as the data used for estimation, I showed that my econometric model is appropriate and capable of estimating overcharges on a class-wide basis.[227] Dr. Stiroh claims, first and foremost, that my direct overcharge model is not capable of identifying uninjured class members, and that "[a] model with a common, unvarying, overcharge across all customers *imposes* rather than tests class-wide impact."[228] Dr. Stiroh goes on to "test" my direct overcharge model by conducting individual customer subregressions. Dr. Stiroh also claims that my overcharge model is flawed because it cannot

[226] Stiroh Report ¶ 91.

[227] *See* Mangum Class Report, Section V.

[228] Stiroh Report, ¶ 13 and Section III.A.

replicate the prices paid by customers in the real world,[229] is not consistent with the length of the supply chain,[230] and fails to account for the price of fed cattle. [231]

134. I explain in this section that Dr. Stiroh's criticisms are unfounded. My direct overcharge regression model properly estimates class-wide impact, is consistent with the alleged conspiracy and beef supply chain, and appropriately accounts for the effect of fed cattle prices. Dr. Stiroh misleadingly quotes and inappropriately applies economic standards in an attempt to justify her flawed "tests."

V.A. My Direct Overcharge Regression Model Properly Estimates Class-Wide Impact

135. Virtually all of Dr. Stiroh's criticisms concerning regression models and overcharge estimation fail fundamentally to address the analysis that I conducted and discussed in the Mangum Class Report. In her report, Dr. Stiroh claims that a "[redacted]"[232] Dr. Stiroh either entirely misunderstands or wholly discards the actual purpose of my regression model. She instead applies a different purpose and then declares that my model fails to accomplish her new purpose.

136. My direct overcharge regression model is one input into my conclusion that impact would be common to the proposed class. My regression model enables me to determine whether there was *impact*, and if so, to estimate the magnitude of that impact to the Class. Further as I explained above, my overcharge model does support my finding of common impact since it is well-specified and does not omit any necessary customer-specific controls.

137. Further, the estimated overcharge is based on an econometric analysis of all direct purchasers' data and therefore is critical to concluding that impact would be class-wide. Each observation in the data represents a price paid by a direct purchaser for beef; if those observations did not exhibit elevated prices, after controlling for nonconspiratorial factors of supply and demand, my regression model would not show an overcharge. In other words, the fact that the combined

[229] Stiroh Report, Section V.A.
[230] Stiroh Report, Section V.B.
[231] Stiroh Report, Section V.C.
[232] Stiroh Report, ¶ 13 and Section III.A.

data from all customers shows an overcharge is, in and of itself, evidence against the "Theory of Many Zeroes" that Dr. Stiroh speculates may be true.[233] As I show in other areas of this report, numerous other analyses can be utilized to further demonstrate how implausible the Theory of Many Zeroes really is, and how my conclusion of common impact is correct.

138. In addition to my overcharge regression model, my finding that impact is common to the Class is also based on my regression robustness checks and all the other analysis, facts and evidence discussed in the Mangum Class Report. Specifically, I reached my conclusion about common impact on the basis of 1) Defendants' domination of the beef market; 2) the existence of high barriers to entry that prevent new suppliers from quickly entering the market and providing competitive pressure; 3) the commodity-like nature of beef products, which makes competition primarily about price; 4) the widespread use of market-based pricing mechanisms; 5) the inelasticity of demand for beef products; 6) the presence of pricing structures that persist across Defendants, products, and geographies; and 7) my analysis of pass-through to downstream customers. All these factors lead to a situation in which individual customers would be unable to avoid paying supracompetitive prices for beef during the Class Period.

139. Dr. Stiroh's repeated assertion that my overcharge model needs to be "tested against individual customers"[234] to avoid "assuming the conclusion it seeks to establish" is incorrect. Dr. Stiroh is unilaterally imposing a burden—that common impact be determined solely through separate subregressions for each individual customer—that is neither necessary nor economically logical or feasible.[235] As described above, my finding of common impact relies on my overcharge estimation *in connection with* numerous other analyses, facts related to the structure of the marketplace, and case-specific evidence. That is, it is not meant to be taken as a standalone analysis of common impact, devoid of consideration of all other case-specific evidence.

[233] See **Section IV** of this report for a discussion of this topic.

[234] Stiroh Report, ¶¶ 35–36.

[235] "Estimating a model that allows each purchaser to experience a different price increase, for example, requires data that are rarely available." ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues*, 2nd ed. (ABA Book Publishing, 2014), p. 359.

V.B. My Regression Model is Consistent with the Alleged Conspiracy and the Beef Supply Chain

140. Dr. Stiroh argues that my regression model is inconsistent with industry characteristics, "including the length of the supply chain."[236] In making this argument, Dr. Stiroh argues that, because of the long duration of the supply chain for live cattle, "it would be implausible for the conduct at issue to have caused a reduction in the supply of beef products beginning as early as August 2014."[237] Per Dr. Stiroh, the supply of beef "would require at least two years to fully adjust to a reduction in demand from beef processors."[238] Dr. Stiroh then concludes:

[redacted][239]

141. There are multiple problems with Dr. Stiroh's opinions here. First, Dr. Stiroh is selectively ignoring the mechanisms of the alleged conspiracy. As previously discussed in the Mangum Class Report, there is substantial evidence that Defendants took actions in 2014 and 2015 that, according to Defendants' own descriptions, had an impact on prices for beef and their own margins.[240] So Dr. Stiroh's conclusion that there could be no impact is directly contradicted by record evidence.

142. Second, as with her insistence on individual customer subregressions, Dr. Stiroh is, without any basis, unilaterally attempting to change the standard for common impact: rather than asking whether all customers are impacted, she has changed the question to whether all customers are impacted *identically at all points in time*.[241] I am unaware of any requirement,

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[236] Stiroh Report, Section V.B.

[237] Stiroh Report, ¶ 75.

[238] Stiroh Report, ¶ 76.

[239] Stiroh Report, ¶ 78.

[240] *See* Mangum Class Report, Section II.

[241] Dr. Stiroh actually takes it even further—she suggests that common impact requires overcharges that are identical across all customers, time, and products ("… impact, if any, would vary by class member, product, and year."). *See* Stiroh Report, Section II.A. Taken at face value, this means that Dr. Stiroh would require a subregression for every year-customer-product combination. Even if "product" in this instance is limited to the four "primals" of beef (as opposed to the thousands of individual product IDs), for a class with 4,000 customers over 6 annual periods, it would be necessary to estimate 4,000 x 4 x 6 = 96,000 individual customer subregressions.

legally, logically, or economically, that common impact requires that the alleged conspiracy have a "uniform" effect on beef prices "for the entire duration" of the Class Period. As a hypothetical, assume that the alleged conspiracy initially affects prices by (say) 3 percent, but grows in effectiveness over time to a maximum of 6 percent, and then wanes to an effect of 4 percent by the end of the Class Period. In this scenario, all customers are clearly affected, but Dr. Stiroh's self-created and self-imposed criteria of instantaneous and uniform impact at all times will not be met.

143. Third, Dr. Stiroh's criticism is based on the false assumption that Defendants had no ability over the short run to affect the quantity of cattle being slaughtered or the volume of beef being sold.[242] As I discussed in the Mangum Class Report, Defendants themselves undertook actions specifically intended to alter the supply of beef available for purchase in the immediate and medium term.[243] Further, Defendants applauded the apparent effectiveness of such efforts. If Defendants had no ability to affect supply, it would make no sense whatsoever for them to engage in cuts to production. For example, in July 2014, Monte Lowe (of National Beef) told industry participant Julian Leopold that National was "[redacted]" to "[redacted]"[244] Thus, National Beef—the smallest of the four Defendants—clearly believed and acted as if its reductions to slaughter would have an effect on the supply of beef and Defendants' margins, even over the short run.[245]

144. Fourth, in the Mangum Class Report, I showed that my model is robust to a specification that estimates overcharges by year.[246] While this approach to overcharge estimation is neither necessary nor superior to a single overcharge variable, it is nevertheless evidence that directly

[242] Stiroh Report, ¶¶ 67–78.

[243] *See* Mangum Class Report, Section III.

[244] NationalBeef01697714 (Lowe Ex. 1763).

[245] Further, such reductions would also send signals and have effects that would affect the larger supply chain and contribute to longer-term, longer-lasting impact. For example, if Defendants reduce slaughter, cattle already in feedlots may remain there for longer than anticipated. In response, feedlots may not accept as many incoming feeder cattle in the same period, causing them to continue grazing, which in turn could reduce the demand for calves, and ultimately lead to further reduced future cattle production. See, for example, Drovers, "Counting Cows: Drought, Costs Will Drive Further Reductions," available at https://www.drovers.com/news/industry/counting-cows-drought-costs-will-drive-further-reductions (Kansas State University economist Glynn Tonsor explains "I think we would have shrunk the herd a little even without the drought magnifier, simply because of the price signals ranchers were seeing.")

[246] Mangum Class Report, ¶ 490.

contradicts Dr. Stiroh's assumption that the alleged conspiracy could not have an effect throughout the Class Period. While this analysis was included in the Mangum Class Report, Dr. Stiroh did not comment on it, much less rebut it, in her own report. In her deposition, Dr. Stiroh acknowledged that she was aware of my analysis of annual overcharges but just did not to respond to it in her report.[247] Dr. Stiroh then argued that differences in the annual estimates—despite being all positive and statistically significant—are evidence against common impact, because they are not identical over time.[248] This is another example of Dr. Stiroh playing a game of "Heads I win, Tails you lose": If a subregression shows "no impact" she concludes that is proof against impact; but if it a subregression does show "impact," she pivots to arguing that the estimated impact does not meet her self-created standard of uniformity.

V.C. My Overcharge Model Appropriately Accounts for the Effect of Fed Cattle Prices on the Price of Beef

145. In the Mangum Class Report I explained the structure of my overcharge model, including how I controlled for cattle prices and addressed endogeneity concerns through an instrumental variable modelling approach.[249]

146. Despite my use of a 2SLS regression model that accounts for the cost of cattle, a technique that Dr. Stiroh admits "[redacted]"[250] and which she uses to attack Drs. Sunding and Williams.[251] Dr. Stiroh argues that my econometric model fails to appropriately account for the price of cattle on beef prices. Specifically, she asserts that I failed to "[redacted]"[252] In making this argument, Dr. Stiroh again either misunderstands or misconstrues the objective of my overcharge model. My overcharge model does not attempt to predict the but-for price of cattle. I understand that there are upstream cattle producer classes that include allegations that may differ in detail and timing

[247] Stiroh Deposition, pp. 305:22–307:11.
[248] Stiroh Deposition, pp. 306:22–307:23.
[249] Mangum Class Report, ¶¶ 413–414.
[250] Stiroh Report, ¶ 59.
[251] Stiroh Report, ¶ 61.
[252] Stiroh Report, ¶ 82.

from the Consumer IPP class. The goal of my model is to evaluate the conspiracy alleged by the Consumer IPP class and as it impacts members of the Consumer IPP class. In a competitive market, Defendants would have competed on the price of beef, taking the price of fed cattle as given. My model demonstrates that Defendants charged a higher price for beef during the Class Period than they would have if confronted with the same input costs and level of demand during the Benchmark Period. This is hardly surprising given the record high profit margins reported by Defendants during the Class Period.[253]

147. By taking cattle prices as given, my model isolates the downstream damages from any harm attributable solely to upstream cattle price manipulation. My model evaluates whether beef prices were artificially inflated based on how they diverged from the relationships with supply and demand factors (including cattle prices) established during a timeframe free of the alleged conspiratorial behavior.

148. Dr. Stiroh claims that, because the coefficient for fed cattle in the first stage of my 2SLS model is positive, my model implies Defendants overpaid for fed cattle, contrary to allegations in this case.[254] While the first stage regression allows for accurate estimation of my model for beef prices, it should in no way be construed as an appropriate, standalone model for fed cattle prices. The first stage regression is merely a mathematical device for correcting the bias in coefficient estimates that would likely occur if I included the price of fed cattle directly in my regression model. For the IV regression methodology to be valid, the instrumental variable should be correlated with the endogenous variable (relevance condition) and uncorrelated with the idiosyncratic error term (exogeneity condition). I go into further detail in the Mangum Class Report about why my instrumental variable—total costs of fed cattle production—is likely to satisfy both conditions.[255] Neither condition requires the first stage regression to be a standalone analysis of *other* hypotheses. For Dr. Stiroh to interpret the results as a model for cattle price impact is economically inappropriate and misleading. The most obvious quality that makes the first stage equation an inappropriate standalone model for fed cattle prices is the data frequency. The cattle prices used in my model are a monthly average reported by Iowa State University, which consists of 120 unique values. However, the first stage regression treats

[253] Mangum Class Report, ¶¶ 373–377.
[254] Stiroh Report footnote 174.
[255] Mangum Class Report ¶ 426, footnote 1034.

the price of cattle as if it was observed in over 3 million unique instances. The first stage regression also includes hundreds of thousands of fixed effects dummy variables. Taken literally, these fixed effects imply the average price of fed cattle—a variable that takes a single unique value in each month—can change depending on the Defendant, customer, product description, or grade of the corresponding observation of beef price. While this is the proper approach for a 2SLS regression, it is not offered as a standalone model for the price of fed cattle.

V.D. Dr. Stiroh Utilizes Obsolete and Cherry-Picked Quotes to Justify a Methodology that is Designed to Fail

149. In my report, I estimated class-wide impact using a regression model which estimates the effect of the alleged conspiracy on prices paid by Direct Purchasers over the entire Class Period.[256] Using the data produced in this case, I estimated that, during the Class Period and after controlling for other factors of supply and demand, beef prices would have been [redacted] percent lower, absent the alleged conspiracy.[257] In her report, Dr. Stiroh takes issue with my approach and analysis, and makes repeated complaints about what she describes as a "single, unvarying overcharge."[258]

150. Dr. Stiroh presents misleading and obsolete citations from purportedly authoritative sources to bolster her misguided views on regression analysis. For example, Dr. Stiroh utilizes the following quote to claim that the ABA Handbook on Econometrics ("ABA Handbook") "explains" how my overcharge regression model "assumes, rather than establishes, class-wide impact and should be tested via more complex modelling…"[259]

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[256] See Mangum Class Report, Section VII.

[257] Mangum Class Report, ¶ 499.

[258] Dr. Stiroh uses a handful of different terms throughout her report, including "common, unvarying overcharge," "constant, unvarying overcharge," "single overcharge," and "average overcharge." Often repeating such phrasing multiple times in the same paragraph, Dr. Stiroh uses these terms pejoratively dozens of times in her report. See paragraphs 11, 13b, 13c, 13f, 13g, 13j, 15, 32, 34, 35, 36, 37, 39, 40, 43, 49, 51, 52, 66, 79, 84, 86, and 110; footnotes 39, 91, 94, 96, 98, and 117; as well as the heading for Section V.B.

[259] Stiroh Report, ¶ 37, citing ABA Antitrust Law Section, Econometrics: Legal, Practical, and Technical Issues, 2nd ed. (ABA Book Publishing, 2014) ("ABA Handbook"), pp. 357–359.

Figure 19. Excerpt From Dr. Stiroh's Report Showing a Misleading Quote

37. As explained in the ABA Handbook on Econometrics, an overcharge regression model such as the one used by Plaintiffs' experts assumes, rather than establishes, class-wide impact and should be tested via more complex modelling that does not make the same assumption:

> If the regression … is specified correctly and includes all appropriate control variables, the coefficient on the conspiracy-related variable will provide a measure of the average effect of the alleged conspiracy on prices. … **Whether this coefficient indicates classwide impact depends on whether the average estimated impact applies to all of the members of the proposed class**. … Other statistical tools, including additional regression specifications, may be used to test whether the average effect represented by a single coefficient from a classwide regression masks widely varying individual effects that require individualized inquiry, or whether it truly reflects common impact. …

[99] The sensitivity tests proffered by Plaintiffs' experts, calculating, for example, the average overcharge by primal or by Defendant similarly fail to assess impact on an individualized basis. Sunding Deposition, 226:24-229:23 ("Q. The reason I'm focusing on particular transactions is just to kind of make sure we have a clear record on how these models work, right, so you can run the model on the whole cohort or you could break it down and run it on, you could pull out five customers, you could break it down by defendants, or things like that, but in all those breakdowns that you did you are still getting an average, you are just getting an average that is applied to a subcategory of the whole category; is that fair? … A. So, sure, for example, if I use the overcharge that I estimated by primal, there are a number of products that are within each, you know, individual products that a consumer might recognize that are within each primal category. Q. I see. … So you calculate an overcharge for primals. That is not the same thing as saying every particular transaction over this class period that involved a primal, that primal was overcharged, that's an average for the primals for the class period? … A. Yes. …").

[100] Plaintiffs' experts do not dispute this fact. Sunding Deposition, 252:10-253:5; Deposition of Russell W. Mangum, III, Ph.D., January 2, 2025 ("Mangum Deposition"), 347:15-348:5.

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> [R]elying solely on the estimated average effect would lead to the incorrect conclusion that all customers were impacted.[100]

151. As shown, Dr. Stiroh uses ellipses ("…") and a single footnote to bring in what appears to be the conclusion of a single quote ("[r]elying on the estimated average effect would lead to the incorrect conclusion that all customers were impacted").[260] However, a review of the underlying text in question shows that the conclusion of Dr. Stiroh's quote is wholly

[260] Stiroh Report, ¶ 37, citing ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues*, 2nd ed. (ABA Book Publishing, 2014), pp. 357–359.

disconnected from the earlier portion of the discussion she cites.[261] Indeed, Dr. Stiroh has sewn together two quotes separated by multiple pages and presented them as one. In reality, the concluding point about relying "solely on the estimated average effect would lead to the incorrect conclusion that all customers were impacted."[262] relates to a specific and extreme hypothetical example that is used by the authors to illustrate a point that bears no relationship whatsoever to the facts of this case.

152. The hypothetical conspiracy to which Dr. Stiroh's quote relates is intentionally contrived and extreme. the authors use a fictional conspiracy on vitamins to illustrate potential issues related to common impact. The example *assumes* that a 5 percent overcharge has previously been estimated over the length of the conspiracy. After discussing the possibility of allowing the overcharge to vary over time, the authors suggest that additional subregressions *could* be tested, and identifies different geographies, suppliers, products, and purchaser types as potential candidates. The text then explains how statistical tests can be implemented to test "whether the effect of an alleged conspiracy should be estimated separately for two or more potential subgroups of customers, products or periods."[263] The authors then write:

> *To illustrate this point, suppose that in the vitamins example discussed above, two types of vitamins (A and B) are implicated in an alleged conspiracy that covered two distinct time periods (1 and 2). Suppose further that the expert begins by regressing the logarithm of vitamin prices on vitamin type, time, and conspiracy dummy variables, and that the estimated average effect of the conspiracy is 5 percent… Suppose now that separate conspiracy dummy variables are introduced for each vitamin type and that the estimated effect of the alleged conspiracy is 0 percent for Vitamin A and 10 percent for Vitamin B. This second regression would reveal that the initial estimated average effect of 5 percent lumped together some prices that may not have been inflated (Vitamin A) and some that may have been inflated (Vitamin B). Thus,* ***relying solely on***

[261] *See* ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues*, 2nd ed. (ABA Book Publishing, 2014), pp. 357–359 (covering two different subsections; the sentence "[R]elying solely on the estimated average effect" appears in the context of a different section discussing a discrete example).

[262] Stiroh Report, ¶ 37, citing ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues*, 2nd ed. (ABA Book Publishing, 2014), pp. 357–359.

[263] ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues,* 2nd ed. (ABA Book Publishing, 2014) p. 358.

> ***the estimated average effect would lead to the incorrect conclusion that all customers were impacted.***[264]

153. There are several important points here. First, nothing in the text that Dr. Stiroh cites prescribes running subregressions at the individual customer level (I address this in more detail in **Section VI** below). The ABA Handbook solely identifies potential economically meaningful subgroups—product categories (*i.e.*, Vitamin A and Vitamin B), geographic regions, suppliers, or customer types. I note that these subgroups are only *potentially* economically meaningful, because meaning would depend on whether the proposed subgroup is motivated by evidence or the allegations in the case. For example, in a textbook market allocation scheme, where competitors agree not to compete in certain regions or product markets, it may be reasonable, or even necessary, to explore anticompetitive effects in the distinct geographic or product submarkets. Similarly, if there is evidence or an allegation that an alleged conspiracy was only directed at certain customers, and arbitrage across channels was not possible, it *could* be reasonable to explore anticompetitive effects across sales channels or customer types. The point, however, is that subregressions of this type should be motivated by facts, evidence, and economic theory.[265] Without evidence to guide the investigation, such subregressions are nothing more than unscientific and indefensible fishing expeditions that decrease the amount of data available and reduce the power of statistical testing.[266]

154. Second, the hypothetical example—two vitamins, one with 10 percent impact and one with no impact—is extremely contrived, and identifying the lack of impact on vitamin A would almost certainly not require regression analysis in the first place. For the pattern described to hold, either the relevant markets for the two products are different, or only one of them was subject to the conspiratorial conduct in the first place. In either case, information indicating fundamentally different market dynamics would likely be identifiable through documentary

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[264] ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues,* 2nd ed. (ABA Book Publishing, 2014), pp. 358–359.

[265] For example, if a conspiracy included list price increases, but documents indicate that the price lists were only disseminated to certain customer types or sales channels, it would be reasonable to investigate differences in impact among customers who received the accused price lists compared to those who did not.

[266] While Dr. Stiroh may argue that her discussion of different pricing mechanisms is evidence, that is not what she ultimately tests. For example, if Dr. Stiroh believed that customers who purchased primarily in the spot market avoided impact, she could have identified those customers and tested them specifically. Instead, she jumps straight to the individual customer level, which still does not address her own claim about purchase types (since many customers purchase beef through multiple methods). As I show elsewhere in this report, controlling for purchase type has no effect on my finding of common impact.

evidence and market research long before any regressions were run in the first place. While extreme hypotheticals can be useful in the classroom or textbook to illustrate broader points, the facts and circumstances necessary to bring them about in the real world are highly unlikely, and simple mathematics can illustrate how absurd this proposition is. Assuming that the "true" overcharge is, in fact, 10 percent for Vitamin B and 0 percent for Vitamin A, a regression model could return an estimate of 5 percent *only* if the sales data used in the model contained *identical* observation totals for Vitamin A and Vitamin B, and that the overcharge for B was *exactly* 10 percent on all transactions, and exactly 0 for A on all transactions.

155. This extreme example is at the root of Dr. Stiroh's analysis, argument, and opinions—she has just replaced product type ("vitamin type") with "individual customer." Dr. Stiroh opined that most customers were actually uninjured and the "average" overcharge of [redacted] hides this fact.[267] But this Theory of Many Zeroes is even more absurd in this case than it is in the hypothetical vitamins example.[268] As noted above, Dr. Stiroh claims to have found that 636 of the top 1,000 customers were "uninjured" (*i.e.*, they had *no* overcharge).[269] By Dr. Stiroh's reasoning that my overcharge is merely an average of the individual customers' overcharges, to offset so many 0 values (for "uninjured" purchasers) and reach an "average" of [redacted] percent (across all 1,000 customers), the remaining 364 customers (for which Dr. Stiroh finds impact) must have incurred an average overcharge of [redacted] percent—nearly three times higher than the actual overcharge I estimated.[270] Further, Dr. Stiroh also opines that the remaining 3,313 untested customers (beyond the top 1,000) are also likely uninjured.[271] If these are added to the equation, the 364 "injured" customers would need to have sustained an overcharge of [redacted] percent in order to have an "average" overcharge of [redacted] percent across all customers (which is what my model finds).[272]

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[267] Stiroh Report, ¶ 86. In her deposition, Dr. Stiroh made this point explicitly: "[T]he average overcharge would have variation around it. I'm looking at the possibility that that variation is - - goes to zero or negative for some customers and finding that it does." *See* Stiroh Deposition, p. 219:1–24.

[268] Stiroh Report, ¶ 86.

[269] Stiroh Report, ¶ 13 and ¶ 40.

[270] "Injured" "average" overcharge has to satisfy the following equation: [redacted] The resulting "average" overcharge for "impacted" customers is [redacted] Stiroh Report, ¶ 13 and ¶ 40; Mangum Class Report Figure 50; Mangum Class Report, ¶ 433.

[271] Stiroh Report, ¶¶ 13, 40, 48. 4,313 direct customers that purchased beefd during the Class Period minus 1,000 customers that Dr. Stiroh tests, leaves 3,313 "untested" customers.

[272] [redacted]

156. In a later section, I address this topic from an econometric perspective to further demonstrate how economically and statistically implausible Dr. Stiroh's Theory of Many Zeroes truly is.[273]

V.E. There Is No Reasonable Basis for Individual Customer Subregressions

157. The ABA Handbook discusses a handful of subregressions that could, in certain evidentiary or allegation-specific contexts, be economically relevant. The ABA Handbook does not, however, not prescribe running any particular subregressions in the absence of evidence for doing so. In this case, there are no allegations, there is no evidence, and there is no economic theory, that would support running subregressions based on geography, product type, or sales channel.[274] Plaintiffs have not alleged that the Defendants constrained beef production for only certain types of customers, or only in certain regions, or only on certain beef primals. Dr. Stiroh has not presented any evidence to support these distinctions.

158. With no basis for even these subregressions, there is obviously no basis for drilling down even further—slicing and dicing the data into economically meaningless subsets without consideration for sample sizes, purchase composition, or scientific theory. Without any case-specific evidence to support her preferred methodology, Dr. Stiroh again relies on misleading quotes from the ABA Handbook as justification. Dr. Stiroh's previous misleading quote (discussed above) comes from the current (2014) ABA Handbook. However, she then pivots to an older (2005) version for the following quote:

> *The reduced-form pricing equation assumes that a conspiracy has the same effect on every purchaser and focuses on an average effect, which may hide variation across class members.* ***If one is attempting to test whether there is an impact on all members of a proposed class, however, that assumption is not valid, as it assumes the very proposition that is being tested. As a result, somewhat more complex models that do not make such an assumption must be used to test for class-wide impact.*** *One approach is to divide the proposed class into categories and use a model that allows the value of the dummy variable to be different for different categories. … In fact, given enough data, it is possible to let the variable that represents the conspiracy's effects assume a different value for every customer. In this way, every member of the class can*

[273] See **Section VI.G**.

[274] To be clear, the mere possibility that the overcharge "might be different" in one subset of the data is not an economic or scientific theory—it is merely conjecture used by Dr. Stiroh as justification to disaggregate data into powerless subsets.

> *present a formulaic estimate of its impact and damages that is grounded in a regression model that is common to all.*[275]

159. Dr. Stiroh's citation to an older version appears to be strategic in nature. When I reviewed the current edition, the language she cited does not appear to be included. While there remains a section describing potential uses for well-defined subregressions, I found no references to the notion that "a somewhat more complex model must" be employed, nor did I find any encouragement for using individual customer subregressions, or suggestions that the use of a single overcharge is "not valid." This makes sense, given that it is demonstrably incorrect—I have utilized single overcharge models to estimate class-wide impact in multiple cases where the Courts have accepted such approaches.[276]

160. In fact, the current edition of the ABA Handbook goes even further than simply removing the claims about customer-specific overcharges that Dr. Stiroh cites—it actually warns against such excessive disaggregation:[277]

> *There are several issues associated with the use of regression and statistical tests to demonstrate classwide impact or lack thereof. First, as the class is divided into more and more subgroups, the need for data increases significantly.* ***Estimating a model that allows each purchaser to experience a different price increase, for example, requires data that are rarely available****. The data demands become even more daunting if possible interactions between customer effects and other supply and demand variables...are considered. Second, because the number of observations per grouping declines* ***as transactions are divided into more and more subgroups, coefficients become less precise, which makes a test of coefficient stability or robustness less reliable****.*[278]

161. Dr. Stiroh sidesteps this warning by simply asserting that "in this matter, such data *are* available" and cites the large number of purchasers who frequently purchased beef throughout the period of analysis.[279] I strongly disagree with Dr. Stiroh's conclusion, and discuss the issue

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[275] Stiroh Report footnote 100 (emphasis added).

[276] For example, in recent years, I have utilized such overcharge variables in cases related to packaged tuna, broilers, pork, and turkey. In each case, the Court certified the class for which I was assessing common impact and overcharges.

[277] While there is no one-size-fits-all answer to the question of what constitutes excessive disaggregation, the point is that Dr. Stiroh immediately jumps to the extreme—the individual customer—without any basis.

[278] ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues,* 2nd ed. (ABA Book Publishing, 2014), p. 359 (emphasis added).

[279] Stiroh Report, footnote 101.

of small sample sizes, and the impact they have on the centerpiece of Dr. Stiroh's report, in extensive detail later in this report.

V.F. Dr. Stiroh's Predicted Price Charts Are Meaningless

162. Dr. Stiroh criticizes my econometric model in her report because she believes it performs poorly when she tried to predict the prices of a handful of beef products using the estimated model coefficients.[280] Dr. Stiroh's criticisms of my model in this regard are weak, ignore the purpose of the model and the basis of the coefficient estimates, and other evidence of model performance. More importantly, Dr. Stiroh's criticisms are effectively a rejection of any regression model in the first place.

163. My econometric model incorporates data from thousands of products and thousands of customers. Like all regression models, the end result of the estimating procedure is a single coefficient estimate corresponding to the effect each explanatory variable has on the data. Dr. Stiroh cherry-picks a handful of individual products and uses these coefficients to create charts of actual and predicted prices. The plot she presents to criticize my mode, her Figure 5.2, is one of over 375 thousand customer product combinations, and is not one of the top customer product combinations at that – her Figure 5.2 is ranked 7,571.[281] Based solely on her own eyeball test, Dr. Stiroh concludes that my model's predicted prices are unacceptable and that therefore, my model is flawed.

164. First, it is unrealistic to expect that estimated coefficients, which are calculated across thousands and thousands of products and customers, would be able to precisely estimate the actual prices of all individual products. If the purpose of the model was solely to estimate predicted prices for one individual product sold by one Defendant to one customer (say, product 3352, a "Boneless Strip Loin" sold by National Beef to Harvest Meats), the model would likely be constructed quite differently. But that is not the purpose of the model, nor would such a model be of any use in assessing class wide anticompetitive impact.

165. Second, Dr. Stiroh's criticism is, fundamentally, that my model fails to account for variation in customer pricing. This is incorrect, because my model explains nearly all of the variation in

[280] Stiroh Report, ¶¶ 67–71, Stiroh Figure 5.2, and Stiroh Exhibits 9B, 10B, and 12C.

[281] See backup production.

the Defendants' sales data. Indeed, as I showed in the Mangum Class Report, my model explains approximately 95 percent of the variation in the data.[282]

166. Third, Dr. Stiroh's criticism is really a criticism of regression in general. No regression model will ever precisely predict all values from the underlying data, because regression models only estimate a single coefficient for each explanatory variable. The only way around this reality is to estimate a different coefficient for every observation, which the regression algorithm cannot do. Thus, in demanding that a regression model precisely estimate prices for thousands and thousands of models, Dr. Stiroh is really just demanding that regression models do things they are not capable of doing and that stochastic relationships are not deterministic.

V.G. My Overcharge Model is Robust to Economically Reasonable Sub-specifications

167. In this section, I demonstrate, for illustrative purposes, that my regression model is robust to additional economically plausible sensitivity tests. Regressions show that across overcharges customer types, product types, geographic regions, and purchase types are all positive and statistically significant and consistent with the pooled overcharge estimate in the Mangum Class Report. While there is no evidence in this case that any of these sensitivities are necessary, justified, or relevant, they do serve two purposes. First they demonstrate that Dr. Stiroh's jump to the customer-specific level was unjustified, as subregressions at an economically plausible level provide no meaningful evidence of a lack of impact. Second, they demonstrate how Dr. Stiroh's results, in which she purports to find no impact for many customers, are driven by small samples. This purported lack of impact vanishes when reasonable sample sizes are maintained.

168. As discussed previously, there is no basis whatsoever for the type of fishing expedition Dr. Stiroh conducts, in which she disaggregates the data into thousands and thousands of individual customer samples to run individual overcharges.[283] As also noted above, the allegations in this case do not support investigation of the other types of data subsets that the ABA Handbook indicates may, in certain contexts, be appropriate. However, in **Figure 20** below**,** I include—

[282] Mangum Class Report, Figure 50.

[283] While her report only includes 1,000 subregressions using my sales data, in her deposition she testified that she has since expanded the number of subregressions she has run. She did not disclose the results of that analysis, however. Stiroh Deposition, pp. 23:10–24:12.

solely for illustrative purposes—additional variants of my regression model based on the distinctions mentioned in the ABA Handbook (*i.e.*, product type (beef primal), geographic regions (time zone), suppliers (Defendants) and customer type). Every subgroup shows a positive and statistically significant overcharge. The widespread findings of positive and statistically significant overcharges are not surprising, because the nature of the alleged conspiracy is such that there would be no reason to find a lack of impact. This is why it is not correct or necessary to disaggregate purely for the sake of disaggregation—there should be a basis in fact or evidence to motivate such disaggregation. Dr. Stiroh has no plausible economic theory or evidence that remotely aligns with "every individual customer is potentially its own relevant market," but that is precisely the methodology she chooses.

Figure 20. Overcharge Model Sensitivities[284]

Interaction Sensitivity		Conspiracy Period Coefficient	Observations
Defendant			
Cargill			
JBS			
National Beef			
Tyson			
Primal			
Chuck			
Loin			
Rib			
Round			
Customer Type	**Channel**		
Cash & Carry	Foodservice		
Distributor	Foodservice		
Redistributor	Foodservice		
Restaurant	Foodservice		
Restaurant Manageme	Foodservice		
Broker/Trader	Multichannel		
Distributor	Multichannel		
Redistributor	Multichannel		
Processor	N/A		
Other	Other		
Cash & Carry	Retail		
Distributor	Retail		
Grocery Coop/Gpo	Retail		
Grocery Store	Retail		
Grocery Wholesaler	Retail		
Time Zone			
Alaska			
Central			
Eastern			
Hawaii			
Mountain			
Pacific			

* p<0.1, ** p<0.05, *** p<0.01

169. In an earlier section of this report, I noted that Dr. Stiroh had accused me of failing to control for the different types of contracts or purchasing mechanisms in my analysis. Beef purchasers utilize multiple contracting or pricing methods to buy beef from Defendants.[285] However, all of these pricing mechanisms would be affected by the alleged conspiracy. As such, there is a fundamental lack of economic or evidentiary basis for disaggregating the data into purchase

[284] See backup production.

[285] Stiroh Report, ¶¶ 25–26; Mangum Class Report, ¶ 57.

type subsamples for analysis. Additionally, the information produced by Defendants is incomplete with respect to purchase type. For example, the databases produced by Tyson, National Beef, and JBS [redacted] Moreover, where the fields do exist, they appear to be somewhat inconsistent across Defendants. Further, the sales data produced by Tyson and National Beef contain only a small number of values in this field, while JBS includes a large number of limited and ambiguously-named descriptions for purchase types that were used during the Class Period.[286]

170. Nevertheless, for three of the Defendants, it is ultimately possible to examine overcharges by purchase type.[287] In **Appendix D**, I show that incorporating information about purchase type into my overcharge estimates yields no meaningful changes. Overcharges across purchase types are positive and statistically significant, as expected. Further, even if purchases under infrequently-used pricing mechanism were somehow shielded from the effects of the alleged conspiracy, it would still not imply non-impact for any individual customer, let alone multiple, many, or most. This is because customers purchased under multiple pricing mechanisms.

171. I note that this information about purchase types was available to Dr. Stiroh, but she chose to criticize me without investigating whether there was any merit. More importantly, in her own analysis—where she runs thousands of individual regressions—Dr. Stiroh does not incorporate any of this information herself, which undermines her credibility in making arguments in the first place. Moreover, it is important to note that running individual customer subregressions does nothing to improve the purported informational shortage Dr. Stiroh complains about. As Dr. Stiroh herself points out, individual customers utilized multiple purchasing methods during the period of analysis.[288] But when she runs individual regressions for each customer, Dr. Stiroh does not make any attempt to include any customer-specific information such as

[286] See backup production.

[287] To do this, I include interaction terms for all purchase types that account for at least 1 percent of the observations and are not missing values. For National Beef, this excludes "CONTRACT," which accounts for approximately one half of one percent of observations. Only one customer purchased beef from National Beef exclusively under the "CONTRACT" purchase type. For Tyson, this approach excludes "HEDGE CONTRACT." "HEDGE CONTRACT" accounts for less than one percent of total observations, and only one customer purchased beef from Tyson exclusively through this purchase type. For JBS, this approach excludes many small and ambiguous purchase types, which are identified in my backup materials. See backup production.

[288] Stiroh Report, ¶¶ 25–26.

contracting details. Accordingly, Dr. Stiroh's individual customer subregressions will conflate any latent or idiosyncratic noise with the overcharge variable, leading to poorer estimates.

172. Taken together, all of these subregressions—across time, products, customer types, purchase types, suppliers, and regions show precisely what I already demonstrated in the Mangum Class Report: beef prices were elevated by approximately 5 percent during the Class Period. There are certain estimated coefficients that are higher or lower than 5 percent, but this is not surprising—smaller samples will lead to larger variances in estimates—nor is it problematic. The nature of this case makes these subregressions entirely unnecessary, but they do serve to highlight how lacking in merit or reason Dr. Stiroh's jump to the individual customer subregression level is. They also demonstrate that Dr. Stiroh's "Theory of Many Zeroes" has no basis in reality.

V.H. My Direct Overcharge Model Appropriately Controls for the Alleged Conspiracy

173. While Dr. Stiroh does not make this argument in her report, I understand that Defendants' counsel has claimed that my overcharge model is unreliable because it "misattributes impact from market factors that are unrelated to the alleged conspiracy"[289] Defendants' counsel claims that non-conspiratorial plant closures that occurred during the benchmark period resulted in price increases around and after the start of the conspiracy period and that my overcharge model inappropriately includes these effects in my direct overcharge estimation.

174. In the Mangum Class Report, I discussed numerous actions Defendants took to eliminate or restrain capacity from the beef market during the Class Period. In particular, I highlighted numerous existing slaughter facilities which were unavailable due to Defendants' actions. Certain (but not all) of these plants were idled prior to July 2014, and it is my understanding that the decisions to idle at that time are not considered part of the alleged conspiracy. These plant closings were not exogenous events – they were business decisions based on market conditions, and as such they are appropriately and explicitly controlled for through relevant controls (*i.e.*, demand and supply variables) in my direct purchaser overcharge model. My model captures the underlying, relevant exogenous economic conditions and factors that lead to a myriad of business decisions, including decisions regarding plant existence, operations,

[289] Defendants' Memorandum of Law in Support of Their Motion to Exclude Certain Portions of the Expert Report and Testimony of Dr. Russell W. Mangum, Jan. 24, 2025, p. 15.

and transfers. Controls for these exogenous economic conditions and factors are appropriate and active during time periods reflecting the benchmark and the alleged conspiracy. My assessment of damages through use of an estimated overcharge is coincident with the alleged conspiracy, and only reflects incremental price effects during the Class Period; *i.e.*, based on price elevation above what is explained by market supply and demand factors. Thus, my model does not "misattribute" price impact from the plant closures that occurred prior to the start of the conspiracy.

175. As I discussed in the Mangum Class Report, Defendants allegedly restrained supply during the Class Period by removing additional capacity and keeping the idled facilities closed, despite increased availability of cattle. Importantly, Defendants allegedly utilized deed restrictions and refused to sell plant capacity to keep beef slaughter capacity and facilities away from would-be competitors (non-conspirators) who would respond competitively to market conditions rather than collusively (*i.e.*, in accordance with collusive "margin over market share" goals). It is my understanding that the effective restraint of capacity after the start of the alleged conspiracy, including the use of deed restrictions and refusal to sell the facilities, are considered part of the allegations in this case. To the extent this behavior inflated beef prices above those that would exist absent the alleged conspiracy, this effect is captured in my overcharge estimate. Thus, the impact of closing of certain facilities during the benchmark is fully accounted for in my model and is not included in my direct overcharge estimate, which is limited to the incremental price effects, above and beyond prices explained by market factors, during the Class Period. Accordingly, my direct purchaser overcharge estimates are untainted by "nonconspiratorial" effects, including the plant closures that occurred prior to the conspiracy.

VI. Dr. Stiroh's Individual Customer Subregressions Are a Meaningless Exercise in Flawed Small Sample Estimation and Low-Power Statistical Testing

176. Dr. Stiroh conducts individual customer subregressions to "test" my direct overcharge model, first by running individual customer subregressions and then by running a customer interaction subregression, in which she interacts each customer with the conspiracy period dummy.[290] I

[290] Stiroh Report, ¶ 41.

explain in this section why Dr. Stiroh's approach is flawed and her conclusions therefore faulty.

177. Dr. Stiroh has erroneously equated "observations" with "data" (or more properly, "information"). The fact that many customers bought beef on a regular enough basis to result in many observations does not mean that running individual regressions is appropriate or reliable. This is because each customer's purchases (and prices) necessarily exhibit idiosyncratic variation over time. Idiosyncratic variation is a normal and inevitable part of econometric and statistical analysis of real-world phenomena. In the context of a case like this, discounts or sales incentives—either regularly occurring or one-off sales—may be the cause of such variation.[291] Additionally, minor recording issues (*e.g.*, incorrect product descriptions or mispriced products) in the data may lead to unexplained variation.

178. When an analysis involves a large and diverse sample of customers, such idiosyncratic variation is less likely to result in poor estimation, because the model will be able to properly differentiate the underlying relationships between prices and the explanatory variables, including the overcharge – from the noise or idiosyncratic variation. The critical point here is that the causes behind such idiosyncratic variation are rarely, if ever, observable—they are latent in nature, and cannot be controlled for in a regression model.[292]

179. Dr. Stiroh's results plainly contradict her assertion that there is sufficient data available to estimate her individual customer overcharges. In her analysis, she estimates customer-level overcharges for the largest 1,000 customers who also meet certain observation requirements.[293] These subregressions are run in two ways: 1) using separate, individual subregressions for each

[291] Grocery retailers may position themselves in different ways when it comes to discounts or incentives from suppliers. For example, suppose that a beef supplier offers $1,000 in promotional dollars over (say) a quarter. A retailer that uses an "every day low price" (or "EDLP") approach would spread that $1,000 evenly across all of that quarter's purchases. In contrast, a "deep discount" retailer may prefer to offer higher everyday prices, but then concentrate the $1,000 in incentives on a few weeks of deeper discounts. Still other retailers may prefer a hybrid approach between these two extremes. These differences in approaches will lead to different prices on a purchase-by-purchase basis, even if they are paying the same price over the course of the quarter.

[292] I note that the fact that it is not possible to control for such idiosyncratic variation does not mean that a model suffers from "omitted variable bias." The regression model will produce reliable estimates so long as the idiosyncratic variation is not correlated with any of the explanatory variables, or when correlation exists but is addressed with an instrumental variable approach. Given the explanatory variables take the same value for each customer in a given time period, it is unlikely that any of them will be correlated with idiosyncratic variation from customer-level heterogeneity.

[293] Stiroh Report ¶ 44.

customer ("individual customer subregressions"), and 2) using a "customer-interaction" subregression. Both of these approaches estimate customer-specific *overcharge* coefficients, but the latter incorporates data from all customers to estimate the non-conspiracy coefficients (*i.e.*, seasonal dummies, costs, and so on), whereas the former relies solely on an individual customer's data to estimate all coefficients in the model.[294] In Dr. Stiroh's Exhibit 7C, she presents the results of her two different approaches: Column C shows results from individual customer subregressions; Column D shows the results of her customer-interaction subregression.

180. When Dr. Stiroh asserts that there is sufficient data in this case for the 1,000 individual customer subregressions she presents in Stiroh Exhibit 7C, she is claiming—without overtly saying so—that her results are not materially or adversely affected by the different samples of information available for those 1,000 customers.[295] That is, Dr. Stiroh equally adopts the results for each of the 1,000 customers as part of her opinion, and does not indicate that any of the estimations are more (or less) reliable than any other estimations. For example, in Dr. Stiroh's analysis, the results for [redacted] are portrayed as equally valid and reliable, despite their vast differences in sample sizes. In her deposition, Dr. Stiroh confirmed this, when she asserted that sample size is not a determining factor in her results, and cited an example (from her analysis of Dr. Sunding's customer data) of a customer ([redacted]) that has thousands of observations but does not show an overcharge as proof.[296] Dr. Stiroh is incorrect in both the specific and in the general. Dr. Stiroh's analysis of my customer data is similar to her analysis of Dr. Sunding's with respect to [redacted] her individual customer subregression for [redacted] shows a positive and statistically significant overcharge, but her customer-interaction subregression shows a positive overcharge that is not statistically

[294] As a simple example, consider a scenario with only two customers (A and B), a conspiracy dummy ("D") and a single explanatory variable ("X"). The interaction model would use transactions from A and B to estimate the coefficient for X, but only use transactions for A to estimate the coefficient for D_A, and only use the transactions for B to estimate the coefficient for D_B. Using individual regressions, only transactions for A would be used to estimate the coefficients for X_A and D_A, and only the transactions for B would be used to estimate the coefficients for X_B and D_B.

[295] Stiroh Report ¶ 41.

[296] Stiroh Deposition, pp. 217:20–218:25.

significant.[297] While the different approaches give slightly different results (both positive, one statistically significant), neither could be used to demonstrate a *lack* of impact.[298] More importantly, [redacted] does not, in fact, have the wealth of observations that Dr. Stiroh claims it does. In my customer data, which is monthly in nature, [redacted] has only [redacted] observations—split roughly evenly across the benchmark and Class Period. While this number of observations clearly exceeds Dr. Stiroh's meaningless "100/30/30" criteria, as I show later in this report, Dr. Stiroh's subregressions struggle to identify overcharges for most customers with this amount of data. More importantly, pointing to a single "large" customer like [redacted] is not proof that small samples are not affecting estimation.

VI.A. Dr. Stiroh's Individual Customer Subregression Results are Driven by Small Sample Problems

181. In this section, I show that Dr. Stiroh's individual customer subregressions are indisputably and negatively plagued by small sample problems that lead to erroneous conclusions. I do this by showing how her findings of impact are directly and negatively related to the number of observations in each customer subregression. This means that her findings are not reliable and do not provide any evidence of non-impact.

182. Dr. Stiroh's Exhibit 7C, which shows her subregression results and lists overcharge estimates by customer, is sorted in descending order of customer size (based on total purchase volume).[299] Even a cursory examination of these results reveals that the frequency of positive and statistically significant overcharge estimates declines as the customers get smaller and smaller (*i.e.*, customers nearer the top of the table are more likely to have positive, statistically significant overcharge estimates). However, the length of Dr. Stiroh's list of customer results (1,000 rows) obscures the strength of the relationship between customer size and statistically significant overcharge estimates. To facilitate a clearer picture of the relationship between a customer's sample size and the likelihood of finding a statistically significant overcharge, I placed Dr. Stiroh's 1,000 customers into 10 groups of 100 customers each. Thus, Group 1 is

[297] Stiroh Report, Exhibit 7C.

[298] I also note that [redacted] is not a relevant customer for the Consumer Indirect Purchaser Class, and thus even if it were the case that Dr. Stiroh's results were valid—which they clearly are not—it would have no bearing on impact for downstream consumer class members.

[299] Stiroh Report, Exhibit 7C.

comprised of the largest 100 customers (by observation count), and Group 10 includes the 100 smallest customers (by observation count).[300]

Figure 21. Results of Dr. Stiroh's Individual Customer Subregressions, Grouped by Size

		Stiroh Individual Customer Subregressions				Stiroh Interaction Subregressions			
Group	Average Total Observations	Positive & Significant	Positive & Insignificant	Negative & Significant	Negative & Insignificant	Positive & Significant	Positive & Insignificant	Negative & Significant	Negative & Insignificant
1	16,811	88%	9%	0%	3%	98%	2%	0%	0%
2	4,145	63%	31%	1%	5%	94%	1%	3%	2%
3	2,528	47%	39%	3%	11%	90%	8%	2%	0%
4	1,696	36%	45%	2%	17%	79%	13%	1%	7%
5	1,219	28%	43%	3%	26%	82%	10%	1%	7%
6	949	18%	42%	1%	39%	72%	24%	1%	3%
7	690	23%	37%	3%	37%	77%	18%	1%	4%
8	483	20%	45%	2%	33%	67%	26%	2%	5%
9	323	21%	36%	4%	39%	62%	31%	3%	4%
10	179	20%	35%	8%	37%	48%	33%	4%	15%

Note: Customers grouped on total number of observations. Each group indicates 100 customers, where Group 1 represents the largest 100 customers.

183. **Figure 21** above shows, for each group of 100 customers, the frequency of the four possible statistical outcomes from Dr. Stiroh's subregressions. Looking first at the individual customer subregressions, as shown in the first row, 88 of the 100 customers in Group 1 (the largest 100 customers by observation count) had a positive and statistically significant overcharge, 9 had a positive but statistically insignificant overcharge, 3 customers had a negative but statistically insignificant overcharge, and 0 customers had a negative and statistically significant overcharge. Thus, 97 percent of customers had a positive overcharge, with only 3 percent showing negative estimates (none of which were statistically significant). What is remarkable is how directly the number of positive and statistically significant overcharge estimates

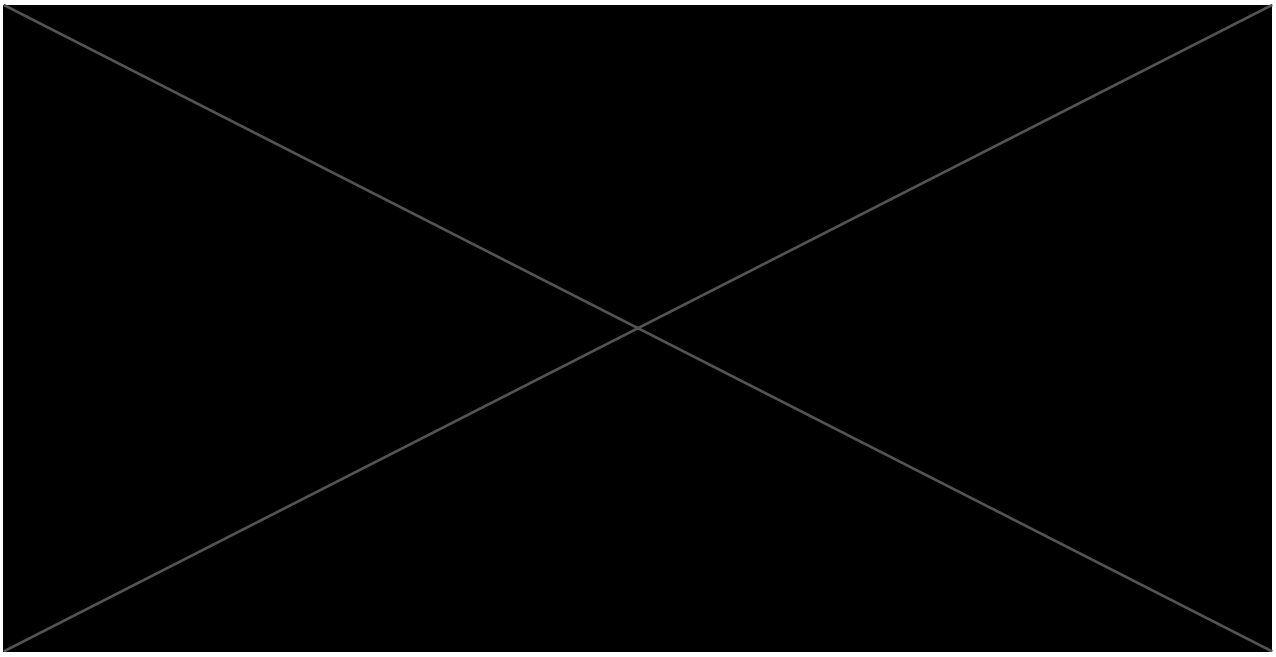
[300] In Dr. Stiroh's analysis, she ranks customers based on purchase volume (*i.e.*, pounds of beef). From a purely econometric perspective, the total quantity of beef purchased by each customer is not really material—what matters is the number of transactions or observations. In practice, these two metrics are typically closely related—customers who bought a lot of beef also bought beef very frequently—but this is not always the case. In fact, the data for some of the top-ranking customers based on their purchase volume could not be used in the individual customer regressions that Dr. Stiroh proposed, since the timing and number of their transactions was still insufficient for the regression analysis. For example, [redacted] all have too few records in the benchmark period to perform individual customer regression analysis by Dr. Stiroh's methodology. *See* Stiroh Report Exhibit 7C backup. While these customers are "untested" in Dr. Stiroh's analysis of my customer data (Stiroh Report, Exhibit 7C), she finds positive and statistically significant overcharges for [redacted] from the individual customer subregressions based on Dr. Sunding's transactional data (Stiroh Report, Exhibit 7A). Further, there are instances where "large" customers in terms of purchase volume do not buy as frequently. For example, [redacted] largest customer in terms of purchase volume. However, in terms of observations, [redacted] is decidedly middle of the road at rank [redacted] among 1,000 customers she tested from my data. See backup production.

declines with customer size (as depicted by the Group rank). Virtually all customers in Group 1 show positive and statistically significant overcharge estimates; Group 2 shows the same for most customers, but the share of statistically significant overcharge estimates declines rapidly for the remaining Groups. Indeed, for Groups 6-10, only about 20 percent show positive and statistically significant overcharges. The reason for this is obvious—as also shown in **Figure 21**, the customers in Group 1 have a wealth of data that allows for highly precise estimates, whereas the customers in lower-ranked Groups have significantly less data. As shown, the difference is extraordinary—the average customer in Group 1 had over four times more data for estimation purposes than the average customer in Group 2, which had roughly 60 percent more data (per customer) than those in Group 3, and the average sample size continues to decline.

184. These results demonstrate clearly that Dr. Stiroh's analysis does not show a lack of anticompetitive impact, a host of uninjured class members, or any other flaws in my methodology or regression model. Rather, they demonstrate that Dr. Stiroh's results are driven by the small-sample problem she manufactured and imposed on the data. Customers with very large samples consistently show positive and statistically significant overcharge coefficient estimates, and customers with smaller samples do not.

185. Dr. Stiroh's flawed approach can be further demonstrated by simply observing the prices paid by Dr. Stiroh's "unimpacted" customers compared to those for which she finds impact. **Figure 22** below shows that, when looking at a specific product, the average monthly price for Dr. Stiroh's "unimpacted" customers is nearly identical to the monthly average price of the impacted customers.

Figure 22. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers[301]



186. As seen, prices paid by Dr. Stiroh's "unimpacted" customers closely track the prices paid by Dr. Stiroh's impacted customers, after controlling for product. While a price level comparison does not control for all of the factors that influence pricing, it does illustrate the similar pricing patterns across the impacted and "unimpacted" customers and further demonstrates that Dr. Stiroh's finding of "no impact" is driven by sample size.

VI.B. Dr. Stiroh's Customer-Interaction Subregression Also Demonstrates the Problem of Small-Sample Regression Analysis

187. Dr. Stiroh's customer-interaction subregressions are similarly plagued by small sample issues. Moreover, Dr. Stiroh fails to address the significant difference in results between her two tests. This is important, because the only explanation for the difference is that small samples are negatively affecting her regressions.

188. While the discussion in the preceding section focuses on Dr. Stiroh's individual customer subregressions, I performed the same analysis using Dr. Stiroh's customer-interaction subregression. There are three key points to discuss with respect to this alternative model. First, as shown in **Figure 21** above, Dr. Stiroh's results are once again dramatically affected by the size of individual customers' data samples. The larger-sample Groups (1-3) show that over 90

[301] See backup production. See **Appendix E** for charts covering the top 100 products.

percent of customers have positive, statistically significant overcharges, but this share steadily declines to just under 50 percent by Group 10.

189. Second, the drop-off in statistically significant overcharge estimates is not nearly as stark for the customer-interaction subregression as it is for the individual customer subregressions. This is unsurprising, because the customer-interaction approach does not impose quite as much of a burden on the regression model in terms of parameter estimation and degrees of freedom. This is because the customer-interaction approach utilizes all customers' data to estimate coefficients other than the conspiracy effect. Nevertheless, because the individual customer conspiracy effects are still estimated solely using each customer's data, they still reflect the negative impact of small sample estimation and are less reliable.

190. Third, the huge difference between Dr. Stiroh's individual customer subregression results and her customer-interaction subregression results is problematic, as is her silence on the matter in her report. Due to the presence of idiosyncratic noise in every customer's data, it is unsurprising that two similar but slightly different approaches would lead to *some* degree of conflict. But in her own report, Dr. Stiroh's two competing methods give her conflicting answers for over 40 percent of direct purchasers ([redacted] percent of customers unimpacted in her individual customer subregressions vs. [redacted] percent of customers unimpacted in her customer-interaction subregression).[302]

191. This chasm in her results—nearly half of the customers in her sample—should have been a major red flag. Whether Dr. Stiroh did not notice this red flag, or simply chose not to investigate or address it, is unclear, but she does not comment on it at all in her report. In her deposition, Dr. Stiroh suggested that showing of impact would require a statistically significant estimate in both tests, as failure in one indicates inconclusive evidence of impact.[303] This is nonsensical, and is another example of Dr. Stiroh making up her own "Heads I win, Tails you lose" rules for showing impact: any purported inconsistency in her results is proof against impact, but never an indication of a flaw in her testing methodology. In her deposition, Dr. Stiroh insisted that small samples are not driving her results.[304] In light of the analysis presented

[302] Stiroh Report, ¶ 13.

[303] Stiroh Deposition, p. 310:5–21.

[304] Stiroh Deposition, pp. 357:6–360:12.

above, such testimony lacks any credibility. Had she investigated her own subregression results, Dr. Stiroh would have readily found that 1) the decreased sample sizes are driving her results, 2) this type of methodology is unreliable for "testing" common impact like she purports to do, and 3) it is misleading to portray outcomes that are so demonstrably plagued by small sample issues as evidence or proof of "uninjured" class members.

VI.C. Dr. Stiroh's Purported Robustness Check Is Meaningless by Construction

192. In this section, I show that Dr. Stiroh's purported robustness check, in which she doubles the number of observations required for each individual customer subregression, is meaningless by construction, because her group of "tested" customers is essentially unchanged. In other words, her conclusions are the same, because the underlying data she evaluates is the same.

193. The discussion in the previous section relates to the results presented by Dr. Stiroh based on her customer subregressions, which requires each "tested" customer to have 100 total observations, with at least 30 in the benchmark and 30 in the Class Period ("100/30/30" criteria).[305] However, as a purported test of her results, Dr. Stiroh doubles this constraint to require customers to have 200 total observations, including 60 in the benchmark and 60 in the Class Period ("200/60/60" criteria).[306] Dr. Stiroh touts the fact that the results of this test are similar to those using the 100/30/30 criteria—she originally found [redacted] percent of customers "unimpacted," and this rises slightly to [redacted] percent under the 200/60/60 criteria.[307] As shown above, Dr. Stiroh's original results were meaningless because of small sample issues; despite the appearance of increased sample sizes, the results of her robustness test are also meaningless, because the results cannot—by construction—change materially in the first place.

194. While Dr. Stiroh doubles the observation requirements for testing, it has very little impact on the actual composition of the 1,000 customers in her regression data. This is because almost all customers who have at least 100 observations also have 200 observations. Dr. Stiroh's original top 1,000 customers (which satisfied her 100/30/30 criteria) only included 68

[305] *See* Stiroh Report, footnote 105.

[306] *See* Stiroh Exhibits 6A and 6B.

[307] Stiroh Exhibit 6A. Using the customer-interaction subregression (Stiroh Exhibit 6B), the percentage of "unimpacted" declines from 23.1 percent to 20.4 percent.

customers with less than 200 total observations, only 36 customers with less than 60 Class Period observations, and only 40 customers with less than 60 benchmark observations. Thus, her robustness test was conducted using almost entirely the same customers and the same data, so any change in the results would be limited to the handful of customers that churned in and out based on the change in sample size requirements.[308]

195. Even these marginal changes would be unlikely to have a significant impact on Dr. Stiroh's results because the handful of customers that do churn in or out of Dr. Stiroh's data under these different criteria largely share a critical common attribute: they have very small data samples in the Class Period, in the benchmark period, or in both. This is essentially a circular condition—the "excluded" customers must have small samples of data, or they would not be excluded in the first place. And the "replacement" customers *must* also have small samples of data, because if they did not, they would likely have been included in the original data set.[309] She is simply swapping between two different measures of small customers: limited observations, or limited purchase totals.

196. Most importantly, since Dr. Stiroh's methodology is constructed to find a lack of impact for customers with small data samples, the "new" (but still small) customers would be expected to show results that are consistent with the customers they replaced, and the overall percentages of "impacted" customers are largely unchanged. In conclusion, Dr. Stiroh's purported robustness test that purports to strengthen her arguments about data sufficiency is a superficial and misleading exercise that cannot, by construction, test the proposition it is intended to test.

VI.D. Dr. Stiroh's Individual Customer Subregressions Demonstrate Common Impact Across Customers

197. Dr. Stiroh applies her individual customer subregressions using my regression model, as well as those put forward by Dr. Williams and Dr. Sunding. Her results are inconsistent across

[308] See backup production.

[309] Had Dr. Stiroh used "observation count" to rank the top 1,000 customers for regression modeling, there would not be any change in the tested or untested customers under either criteria. Indeed, the reason there are a few new customers under the 200/60/60 criteria is that Dr. Stiroh ranked her 1,000 largest customers based on purchase volume instead of observation total. While there is a very strong positive correlation between purchase volume and transaction counts, it is not perfectly 1 to 1, and thus there is some minor churn among high volume/low frequency and low volume/high frequency customers at the bottom of the top 1,000 customers.

models for the same customer. By comparing results across models, however, virtually all of the customers tested by Dr. Stiroh are impacted in her subregressions.

198. Dr. Stiroh runs individual customer subregressions for the top 1,000 customers using each expert's data. In this process, she analyzed a total of 1,291 customers across three experts.[310] While the methodology she adopts is the same for each expert—run 1,000 individual regression models—the results are noticeably different in terms of how many customers she finds to be impacted. For example, while Dr. Stiroh finds that [redacted] percent of customers are uninjured using her variant of my regression model, that share is reduced to [redacted] percent using her variants of Dr. Sunding's and Dr. Williams' models, respectively.[311] But Dr. Stiroh's models also differ considerably in terms of which customers she finds to be impacted. Specifically, when focusing on individual customer regression results, only 69 customers appear as "not impacted" across all three experts' models.[312] This is only 1.6 percent of the DPP class (which includes 4,313 class members) and these customers, unsurprisingly, are generally quite small in terms of observation count.[313] When focusing on Dr. Stiroh's customer-interaction subregressions instead, only 14 customers are never impacted across all three experts' customer models.[314] This is only 0.3 percent of the DPP class (which includes 4,313 class members). Taken together, of the 1,291 customers that are tested in *any* of the 6 models, 1,248 (over 96 percent) show impact in at least one individual customer subregression.[315] Of the 43 customers that do *not* have impact in any regression, most (40) were not tested across other models, due to differences in class definitions or Class Periods. Of the 711 customers that are tested across *all* 6 models, only 3 customers are never

[310] Stiroh Exhibits 7A, 7B, and 7C.

[311] Stiroh Report ¶ 41. Dr. Stiroh calculates 36 percent uninjured for Dr. Sunding's individual customer model, but then she points out a necessary correction to his model in controlling for inflation (see Stiroh Report Appendix B). The results I discuss for Dr. Stiroh's subregressions using Dr. Sunding's model are based on an updated version of Dr. Sunding's model, correcting for the inflation adjustment that Dr. Stiroh identified. See backup production.

[312] See backup production.

[313] They range in observation count (in the Mangum data) between [redacted] See backup production.

[314] See backup production. These customers range in observation count (in the Mangum data) [redacted]

[315] See **Appendix F** for a complete list of all tested customers and their impact status according to Dr. Stiroh's 6 subregressions.

impacted.[316] This is only 0.07 percent of the DPP class members (which includes 4,313 class members). Those 3 customers purchased [redacted] during the Class Period, which is only 0.03% of the overall commerce – this is relevant because it demonstrates how small their footprint of potential sales to indirect purchasers is.[317] Further, Defendants' data suggest that at least 2 out of these 3 customers may not be in the Consumer IPP supply chain because they are not in a Class State.[318]

Figure 23. Customers for Which Dr. Stiroh's Individual Customer Subregressions Find No Impact[319]

[redacted]

[316] See backup production. Again, all of these customers are quite small in terms of observations, ranging from 157 observations ([redacted]) to 1,506 observations ([redacted]) in the Mangum data. See backup production.

[317] See backup production.

[318] These customers are [redacted] Defendant sales of relevant beef products to [redacted] See backup production.

[319] See backup production.

[320] See backup production.

199. To be clear, I recognize that there are differences in the models put forward by Dr. Sunding and Dr. Williams. In my opinion, the fact that multiple economists independently specified different models that all show impact on beef prices during the specified Class Periods is itself evidence of impact. However, it is important to note that differences in the percentage of "unimpacted" customers in Dr. Stiroh's subregressions using applications of our respective econometric models is not an indication that one model is superior or inferior to the others. Rather, the differences in Dr. Stiroh's results across the Plaintiffs' experts' data flows entirely from the magnitude of the estimated overcharge. Dr. Sunding and Dr. Williams both have specified a model that leads to an estimated overcharge of approximately [redacted] percent.[321] In contrast, my econometric model finds a more modest overcharge of [redacted] percent. It is unsurprising that Dr. Stiroh's individual customer subregressions using Dr. Sunding's and Dr. Williams' models show similar shares of "unimpacted" customers, and that it is considerably lower than she finds using her application of my regression model. The size of this estimated effect directly affects the proportion of customers Dr. Stiroh claims are unimpacted in her subregressions. As I show later in this report, this is yet another fundamental flaw in Dr. Stiroh's "testing" methodology—it fails to detect modest overcharges.[322] This is problematic both econometrically and as a matter of justice—class certification and common impact should not be available only to plaintiffs who suffered enormous overcharges. Thus, a tool or approach that systematically fails to find impact for more modest overcharges is inappropriate.

VI.E. Subsetting Injured Customers in Line with Dr. Stiroh's Small Customers Demonstrates that Sample Size Drives Her Finding of No Impact

200. In this section, I use regression analysis to illustrate directly how small samples affect overcharge estimation. Specifically, using customers that Dr. Stiroh herself finds "impacted" in her subregressions, I "shrink" the available sample of data to match the size of smaller customers, which largely were not impacted in Dr. Stiroh's subregressions. I show that shrinking the available sample size causes "impacted" customers to become "unimpacted" in

[321] Stiroh Report, footnote 20. Dr. Sunding's inflation corrected model finds an overcharge of [redacted] percent, see Stiroh Report Appendix B, Figure B.2.

[322] I address the topic of "threshold" overcharges in **Section VI.H.6** of this report.

Dr. Stiroh's tests. This definitively shows that Dr. Stiroh's own methods are the cause of her finding of non-impact, and not the absence of an overcharge.

201. Dr. Stiroh runs thousands of regressions (across all purchaser Plaintiff expert reports) to estimate separate "overcharge" coefficients for each individual customer. Dr. Stiroh then tallies up the number of coefficients that are not positive or statistically significant, and concludes that each customer associated with these coefficients was "uninjured" by the alleged conspiracy.[323] As I have shown above, Dr. Stiroh's results are driven by small sample problems: customers with lots of data tend to show positive, statistically significant overcharge coefficients, customers with less data tend to not show positive, statistically significant overcharge coefficients. *In this way, Dr. Stiroh's analysis effectively implies a world in which the largest customers are harmed, while smaller customers are not.* This outcome is economically implausible from the perspective of bargaining power, volume-based discounts, or any other economic logic. Nevertheless, given that it is central to Dr. Stiroh's conclusions, I have conducted an additional analysis to demonstrate that the only material difference between the largest purchasers and the smaller purchasers, in terms of Dr. Stiroh's finding of impact, is sample size.

202. To do this, I take the top 100 customers (based on number of observations) for which Dr. Stiroh finds positive and statistically significant overcharges in her individual customer subregressions, and draw samples of data to match the corresponding samples for the bottom 100 customers.[324] For example, the 9th and 909th ranked customers (by observation count) are [redacted] and [redacted]. In Dr. Stiroh's individual customer subregressions, [redacted] had a statistically significant overcharge of [redacted] percent, while [redacted] had a negative (but statistically insignificant) "overcharge" of [redacted] percent. After taking steps to ensure general product comparability for statistical estimation purposes,[325] I then draw 100 samples of 242 observations from [redacted]

[323] Stiroh Exhibit 7A, Exhibit 7B, and Exhibit 7C.

[324] See backup production. Note that these rankings are based on observation counts, not the purchase volume rankings in Dr. Stiroh's analysis.

[325] Larger customers tend to buy not only more beef, but more individual products (*i.e.*, SKUs). Thus, to ensure well-defined clusters for estimation purposes, I took steps to ensure that the number of unique defendant-customer-product-grade combinations in the larger customer's drawn sample was equal to the number in the corresponding smaller customer sample. This was accomplished by first sampling the same number of clusters from the large

data to mimic the sample size of [redacted].[326] I repeat this process for the other 99 largest customers (and corresponding 99 smallest customers). I then use Dr. Stiroh's code to run individual customer subregressions (for each of [redacted] 100 small customer samples, as well as the 100 samples drawn for the remaining 99 large customers), and estimate overcharge coefficients. This process, using [redacted] as an example, is illustrated in **Figure 25** below.

Figure 25. Applying Dr. Stiroh's Methodology to Customer Subsamples Illustrates the Effect of Small Sample Sizes

[redacted]

203. In her deposition, Dr. Stiroh insisted that small samples had no effect on her results, and that she had taken steps to ensure that.[327] If Dr. Stiroh is correct, then customers that were "injured"

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customer as are in the small customer sample, with each matched to a cluster in the small customer sample. Clusters from the large sample were eligible for matching if they contained at least as many benchmark and Class Period observations as the corresponding small sample cluster. Observations from the large customer cluster were then sampled to produce a cluster with the same number of observations in the Benchmark and Class Periods as the small sample cluster. The difference in rank (by observation count) is a multiple of 100 by default. However, the situation occasionally arises in which the large customer sample does not have the required cluster composition to match the cluster composition of the small customer sample. In these instances, the sampling procedure attempts to find a large customer sample with the required cluster composition by cycling through the remaining large customers until a match is found.

[326] This is done to avoid the possibility of a single draw giving improbable results.

[327] Stiroh Deposition, pp. 217:20–219:24.

in Dr. Stiroh's analysis should remain so when their sample sizes are reduced. In contrast, if customers who were "injured" in Dr. Stiroh's customer subregressions are no longer injured based on a subset of their data, it demonstrates that her small samples are driving her results. For example, because Dr. Stiroh's individual customer subregression finds impact for [redacted] if Dr. Stiroh is correct about sample sizes being a nonissue, a subregression run on a sample of [redacted] data (drawn to include the same amount of information that she relies on for smaller customers) should also show impact. This is demonstrably *not* the case. Indeed, the subregressions based on subsets of Dr. Stiroh's own impacted customers fail to find a positive and statistically significant overcharge nearly 90 percent of the time.[328]

204. As shown above, the smaller the sample, the less likely it is to find a positive and statistically significant overcharge, despite the fact that all of the customers from whom data is being drawn (the largest 100) have positive and statistically significant overcharges in Dr. Stiroh's customer subregressions.[329] Indeed, even when the customers are quite large—those in the second largest group of 100 impacted customers—Dr. Stiroh's approach still fails to find an overcharge in nearly half of all subregressions. Again, these results are exclusively based on data from customers Dr. Stiroh herself finds "impacted" in her individual customer subregressions. Given the already-flawed nature of her finding of "impact," drawing samples from all customers—not only those "impacted" in her subregressions—would lead to even more extreme results.

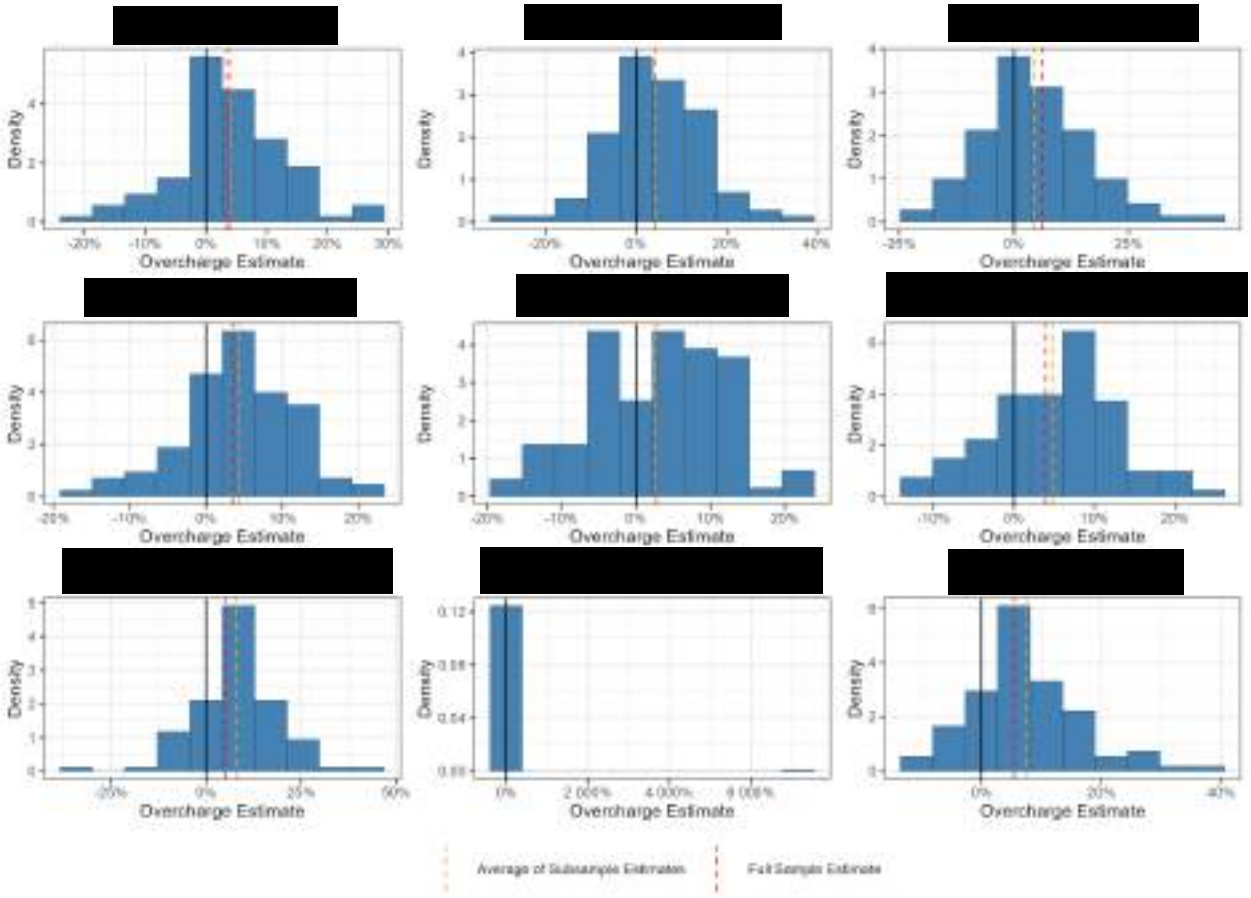
205. In **Figure 26** below, I show histograms showing the distribution of the estimated overcharges that result from this exercise. These histograms are a useful way of understanding why Dr. Stiroh's results are so misleading. In each histogram, the blue bars represent the range of estimated overcharge outcomes, the solid black line represents a value of 0, the dotted red line represents the overcharge estimate using the customer's full sample of data, and the dotted

[328] See the far-left column of **Figure 28**. See backup production.

[329] This approach is consistent with Dr. Stiroh's own writing in a chapter she authored in the ABA Handbook on Econometrics. Dr. Stiroh explained that "Parameter estimates tend to be less precise with smaller samples, and an analysis may be less likely to find statistical significance in a small dataset compared to the same model estimated on a larger dataset." This is precisely what I am showing: for a customer that shows impact, reducing the sample size of data decreases the likelihood of finding impact. By forcing the analysis to be done at the individual customer level, Dr. Stiroh is choosing a method that ensures her tests will reject the finding of impact. *See* ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues*, 2nd ed. (ABA Book Publishing, 2014), p. 111 and p. xvii (noting that Dr. Stiroh authored that chapter).

orange line represents the average of the 100 overcharge estimates using 100 different sample draws.

Figure 26. Distribution of Overcharge Estimates for Subsamples of Top 9 Impacted Customers[330]



206. This is sufficient evidence to establish how Dr. Stiroh's methodology is fundamentally flawed and is biased to show a lack of impact by design. Nevertheless, I extend the analysis further by re-estimating overcharges using Dr. Stiroh's model for the top 100 customers, but draw increasingly large samples of data based on larger customers. I have shown that impacted customers in Dr. Stiroh's analysis are no longer impacted according to her models if you simply limit the data (in accordance with the smallest customers Dr. Stiroh's regression utilizes). Next, I demonstrate how *increasing* the sample size (in accordance with customers in Dr. Stiroh's

[330] See backup production. [redacted] This demonstrates the challenges of limited sample sizes.

regression dataset) *increases* the likelihood of Dr. Stiroh's individual customer subregressions returning positive and statistically significant overcharge values. For example, instead of [redacted] I subset [redacted] to mimic increasingly larger customers: [redacted] then [redacted] and then [redacted] and so on.[331] Again, I do this for all "impacted" customers in the top 100 (based on observation total). This enables me to evaluate how the results of top "impacted" customers in Dr. Stiroh's analysis (*e.g.*, [redacted] are affected by the change in sample size alone (in correspondence with the size of actual customers included in Dr. Stiroh's customer specific regressions).

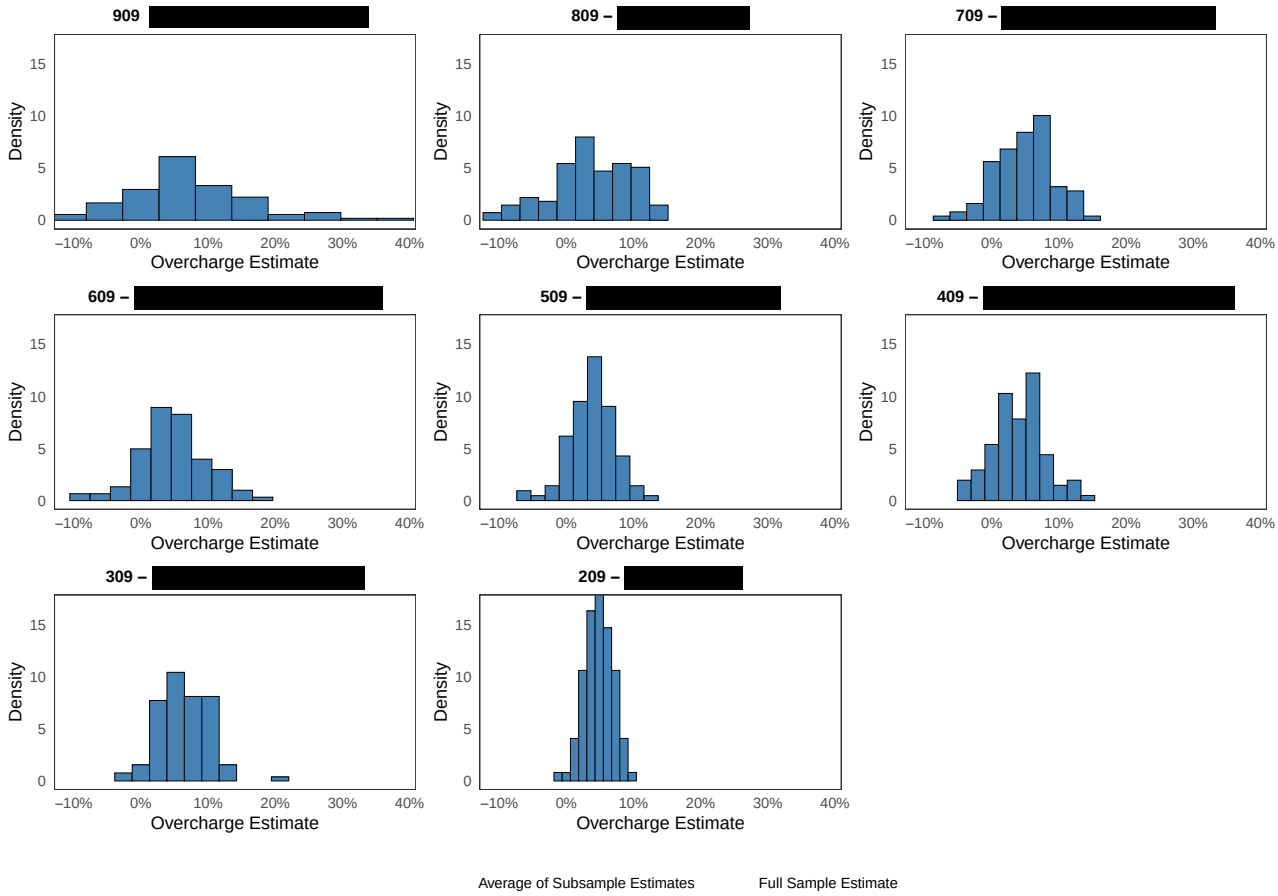
207. **Figure 27** below illustrates this process for [redacted] depicting the distribution of the estimated overcharges across the 100 runs for each of [redacted] subsamples. The top left chart shows that when I subset [redacted] data to match a customer in the smallest group ([redacted] the estimates range significantly, and there is a higher probability of the model (inappropriately) returning a negative and/or insignificant overcharge estimate. Of these 100 runs for [redacted] subsample, 70 percent show no impact for [redacted] (based on Dr. Stiroh's definition of no impact being negative or insignificant overcharges).[332] Moving from 909 to 809 (top center) and then 709 (top right), and continuing all the way to 209 (bottom center) shows that, as size of the [redacted] subsample increases (*i.e.*, the rank of the subsample customer increases) the distribution of the estimated overcharges narrows and centers around the value for [redacted] subregression in Dr. Stiroh's analysis: [redacted] percent.[333]

[331] As I subset based on increasingly larger customers, the sample size begins to approach [redacted] actual subsample, which should lead to overcharge estimates close to that found in Dr. Stiroh's [redacted] specific customer subregression.

[332] See backup production.

[333] *See* Stiroh Report, Exhibit 7C in Column C.

Figure 27. Distribution of Overcharge Estimates Based on Subsamples of [redacted] Data [334]



208. When I perform this analysis for the other 99 top impacted customers aside from [redacted] it is not a surprise that I find the same pattern. **Figure 28** below shows the portion of positive and statistically significant overcharges found for each subsample group of impacted customers. When the top 100 impacted customers are subset to match the Stiroh's smallest customers (ranked 901 – 1,000) only 11.9 percent of the *impacted* customer subregressions return positive and statistically significant overcharges (see the far-left column of **Figure 28**). As the subsample size of the impacted customers grows (and the rank increases from 900s, to 800s, to 700s, and so on through to 200s - from left to right in **Figure 28** below) the portion of *impacted* customer subregressions that accurately return a positive and statistically significant overcharge increases.

[334] See backup production.

Figure 28. The Finding of Overcharges for Impacted Customers Increases with Sample Size [335]

	Sample Size Customer Rank								
	Smallest Customers → Larger Customers								100 Largest Impacted Customers
	901-1,000	801-900	701-800	601-700	501-600	401-500	301-400	201-300	
Portion of Positive & Significant Overcharges	11.9%	14.4%	18.5%	21.0%	26.3%	32.2%	41.3%	54.0%	100%

209. It is worth emphasizing, as I explained above, that these subregressions are all run using the data of customers that Dr. Stiroh finds *are impacted*. The only change I make is subsetting the data of the impacted customers to correspond to the size of other, smaller, customers that Dr. Stiroh uses in her individual regression. When I do subset the data, the subregressions for the impacted customers no longer return positive and statistically significant overcharges the majority of the time. The subregressions based on the smallest customers (ranked 901 – 1,000) only show impact on 11.9 percent of the time, and even the largest subsamples (customers ranked 201 – 300) only return a positive and statistically significant overcharge 54 percent of the time.

210. **Figure 28** shows that the likelihood of finding impact (for customers Dr. Stiroh has already established were impacted) increases with sample size. This is precisely what is expected for a well-specified regression model like the one I have used in this case: as the richness and availability of data improve, the estimates become increasingly accurate; as the data quality and abundance wanes, the estimates become increasingly inaccurate. Dr. Stiroh chose to adopt and present a methodology that exploits this reality and then testified that it was having no impact on her results. This is clearly incorrect.

VI.F. Dr. Stiroh's Analysis of "Untested Customers" is Flawed and Misleading

211. In this section, I demonstrate that Dr. Stiroh's claims about "untested" customers likely avoiding an overcharge are false and misleading. I first discuss how she portrays purported differences in price in a misleading fashion that exaggerates any differences. I then demonstrate that, outside of regression analysis, the evidence suggests that "untested" customers experienced price elevation at a higher rate that the "impacted" customers in Dr. Stiroh's subregressions.

[335] See backup production.

212. As discussed above, Dr. Stiroh conducted her analysis of individual overcharges using the largest 1,000 customers (based on purchase volume). As also shown above, her analysis was rigged from the outset—her choice to disaggregate the data into increasingly small sample sizes delivered results that were entirely predictable. In her report, Dr. Stiroh further opines that, based on the results of her purported "testing" of the 1,000 largest customers, the remaining "untested" customers likely include many that were also "uninjured."[336] To reach this conclusion, Dr. Stiroh compares the average prices paid by "untested" customers with the average prices paid by "tested" customers. According to Dr. Stiroh, "untested" customers paid prices that were 1) lower than those paid by "tested" customers who had positive and statistically significant overcharge estimates, and 2) almost identical to those paid by "tested" customers who did not have positive and statistically significant overcharges. In other words, Dr. Stiroh argues that "untested" customers are more like the purportedly "uninjured" class members than the injured class members she finds.

213. First, as I have demonstrated thoroughly, Dr. Stiroh's purportedly "uninjured" class members are not uninjured—they are customers who fell victim to Dr. Stiroh's inappropriate regression methodology. This has implications for comparing prices to Dr. Stiroh's "untested" customers, because they are a combination of 1) even smaller customers who did not make it to the "top 1,000 customer" threshold based on the volume of their purchases, *and 2)* a mix of other customers with higher purchases yet insufficient data to meet the "100/30/30" or "200/60/60" observation threshold due to their lack of observations in the benchmark or Class Period. Dr. Stiroh is therefore merely comparing the average prices of (mostly) small customers or infrequent buyers to the average prices of (mostly) larger customers with more data. It is unclear what purpose such a comparison could serve in any context, but it is certainly devoid of any meaning in the context of evaluating overcharges in an antitrust case.

214. Second, even ignoring the empty nature of the comparison, Dr. Stiroh describes this analysis in a highly misleading fashion. In the text of her report, Dr. Stiroh emphasizes the difference in a weighted average across "impact" groups and calculates a very large difference ($4.07 vs. $3.54 per pound, a difference of approximately 15 percent) across impacted and untested

[336] Stiroh Report, ¶ 13.

customers in the body of her report.[337] This comparison is meaningless because it fails to control for significant differences across the product mixes purchased by different customers. In a footnote to her report, Dr. Stiroh acknowledges this and concedes that the calculated difference is closer to 3 percent after controlling for product mix.[338]

215. Setting aside Dr. Stiroh's overstatement of any purported difference in prices, Dr. Stiroh's analysis does not ask a relevant question in the first place. As Dr. Stiroh and I have both discussed, price dispersion exists across customers, even for similar cuts of beef. For example, if two customers purchase identical cuts of beef on the same day, but differ materially in volume, it is possible—or even likely—that the price will vary between the customers due to volume discounts. Whether a customer paid more or less than another customer (or a group of customers) is not, on its own, informative to the question of antitrust impact. The relevant question is whether a customer paid higher prices than it would have in the but-for world where the alleged conspiracy does not take place.

216. In situations where it is not possible to directly address this question through individual regressions model due to data insufficiency—as Dr. Stiroh herself acknowledges for at least these smaller customers that don't make her top 1,000—it is possible to at least compare the average prices during the benchmark to those during the Class Period across Dr. Stiroh's purported "impact" groupings. As shown in **Figure 29,** customers who were (according to Dr. Stiroh) "impacted" saw their average price of beef rise [redacted] percent during the Class Period. However, those who were "untested" by Dr. Stiroh paid prices that were [redacted] percent higher during the Class Period.[339] While this analysis does not hold "all else equal" in the way a regression model would, it does indicate that the mechanisms that led to higher prices during the Class Period had a relatively larger effect on Dr. Stiroh's "untested" customers. This is a clear contradiction of Dr. Stiroh's flawed analysis of pricing for "untested" customers and her conclusion that such "untested" customers were likely unaffected by the alleged conspiracy.

[337] Stiroh Report, ¶ 47. These calculations are based on Dr. Sunding's data.

[338] Stiroh Report, ¶ 47 and footnote 127.

[339] See backup production.

Figure 29. Price Change by Dr. Stiroh Individual Customer Subregression Result[340]

[redacted]

VI.G. Pooled Regression Analysis Based on "Untested" and "Unimpacted" Customers Demonstrates Dr. Stiroh's Flawed "Masking" Theory

217. In this section, I use regression analysis to demonstrate that Dr. Stiroh's conclusions about "unimpacted" and "untested" customers are incorrect. I combine the data of customers Dr. Stiroh has specifically identified in her subregressions as having "no impact," and show they do exhibit statistically significant overcharges when pooled together. This demonstrates that Dr. Stiroh's findings are again driven by small sample estimation issues, since if each individual customer was indeed "unimpacted" then combined they should also show "no impact." I also demonstrate that Dr. Stiroh herself tested this, but did not disclose or discuss it in her report. After doing so, I demonstrate that all subgroups of customers from Dr. Stiroh's analysis—"impacted," "unimpacted," and "untested"—show overcharges consistent with my original estimate.

218. Earlier in this report, I noted how Dr. Stiroh misleadingly utilized a contrived example from the ABA Handbook, in which a conspiracy alleged to have affected prices for two vitamins (A and B) actually only affected one (B).[341] In that illustrative example, an initial overcharge of 5 percent was found to be comprised of an overcharge of 10 percent on Vitamin B, with but no overcharge for Vitamin A. I illustrated, using basic mathematics, how economically nonsensical Dr. Stiroh's concern that my estimated overcharge of [redacted] percent is "masking" legions of uninjured customers. This "Theory of Many Zeroes" can be tested more rigorously with regression. In the backup materials for Dr. Stiroh's individual customer subregressions, she identified three groups of customers: 1) "injured" (*i.e.*, customers *with* a positive and statistically significant overcharge coefficient in her customer subregressions), 2) "uninjured" (*i.e.*, customers *without* a positive and statistically significant overcharge coefficient in her

[340] See backup production.

[341] Stiroh Report, ¶¶ 37–38; ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues,* 2nd ed. (ABA Book Publishing, 2014), pp. 357–360.

customer subregressions), and 3) "untested" (*i.e.*, customers who were not included in her customer subregressions).

219. It is straightforward to test Dr. Stiroh's Theory of Many Zeroes by comparing regressions results comprised of these three distinct groups. If my estimated overcharge is the result of "averaging" the outcomes of "injured" customers and "uninjured" customers, as Dr. Stiroh asserts, then removing all "injured" customers from the dataset should result in an estimated overcharge of zero. Stated another way, a regression that only includes customers Dr. Stiroh determined are uninjured, it should not be possible for a pooled regression model to return a result showing injury. However, if the pooled model using only "uninjured" customer data returns a positive and statistically significant overcharge estimate, that would be compelling evidence that disaggregation is the principal cause of Dr. Stiroh's findings of non-impact.

220. To conduct this test, I estimated my regression model in the following manner: First, I estimated the model using only data from Dr. Stiroh's 636 "unimpacted" customers.[342] Second, I estimated the model again but used data from both "unimpacted" customers and "untested" customers, which Dr. Stiroh asserts are likely unimpacted. Third, I estimated the model using only data for Dr. Stiroh's "untested" customers. According to Dr. Stiroh's report and opinions, *none of these models should show a positive and statistically significant overcharge*. However, as seen in **Figure 30**, the estimated overcharge for all three data samples is positive and statistically significant.[343]

[342] This group of 636 customers includes those with positive but statistically insignificant results, negative but statistically insignificant results, and negative and statistically significant results.

[343] I note that the fact that the estimated overcharges for the two models that include "unimpacted" customers are lower does not, in fact, mean that they truly incurred a lower overcharge. Rather, it is likely due to selection bias. That is, it is more likely that they experienced relatively more negative idiosyncratic errors during the Class Period. This increased likelihood of relatively more negative idiosyncratic errors for the "unimpacted" customers and relatively more positive idiosyncratic errors for the "impacted" customers follows from the very definition of the two groups. This can be seen by observing the "untested" customers, which were not selected based on Dr. Stiroh's testing results, and which exhibit an overcharge that is close to that found from the full sample.

Figure 30. Overcharge Estimates for Dr. Stiroh's "Unimpacted" and "Untested" Customers[344]

	Unimpacted	Unimpacted and Untested	Untested	Impacted
Aug 2014-Dec 2019 Period Indicator Estimate	[redacted]			
Aug 2014-Dec 2019 Overcharge Rate [1]				

* denotes a statistical significance of 5%.

[1] The Overcharge Rate is calculated by the following formula that accounts for the semi-log functional form of the regression model:

$$1 - \frac{1}{exp^{(Period\ Indicator\ -0.5 \times Var(Period\ Indicator))}}$$

221. I note that Dr. Stiroh appears to have recognized this type of test herself, as her supporting materials include code that indicates she ran the test with respect to the "untested" customers, but chose not to discuss or disclose this fact.[345] While the results shown here utilized a pooled regression approach, Dr. Stiroh effectively ran the same analysis by grouping all customers outside of the Top 1,000 into a single "untested" category which she then included in her customer-interaction subregression. In this model, Dr. Stiroh estimated an overcharge of over [redacted] for the "untested" customers.

222. As a final step, I estimated the model using only data from the 364 customers Dr. Stiroh finds to be "injured" in her analysis. For Dr. Stiroh's Theory of Many Zeroes to work, these customers would need to have an estimated overcharge substantially higher than [redacted] But, as shown in **Figure 30**, the estimated overcharge is only [redacted]

223. There are a few key points to be made here. First, these results clearly contradict Dr. Stiroh's opinion that her purported "uninjured" customers were not affected by the alleged conspiracy. Second, these results contradict her claim that there is enough data to run individual overcharge models, because it shows how her results are completely overturned when the estimation procedure is improved by utilizing larger samples of data. Third, these results contradict Dr. Stiroh's opinion that her "untested" customers were unlikely to be impacted by the alleged conspiracy. Their collective overcharge was not only positive and statistically significant, but

[344] See backup production.

[345] See Stiroh Report backup for Figure 3.2. While the results shown here utilized a pooled regression approach for each customer impact status group, Dr. Stiroh effectively ran the same analysis by grouping all customers outside of the Top 1,000 into a single "untested" category which she then included in her customer-interaction subregression. In this model, Dr. Stiroh estimated an overcharge of over [redacted] percent for the "untested" customers using my data. Despite conducting this analysis and getting this result, Dr. Stiroh failed to mention it anywhere in her report and instead speculated that "untested" customers were likely unimpacted by the alleged conspiracy.

it is also very close in magnitude to the estimated overcharge found in the Mangum Class Report. Fourth, Dr. Stiroh knew this, because she tested it herself, but still chose to speculate that "untested" customers were likely unimpacted by the alleged conspiracy.[346]

224. Finally, these results contradict Dr. Stiroh's Theory of Many Zeroes that my estimated overcharge is "masking" meaningful variation or "averaging across all customers" to hide scores of uninjured class members. The estimated overcharge for the customers Dr. Stiroh finds to be impacted is approximately [redacted] percent. While this is slightly higher than my estimated class-wide overcharge, 1) that is likely only due to sample selection,[347] and 2) it is not remotely high enough to "mask" hundreds (let alone thousands) of purportedly "uninjured" customers. If anything, these subregressions ("injured," "uninjured," and "untested") demonstrate that there is broad consistency in the overcharge across customers. While the results *could* indicate minor variation in the overcharge across customers, 1) the variation is wholly consistent with how the samples were constructed, and 2) if the effect of sample selection is ignored, the variation that does exist suggests that the estimate is between 4 and 5 percent, *and would not range as much as* between 0 and 57 percent, as Dr. Stiroh's Theory of Many Zeros would require.[348]

VI.H. Low Power & Insufficient Data: Determinations about Data Sufficiency Require More than Observation Counts

225. Dr. Stiroh opined, in both her report and her deposition, that there was enough data in this case for individual customer subregressions, and claimed to have taken steps to ensure that each customer had a valid and sufficient sample of data.[349] As I have shown above, the results of her individual customer and customer-interaction subregressions are heavily and indisputably plagued by small sample issues. Further, Dr. Stiroh failed to identify any steps—beyond simply counting observations—that she took to ensure that each customer had enough data to ensure

[346] The results of Dr. Stiroh's test are included in the backup materials for Stiroh Exhibit 6B. The estimate coefficient for "OTHER CUSTOMERS" is 0.0648 and is found in row 692 of tab "Sheet1 (100-30-30, Mangum)" in Stiroh's "Exhibit 6B.xlsx." For Dr. Stiroh's robustness check using her 200/60/60 observation criteria, the estimated coefficient is 0.0645 and is found in row 691 of tab "Sheet1 (200-60-60, Mangum)" in the same file.

[347] By only analyzing customers that were found to be impacted by Dr. Stiroh's analysis, we have focused on customers with higher than average estimated overcharges. This is likely due to higher than average positive shocks (*i.e.*, idiosyncratic error terms) during the Class Period.

[348] See paragraph 155 above.

[349] Stiroh Report ¶¶ 38, 40, footnote 105; Stiroh Deposition pp. 181:19–183:12.

reliable results.[350] In this section, I discuss several of the specific shortcomings in Dr. Stiroh's assumptions about data sufficiency and demonstrate that her results are overturned when I improve the testing procedure she utilizes.

VI.H.1. Dr. Stiroh's Criteria Fails to Account for Data Variation

226. Counting observations is an insufficient standard for data selection. Some customers might meet Dr. Stiroh's arbitrary[351] "100/30/30" or "200/60/60" thresholds, yet their data is still insufficient to estimate the individual customer regressions. Dr. Stiroh alludes to the "technical complexity of the models" which prevents certain individual customer subregressions from estimating, when in fact it is the lack of observations *and* insufficient variation in her artificially limited data that prevent the estimation.[352] But she did not take any steps to assess the degree of variation in the data. For example, [redacted] purchased 801,462 pounds of beef and had 108 observations during the Class Period, with an additional 1.4 million pounds of beef and 306 observations during the benchmark. These purchases were sufficient to be ranked among the top 1,000 customers in Dr. Stiroh's analysis.[353] However, [redacted] was ultimately excluded from her individual customer subregressions because the overcharge coefficient would not estimate. As another example, [redacted] purchased over 8 million pounds of beef products and had 2,213 observations during the Class Period, with additional 346,067 pounds of beef and 51 observations in the benchmark. Again, [redacted] purchases were sufficient to be ranked among the top 1,000 customers by Dr. Stiroh, yet this customer is excluded from her individual customer subregressions. This is because, while [redacted]

[350] Stiroh Deposition pp. 184:25–185:9.

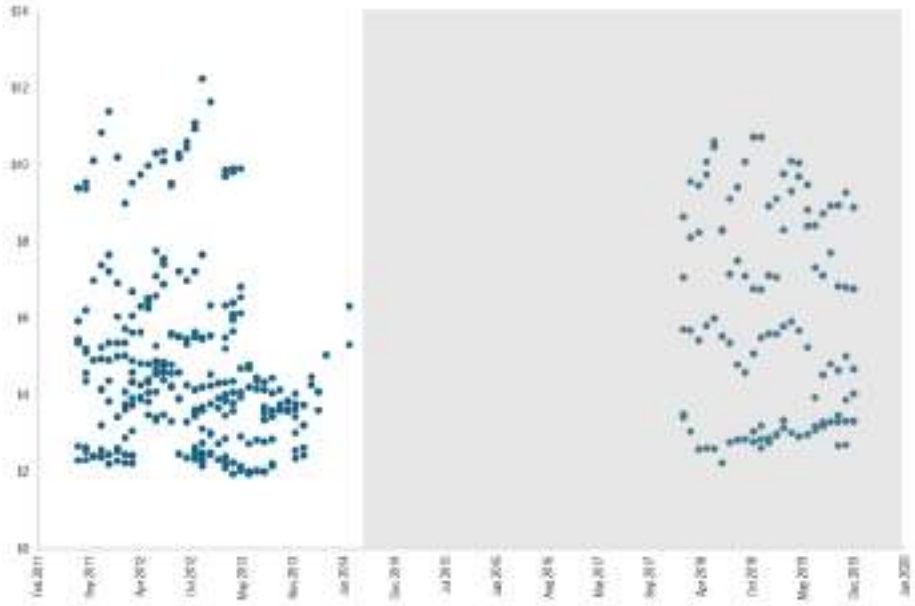
[351] Dr. Stiroh does not explain why she adopted these particular criteria anywhere in her report. In the Federal Judicial Center and National Research Council's *Reference Manual on Scientific Evidence, Third Edition* (2011), Professor Rubinfeld provides some commentary on the conditions or assumptions under which "the least squares estimators provide 'best' determinations of the true underlying parameters." (p. 342) He then writes, "If the further assumption is made that the probability distribution of each of the error terms is known, statistical statements can be made about the precision of the coefficient estimates. For relatively large samples (often, thirty or more data points will be sufficient for regressions with a small number of explanatory variables), the probability that the estimate of a parameter lies within an interval of 2 standard errors around the true parameter is approximately .95, or 95%." (p. 342). This statement is not applicable in this case, because there is not a "small number" of explanatory variables—there are as many explanatory variables as there are combinations of defendant, product, customer, and grade, as well as the other explanatory variables in my model. Further, the statement from the reference manual only relates to the accuracy of tests with respect to "type 1" errors, and does not account for the risk associated with "type 2" errors in hypothesis testing. I address this topic in detail in **Section VI.H.5** of this report.

[352] Stiroh Report, footnote 125. In her deposition, Dr. Stiroh acknowledged that insufficient variation in data leads to meaningless results. Stiroh Deposition, p. 183:1–8.

[353] See Stiroh Report, Exhibit 7C backup.

made numerous purchases in both the benchmark and Class Period, there was not a single product that was purchased in both periods.[354] This created a situation of perfect collinearity between the conspiracy period dummy and the product fixed effects included in the model.[355] This means that due to the timing of observations, or gaps in the data, the regression model cannot disentangle the effect from these two variables – it cannot estimate the marginal effect of the conspiracy period indicator while holding the product fixed effects variable constant. The gaps in in data for [redacted] are shown in **Figure 31** and **Figure 32** below, and demonstrate the limitations of relying on individual customer data, even when simplistic observation count criteria are met.

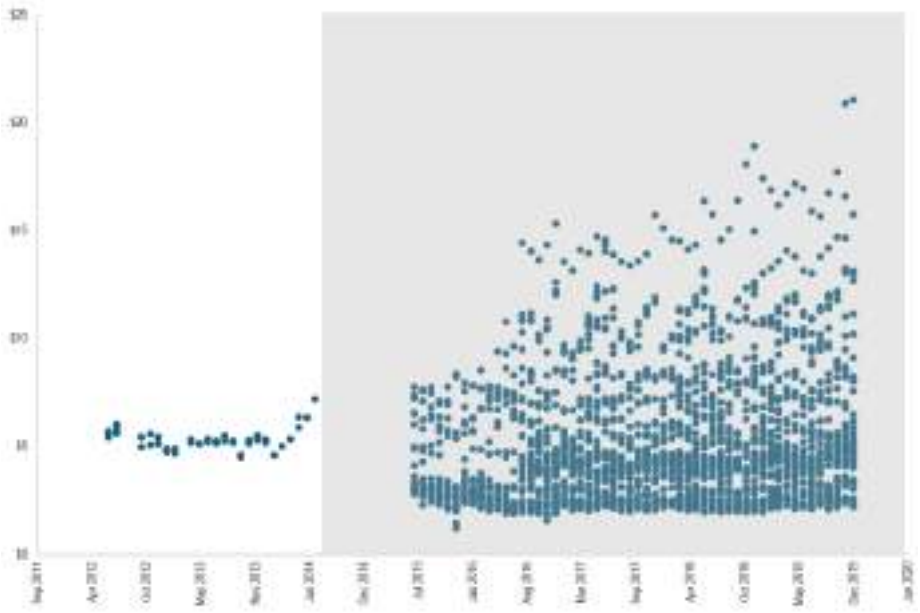
Figure 31. Monthly Product Net Prices Paid by [redacted]



[354] Dr. Stiroh acknowledged in her deposition that she did not impose any limitations on the number of observations per product for a customer. Stiroh Deposition p. 190:20-24.

[355] [redacted] are not unique in this regard. There are other purchasers, such as [redacted] who seemingly would fit in Dr. Stiroh's criteria, but which are excluded from her analysis. See backup production. *See also* Stiroh Report Exhibit 7C backup.

Figure 32. Monthly Product Net Prices Paid by [redacted]



227. This illustrates the issue of relying on the count of observations alone, since there are other parameters of the data (such as the timeframe covered or ensuring comparable products) that are critical (depending on the analyses) in order to ensure reliable results. On the individual customer level, hundreds of observations may not be enough data to estimate an overcharge, let alone estimate one reliably. Utilizing the entirety of the data, as I do in my regression model, eliminates the negative effects of idiosyncratic and customer-specific noise and allows for more accurate and reliable estimation of model parameters, including the overcharge.

VI.H.2. Dr. Stiroh's 100/30/30 Criterion is Misleading about the Information Content of the Subsamples it Produces

228. It is worth noting that Dr. Stiroh herself acknowledges—albeit indirectly—that small samples have a negative impact on the reliability of statistical analysis.[356] This is evident from the fact that she does not include all possible class members in her individual customer subregressions.

[356] Dr. Stiroh also explicitly stated this fact in a chapter she authored in the ABA Handbook on Econometrics. Dr. Stiroh explained that "A common problem associated with small sample sizes is that there may be insufficient variation in the sample data to allow the researcher to isolate the incremental effects of all of the variables of interest on the dependent variable. Parameter estimates tend to be less precise with smaller samples, and an analysis may be less likely to find statistical significance in a small dataset compared to the same model estimated on a larger dataset." (emphasis added). *See* ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues*, 2nd ed. (ABA Book Publishing, 2014), p. 111 and p. xvii (noting that Dr. Stiroh authored that chapter).

Rather, she constructs a criterion—based on observation counts—for determining whether a customer-specific subsample contains enough information to run an individual regression.[357] Unless she had concerns about data sufficiency and small sample estimation, there would be no reason to limit her subregressions in this way. Dr. Stiroh assumes that her 100/30/30 criteria ensures that a customer-specific subsample contains sufficient information.[358] While it is undeniable that the precision of regression estimates increases with sample size, sample size alone does not determine the reliability of statistical estimates. The precision of regression estimates is a function of sample size as well as the variation of the idiosyncratic error term, the degree of correlation between the explanatory variables, the amount of variation in the explanatory variables, and the number of explanatory variables.[359] Dr. Stiroh cannot adequately conclude that the customer-specific subsamples upon which she relies contain sufficient information without taking all of these factors into account, and she testified in her deposition that she took no steps to do so.[360]

VI.H.3. Dr. Stiroh Fails to Appropriately Account for Degrees of Freedom

229. An important concept in regression modeling is the "degrees of freedom" of a model. Whenever a researcher adds an explanatory variable to a regression model, the information requirements of the data increase. Since the model is now trying to do more with the same amount of data, it is as if an observation has been deleted. Statisticians refer to the sample size net of the number of explanatory variables as the degrees of freedom.[361] Degrees of freedom is still an insufficient criterion for determining the information content of a subsample for all the reasons discussed previously, but it is a more appropriate measure than sample size alone. In conducting econometric analysis in antitrust cases like this, where there are multiple products at issue, it is of particular importance to consider the impact of the product-specific fixed effects on the degrees of freedom. The number of degrees of freedom decreases by 1 for each fixed effect included in a model. The sample size in my overcharge regression model is

[357] *See* Stiroh Exhibit 6A and Exhibit 6B.

[358] Stiroh Report, footnote 105.

[359] Jeffrey M. Wooldridge, "Multiple Regression Analysis: Estimation," chap. 3 in *Introductory Econometrics: A Modern Approach*, 6th ed. (Cengage Learning, 2016).

[360] *See generally* Stiroh Deposition, pp. 182:5–193:21.

[361] Jeffrey M. Wooldridge, "Multiple Regression Analysis: Estimation," chap. 3 in *Introductory Econometrics: A Modern Approach*, 6th ed. (Cengage Learning, 2016).

large enough that the model still has over 3 million degrees of freedom, however the impact of numerous explanatory variables becomes more noticeable in small samples. Of the 375,123 "clusters" utilized in my direct overcharge regression model, defined by combinations of Defendant, customer, product, and grade, 152,633 contain only a single observation.[362] This means any model that includes a fixed effect for one of these clusters is identical to one in which those observations are simply deleted. Dr. Stiroh fails to account for the individual observations that are lost due to clusters with 1 observation, as well as the overall loss of degrees of freedom from the inclusion of fixed effects. Accordingly, numerous of the customer-specific subsamples considered by Dr. Stiroh fail to satisfy her own 100/30/30 criterion *after* correcting for degrees of freedom.[363]

230. While the minimum numbers of observations Dr. Stiroh imposes on her subsamples may produce sufficient information in certain settings, it also completely ignores the reality of repeated observations within the same time period. Using Dr. Stiroh's methodology for sufficient sample size, one would determine that a subsample for a customer that purchased 30 products in one month of the conspiracy and none in all others would produce statistically precise regression estimates. In reality, the regression estimates will be heavily influenced by period-specific shocks. This is not an issue for my direct overcharge regression since it utilizes all the available data and is not susceptible to idiosyncratic changes the way smaller samples are. 39.9 percent—nearly two-fifths—of the customer-specific subsamples considered by Dr. Stiroh fail to satisfy her 100/30/30 criterion when it is applied to months rather than observations (after removing singletons).[364]

VI.H.4. The Insufficient Information Content of the Customer-Specific Subsamples can be Illustrated by Calculating Threshold Overcharge Coefficients

231. In this section, I discuss in some detail some shortcomings in the individual customer subregression "testing" methodology that Dr. Stiroh employs, which allow her to exploit small sample estimation issues to support her conclusion of non-impact. I first review basic concepts related to hypothesis testing. Then I discuss the concept of "threshold" coefficients, which can

[362] See backup production.

[363] See backup production. *See also* Stiroh Report, Exhibit 6A and Exhibit 6B.

[364] See backup production.

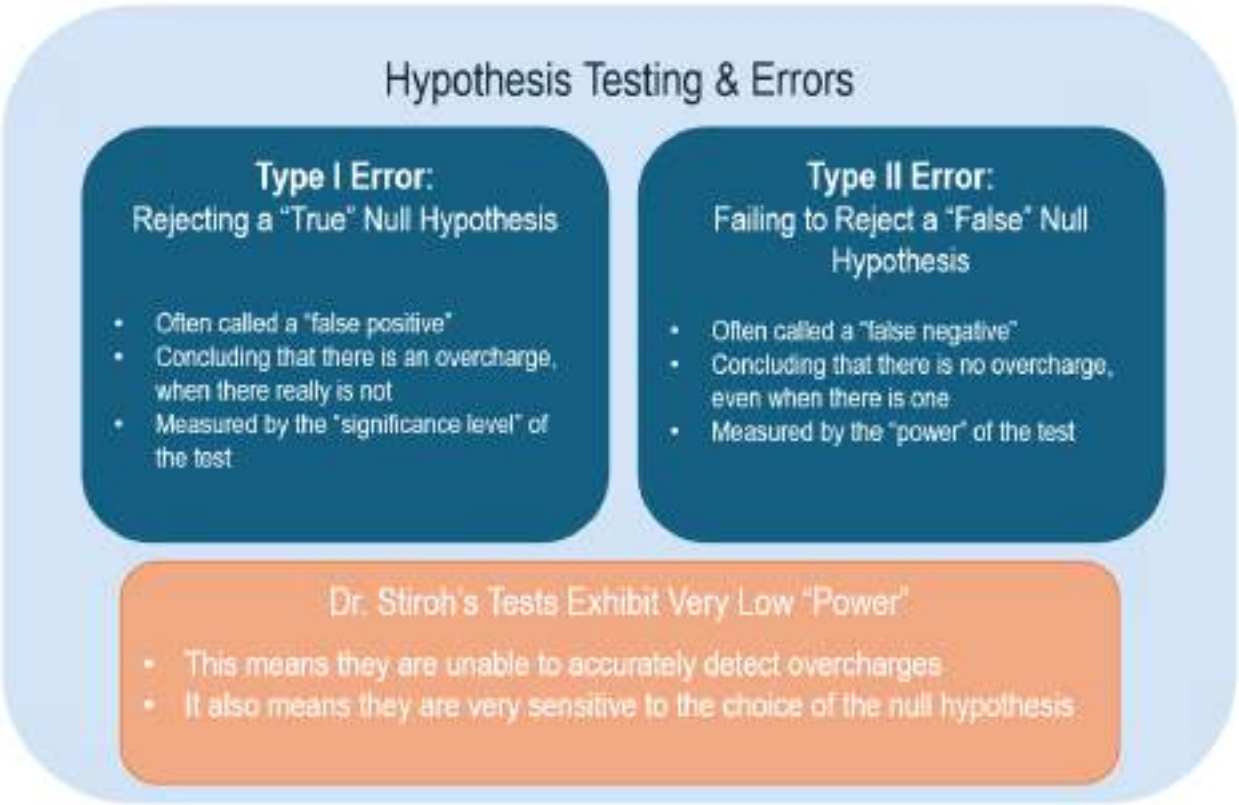
be used to demonstrate that Dr. Stiroh's testing approach is incapable of detecting overcharges of the magnitude I have estimated. In other words, her approach is rigged to deliver "false negatives"—it will fail to find overcharges even when they exist. Following that discussion, I demonstrate how Dr. Stiroh fundamentally misinterprets the results of her low-power statistical tests, which causes her to erroneously conclude that there is no impact. Finally, I demonstrate that, taking Dr. Stiroh's own subregression and testing methodology at face value, her results indicate that over 96 percent of the customers she tested paid an overcharge that cannot be distinguished from the overcharge I estimated.

VI.H.5. Overview of Hypothesis Tests

232. Hypothesis tests are an invaluable part of an applied economist's toolkit. Any statistical estimate—such as the overcharge coefficient—will differ somewhat from the true value due to the influence of random variation. A hypothesis test is one way of determining if the estimated coefficient (b) can be trusted as a reliable estimate of the "true" value. The basic idea is to compare a baseline (or *null hypothesis*) to some *alternative hypothesis* and test whether one can reject the null hypothesis (and therefore accept the alternative hypothesis).

233. Hypothesis testing involving statistical models necessarily involves some degree of uncertainty or risk of getting the "wrong answer." A common error, called a "Type 1" error, occurs when the investigator rejects a *true* null hypothesis. When this happens, the investigator concludes that something (an effect) exists when it actually does not. The probability of making a Type 1 error is typically denoted by the Greek letter alpha (α), which is the "significance level" chosen for the test. Another common error, called a "Type 2" error, occurs when the investigator fails to reject a *false* null hypothesis. When this happens, the investigator concludes that there is no effect, even when there really is. The probability of a Type 2 error is related to the "power" of a test (power = 1 – probability of a Type 2 error).

Figure 33. Dr. Stiroh's "Tests" Fail to Detect Overcharges Due to Low Power



234. In Dr. Stiroh's customer subregressions, she (implicitly) adopts the null hypothesis: true overcharge $= \beta_c = 0$, which indicates no overcharge for a particular customer. She then (implicitly) adopts the alternative hypothesis: true overcharge $= \beta_c \neq 0$, which indicates the alleged conspiracy did impact beef prices for a particular customer. The specific type of hypothesis test employed by Dr. Stiroh is a z test, which is done by constructing a z statistic, $z = \frac{b-0}{se(b)}$ and comparing to a user-selected number or "critical value." If the magnitude of z is greater than the critical value, the null hypothesis is rejected in favor of the alternative hypothesis.

235. In her report, Dr. Stiroh chooses a significance level (α) of 5 percent, which means the test will only reject the null hypothesis 5 percent of the time if the true value of the overcharge coefficient is in fact 0 (*i.e.*, it will only lead to a "false positive" 5 percent of the time).[365] By inspecting the formula for the z statistic, one can see it is impacted by 3 values: the coefficient estimate b, the "true" value assumed for the null hypothesis (0 in this case), and the standard

[365] Stiroh Report, footnote 115.

error of b ($se(b)$), which measures how much the value of b is determined by random chance. Because the size of the standard error is directly and negatively impacted by sample size, as the number of observations increases, the standard error decreases and we can have more confidence in b as a more reliable estimate of the impact of the alleged conspiracy. Conversely, the value of b can be driven almost entirely by chance if the sample size is too small. The formula for the z statistic tells us that there are 2 situations in which the null hypothesis will be rejected: 1) when b is substantially different from the null hypothesis of $\beta_c = 0$ and 2) when *any* difference exists and the standard error is sufficiently small. Thus, when the standard error is very large, it becomes difficult to reject the null hypothesis *regardless of whether the null hypothesis is actually correct*. In the context of this case, this means that a subregression will find that there is no overcharge, even when there actually is one.

VI.H.6. Calculation of "Threshold" Coefficients

236. Dr. Stiroh's claim that her 100/30/30 criterion creates adequately large samples implies that her tests to determine injured customers also have sufficient *power*. The power of a test is "the probability of rejecting the null hypothesis when the alternative hypothesis … is right."[366] Put another way, *if* Dr. Stiroh is correct that all her customer subsamples are sufficiently large, the standard errors from her regressions *must* be small enough that increasing the sample size would not alter her results. But, as I showed previously, Dr. Stiroh's subregression results are directly and negatively related to the size of the customer subsamples.[367] This is evidence that Dr. Stiroh's tests are suffering from low power—and therefore failing to reject false null hypotheses (*i.e.*, they are rejecting the presence of an overcharge that actually exists). To see this, I calculate the "threshold coefficient," which illustrates the impact of sample size on power and shows that Dr. Stiroh's testing approach will fail to detect overcharges.[368]

237. For a given statistical test and standard error, the threshold coefficient is the largest possible value an estimated coefficient can take for which the test will *not* reject the null hypothesis.

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[366] National Research Council, *Reference Manual on Scientific Evidence,* 3rd ed. (The National Academies Press, 2011), p. 254, footnote 106.

[367] See **Section VI.A** above.

[368] Calculation of the power of a test requires the assumption of a specific data generating process for the alternative hypothesis. See Russell Davidson and James G. MacKinnon, *Estimation and Inference in Econometrics* (Oxford University Press, 1993), p. 407.

The test used by Dr. Stiroh rejects the null hypothesis of "no impact" if the estimated overcharge coefficient (b) is positive and significant at the 5 percent level. The associated critical value (which ensures the test will only incorrectly reject the null hypothesis 5 percent of the time, assuming the null hypothesis is true) is 1.96. This means that Dr. Stiroh's test will conclude there is impact if $z = \frac{b-0}{se(b)} > 1.96$.[369] We can therefore compute the threshold level (b^*) of b as

$$b^* = 1.96 \times \text{standard error}(b).$$

Framing the analysis in terms of the threshold coefficient b^* allows one to determine if Dr. Stiroh's test will detect overcharges of the magnitude I estimated in the Mangum Class Report.

238. Calculation of b^* for each of Dr. Stiroh's individual customer subregressions demonstrates that her tests are woefully lacking in power and her methodology is *incapable of detecting overcharges that are of the magnitude I estimated*. Specifically, Dr. Stiroh's testing would reject the existence of an overcharge coefficient of 4.9 percent in 67.2 percent of her subregressions.[370] If the overcharge was only 2 percent, her testing procedure would fail to detect impact in 96.8 percent of her subregressions.[371] In contrast, my main regression model has substantial statistical power—it has a threshold overcharge coefficient of only 0.14 percent,

[369] Jeffrey M. Wooldridge, "Multiple Regression Analysis: Inference," chap. 4 in *Introductory Econometrics: A Modern Approach*, 6th ed. (Cengage Learning, 2016); Jeffrey M. Wooldridge, "Instrumental Variables Estimation and Two Stage Least Squares," chap. 15 in *Introductory Econometrics: A Modern Approach*, 6th ed. (Cengage Learning, 2016).

[370] Across her 1,000 subregressions, the threshold coefficient b^* reaches levels as high as 4,136.78 percent, with an average value of 21.58 percent and a median value of 6.33 percent. This means that, for one customer, Dr. Stiroh's subregression could estimate an overcharge of over 4,000 percent and her testing approach would still conclude that there was no impact. Further, in half of Dr. Stiroh's 1,000 subregressions, the estimated overcharge would have to be bigger than 6.33 percent to be found statistically significant. See backup production. It should be noted that I am referring to the overcharge *coefficient* and not the overcharge *rate* reported in the Mangum Report. This is done for consistency with Dr. Stiroh's analysis. She merely takes her estimated coefficient for the class dummy, multiplies it by 100 percent, and reports that as the overcharge. An overcharge coefficient of 6.33 percent is thus equivalent to 0.0633. For a given overcharge coefficient b, the overcharge rate is equal to $1 - \frac{1}{\exp(b-0.5\times se(b)^2)}$. There is little practical difference between the overcharge coefficient and the overcharge rate when the overcharge coefficient is less than 10 percent. This can be seen by noting that the [redacted] percent overcharge coefficient from my main regression produces an overcharge rate of [redacted] percent.

[371] See backup production.

which means it is able to detect an overcharge from the alleged conspiracy of essentially any magnitude.[372]

VI.H.7. Dr. Stiroh Misinterprets the Results of Her Low Power Tests

239. Given the low statistical power demonstrated by Dr. Stiroh's tests, it is not surprising that many of her tests fail to reject the null hypothesis of $b = 0$ (*i.e.*, fail to find impact). Due to the low power of her tests, Dr. Stiroh's finding that many of her chosen null hypotheses ($b = 0$ for each individual customer) cannot be rejected should thus not be taken as strong evidence that they are actually true.[373] This is merely a consequence of the considerable coefficient uncertainty Dr. Stiroh has manufactured by reducing the sample size. According to the *Reference Manual on Scientific Evidence,* "[w]hen a study with low power fails to show a significant effect, the results may therefore be more fairly described as inconclusive than negative. The proof is weak because power is low."[374] I note that Dr. Stiroh's opinions are inconsistent with this view since she does not describe her results as "inconclusive" in her report or deposition, where she emphasizes that they mean there is no impact.[375]

VI.H.8. Dr. Stiroh's Own Testing Methods Can Be Used to Demonstrate Common Impact

240. It is my opinion that running individual customer subregressions and performing hundreds and thousands of individual hypothesis tests is neither necessary nor economically or statistically justified. As I have shown in the previous sections, Dr. Stiroh's adoption of this approach leads to numerous errors and misleading conclusions. In this section, I show that, even taking her own testing methods at face value, the results of individual customer subregressions can be used to demonstrate the widespread impact she insists does not exist.

241. The lack of power in Dr. Stiroh's tests, which stems from large standard errors (and not from a lack of impact), means it is also difficult to reject *other* null hypotheses. A different choice of null hypothesis worth considering—and one potentially more conceptually relevant to the

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[372] See backup production.

[373] *I.e.*, Dr. Stiroh's failure to reject a null hypothesis that there is no overcharge is not meaningful evidence that there was not actually an overcharge.

[374] National Research Council, *Reference Manual on Scientific Evidence,* 3rd ed. (The National Academies Press, 2011), p. 254.

[375] *See, e.g.,* Stiroh Report, Section III.B. and III.C. headings (describing the results of her "tests" as demonstrating that customers were uninjured); Section VI. heading (describing Dr. Stiroh affirming her "finding" that many class members are uninjured); Stiroh Deposition, pp. 89, 91-92, 99, 102, 193, 197, 309-310, and 363.

question of common impact itself[376]—is that each customer-specific overcharge is equal to the overcharge estimated by my main regression. This is not substantially different in practice from Dr. Stiroh's procedure; she also assumes a common overcharge coefficient under the null hypothesis, only she sets it to 0.[377] If one merely starts with the assumption of a common overcharge coefficient, a z-test demonstrates that the estimated coefficient from my direct overcharge regression is a more reasonable choice than 0.

242. Dr. Stiroh reaches her conclusions about individual customer impact after running 1,000 hypothesis tests without a correction for multiple hypothesis testing.[378] To conduct my analysis, I repeated her tests but included a correction for multiple hypothesis testing and then tested her supposedly "unimpacted" customers against the pooled overcharge. This procedure finds that of the 1,000 customers she analyzed, 94.8 percent of those she deemed to be "uninjured" have an estimated overcharge coefficient that is not statistically different from the estimated overcharge in my regression model.[379] Thus the number of Dr. Stiroh's "unimpacted" customers for which the estimated overcharge is statistically *different* from the

[376] As I explained previously, the estimated overcharge is based on an econometric analysis of all direct purchasers' data and implies widespread impact. The data used in my regression model comes from all direct purchasers, and thus reflects the prices paid for beef in the marketplace by all direct purchasers. My model finds that, after controlling for nonconspiratorial factors of supply and demand, prices were elevated. If they were not, my regression model would not show an overcharge. Thus, the results of my model are evidence of *impact*. Dr. Stiroh's approach fundamentally rejects this evidence by testing a null hypothesis that the overcharge is equal to 0. A potentially more appropriate null hypothesis—after having found impact already—is whether any of the individual customers have an overcharge that statistically different from the pooled model.

[377] In a chapter she authored in the ABA Handbook on Econometrics, Dr. Stiroh explained that the choice of null hypothesis to test is often "0" but that any value can be tested. "If the estimated coefficient on the conspiracy variable is far from 0 (*i.e.*, the hypothesized true value of the effect of the conspiracy on prices), then the null hypothesis should be rejected. More generally, the null hypothesis typically tested is that the true population value of a coefficient of interest is equal to some value a," and "can be stated more generally as $H_0 : \beta_c = a$." *See* ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues*, 2nd ed. (ABA Book Publishing, 2014), p. 97 and p. xvii (noting that Dr. Stiroh authored that chapter). In this instance, I am testing whether individual subregression coefficients are statistically different from 4.9 percent, the overcharge I estimated in the Mangum Class Report.

[378] When running multiple hypothesis tests, the probability that the null hypothesis is incorrectly rejected in at least one of the tests is often significantly higher than the size of the test—the probability of this occurring in a particular test. If all the statistics in Dr. Stiroh's tests are independent, which is the case under the null hypothesis, the likelihood that at least one of Dr. Stiroh's hypotheses is incorrectly rejected is $1 - (1 - .05)^{1000}$. This means there is a greater than 99.99% chance of making a mistake. Multiple hypothesis testing corrections help mitigate this phenomenon. I employed the Benjamini-Hochberg correction, which allows the researcher to control the false discovery rate, the proportion of rejected null hypotheses that are actually true. I conducted all multiple testing corrections so as to ensure the false discovery rate is no greater than 5%. Trevor Hastie, Robert Tibshirani, and Jerome Friedman, *The Elements of Statistical Learning: Data Mining, Inference, and Prediction*, 2nd ed. (Springer-Verlag, 2009), pp. 683–690.

[379] See backup production.

pooled overcharge coefficient of 4.9 percent is only 36, or a 3.6 mere percent of total customers analyzed by Dr. Stiroh.[380] Expanding this test, when the null hypothesis is changed to 3 percent, *none* of Dr. Stiroh's "unimpacted" customers have an estimated overcharge coefficient that is statistically different.[381]

243. The results discussed above demonstrate the lack of power in Dr. Stiroh's tests for impact. A reliable method for the question at hand should not produce results that vary so widely in economic significance. For instance, my direct overcharge regression produces very precise estimates of the overcharge coefficient. The only alternative overcharge coefficients that cannot be rejected fall between [redacted][382] By contrast, nearly all of Dr. Stiroh's tests that conclude a customer is "unimpacted" cannot reject the coefficient of [redacted] percent found by my regression model. The wide variability in economic significance that can result from the application of Dr. Stiroh's method demonstrates its inherent unreliability.

VI.I. Dr. Stiroh's Justification for Individual Customer Subregressions Incorrectly Conflates the Concepts of Exogeneity and Independence

244. Dr. Stiroh cites the assumed exogeneity of the conspiracy dummy as justification for her individual customer subregression approach. She asserts:

[redacted]

[380] See backup production. Further, even smaller numbers of customers cannot reject other economically significant overcharge coefficients. The number of customers that Dr. Stiroh' method deems "unimpacted" and also for which the estimated overcharge is statistically different from 4 percent accounts for only 0.3 percent of the total customers analyzed by Dr. Stiroh.

[381] See backup production.

[382] This is the 95 percent confidence interval, calculated as $b \pm 1.96 \times se(b)$. See Mangum Report backup production for my direct overcharge regression results.

[redacted]383

245. Dr. Stiroh incorrectly conflates the concepts of structural *exogeneity* and statistical *independence*. Structural exogeneity, the assumption that one variable does not causally impact another, does not imply statistical independence, which occurs when knowing the value of one random variable provides no information about the outcome of another random variable. In the context of linear regression, the relevant measure of statistical dependence between two variables is the correlation coefficient, which measures the degree to which the variables move together in a linear fashion. All that is required for the conspiracy dummy to be correlated with another explanatory variable is that the average value of the explanatory variable during the benchmark period is different from the average value of the explanatory variable during the conspiracy period. This is virtually guaranteed to occur in practice, especially when the explanatory variable exhibits a large degree of autocorrelation or persistence and can deviate from its average value for long periods of time. Correlation can also occur at a structural level if the explanatory variable impacts the probability of the onset or continuance of a conspiracy. Possible correlation between the conspiracy dummy and other relevant explanatory variables is what necessitates the use of multiple regression in the first place. If one could reasonably assume the conspiracy dummy is independent of all other explanatory variables, the impact of the conspiracy could be estimated through the simple regression of beef prices on a constant and the conspiracy dummy. This would be equivalent to simply calculating the difference in average beef prices during the conspiracy period and the benchmark period.

246. A simple way to explore the distinction between exogeneity and independence is by considering the relationship between the conspiracy dummy and the Holcomb fire dummy. The Holcomb fire was a temporary but significant disruption to the beef industry. Given the sharp negative impact this event had on industry-wide capacity, economic theory would predict that this event elevated beef prices. Since the fire occurred during the Class Period, failure to account for its effects could lead to overestimation of the conspiracy effects. Given the fire occurred entirely during the Class Period, the conspiracy dummy and the fire dummy are positively correlated. By Dr. Stiroh's apparent logic, this positive correlation implies the

383 Stiroh Report, footnote 117.

conspiracy *caused* the Holcomb fire, an assertion that I am not aware of any plaintiff or Defendant making thus far. This example highlights the danger of Dr. Stiroh's interchangeable use of exogeneity and independence.

247. The unsurprising fact of statistical dependence between the conspiracy dummy and other explanatory variables likely to impact beef prices underscores the value of multiple regression and the importance of a large sample size. Dr. Stiroh is correct to point out that more observations lead to better estimates of the effects of common factors. But she has failed to understand that this improved estimation of common factors also leads to better estimates of the conspiracy effects through better estimates of the portion of beef prices not explained by other explanatory variables, as well as the relationships between those variables and the conspiracy dummy.

VI.J. Conclusion Regarding Sample Size, Data Sufficiency, and Dr. Stiroh's "Testing"

248. As I have addressed in detail above, the question of whether there is "enough" data for a particular analysis in this (or any other) case cannot be answered solely by an appeal to a specific number of observations. In some econometric applications, a given number of observations may be enough; in other applications, that same number may be woefully insufficient. It simply depends on the richness and quality of the data being used, as well as the questions the model is being asked to answer. In this case, the results of Dr. Stiroh's subregressions alone are proof that she did not have enough data to reach the conclusions she did. The relationship between customer sample sizes and Dr. Stiroh's finding of "no impact" is obvious and entirely predictable.

249. Dr. Stiroh emphasizes the number of customers she purports to find unimpacted, but she does not discuss who these customers are or offer any explanation or theory as to why those specific customers might be unimpacted. In her report, Dr. Stiroh asserts that individualized negotiations, different contracting or pricing mechanisms, and bargaining power *could* lead to customers avoiding anticompetitive impact from the alleged conspiracy,[384] but she does not

[384] Stiroh Report, ¶ 24.

actually test such speculation against her results.[385] Once the association between an apparent lack of impact and sample size is recognized, it becomes clear that Dr. Stiroh's results rely on the false assumption that bargaining power is *inversely* related to customer size.

250. In conclusion, Dr. Stiroh's purported finding of non-impact is not based on the actual effects the Defendants' alleged conspiracy had on prices for beef; it is based on her choice of a statistical method that is engineered to fail a "test" of "common impact" that she has created herself.

VII. All or Nearly All Indirect Class Members Were Impacted by Artificially Inflated Beef Prices

251. In addition to analyzing the beef market and the prices charged by Defendants, my assignment in the Mangum Class Report included evaluating theoretical, documentary, and empirical evidence related to pass-through in order to determine whether the effect of the alleged conspiracy on prices paid by direct purchasers for beef products would be passed on, in whole or in part, to downstream customers, and to the Consumer IPP class in particular. Dr. Stiroh does not address my pass-through analysis.[386]

VII.A. Dr. Stiroh Fails to Meaningfully Address My Pass-Through Analysis

252. In the Mangum Class Report, I explained that economic theory predicts that some amount of an overcharge would pass through the distribution chain down to the consumer since resellers of a product must recover their short-run variable costs when they price their products for the market.[387] I explained that basic principles of economics emphasize that cost pass-through is not simply the expected outcome, but a necessary outcome for distributors to remain in business. While an individual firm may, in certain contexts, be willing or able to absorb costs over the short run, that is not an economically viable long-run strategy. As such, economic theory predicts that at least some portion of beef purchase costs are passed from direct

[385] Running individual customer subregressions does not, for example, test any of the potential hypotheses that Dr. Stiroh raises in her report. For example, none of her subregressions answer questions about whether small customers (¶ 26) were unimpacted, whether different products (¶ 23) were impacted differently, or whether controlling for purchase or contract types (¶ 24) make any difference.

[386] Stiroh Deposition, p. 303:23–304:4.

[387] Mangum Class Report Section VI.A.

purchasers to Consumer IPP class members. Therefore, if Defendants were able to successfully raise prices to supracompetitive levels through collusion, all or virtually all Consumer IPP class members would suffer at least some harm as a result.

253. I showed that overcharges would be passed through to downstream customers, leading to impact on consumers. I utilized extensive third-party data productions to demonstrate that the degree of pass-through was very high, with most estimates at or above [redacted] percent.[388] In her rebuttal report, Dr. Stiroh comments only briefly on the topic of pass-through—indeed, she writes only two brief paragraphs, which are largely duplicative in content.[389]

254. First, Dr. Stiroh opines that my overcharge model is flawed and fails to find impact for many customers, and therefore there is no overcharge to pass-through to indirect purchasers. However, Dr. Stiroh's sole basis for concluding that many customers were uninjured is her individual customer subregression analysis, which is fatally flawed. As I have demonstrated, Dr. Stiroh's individual customer subregressions are rigged to find a lack of impact. Her work does nothing more than demonstrate how variances become very wide when small samples are forced to answer questions that require lots of data. Thus, Dr. Stiroh's results do not provide any evidence of uninjured class members. If anything, they demonstrate the opposite—the largest and most powerful customers in the industry consistently show "overcharges" in Dr. Stiroh's methodology. Dr. Stiroh has not offered any explanation or reasoning for how the most powerful customers in the marketplace could not avoid impact, but the smallest and least powerful customers were purportedly able to do so.[390] Regardless, it is worth noting that even taking Dr. Stiroh's analysis at face value, most consumers purchase beef through massive retailers like [redacted] and other supermarkets, which consistently show overcharges in Dr. Stiroh's subregressions.

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[388] [redacted] *See* Mangum Class Report, Figure 56 and Figure 60.

[389] Stiroh Report, ¶¶ 50 and 51.

[390] Dr. Stiroh's failure to question these results is inconsistent with her own writings elsewhere. In a chapter she authored for the ABA Econometrics Handbook, Dr. Stiroh wrote that "The first step in evaluating and interpreting regression results is simply assessing the estimated coefficients to ensure that they appear to be reasonable, logical, and consistent with economic theory." (ABA Antitrust Law Section, *Econometrics: Legal, Practical, and Technical Issues*, 2nd ed. (ABA Book Publishing, 2014) p. 90 and p. xvii (noting that Dr. Stiroh authored that chapter)). The fact that Dr. Stiroh's subregressions appear to indicate impact for largest and most powerful customers and no impact for smaller customers is contrary to economic theory and should have been a red flag.

255. Second, Dr. Stiroh opines that "there is substantial variation in the extent to which the alleged overcharges would have been passed through."[391] It is unclear how Dr. Stiroh's comment here is actually a criticism. **Figure 35** below depicts the estimated pass-through rates for 35 different third parties—retailers, distributors, and grocery wholesalers. There were four entities with estimated pass-through rates below [redacted] percent (the lowest estimate being [redacted] percent), and seven entities with estimated pass-through rates over [redacted] percent. The remaining 24 entities all have estimated pass-through rates sandwiched more closely around [redacted] percent.

256. The degree of variation in estimated pass-through rates in the Mangum Class Report is neither surprising nor concerning. While I used a consistent model to conduct my analysis, the quality and quantity of the data varies from third party to third party, depending on how each firm maintained records. As such, it is not expected that the estimated pass-through rates would all be identical—it would be surprising if the estimated rates were identical. But estimates of pass-through are helpfully informed by economic theory and voluminous economic literature, which point toward competitive markets exhibiting full pass-through. Thus, reasonable variation in empirical estimates can be more confidently understood to represent shortcomings in the data and idiosyncratic noise than evidence that resellers fail to act as resellers by not marking up their wholesale purchases. Finally, and most importantly, it is not my understanding that pass-through rates must be exactly 100 percent to begin with. As I noted in the Mangum Class Report, it is my understanding that any pass-through above 0 percent would indicate harm to the class.[392] Since none of the estimated pass-through rates are lower than [redacted] percent, and almost all are above [redacted] percent, there is no valid economic basis for concluding that overcharges would not be passed through to indirect purchasers.

VII.B. Class Products Make Up the Majority of Consumer Primal Beef Purchases

257. Dr. Stiroh criticizes the Class Definition for excluding certain beef primals, claiming that "[e]xcluded products overall account for approximately 30 to 40 percent of Defendants' sales of products derived from fed cattle over the Class Period."[393] As an initial point, it is unclear how a class definition that excludes certain products is a valid criticism to begin with. It is not

[391] Stiroh Report, ¶ 50.
[392] Mangum Class Report, ¶¶ 438 and 490.
[393] Stiroh Report, ¶ 10.

my understanding that a class definition must necessarily include all products in a market or even all potentially affected products. Further, Dr. Stiroh fails to acknowledge that Class Products are responsible for a large majority of primal sales and the excluded primals are a relatively minor component of beef sales. Over 75 percent of the primal beef products sold by Defendants are Class Products, meaning less than a quarter of primal cuts sold come from excluded primals. Grocery store point-of-sale data provided by IRI confirms the pervasiveness of Class Products. According to IRI data, Class Products constitute over 78 percent of beef primal purchases made by consumers at grocery stores.

Figure 34. Portion of Primal Beef Products Included in the Class [394]

258. Additionally, the 4 primals that constitute all Class Products are known as the "major" primals, while the excluded primals are referred to as "[redacted]."[395] It is unsurprising then that Class Products form the majority of Defendant primal sales. Due to the ubiquity of Class Products, any overcharge applied to the Class Products would be incredibly difficult for beef consumers to avoid.

259. The fact that Class Products account for a large portion of primal sales at the start of the Consumer IPP supply chain and a similarly large portion of primal sales at the end of the supply chain, suggests Class Products maintain its majority of primal sales throughout the supply

[394] See backup production.

[395] Kevin Hueser (Tyson), "Beef 101" (presentation, Jun. 20, 2014), slides 30–36 (TYSONBEEF02460541–92 at 70).

chain. Class Products reflect the commonly purchased and commonly consumed beef primals. While it is likely that beef products excluded from the Class Products were affected by the alleged wrongdoing in this case, their exclusion does not detract from the accuracy or highly representative nature of my analysis.

VII.C. Updated Pass Through Analysis

260. After the Mangum Class Report was filed, I received new or additional data from 4 third parties: [redacted] I have updated my pass-through analysis to include this data.[397] I now estimate pass-through for 35 third parties that represent the range of reseller types including wholesalers, retailers, distributors, and re-distributors. These third parties account for around 70 percent of Defendant sales into the Consumer IPP supply chain, and grocery stores included in my analysis remain 51 percent of the US retail commerce for consumers.[398] The fact that third parties in my analysis cover over two thirds of relevant commerce ensures that pass-through results are a robust and reliable measure of the overcharge that has been passed through to virtually all Consumer IPP class members.

261. The results from my updated pass-through regression analyses further support the existence and consistency of high pass-through rates that I established in the Mangum Class Report.[399] Pass-through rates for all 4 updated third parties are consistently high, ranging from [redacted] percent, and statistically significant at the 99 percent level.[400] Additionally, the updated weighted average, median, and simple average pass-through rates remain around 100 percent.

[396] [redacted]

[397] See **Appendix G** for a summary of the new and updated third-party data.

[398] See backup production; Mangum Class Report ¶ 483 and footnote 1127. This also accounts for a correction to include a third party company, Nash Finch, that was inadvertently excluded in my original calculation.

[399] For a discussion on pass-through regression results and their interpretation, see Mangum Class Report, Section VI.C.4.c.

[400] See backup production.

Figure 35. Pass Through Regression Results[401]

Third Party	Coefficient Estimate	Price to Cost Ratio	Pass Through	Observations Used	Sales Used	Time Period
Distributors and Grocery Wholesalers						
[redacted]						
Retailers						
[redacted]						

262. After updating my analysis to include the additional data, I find the weighted pass-through rate remains [redacted] percent across the 35 third parties. A pass-through rate of 100 percent indicates every penny increase (or decrease) in the price paid by a distributor for the beef product results

[401] See backup production.

in a penny increase (or decrease) in the price charged by that distributor, and a pass-through rate above 100 percent is consistent with a percentage markup. I explained in the Mangum Class Report, that while 100 percent pass through is how distributors generally operated, there are a number of reasons why one may not necessarily expect to see 100 percent pass-through across the board, including slight delays in implementing price adjustments or differences across third parties in that timing.[402] Further, based on observation count, over 95% of the third party data have pass through estimates that fall between [redacted][403]

263. My finding of a pass through rate of [redacted] percent is consistent with the well accepted economic theory of pass through and the qualitative evidence of the behavior of distributors, grocery wholesalers and retailers I discussed in the Mangum Class Report.[404] My updated pass-through analysis further demonstrates that purchasers of beef passed through [redacted] in cost and therefore [redacted] percent of the overcharge on beef products would have been passed through to Consumer IPP class members.

VII.D. Estimation Of Relevant Sales and Damages

264. In the Mangum Class Report, I presented estimates of 1) the relevant sales to be used in a class-wide damages calculation, and 2) class-wide damages, using the relevant commerce, my estimated overcharge, and a pass-through rate of [redacted] percent.[405] *Dr. Stiroh does not comment on, let alone dispute, either of these calculations*.[406]

265. Since filing the Mangum Class Report, I have received updated or additional data for 2 third parties that are multichannel distributors and have customer information: [redacted][407] I have updated my multichannel analysis to include this data. As explained further in the Mangum Class Report, I classified the customers comprising the top 70 percent (or more) of sales as "retail channel" or not. Taking a weighted average of the

[402] Mangum Class Report, ¶490

[403] See backup production.

[404] Mangum Class Report, Section VI.

[405] Mangum Class Report, Section VII; Mangum Figure 60.

[406] Stiroh Deposition, pp. 79:15–80:10.

[407] The [redacted] sales data that I relied on for the Mangum Class Report did not have any customer information. The updated [redacted] sales data has fields for customer name and customer category, allowing for customer categorization.

estimate for each of these multichannel distributors yields 69.5 percent of sales to the retail channel.

Figure 36. Multichannel Relevant Commerce [408]

Third Party	% of Sales Categorized	% of Sales to Retail Channel
[redacted]	90.04%	84.60%
[redacted]	90.11%	61.77%
[redacted]	100.00%	28.81%
[redacted]	74.78%	75.59%
[redacted]	95.64%	71.77%
[redacted]	99.02%	86.00%
Simple Avg	**91.60%**	**68.09%**
Weighted Avg	**88.25%**	**69.46%**

Source: Third Party Data.

266. I apply 69 percent estimate to all customers that I have categorized as multichannel distributors to estimate the portion of commerce sold to retail.

Figure 37. Relevant Nationwide Consumer IPP Purchases by Primal, Aug 2014 - Dec 2019

[redacted]

Source: Defendant Transaction Data.
Note: Totals include 100% of sales to grocery stores and grocery wholesalers, 69% of sales to multichannel distributors, and 0% of sales to all other channels.

267. The portion of relevant sales attributable to the Class States can be reliably estimated using available data on the nationwide population for each state. As discussed in the Mangum Class

[408] See backup production. Note that the percentages for the relevant retail commerce for [redacted] and [redacted] differ slightly from what I presented in Figure 57 of my Class Certification Report. This because I use the total average percentage for multichannel commerce in the calculation of relevant commerce for these third parties that sold *to* multichannel distributors.

Report, I use 55 percent for the portion of nationwide commerce that is attributable to the Class States.[409]

268. Damages are found by applying the relevant overcharge and the pass-through rate to the relevant sales. For example, the average price during the Class period of Tyson's boneless loin strip (choice grade) product was $5.92 per pound and had a total of $398,624,431 in relevant sales (limiting to Consumer IPP Channels and Class states). I estimated an overcharge of [redacted] percent which means that the price direct purchasers would have paid for Tyson's boneless loin strip (choice grade), absent the alleged conspiracy, is $5.65 per pound. This difference in price means that direct consumers in the Consumer IPP sales channels were overcharged a total of $19,165,863 on this product alone. Lastly, based on my finding of [redacted] percent pass through, these overcharges were passed entirely on to Consumer IPP class members who therefore incurred damages in the amount of [redacted] for their purchases of Tyson's boneless loin strip (choice grade). The table below shows the calculation of damages for the top product for each primal purchased during the Class Period.

Figure 38. Illustration of Consumer IPP Damages Calculation for Top Products by Primal, August 2014 - December 2019[410]

Primal	Product Description	Average Price Per LB	[redacted]
CHUCK	BF CHUCK ROLL NECK-OFF CHOICE	$2.83	
LOIN	BNLS LOIN STRIP 0X1 CHOICE	$5.92	
RIB	RIB EYE LIPON HVY CHOICE	$7.45	
ROUND	INSIDE ROUND XT	$2.30	

Source: Defendant Transaction Data.

Note: Damages are calculated as relevant nationwide Consumer IPP purchases from Defendants apportioned to relevant IPP states based on population. This relevant state commerce is then multiplied by the overcharge rate [redacted] from Mangum Class Report Figure 50 and the pass through rate of [redacted]

269. The table below depicts damages to Consumer IPP class members during the Class Period, updated to reflect additional third-party data I have received since filing the Mangum Class Report. Damages are calculated as the relevant nationwide Consumer IPP purchases reduced by 45 percent to ensure damages attributable only to the Class States. This commerce total is

[409] Mangum Class Report, ¶ 498 and Figure 59.

[410] See backup production.

then multiplied by the overcharge percentage of [redacted] percent and a passthrough rate of [redacted] percent.[411]

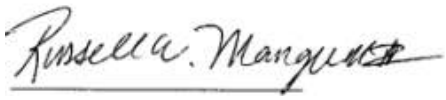
Figure 39. Consumer IPP Damages, August 2014 – December 2019

[redacted]

Source: Defendant Transaction Data.

Note: Damages are calculated as relevant nationwide Consumer IPP purchases from Defendants reduced by 45% to account for purchases that occurred outside of Class States and then multiplied by the overcharge rate [redacted] from Mangum Class Report Figure 50 and the pass through rate of [redacted].

I declare under penalty of perjury under the laws of the United States that the foregoing is true and correct to the best of my knowledge and belief and that this report was executed in Irvine, California, this 4th day of April 2025.



Russell W. Mangum III

[411] Mangum Class Report Figure 50. See **Appendix H** for a breakdown of damages by Consumer IPP state.

Appendix A

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CURRENT POSITION

Executive Vice President, Cirque Analytics; 2021–present

EDUCATION

Ph.D., Economics, University of Southern California, 1995
M.A., Economics, University of Southern California, 1992
M.A., Christian Apologetics (highest hons) Biola University, 2023
B.A., Economics (hons), Calif. State University, Fullerton, 1988

SPECIALIZED EXPERIENCE, RESEARCH, OR INTEREST

Antitrust; Commercial Disputes; Intellectual Property; Statistics and Econometrics, Valuation

PAST POSITIONS

2013–2022	Professor, Economics, Concordia University Irvine (retired)	Irvine, CA
2021	Visiting Researcher, Univ of Winchester, Sch of Bus, Law, and Tech	Winchester, UK
2007–2021	Senior Vice President, Nathan Associates Inc.	Irvine, CA
2002–2012	Associate Adjunct Professor, Economics, Univ of Southern California	Los Angeles, CA
2001–2007	Vice President, Analysis Group, Inc.	Los Angeles, CA
2001	Manager, PricewaterhouseCoopers, Financial Advisory Services	Los Angeles, CA
1998–2001	Managing Associate, Nathan Associates Inc.	Arlington, VA
1998–2000	Adjunct Professor, Economics, Johns Hopkins University	Washington, DC
1995–1998	Economist, U.S. Federal Trade Commission	Washington, DC

COURSES TAUGHT

Principles and Intermediate Economics, Managerial Economics, Statistics and Econometrics, Finance and Financial Markets, Environmental Economics, Business Info. Technology, Advanced Topics in Economics

EXPERIENCE SUMMARY

Dr. Mangum has over 30 years of experience in economic analysis, research, and teaching. His consulting practice centers on economic analysis and damages in matters related to IP and technology, antitrust, and complex commercial disputes, with particular application to class certification, statistical analysis, and use of survey data. Dr. Mangum's experience as an expert is extensive, with testimony in over 125 matters before local, state, and federal courts. Dr. Mangum has taught graduate and undergraduate courses in economics, statistics, finance, and econometrics at several universities, including Johns Hopkins, USC, Pepperdine and Concordia University Irvine. Dr. Mangum's career has included employment by Nathan Associates, Inc., Analysis Group, PricewaterhouseCoopers, and The US Federal Trade Commission.

PROFESSIONAL EXPERIENCE

Intellectual Property

Dr. Mangum has substantial experience in the area of intellectual property damages, including claims related to infringement of patents; FRAND licensing commitments; patent pools; copyrights; and trademarks; as well as theft of trade secrets; false designation of origin; and false advertising. The case contexts in which Dr. Mangum has performed these analyses include:

- Patent infringement and disputes related to:
  - Computer, electronics, and telecommunication industry:
    - Cellular communication network technology;
    - Modem communication devices;
    - Wireless communication network devices (routers, cards, including under FRAND licensing commitments);
    - Handheld device navigation applications;
    - NIC hardware and chipsets;
    - Semiconductors;
    - Webswitching and IP router network hardware;
    - Wired and wireless portable electronic temperature sensor devices;
    - Electronic eReader devices;
    - Digital TV Tuners under FRAND licensing commitments;
    - Automated lip-sync animation used in video games;
    - Data encryption devices.
    - VoIP network telephony services.
    - Video streaming services and applications.
  - Medical devices:
    - Artificial vertebral disc implants;
    - Trocar seals for laparoscopic surgery;
    - Spinal fusion implants;
    - Breast biopsy devices;
    - Remote medical information monitoring technology;
    - Intraarterial guidewire and embolic filter devices.
  - Energy
    - Specialized valves used in oil refining;
    - Electric utility management systems;
    - Wide-area real time phasor measurement and monitoring.
  - Food and agriculture:
    - Additive-infused confections;

    - Nutritional supplements;
    - New variety of late ripening white grapes;
    - Structures and methods utilized in the growing of grapes and raisins;
    - Cold extraction coffee processes.
  - Business software
    - eProcurement;
    - Business intelligence;
    - Design and simulation;
    - Call routing software;
    - Computer tracking;
    - Program and application management;
  - Clothing and clothing design
    - Padded athletic shirts/pants; shoes; headwear; accessories;
  - Miscellaneous
    - Electronic nicotine delivery systems (NDS)
    - Personal watercraft devices and accessories
    - Consumer advertising design via use of digital media
    - Automated stapling machines used in bed manufacturing
    - Specialized hardware and control systems used in high-rise elevators
    - Electronic exchange systems for trading of commodities futures contracts
    - Electronic data management system used in public transportation projects
    - Document and print inspection systems
- <u>Trademark, trade dress, or copyright infringement related to:</u>
  - Environmental Cleanup/Transport/Disposal Services
  - IT Network equipment (transceivers and switches);
  - Sponsorship with motorsports, automotive repair tools and devices, beverages and snacks, and apparel;
  - Banking services;
  - Skincare and cosmetics;
  - Real estate property acquisition services;
  - Online dining reservation and payment services;
  - Internet search engine terms related to retail sales of food and arranged food products;
  - Enterprise Resource Planning (ERP) software;
  - Veterinary Teleradiology (online/internet) Services;
  - Devices and software for online mobile device data extraction and IT network devices;

- Clothing, shoes, and jewelry;
- Advertising and marketing through wireless mobile communications;
- Motion picture trademarks in the manufacture of clothing;
- Furniture products (mechanized and non-mechanized);
- Portable combustion engines and portable air compressors
- Infant care products;
- Homeopathic products;
- Postal measuring products;
- Scented candle products;
- Children's toys;
- Art and art exhibits
- Design plans for a theme amusement park.

- Theft of trade secrets related to:
  - Neuroendovascular coils and catheters;
  - Cold extraction coffee processes
  - Electronic mechanisms for payment processing;
  - Technical documents, Product features, customer data, and marketing methods/models related to Systems for General Floor Hospital Monitoring of patient vital statistics;
  - Training methods, pricing models, and customer status databases related to Enterprise Resource Planning (ERP) software;
  - Customer data and information, and pricing models related to employee pension and benefits insurance brokerage services;
  - Government contracted research into laser vibrometry;
  - Devices and software for mobile device data extraction;
  - IT system design and implementation for the US defense industry;
  - Electronic engineering and CAD packages used in US naval warcraft architecture;
  - Methods for mathematical simulations for the pricing of mortgage backed securities;
  - Soy coffee alternative products;
  - Design, development, marketing, and manufacturing of toys;
  - Computer game accessories.
- False advertising, false designation of origin, or unauthorized use of likeness related to:
  - Chemical dependence treatment services;
  - Real estate property acquisition services;
  - Security monitoring systems and services;
  - Consumer appliances;
  - Medical data printer systems;

- Furniture products;
- Composed music and lyrics used in television commercials;
- Restaurant meals and shopping services;
- Internet advertising services via advertorial placement on publishers' websites;
- Nutritional supplements and beverages;
- Orthodontic and Dental devices and services;

Data Storage/Unauthorized Usage/Breaches

- Contractual dispute of data storage practices (data storage services industry)
- Contractual dispute of data collection and use practices (internet browsing services industry)
- Improper/fraudulent data security measures and resulting data breach (non-profit data maximization services industry)
- Improper/fraudulent data security measures and resulting data breach (file-sharing and data transfer services industry)
- Unauthorized capture of personal data (internet browsing services and social media services industry)

Competition/Antitrust

Dr. Mangum has substantial experience in the area of competition and antitrust, including analyses of relevant product and geographic markets, market power, monopolization, and likelihood of monopolization from impending events. These analyses usually include statistical and econometric analysis of market data to identify the extent of competition, and the magnitude of competition. The case contexts in which Dr. Mangum has performed these analyses include:

- Evaluated common impact and estimated damages, for direct and indirect purchasers, from price fixing and other conspiracies, including the markets for commercial tissue paper, bulk vitamins, high-end automobiles, ready mix concrete, consumer apparel, Korean noodles, packaged seafood, meat products, interior molded doors, airline travel, and pharmaceuticals.
- Evaluation of alleged competitive foreclosure in the market for sleep apnea products, including relevant markets, market power, and lost profits damages.
- Evaluation of alleged price discrimination across dealers of hardscape building materials.
- Evaluation of antitrust claims and affirmative defenses of patent misuse related to required terms in patent license programs for flash memory semiconductors and systems.
- Evaluation of market segments, market channels, and cost pass-through in the market for DRAM-containing products and NFL brand apparel.
- Estimation of damages related to:
  - A conspiracy to boycott developments in DRAM packaging;
  - Foreclosure of competition in market for footwear insoles and inserts.
- Evaluation of competitive effects of exclusive dealing clause in a franchise agreement.
- Evaluated the competitive effects of exclusive dealing policies regarding:

- Acute care hospital and physician services;
- Customer purchase data exchange related to direct mail advertising and sales;
- Free standing insert advertising (coupon) services;
- Replacement parts for 3-piece body welder systems;
- Interconnect agreements between internet backbone communication services;
- Supply of biological inputs used in creating generic biologic therapeutic treatments;
- Professional sports branded athletic apparel;
- Durable medical equipment;
- Pharmaceuticals.

- Analyzed the competitive effects from wrongful patent application and issuance (fraud on the patent office) related to processes and mechanisms for food preparation and processing.
- Analyzed the likely competitive effects of proposed mergers in various industries, including hospital services, physician services, pharmaceuticals, medical insurance, construction aggregates, supermarkets, auto parts, cable systems and programming, industrial refractories, and computer game software.

Commercial Disputes

- Evaluated damages related to a data breach and the improper capture and transmission of personalized information held and maintained in commercial database(s).
- Evaluated damages related to alleged breach of contract involving collection and retention of personalized information connected to web browsing activity.
- Evaluated damages related to alleged breach of contract involving online storage agreements.
- Evaluated claims of damages related to attempted sale of cold extraction coffee company and the discovery of patents allegedly based on confidential information.
- Evaluated claims of damages related to failure to close a sale for multiple solar energy production properties/companies.
- Estimated damages in the form of lost profits from breach of contract in a services joint venture involving use of indexes and associated data for creation and analysis of international financial securitized and derivatives.
- Estimated damages in the form of disgorgement and lost company value related to brokerage services involving employee pension and benefit programs.
- Evaluated claims of replacement cost and lost profits damages related to alleged interference in the market for femtocell wireless communication products.
- Evaluated claims of damages in the form of lost profits and disgorgement of compensation and benefit from alleged unauthorized use of confidential materials in the market for government contracts for research into laser vibrometry.
- Estimated damages from employee theft of HDD computer memory products from s research/testing facility. Calculated value based on historical in-channel market price and on historical costs of manufacturing and sales.
- Evaluated claims of lost profits damages arising from alleged professional malpractice related to commercial development and land use.

- Provided statistical and data analysis of invoices for disaster recovery and construction services. Estimated lost profits related to alleged fraud, breach of contract, and tortious interference.
- Estimated damages related to alleged breaches of contract, including:
  - Contract involving the development and sale of solar power generation projects;
  - Contract involving the supply of active ingredients in nutraceuticals;
  - Non-solicitation agreement between government defense contracting companies;
  - Contract for concession services at amusement parks;
  - Contract for creation and promotion of credit reporting services;
  - Contract for supply of MLB jerseys used in creation of sports memorabilia;
  - Contract for blending and supply contracts for specialized non-dairy beverages;
  - Non-compete clauses (restaurant lease, franchising, structural steel fabrication);
  - Contract for earning and redeeming of frequent flyer miles;
  - Contract for purchase of television airtime on a local over-the-air station;
  - Contract for representation and sale of television programming;
  - Royalty contract regarding design and functionality elements use in toys;
  - Contract for technology and support from software conference bridge systems;
  - Contract for conference calling services and long-distance calls connection services.
- Estimated damages from defamation related to the launch of a clinic for medical disorders.
- Evaluated claims by the CA Coastal Commission related to lost recreational value from proposed coastal bluff seawall construction.
- Evaluated concepts and methods for calculating proceeds from a Qi Tam suit related to improper medical lab billing practices.
- Estimated damages related to Quantum Meruit claims involving use of software to manage viewing and storage of electronic medical images.

Employment and Labor

- Estimated damages related to lost profits; lost company value, employee training and hiring expense, and/or disgorgement of defendant's profits in multiple cases related to the alleged breach of non-solicitation agreements and unauthorized use of confidential information by departing employees the insurance and MLM health and wellness industries.
- Estimated lost profits damages and/or disgorgement of defendant's profits in multiple cases related to the alleged breach of non-solicitation agreements and unauthorized use of confidential information involving government defense contracting companies.
- Estimated plaintiff's lost profits and disgorgement of defendant's profits related to the theft of trade secrets by departing employees in the automated emergency/disaster response industry.
- Estimated disgorgement of defendant's profits related to the theft of trade secrets by departing employees in the naval engineering industry.
- Provided statistical analysis of employee timecard and pay data to estimate instances of underpayment or missed breaks.

- Estimated lost earnings and compensation damages related to an alleged wrongful termination of an employee; evaluated lost wages/earnings, lost retirement benefits, and lost compensation through stock options.
- Estimated damages to an employee/inventor related to exclusion as an inventor from PCT applications following termination from a start-up medical devices company; evaluated the plaintiff's claims of lost share of proceeds from technology share.

Statistical and Econometric Analysis

- Performed regression analysis to evaluate class-wide damages related to class certification in the context of various alleged conspiracies, in several industries and markets including:
  - Ready-mix concrete; Korean Ramen; Interior molded doors; Packaged Seafood; Trans-Pacific airline travel; Broiler chickens; Domestic airlines travel; Pork; Turkey;
- Performed regression analysis to evaluate class-wide cost pass-through in the context of multiple alleged antitrust conspiracies (including building materials, airline travel, food products, apparel, and DRAM).
- Evaluated regression and statistical analysis offered in support of damages related to an alleged breach of non-solicitation agreements and unauthorized use of confidential information by departing employees the insurance and MLM health and wellness industries.
- Evaluated regression and statistical analysis offered in support of damages and common impact in an indirect purchaser class action related to alleged false advertising for nutritional supplement beverages.
- Provided statistical analysis of employee timecard and pay data to estimate instances of underpayment or missed breaks.
- Provided sampling techniques and statistical analysis of customer service database to estimate the extent of use of an allegedly infringing feature in a commercial router.
- Evaluated sampling techniques and extrapolation estimates related to allegedly improper medical billing practices and in the context of damages related to construction defects.
- Provided statistical and econometric analysis of survivorship related to consumer membership attrition in credit reporting programs.
- Provided statistical and econometric analysis of the correlation between purchase of infringing products and consequential purchase of related services.
- Provided statistical analysis and estimate of medical product sales in the absence of data from third party sales force.
- Provided statistical and econometric analysis of conference calling minutes related to alleged intentional interference and unfair competition.
- Conducted statistical analysis of incremental costs in support of lost profits calculations.

EXPERT WITNESS EXPERIENCE (SINCE 2020)

- *In Re Accellion Inc. Data Breach Litigation,* United States District Court, Norther District of California (2025). Provided deposition testimony on behalf of a plaintiff class related to damages involving a data breach and the improper capture of personalized information.

- *Touchstream Technologies, Inc., v. Comcast Cable Communications, LLC, D/B/A Xfinity, et al.,* United States District Court, Eastern District of Texas, Marshall Division (2025). Testified at trial and deposition on behalf of plaintiff related to damages for alleged patent infringement involving video streaming technology.

- *Touchstream Technologies, Inc., v. Charter Communications, Inc., et al.,* United States District Court, Eastern District of Texas, Marshall Division (2025). Testified at trial and deposition on behalf of plaintiff related to damages for alleged patent infringement involving video streaming technology.

- *In Re Cattle and Beef Antitrust Litigation,* United States District Court, District of Minnesota (2025). Provided deposition testimony on behalf of an indirect plaintiff class related to the economic effects and class commonality of an alleged price fixing conspiracy in the beef industry.

- *Bernadine Griffith, et al. v. TikTok, Inc. and Bytedance, Inc.,* United States District Court, Central District of California (2024). Provided deposition testimony on behalf of plaintiff class involving damages and common impact related to the collection and use of personalized information connected to web browsing activity.

- *Route Four LLC, v. Brandon Burkhart, et al.,* Superior Court of California, Orange County (2024). Provided deposition testimony on behalf of defendants related to causation of damages and damages for alleged breach of contract involving the sale of a hydroponic retail business.

- *Susan Iliff, et al. v. Dominium Management Services, LLC, et al.,* District Court of Minnesota, Hennepin County (2024). Submitted an expert report on behalf of defendant addressing allegations of low-income housing tax credit (LIHTC) manipulation.

- *In Re Turkey Antitrust Litigation,* United States District Court, Northern District of Illinois, Eastern Division (2024). Testified at a bench trial (evidentiary hearing), and in depositions, and issued multiple expert reports on behalf of plaintiff class related to the economic effects of an alleged conspiracy to increase prices and restrain supply in the turkey industry.

- *MGA Entertainment Inc. v. Clifford "T.I." Harris, et al.,* United States District Court, Central District of California, Western Division (2024). Testified at trial and deposition, and issued multiple expert reports on behalf of counter-defendant related to misappropriation of intellectual property (name, likeness, trade dress) related to the promotion and sale of toys.

- *American Compliance Technologies Inc., v. Advanced Chemical Transport Inc.,* United States District Court, Middle District of Florida, Tampa Division (2024). Submitted an expert report on behalf of counterclaimants related to the alleged trademark infringement involving environmental clean-up, transport, and disposal services.

- *In Re Pork Antitrust Litigation,* United States District Court, Northern District of Minnesota (2024). Provided testimony in depositions (3) and issued multiple reports on behalf of plaintiff class related to the economic effects of an alleged conspiracy to constrain capacity and increase prices in the pork industry.

- *Lisa Bodenburg v. Apple Inc.,* United States District Court, Northern District of California (2024). Submitted an expert report on behalf of plaintiff class related to damages involving alleged breach of contract for cloud storage services.

- *Tyler Baker et al. v. ParkMobile LLC,* United States District Court for the Northern District of Georgia, Atlanta Division (2024). Submitted an expert report on behalf of plaintiff class related to damages involving a data breach and the improper capture and transmission of personalized information held and maintained in commercial database(s).

- *In Re Broiler Chicken Antitrust Litigation,* United States District Court, Northern District of Illinois (2024). Testified at a bench trial (evidentiary hearing), and in depositions (4), and issued multiple expert reports on behalf of plaintiff class related to the economic effects of an alleged conspiracy to constrain capacity and increase prices in the broiler chicken industry.

- *In Re: Packaged Seafood Products Litigation*, United States District Court, Southern District of California (2024). Provided testimony in depositions (3), testified at trial (evidentiary hearing), and issued multiple expert reports on behalf of direct purchaser plaintiff class related to class certification and estimation of class wide damages in an antitrust case involving alleged collusion on the prices for packaged seafood.

- *MSP Recovery Claims Series v. Express Scripts Inc., et al.,* United States District Court, Northern District of Illinois, Western Division, (2023). Provided deposition testimony on behalf of plaintiff classes (direct and indirect) and issued multiple expert reports related to the economic effects of an alleged anticompetitive practices involving the pharmaceutical drug Acthar.

- *Q Industries Inc. v. O'Reilly Automotive Inc., et al.,* United States District Court, Northern District of California (2023). Provided deposition testimony and issued an expert report on behalf of O'Reilly defendant related to alleged trademark infringement involving retail supply of automotive equipment.

- *Q Industries Inc. v. Test-Rite Products Corp., et al.,* United States District Court, Northern District of California (2023). Provided deposition testimony, and issued an expert report on behalf of defendant Test-Rite Corp. related to alleged trademark infringement involving wholesale supply of automotive equipment.

- *Microvention Inc. v. Balt USA, LLC, et al.,* United States District Court, Central District of California (2023). Provided deposition testimony, and issued an expert report on behalf of Defendant involving damages related to alleged misappropriation of trade secrets involving neuroendovascular medical devices.

- *In Re Blackbaud, Inc. Customer Data Security Breach Litigation*, United States District Court for the District of South Carolina, Columbia Division (2023). Provided deposition testimony, and issued an expert report on behalf of plaintiff class related to damages involving a data breach and the improper capture and transmission of personalized information held and maintained in commercial database(s).

- *Dr. Joseph Ciccio, et al., v. SmileDirect Club Inc., et al.,* United States District Court, Middle District of Tennessee, Nashville Division, (2022). Provided deposition testimony, and issued an expert report on behalf of plaintiff class involving damages related to allegations of false statements regarding orthodontic and dental treatment.

- *Lauren Adele Oliver v. Meow Wolf Inc., et al.,* United States District Court for the District of New Mexico (2022). Provided deposition testimony, and issued an expert report on behalf of plaintiff involving damages related to alleged copyright infringement involving art and art exhibits.

- *Sprint Communications Company LP v. Cequel Communications, LLC, et al.*, United States District Court, District of Delaware (2022). Testified at a bench trial (evidentiary hearing), and in deposition, and issued multiple expert reports on behalf of plaintiff related to lost profits and royalty damages from alleged patent infringement involving VoIP telephony network services.

- *VRtoysone, LLC, et al v. Disney Interactive Studios, Inc.,* United States District Court, Central District of California, Western Division (2022). Submitted an expert report on behalf of Defendant involving damages related to alleged patent infringement involving video games.

- *Patrick Calhoun, et al. v. Google LLC,* United States District Court, Northern District of California (2021). Provided testimony in depositions (2) , and issued multiple expert reports on behalf of plaintiff class involving damages and common impact related to alleged breach of contract involving the collection and retention of personalized information connected to web browsing activity.

- *CPI Security Systems Inc. v. Vivint Smart Home Inc. et al.,* United States District Court, Western District of North Carolina, Charlotte Division (2021). Provided deposition testimony, and issued an expert report on behalf of plaintiff involving unjust enrichment and royalty damages related to alleged false advertising and unfair competition related to security monitoring systems and services.

- *Martifer-Silverado Fund I, LLC and Silverado Power LLC v. Talesun Solar USA, Ltd.,* Superior Court of California, San Francisco County (2021). Provided trial and deposition testimony on behalf of Defendant, related to alleged breach of contract involving solar energy projects.

- *Javo Beverage Co., Inc., v. Stephen Corey,* American Arbitration Association (2021). Provided trial (arbitration) and deposition testimony on behalf of respondent related to breach of contract and misappropriation of confidential information and technology in the market for coffee extracts.

- *Andrea Williams and James Stewart (class reps) v. Apple Inc.*, United States District Court, Northern District of California (2021). Submitted an expert report, and issued an expert report on behalf of plaintiffs related to damages involving alleged breach of contract regarding provision of electronic file storage.

RESEARCH PAPERS AND PUBLICATIONS

- "FRAND Commitments and Royalties for Standard Essential Patents", with S. Bosworth and E. Matolo, in Complications and Quandaries in the ICT Sector, Bharadwaj, Gupta, and Devaiah eds., Ch. 2, Springer Open, ISBN 978-981-10-449570, 2018.
- "Corrective Advertising in Lanham Act Damages: The Use and Misuse of Past Advertising Expenditures" with S. Bosworth and E. Matolo, *The Trademark Reporter*, May-June Volume, 2017.
- "The Case for Admitting Settlement License Agreements in a Reasonable Royalty Analysis," with S. Conroy and R. Knudsen, 2011, *Les Nouvelles,* Volume XLVI No. 4, 2012.
- "Cost Analysis," with J. Kinrich and A. Meister, in Intellectual Property Damages, Guidelines and Analysis, 2004 supplement, M. Glick, L. Reymann, and R. Hoffman, eds., Chapter 14a, Wiley: New York.
- "Analysis and Measurement of Damages in Patent Infringement Actions," with J. Kinrich, 2003, proceedings of Practicing Law Institute.

PAST OR PRESENT AWARDS, PROFESSIONAL MEMBERSHIPS

Outstanding Antitrust Litigation Achievement in Economics, American Antitrust Institute, 2023
Outstanding Antitrust Litigation Achievement in Economics, American Antitrust Institute, 2019
American Antitrust Institute, advisory board member
American Bar Association, member
American Economic Association, member
Licensing Executives Society, member, chapter chair
Los Angeles County Bar Association, member
Los Angeles Intellectual Property Law Association, member
Orange County Bar Association, member
Orange County Patent Law Association, member

Appendix B

Appendix B. Materials Relied Upon

Court Documents

- Consumer Indirect Purchaser Plaintiffs' Memorandum of Law in Support of Their Motion for Class Certificationt, *In re: Cattle and Beef Antitrust Litigation*, United States District Court for the District of Minnesota No. 0:22-MD-3031, Sep. 25, 2024.
- Defendants' Memorandum of Law in Support of Their Motion to Exclude Certain Portions of the Expert Report and Testimony of Dr. Russell W. Mangum, Jan. 24,

Expert Reports and Related Productions

- Expert Report of Russell L. Lamb, Ph.D., Sep. 25, 2024.
- Expert Report of Gareth Macartney, Ph.D., Sep. 25, 2024.
- Corrected Expert Report of Russell W. Mangum, Ph.D., Sept. 25, 2024
- Expert Report of David L. Sunding, Ph.D., Sep. 25, 2024
- Corrected Expert Report of Michael A. Williams, Ph.D., Sep. 25, 2024.
- Expert Report of Lauren J. Stiroh, Ph.D., Jan. 24, 2025
- Expert Report of Justin McCrary, Ph.D., Jan. 24, 2025

Bates Documents

- BUTTS_Beef0591152–154
- CARGILL000098347
- CARGILL000100232
- CARGILL000101586
- CARGILL000102926
- CARGILL000104825
- CARGILL000106023
- CARGILL000110279
- CARGILL000110924
- CARGILL000114213
- CARGILL000134276
- CARGILL000134732
- CARGILL000137500
- CARGILL000139017
- CARGILL000140038
- CARGILL000144797
- CARGILL000146732
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- CARGILL000151945
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- CARGILL000158597
- CARGILL000159316
- CARGILL000161177
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- JBS-0000071922
- JBS-0000091706
- JBS-0000091980
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- CARGILL001183367
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- CARGILL001200660
- CARGILL001238280
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- CARGILL001274706
- CARGILL001277455
- CARGILL001280702
- CARGILL001291549
- CARGILL001292273
- CARGILL001298212
- CARGILL001298602
- CARGILL001305834
- JBS-0000753474
- JBS-0000806473–478
- JBS-0000806473–478
- JBS-0000872481
- JBS-0000995245
- JBS-0000995266
- JBS-0001063244
- CARGILL001308969
- CARGILL001311068
- CARGILL001316895
- CARGILL001322497
- CARGILL001322516
- CARGILL001327760
- CARGILL001328350
- CARGILL001329197
- CARGILL001331670
- CARGILL001336884
- CARGILL001341910–911
- CARGILL001343947
- CARGILL001344007
- CARGILL001345806
- CARGILL001352266
- CARGILL001363709
- CARGILL001366712
- CARGILL001371956
- CARGILL001372432
- CARGILL001372593
- CARGILL001375701
- CARGILL001382279
- CARGILL001384507
- CARGILL001386937
- CARGILL001389932
- CARGILL001395586
- CARGILL001396010
- CARGILL001397335
- CARGILL001397437
- CARGILL001401701
- CARGILL001406645
- CARGILL001407379
- CARGILL001447414
- CARGILL001447418
- CARGILL001447434
- CARGILL001785615
- CARGILL001805227
- CARGILL001841651
- CARGILL001949678–682
- CBBC_0000755101–103
- HALES204856
- HALES270152
- HALES270331
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- TYSONBEEF02460541–592
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Depositions

- Deposition of Jerry Holbrook, Mar. 13, 2024
- Deposition of Al Byers, Aug. 5, 2024
- Deposition of Al Byers, Jun. 26, 2024
- Deposition of David Neitzel, Jul. 25, 2024
- Deposition of Donald Kieffer, Mar. 21, 2024
- Deposition of Dr. Lauren Stiroh, Mar. 19, 2025
- Deposition of Hal Sankey, Nov. 6, 2024
- Deposition of Karl Skold, May 9, 2024
- Deposition of Larry Rose, Jul. 23, 2024
- Deposition of Lawrence Klotz, Aug. 1, 2024
- Deposition of Nicholas Birkhofer, Dec. 12, 2024
- Deposition of Patrick Pottgieser, Feb. 22, 2024
- Deposition of Russell W. Mangum, Ph.D., Jan. 2, 2025
- Deposition of Shawn Spencer, Mar. 28, 2024
- Deposition of Steven Cohron, Jan. 7, 2025
- Deposition of Timothy Klein, Aug. 7, 2024
- Deposition of Todd Nogelmeier, Jun. 28, 2024
- Deposition of William McLaurin, Jun. 25, 2024

Third Party Data

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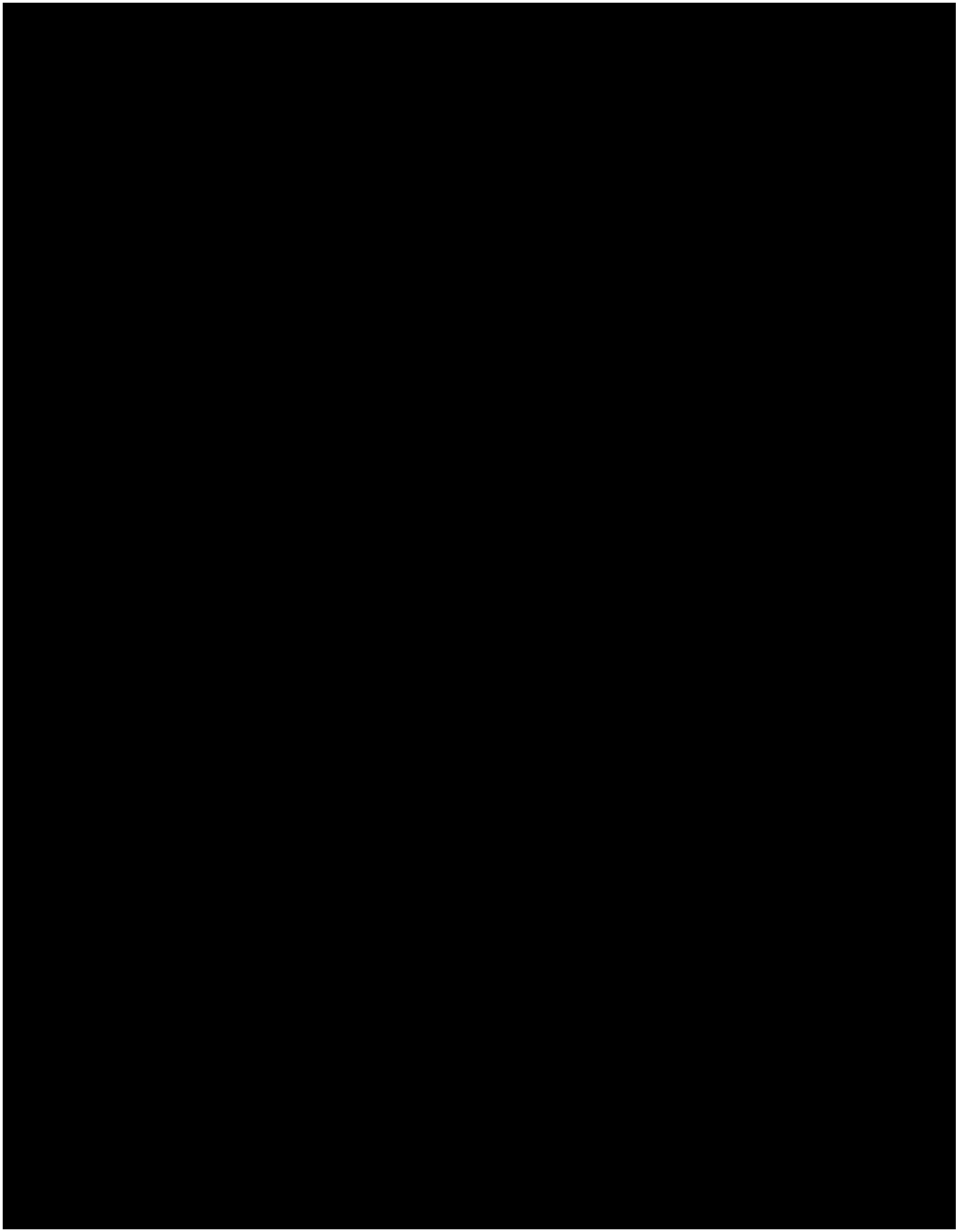

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And answers to data questions for the above data.

And all footnotes cited herein and in the Mangum Class Report.

Appendix C

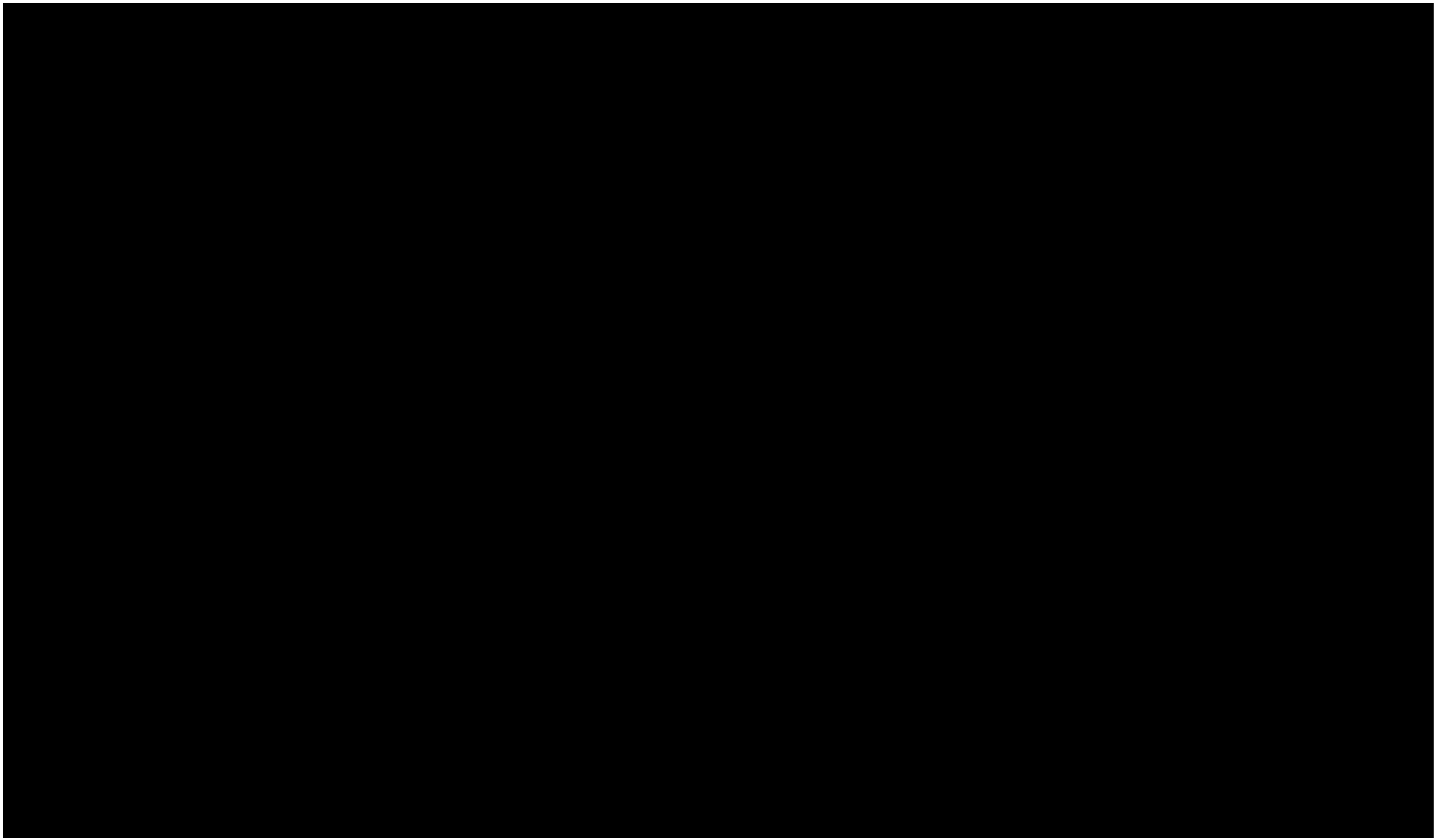
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 1, BF CHUCK ROLL NECK–OFF CHOICE

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

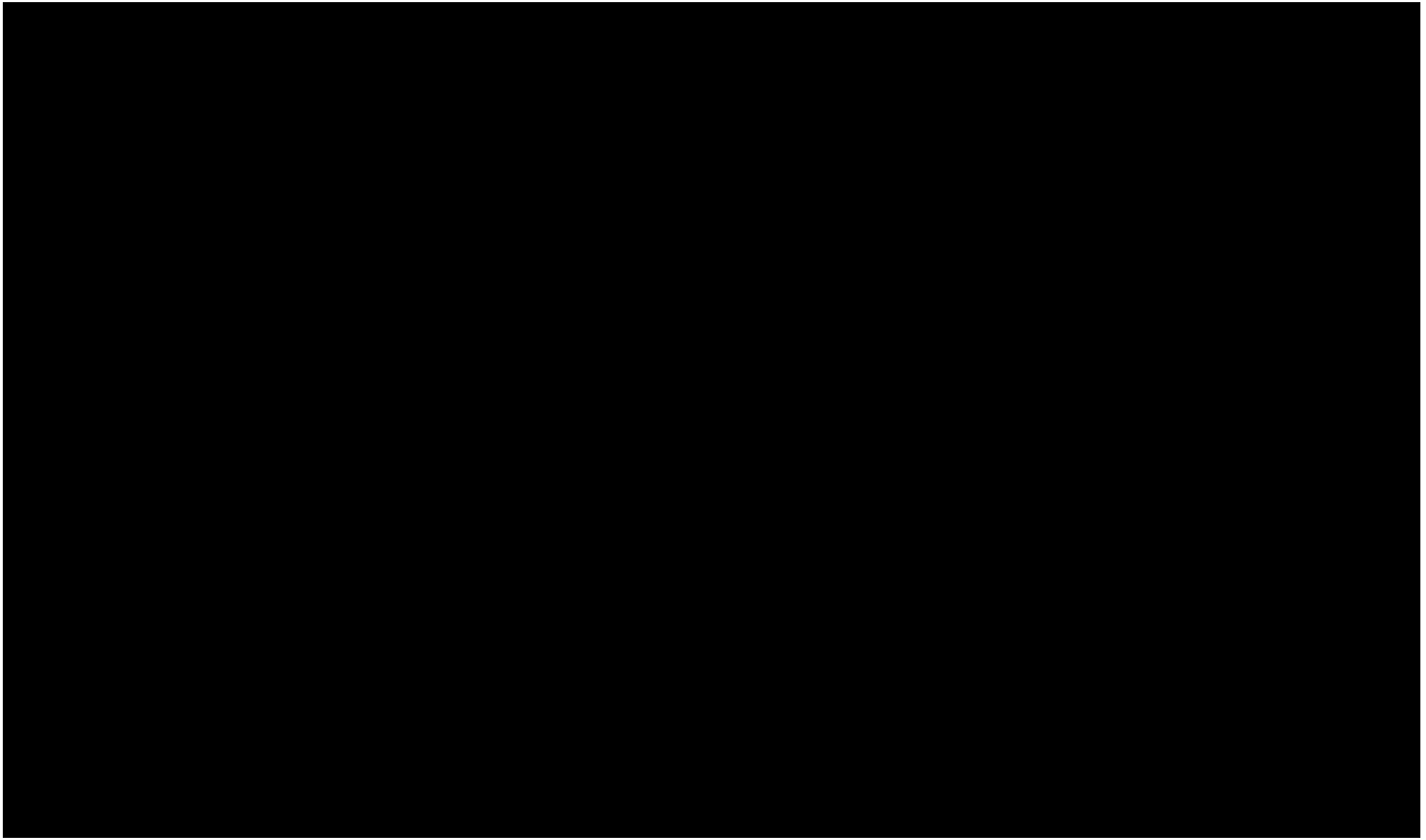
Product Rank 2. CK ROLL, TRMD N/O USDA SELECT

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 3, INSIDE ROUND XT



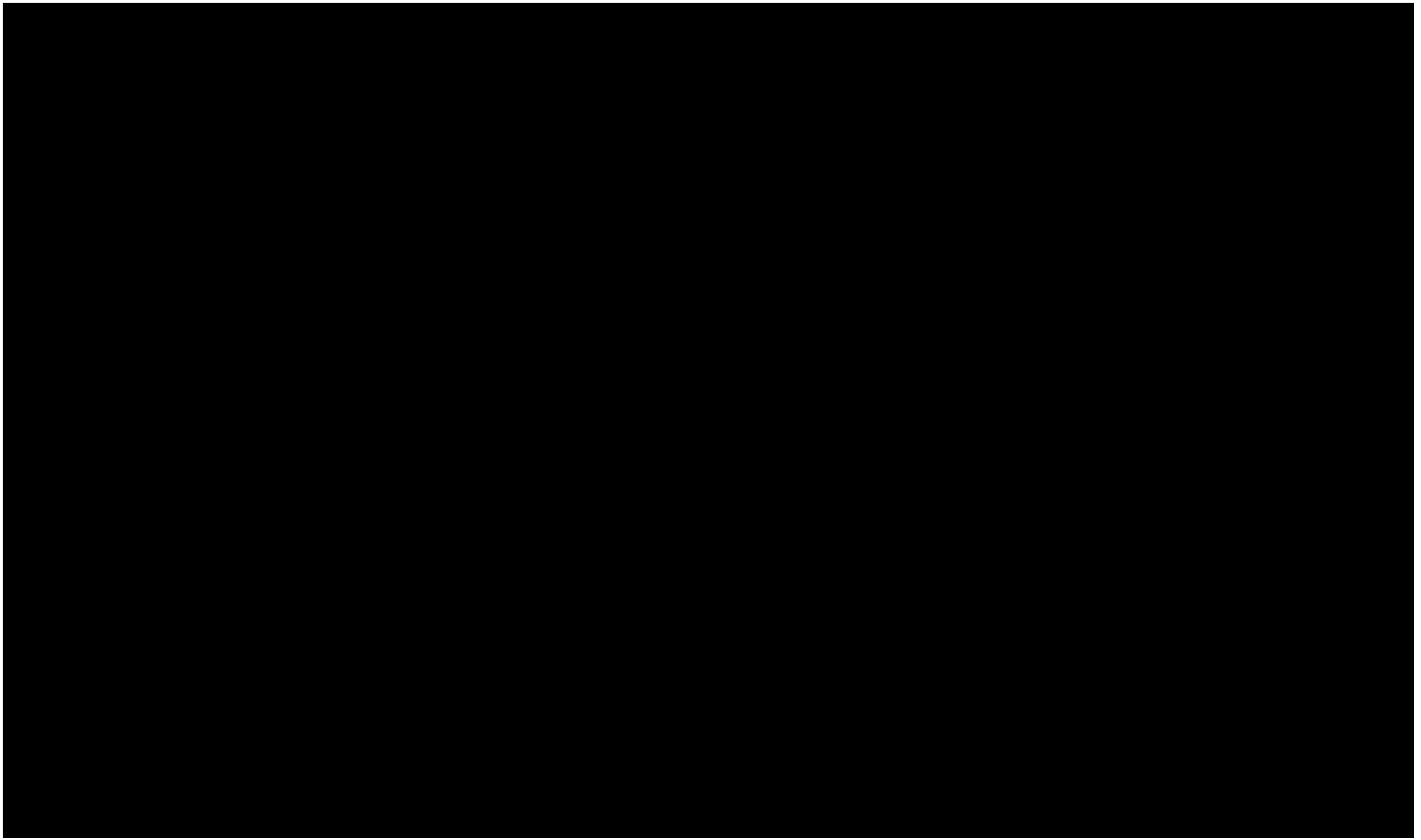
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 4, BEEF INSIDE ROUND

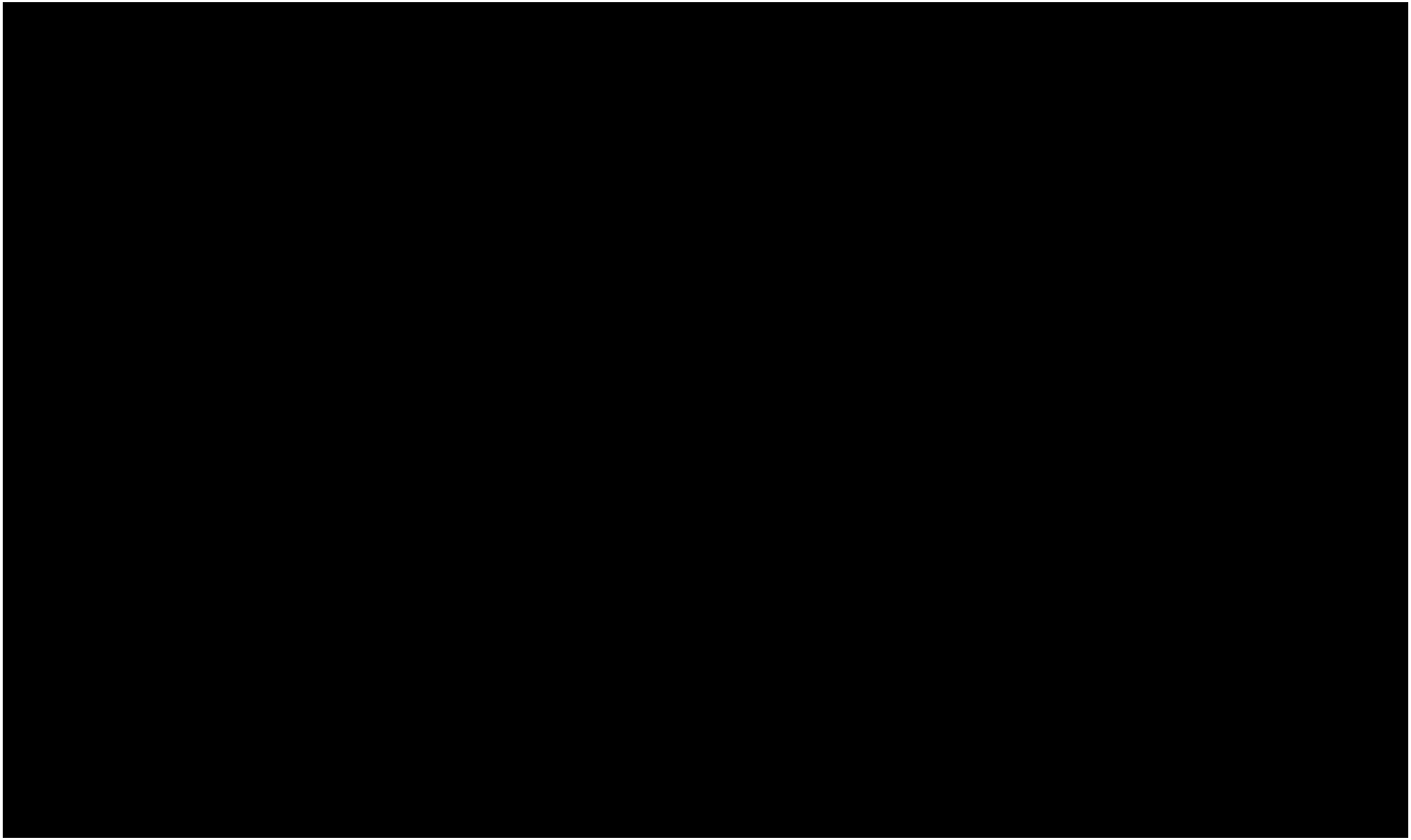


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

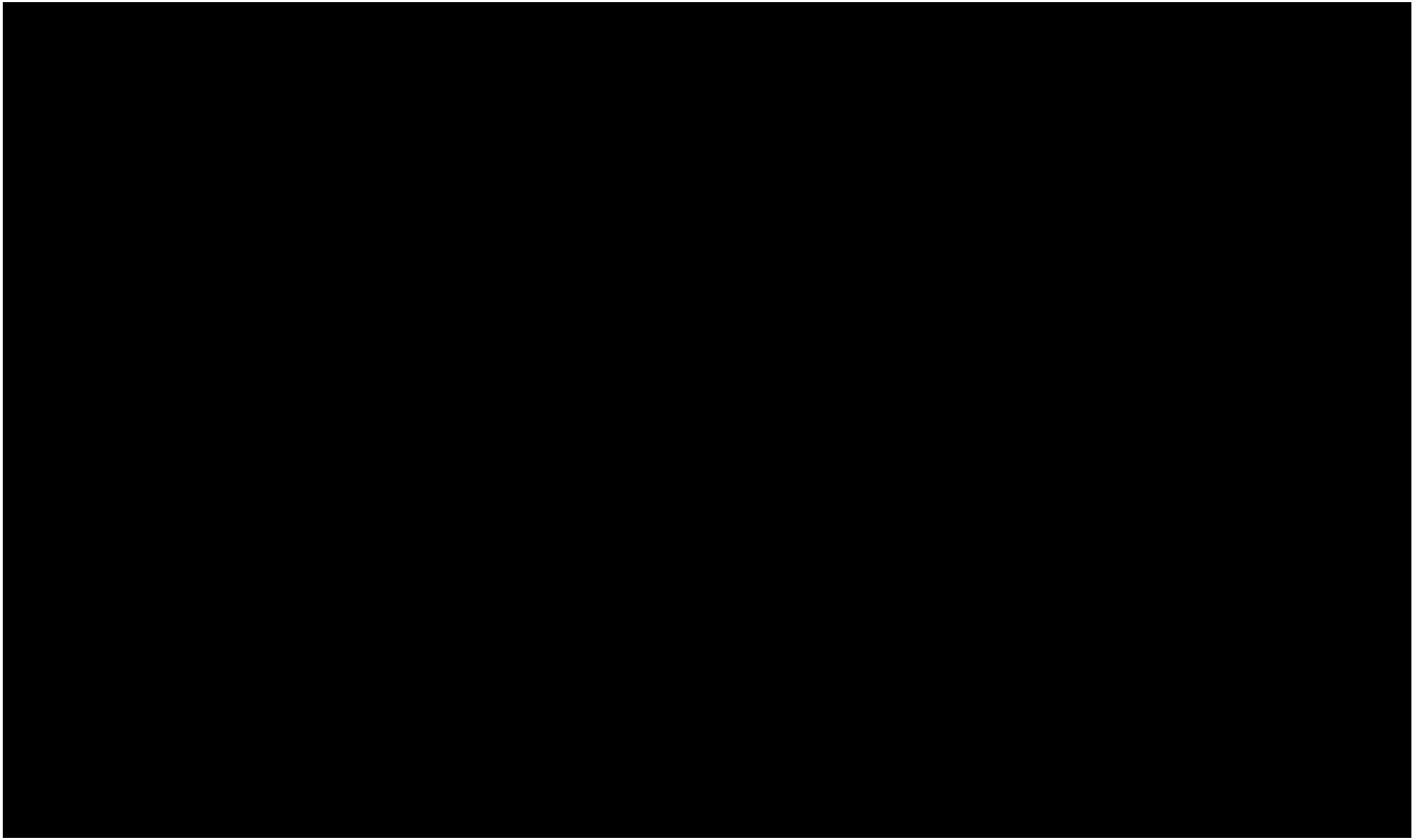
Product Rank 5, BEEF BOTTOM ROUND–FLAT



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 6, RIB EYE LIPON HVY CHOICE

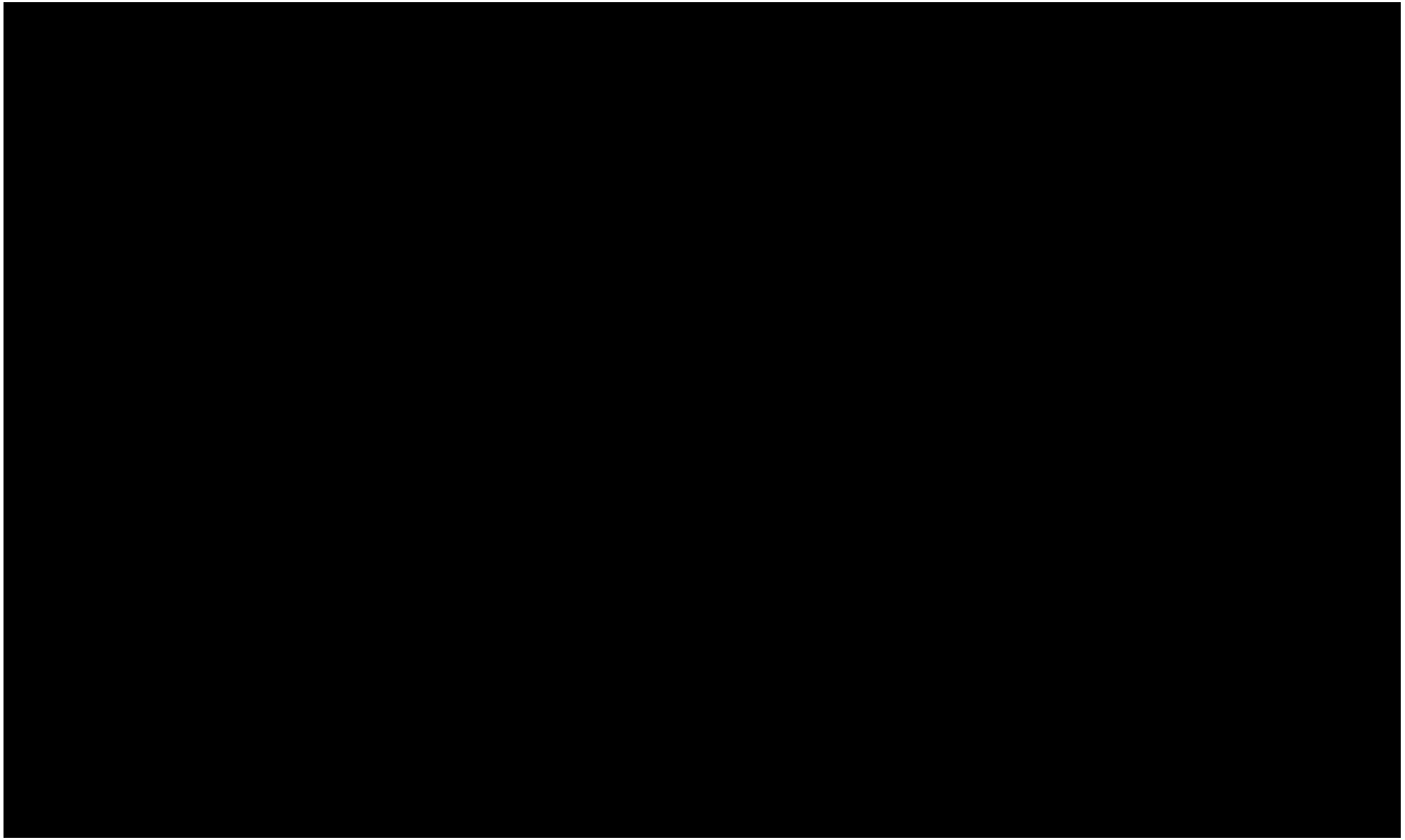


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 7, BEEF KNUCKLES PEELED CHOICE

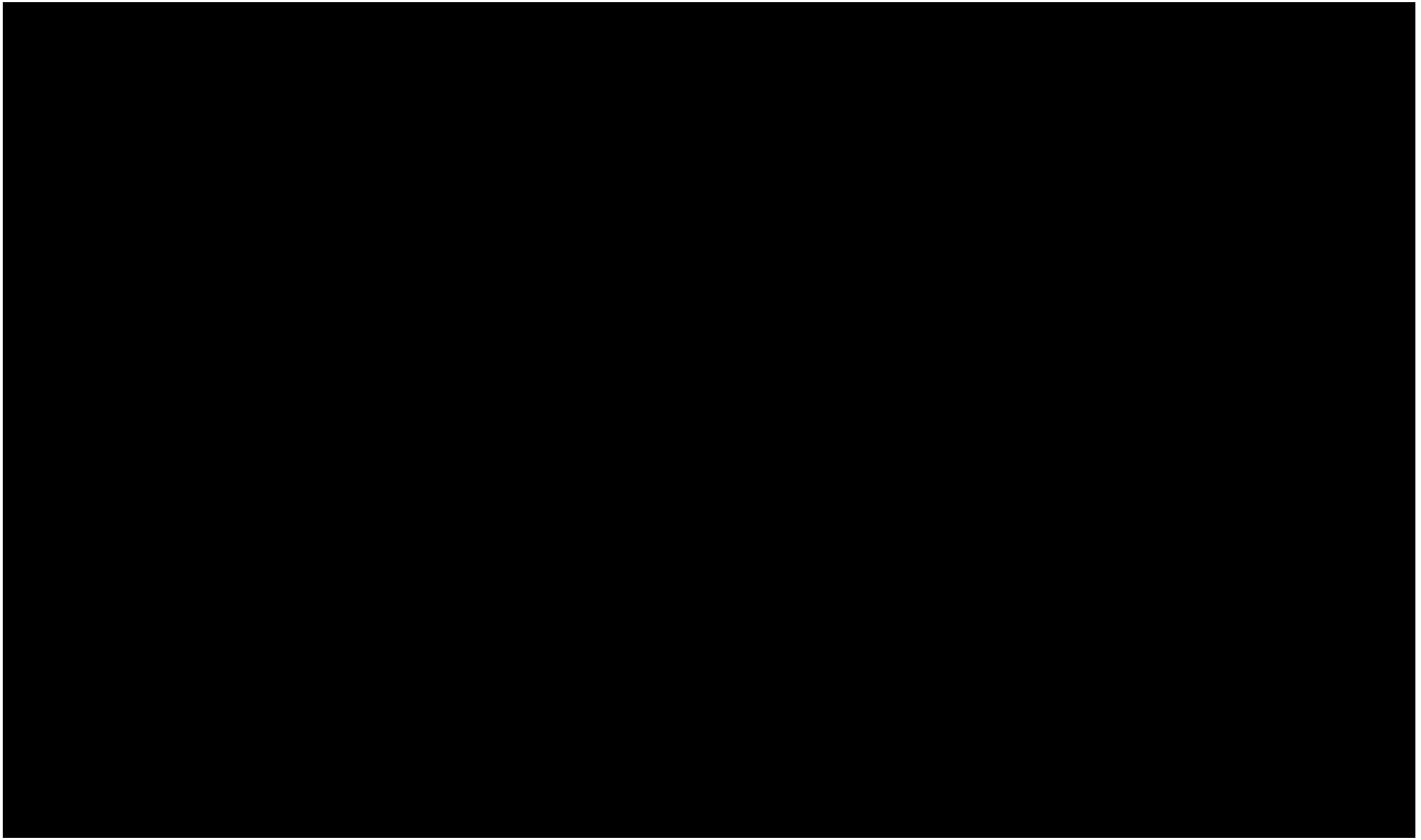


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 8, SHOULDER CLOD XT CHOICE

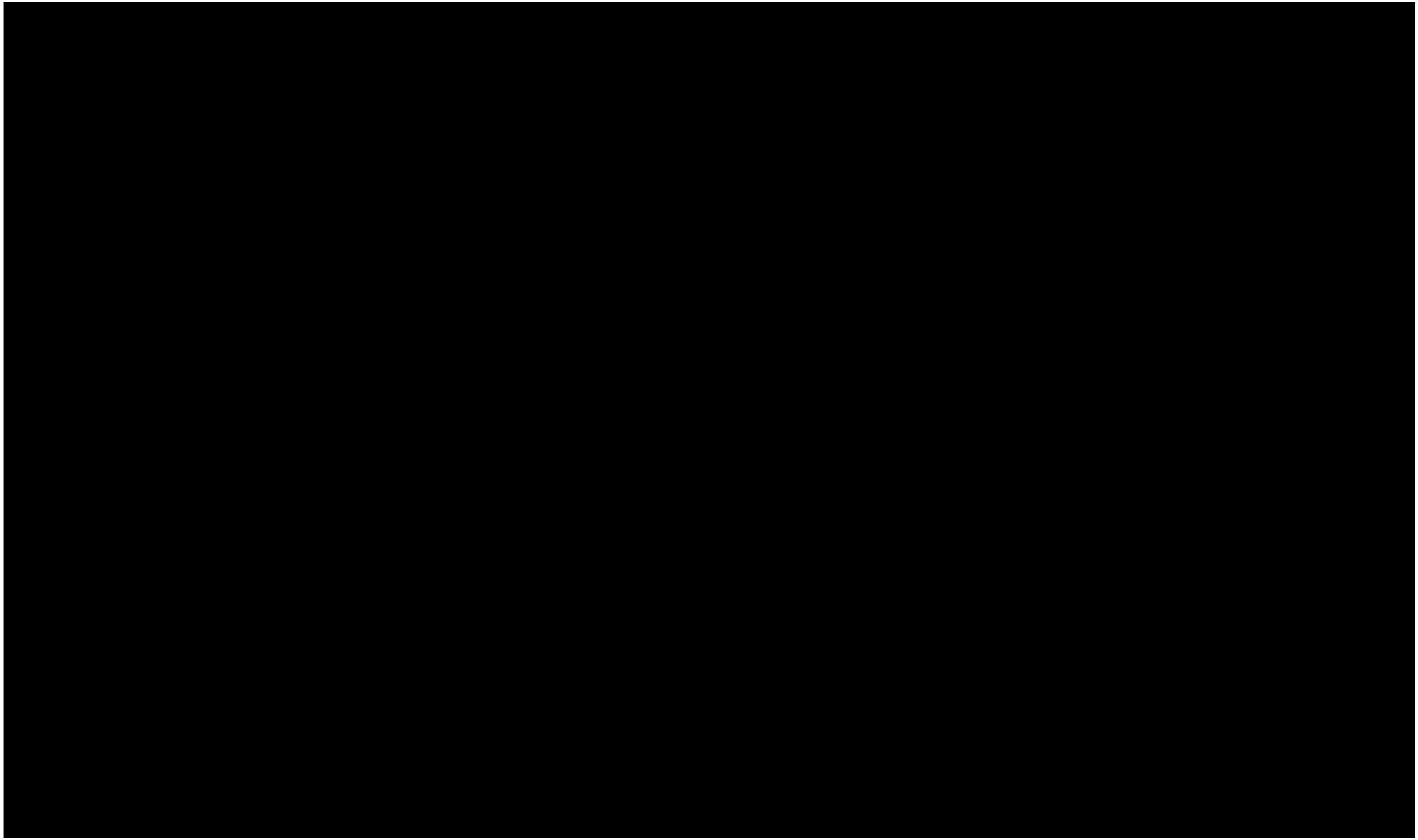


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 9, BNLS LOIN STRIP 0X1 CHOICE



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 10, CH/HI BNLS BEEF BOTTOM ROUND FLAT S/T

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 11, CH/HI BNLS BEEF TOP ROUND S/T



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 12, SE BNLS BEEF CHUCKEYE ROLL S/C

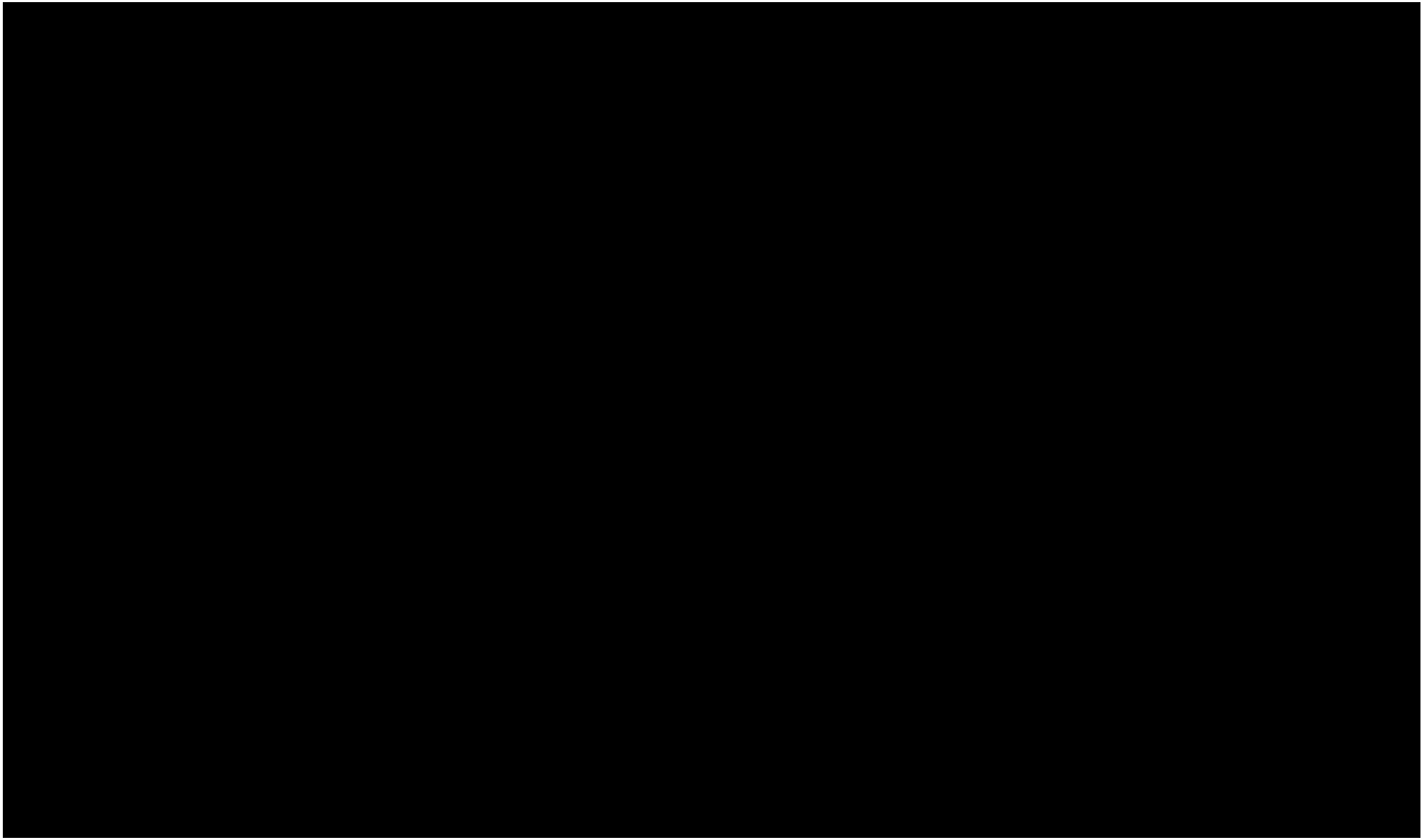
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 13, SHOULDER CLOD XT USDA SELECT

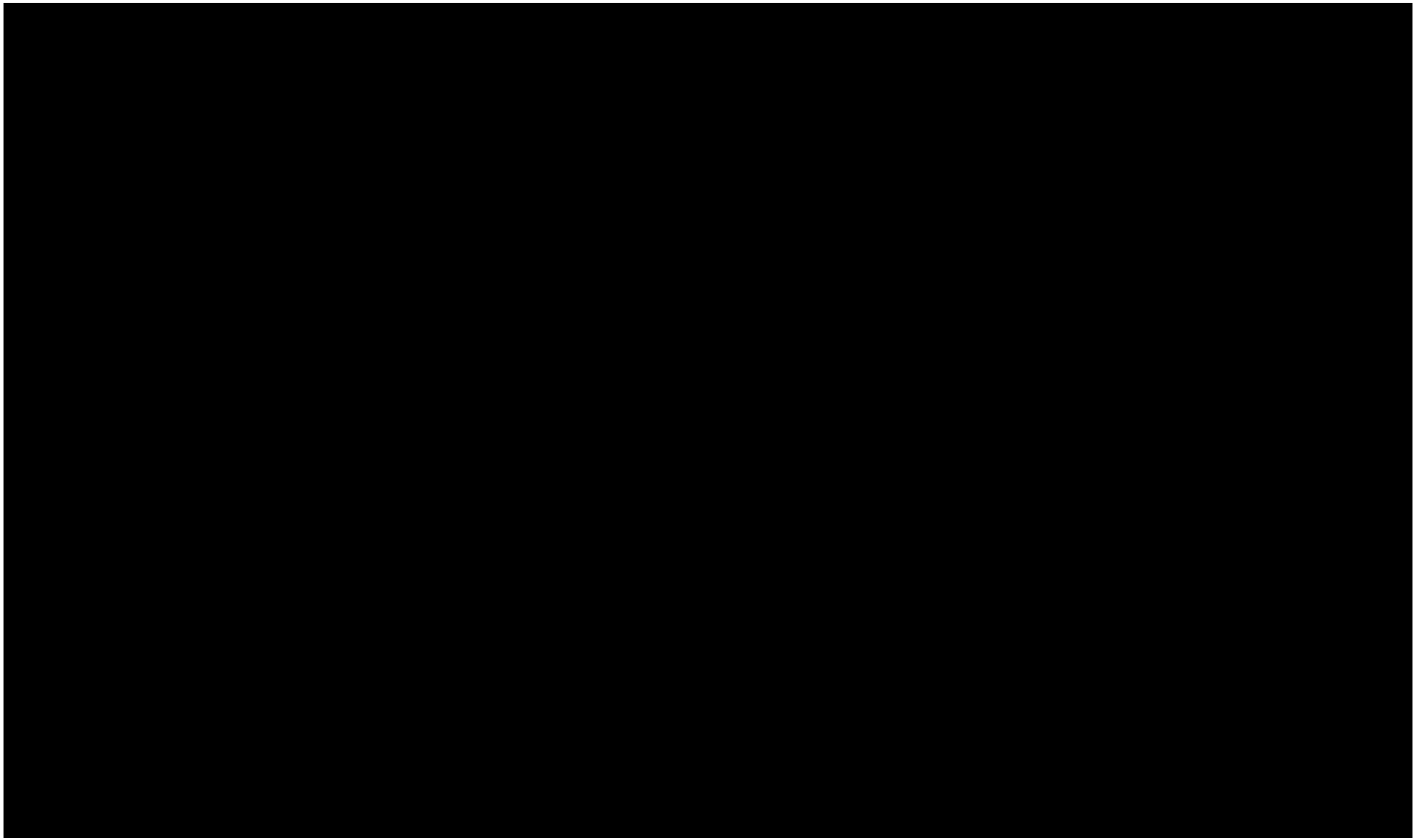
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 14, CH/HI BNLS BEEF CHUCKEYE ROLL S/C

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 15, CAB CHUCK ROLL NECK OFF

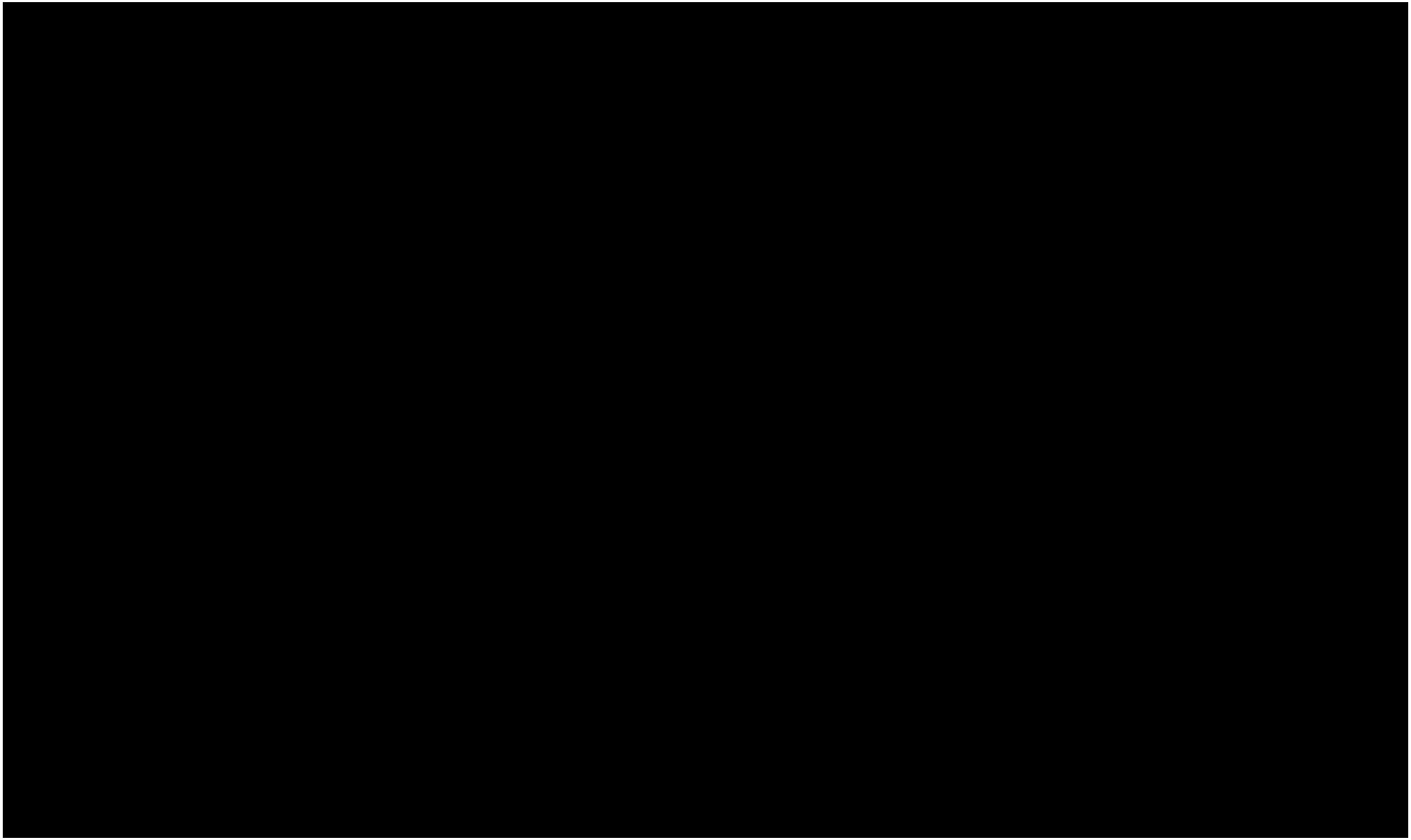
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 16, BTTM SRLN BUTT FLAP MEAT



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 17, BF RND–OUTSIDE FLAT PREFD



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 18, CHOICE BALL TIP 1.5/U



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 19, BEEF ROUND – EYE CHOICE

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 20, CH/HI B/L BEEF RIBEYE L/O 2X2

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 21, CH/HI BNLS BEEF STRIP 0X1 S/T

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 22. RIBEYE LIPON BI CHOICE

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 23, CH/HI BNLS BEEF KNUCKLE PEELED

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 24, BEEF TENDERLOIN PSMO CHOI

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 25, BF CHUCK–CHUCK ROLL–N/O PREF

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 26, BF CHUCK–CLOD 1PC PREF CH/HI

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

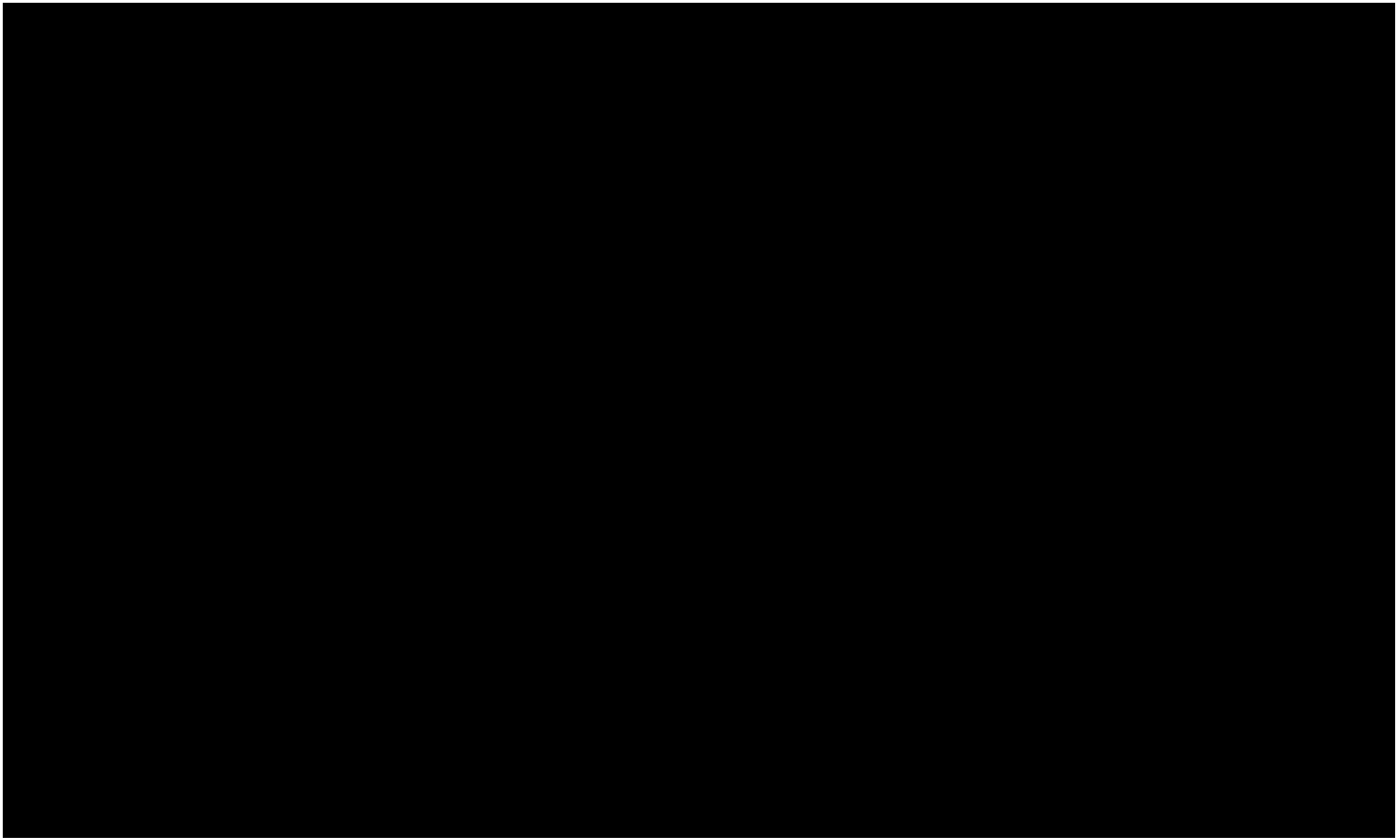
Product Rank 27, BEEF INSIDE ROUND SELECT

[redacted]

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 28, BEEF SHORT LOIN 0X1

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 29, CH/HI BNLS BEEF RIB LIFTER MEAT



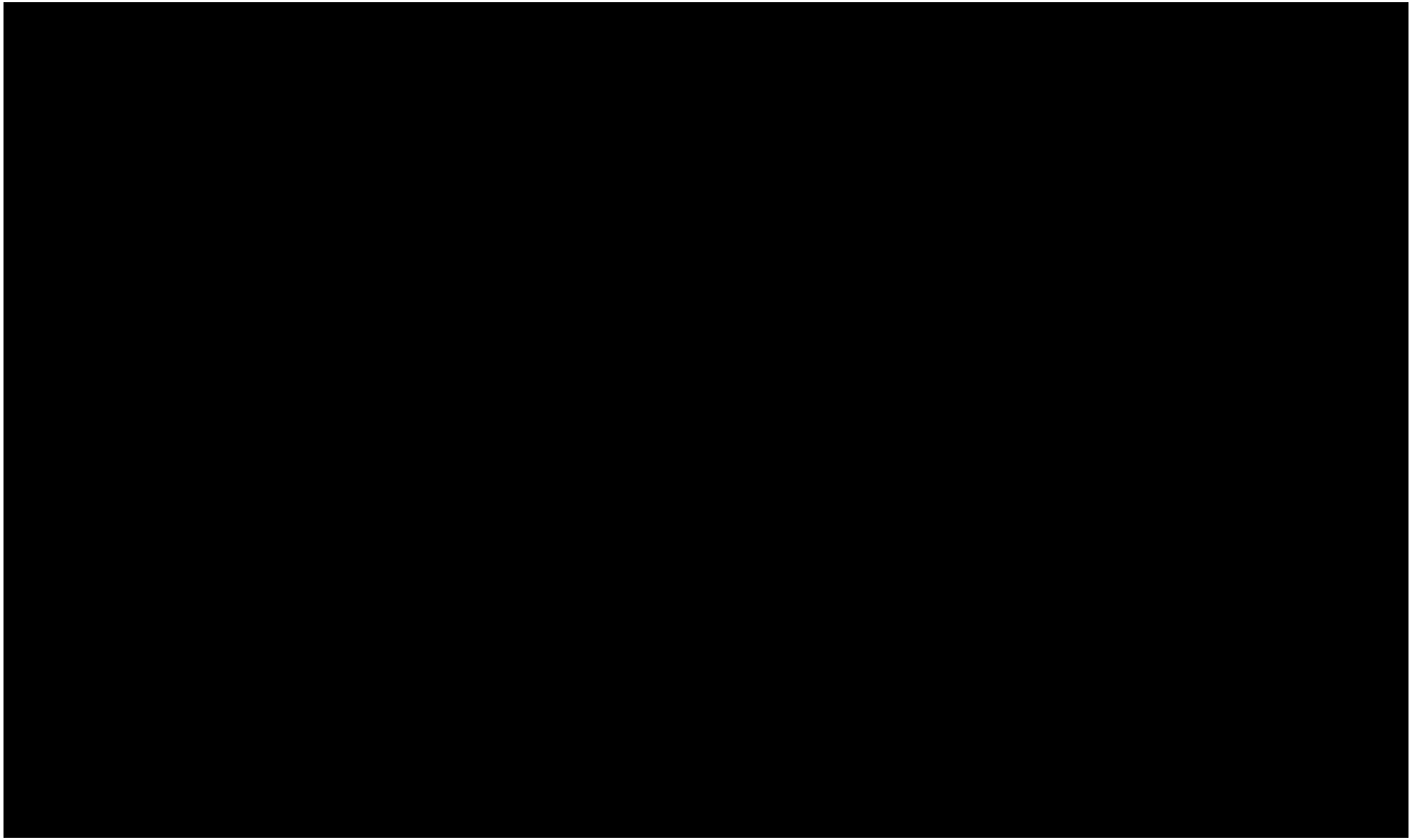
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 30, BF ROUND–KNUCKLE CAP OFF CH/HI

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products
Product Rank 31, 00016925 CH SmtCh FLAT of Round

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 32, 00016826 CH SmtCh INSIDE

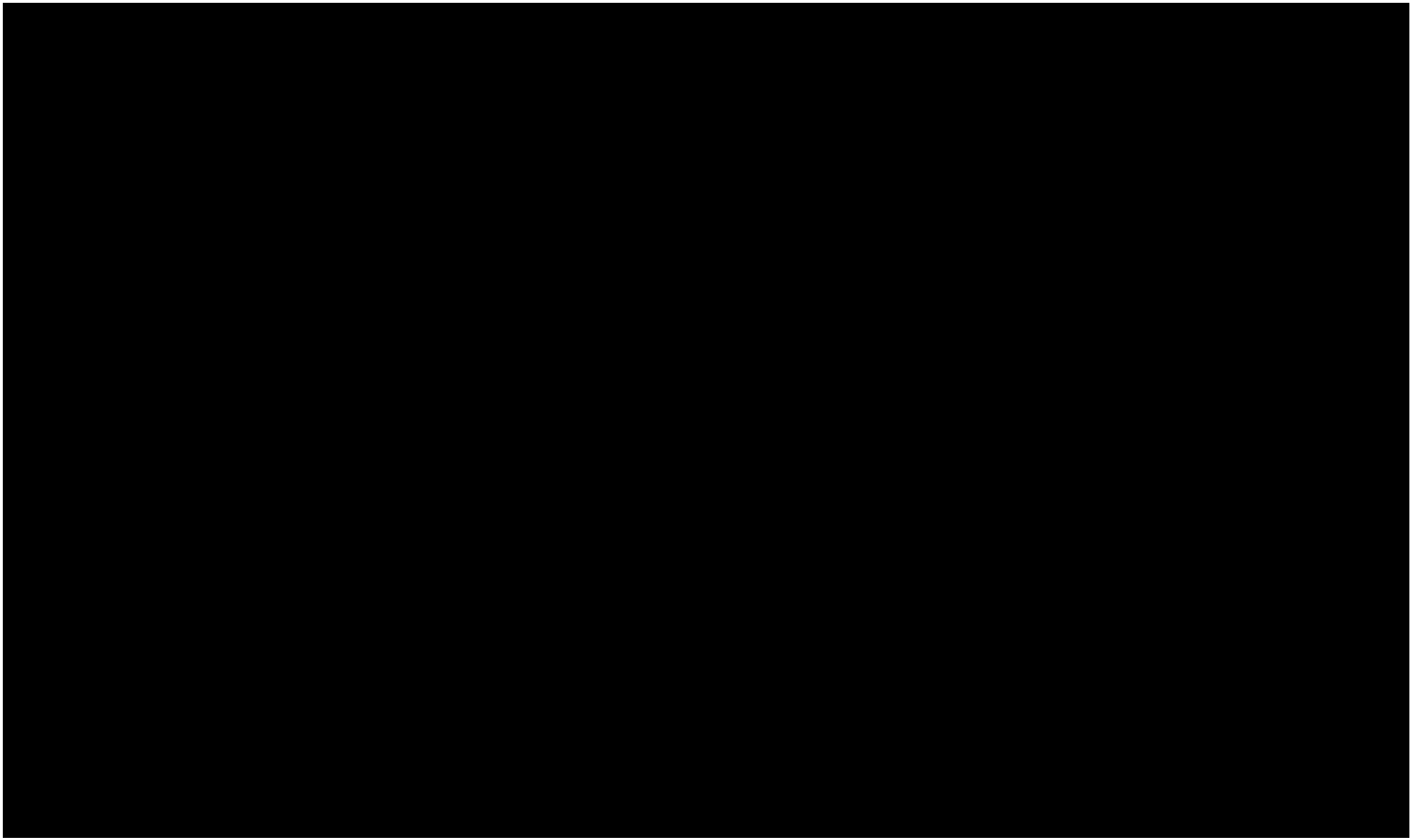
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products
Product Rank 33, BEEF CHUCK SEMI BNLS 2 PC



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

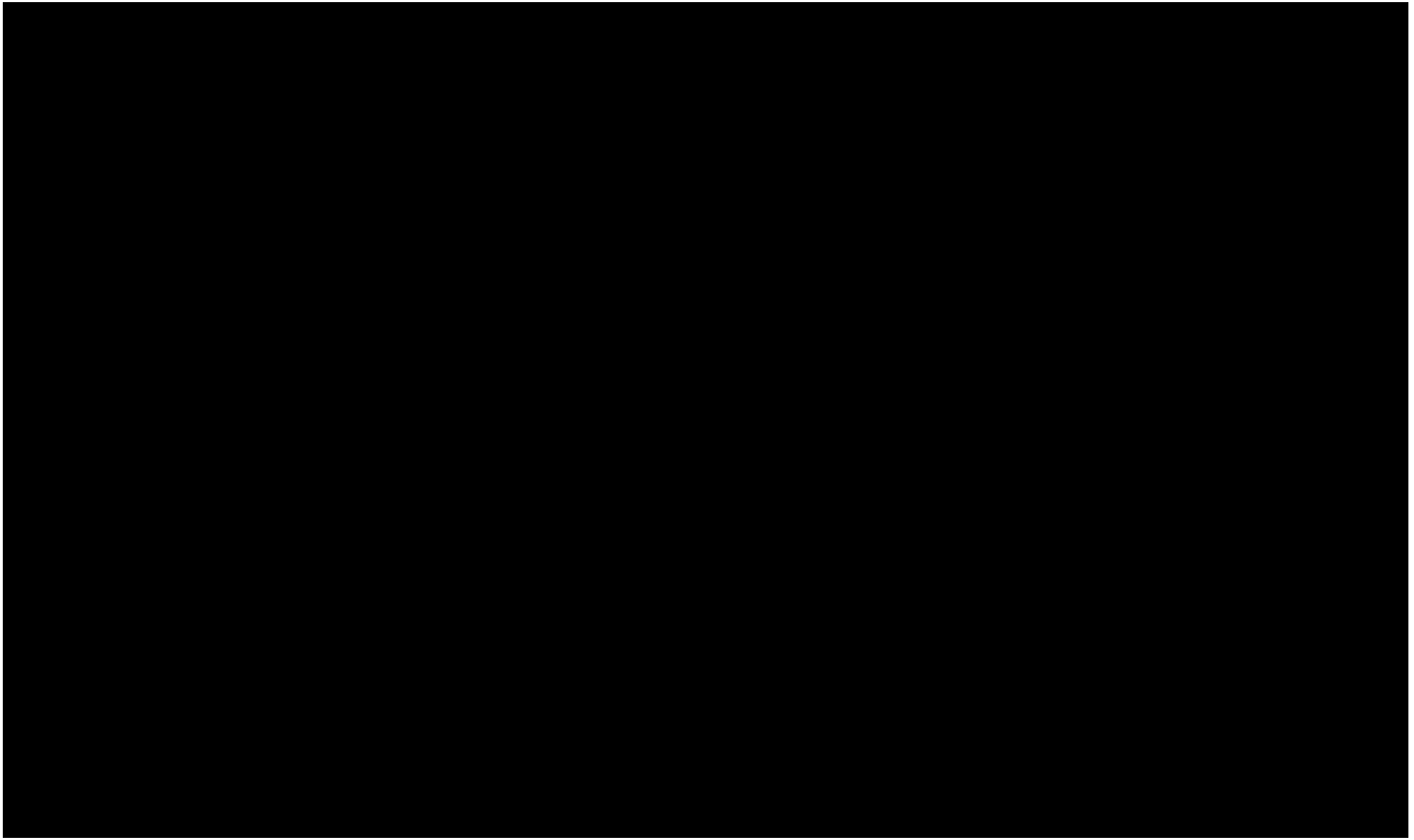
Product Rank 34, CAB BEEF ROUND – FLAT

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 35, CH BNLS BEEF TOP SIRLOIN BUTT S/T

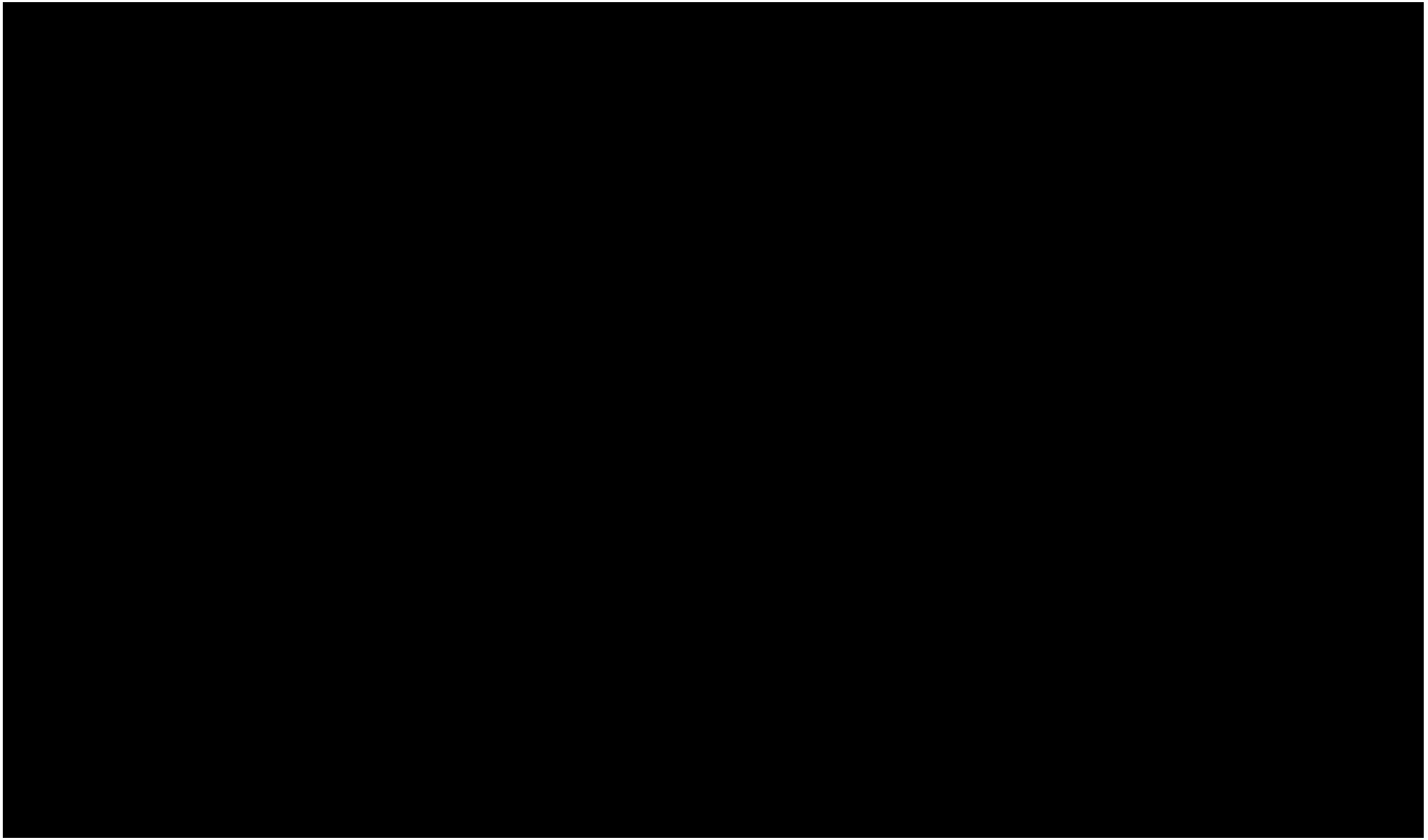


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 36, CAB INSIDE ROUND XT

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 37, CH/HI BNLS BEEF SHOULDER CLOD XT S/T

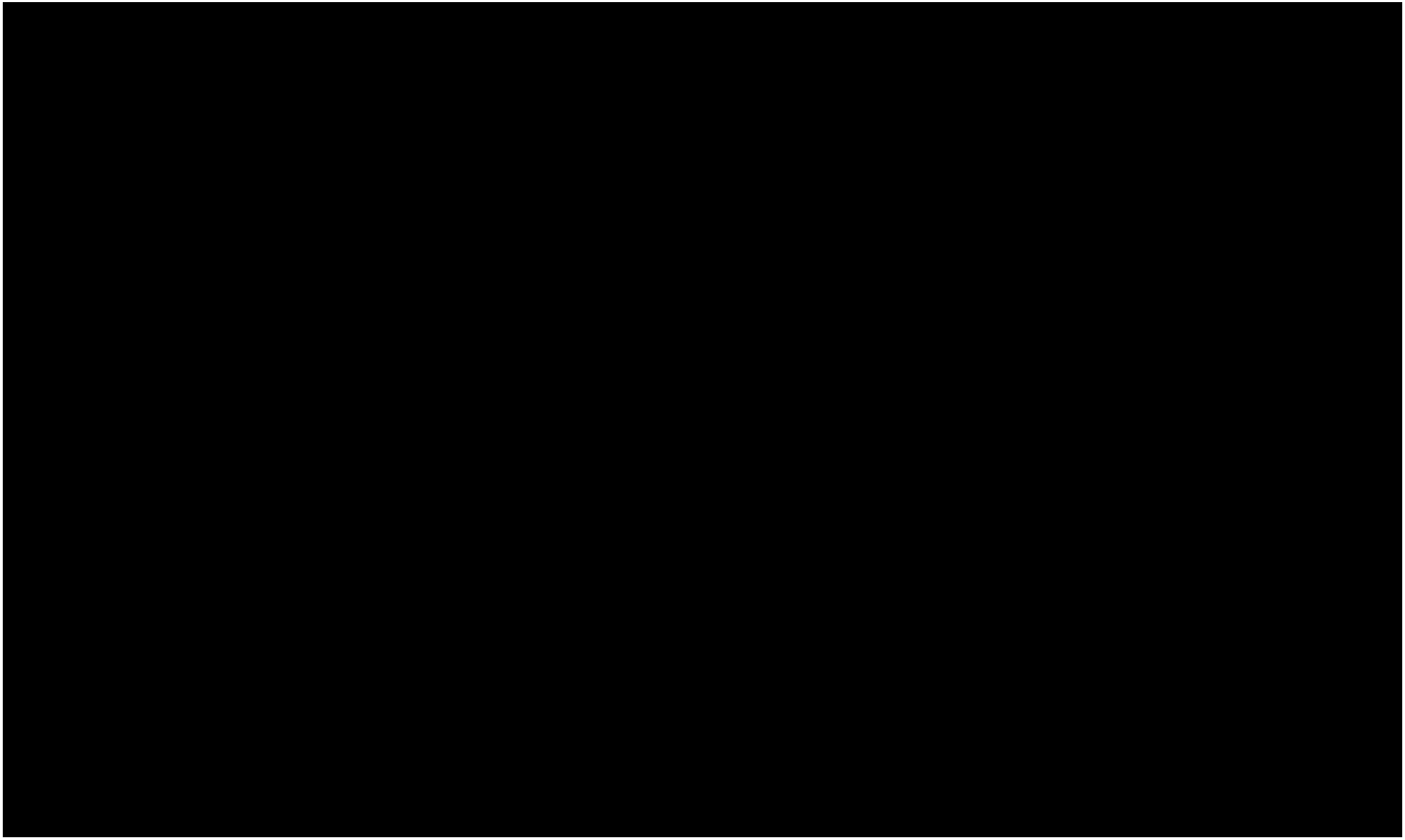


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 38, CH/HI BNLS BEEF CHUCK EYE ROLL B/R



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 39, CH/HI BNLS BEEF EYE OF ROUND S/T

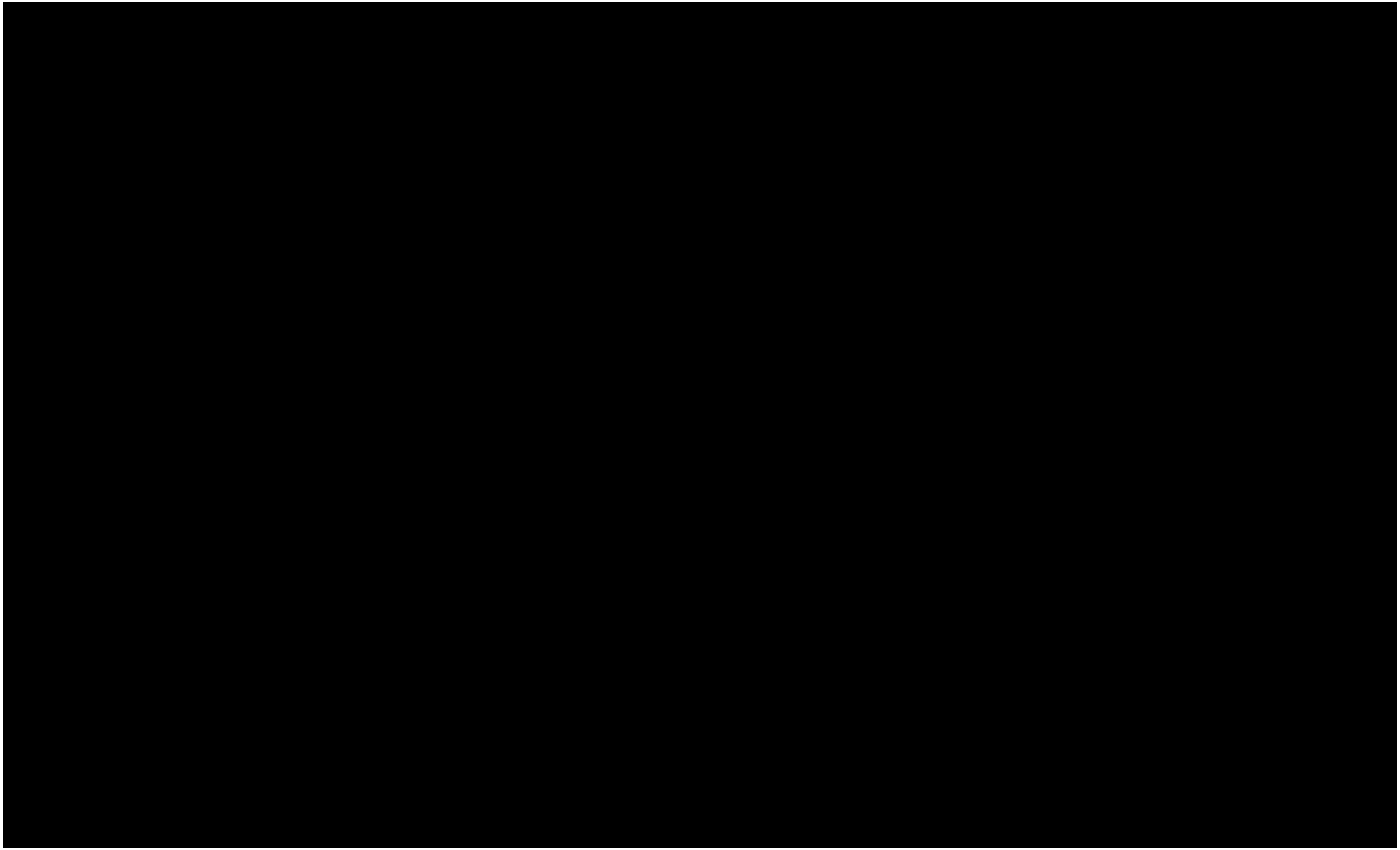
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 40, CAB SHOULDER CLOD XT



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 41, BEEF RIB BNLS LIPON RIBEYES DN

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 42, 00011722 CH SmtCh CHUCKEYE

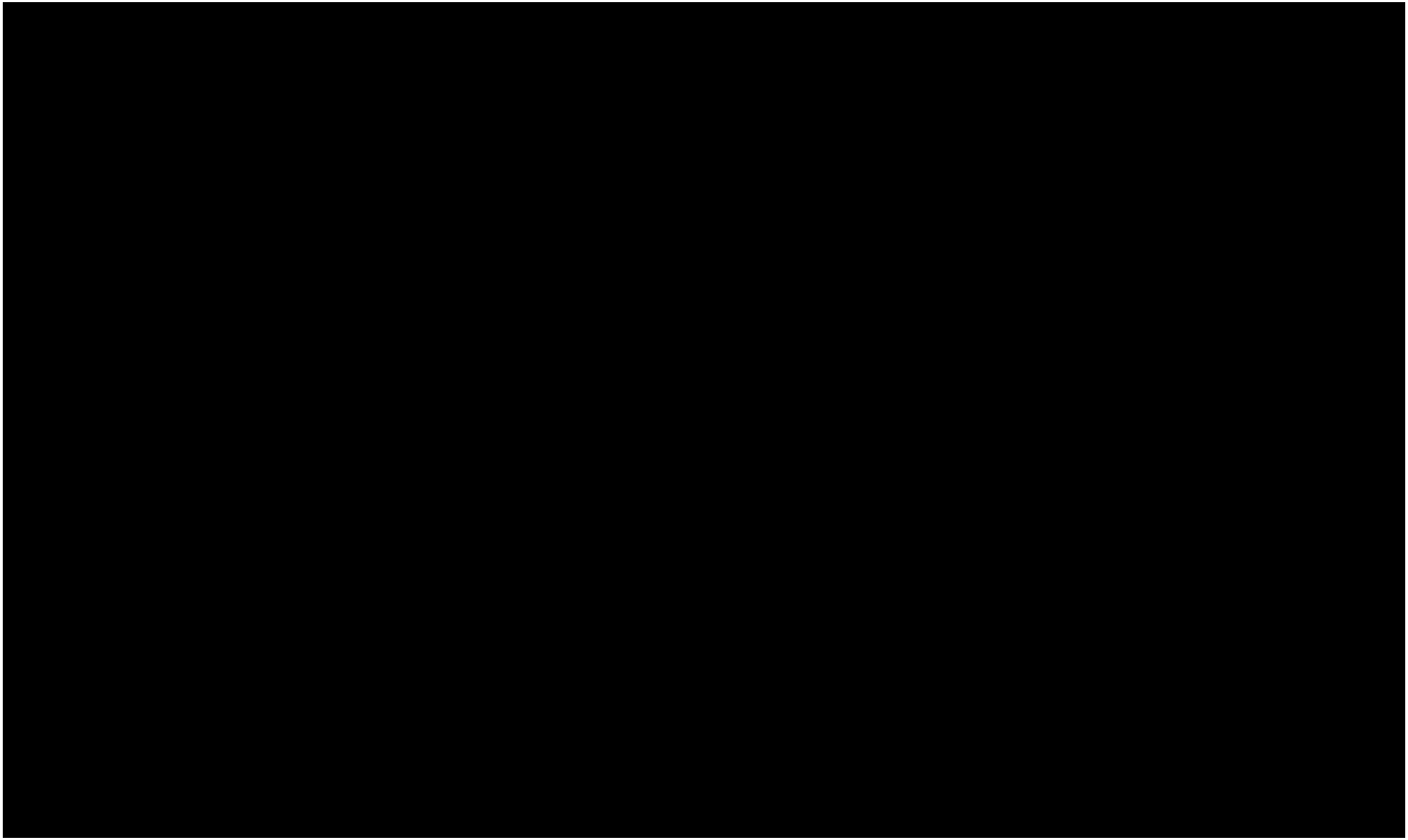


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

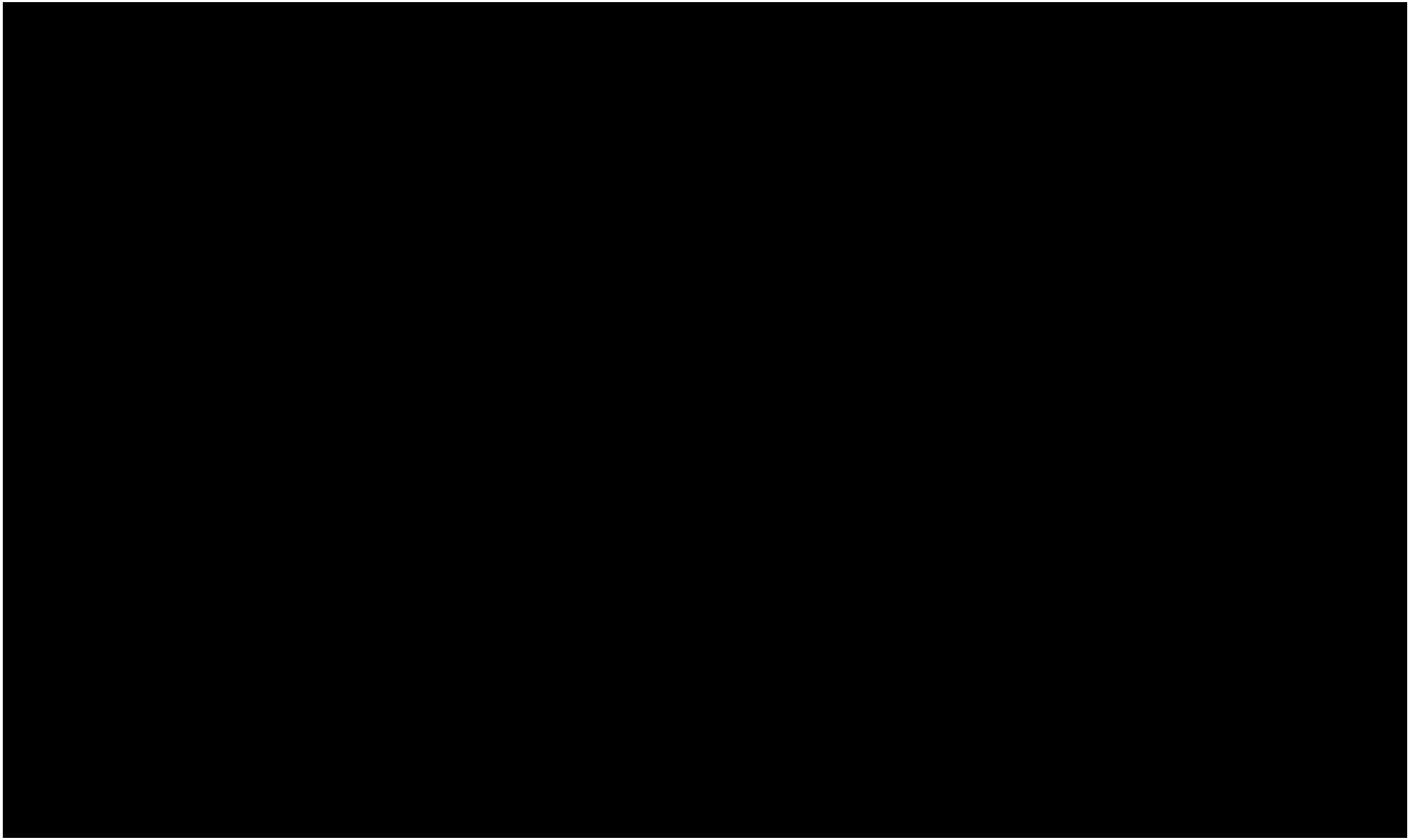
Product Rank 43, BEEF INSIDE ROUND XT SELECT

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 44, BEEF RIB BNLS LIPON RIBEYES UP

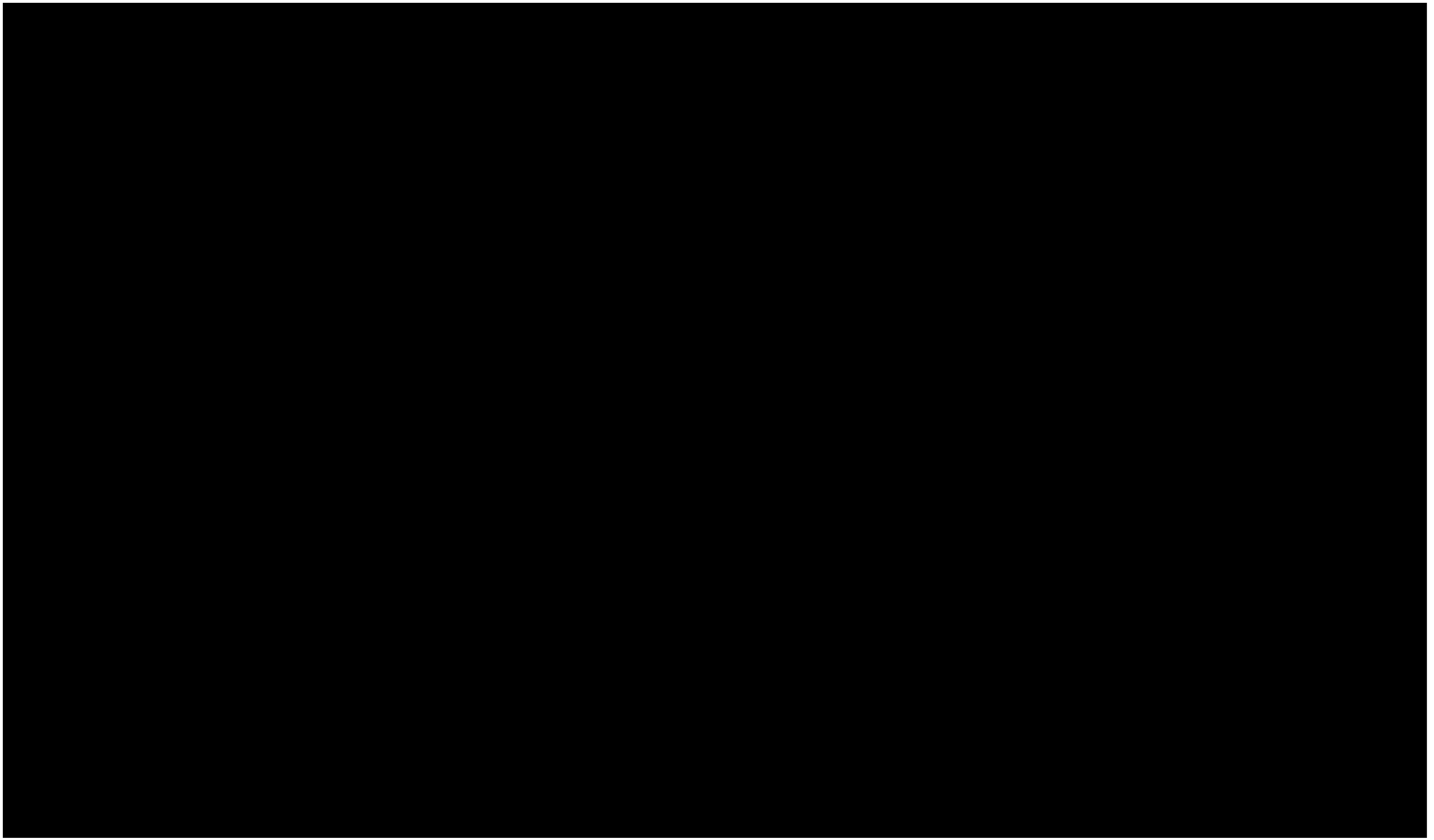
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 45, BOTTOM ROUND FLAT USDA SELECT



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 46, RIB EYE LIPON HVY SELECT

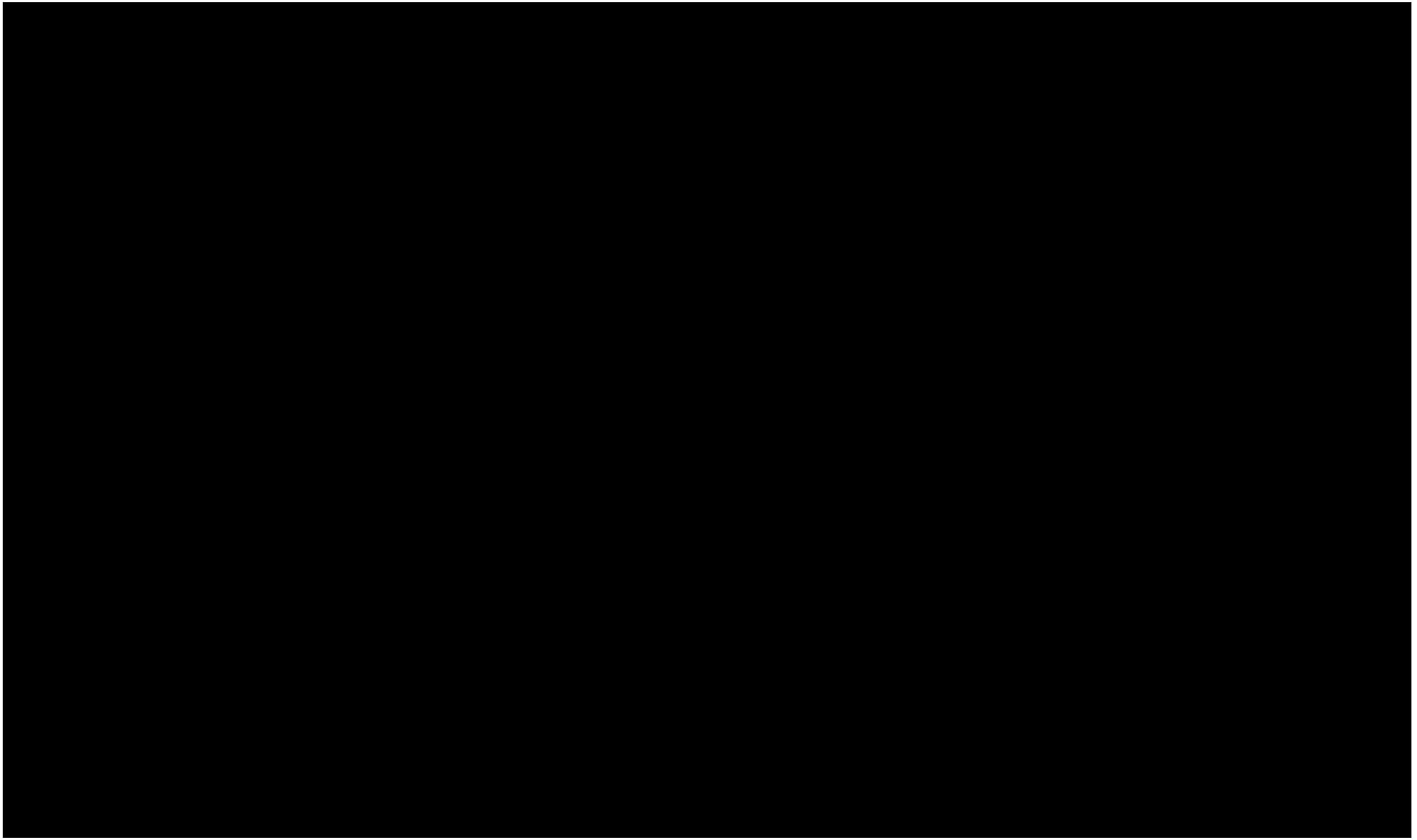


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 47, BNLS RIBEYE LIPON LGT



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 48, CAB CH/HI BNLS BEEF CHUCK EYE ROLL S/C

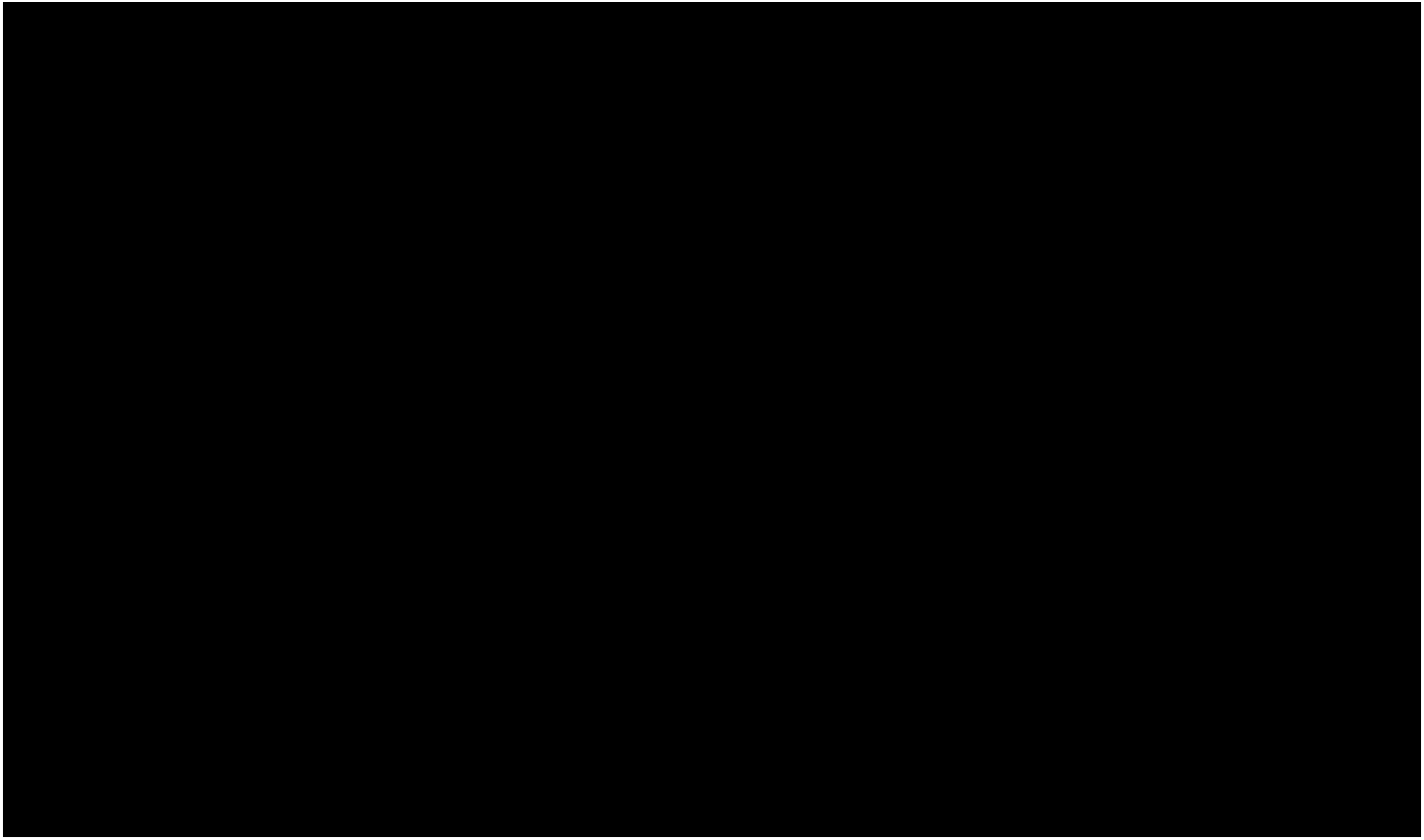
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 49, BEEF CHUCK ROLL NECK–OFF



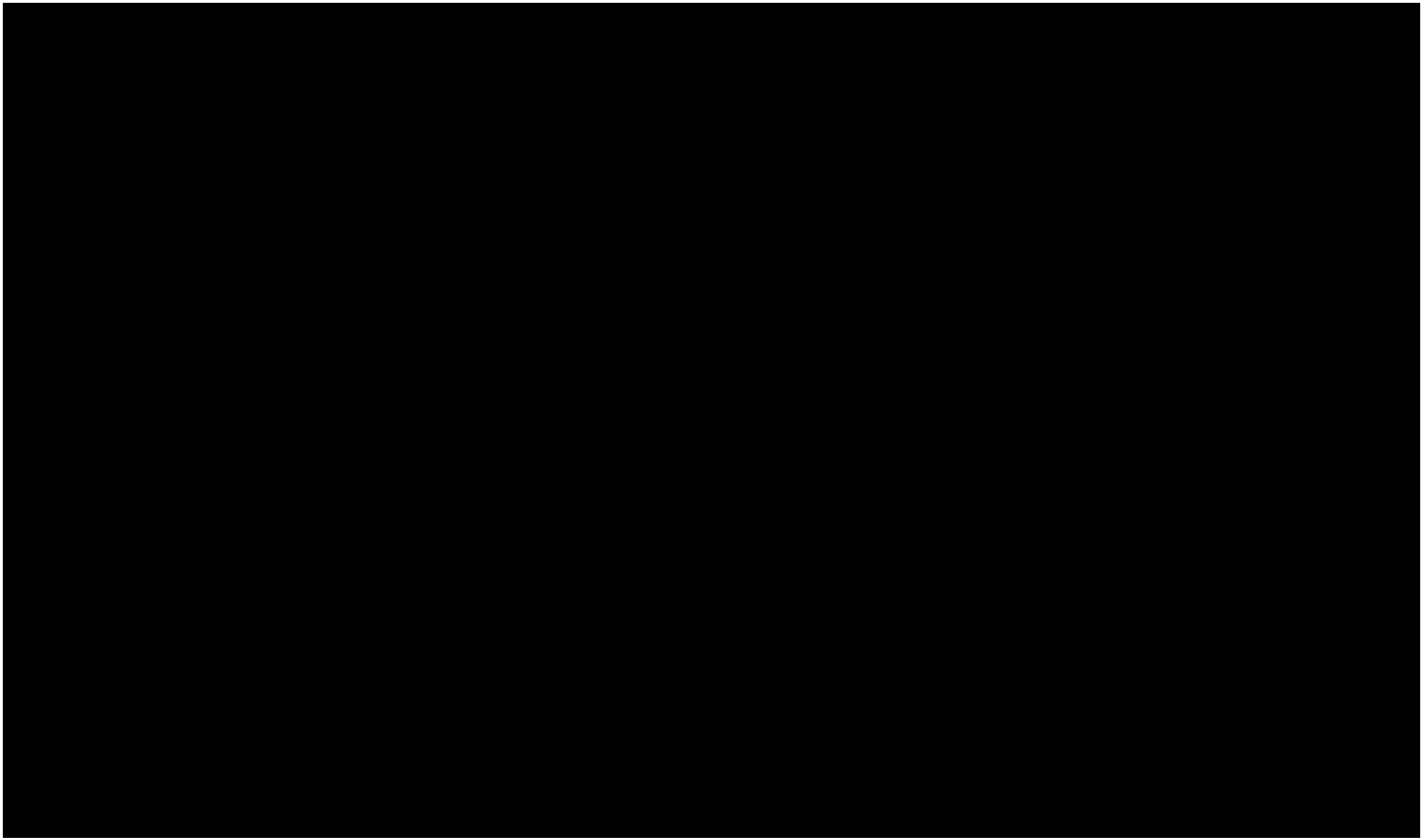
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 50, CH/HI BNLS BEEF TENDER PEELED 5/UP

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 51, SE BNLS BEEF ROUND FLATS

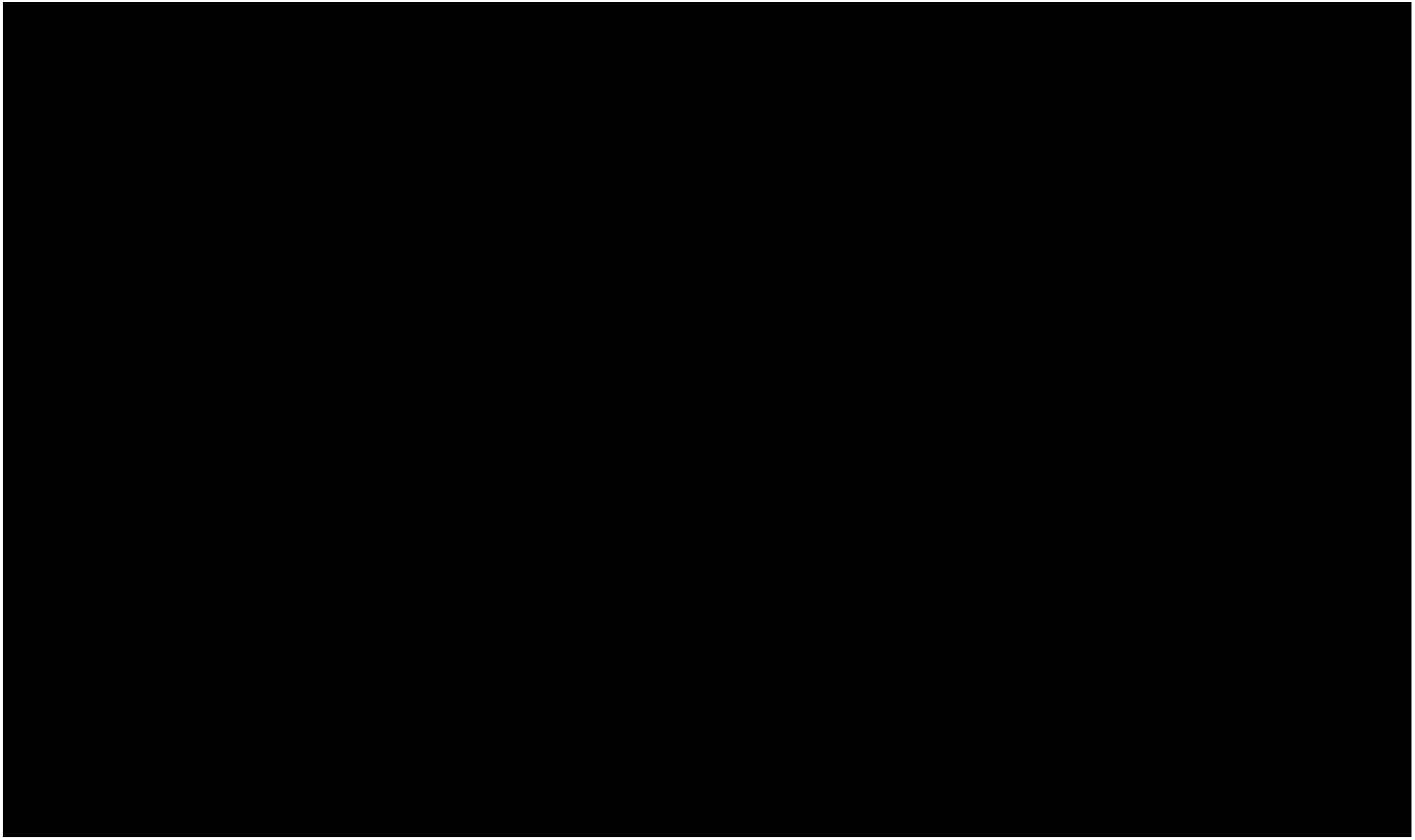


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 52, CH/HI BB BOTTOM SIRLOIN BUTT FLAP MEAT



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

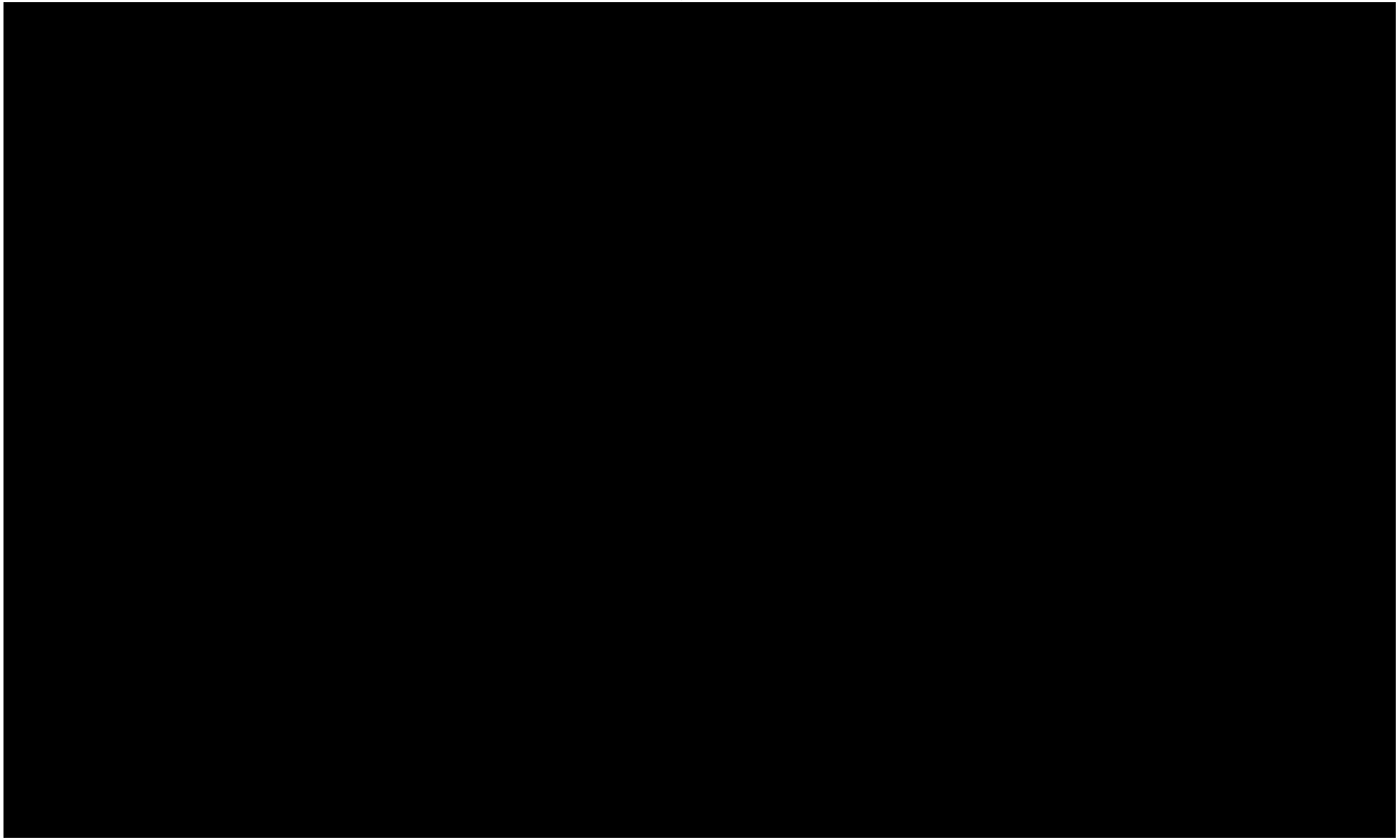
Product Rank 53, BF ROUND–INSIDE 18 UP CH/HI



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 54, 00011742 SE SmtCh CHUCKEYE

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

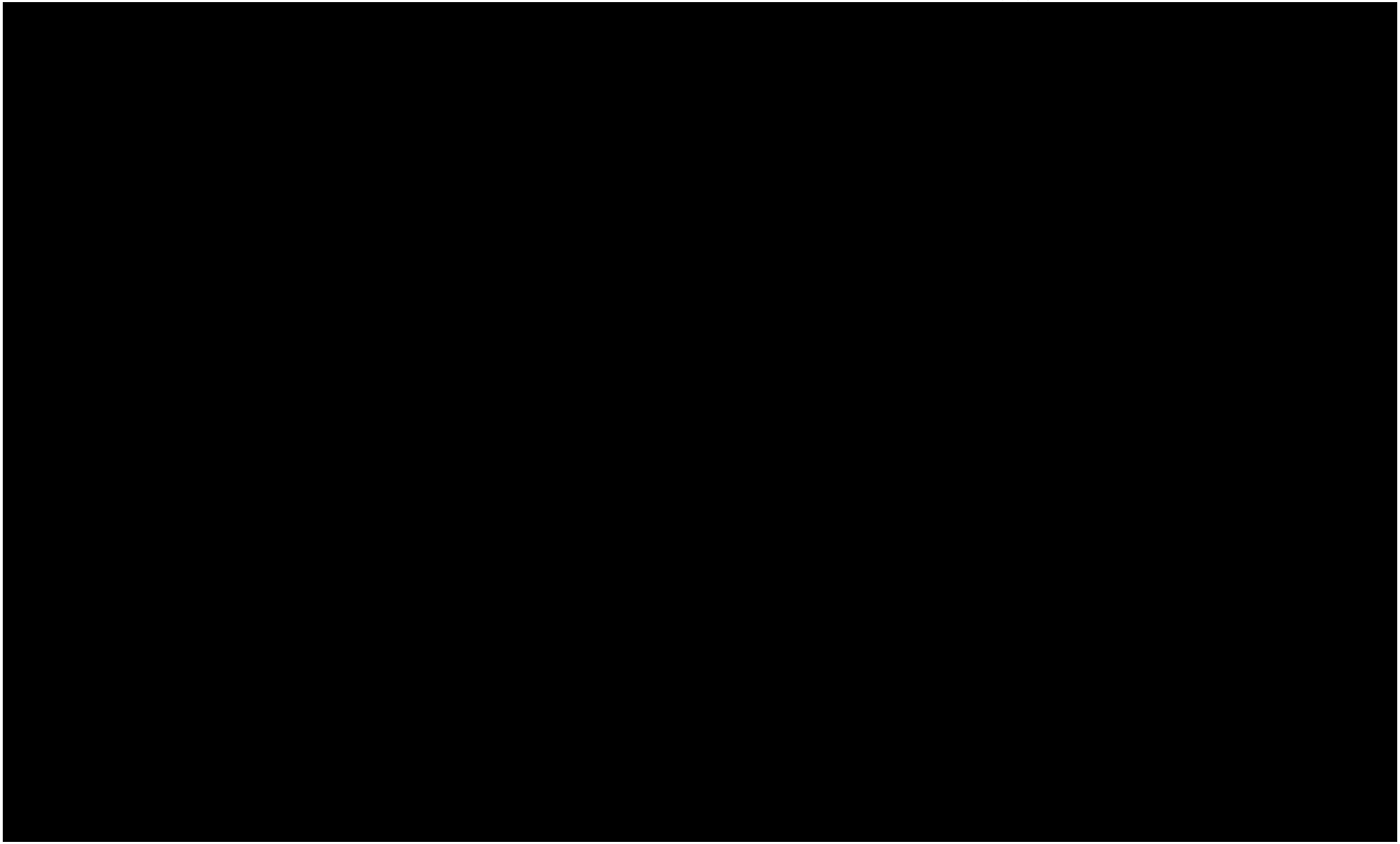
Product Rank 55, PLD TOP(CENTER CUT) & CULOTTE



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

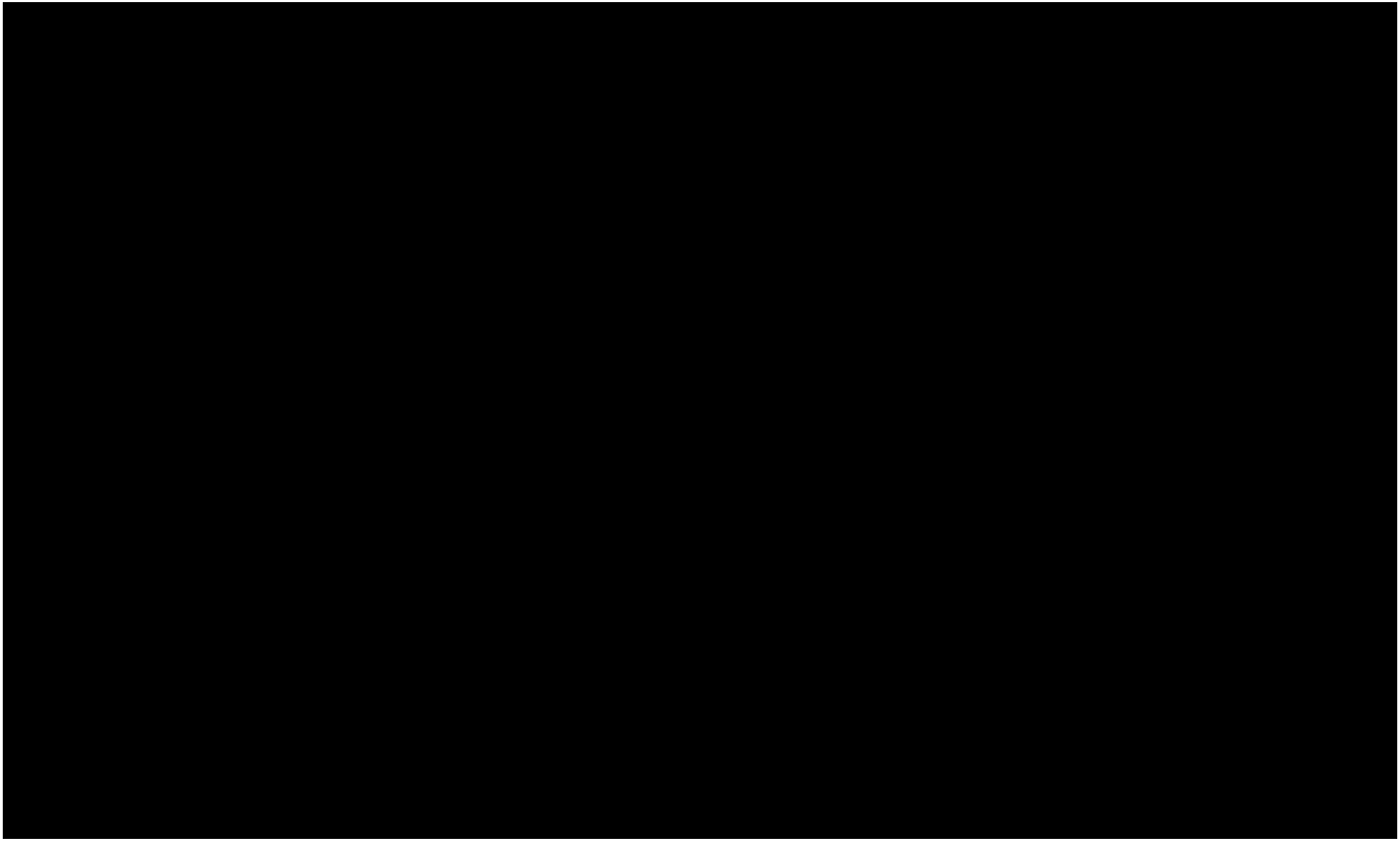
Product Rank 56, CAB CH/HI BNLS BEEF TOP INSIDE ROUND S/T

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 57, SHOULDER CLOD MUSCLE (H)

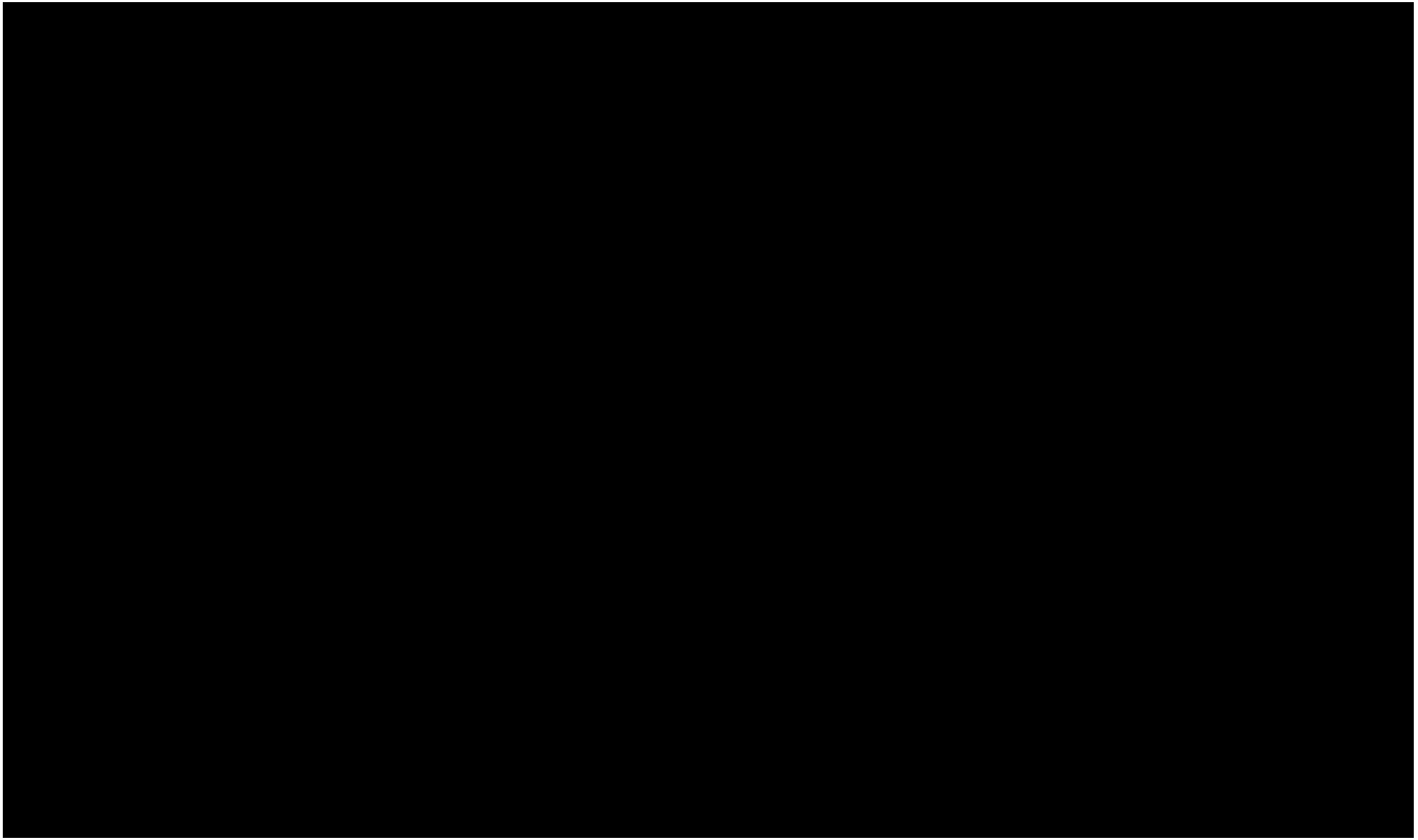


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 58, SEMI–BNLS NECK–OFF CP CHUCK



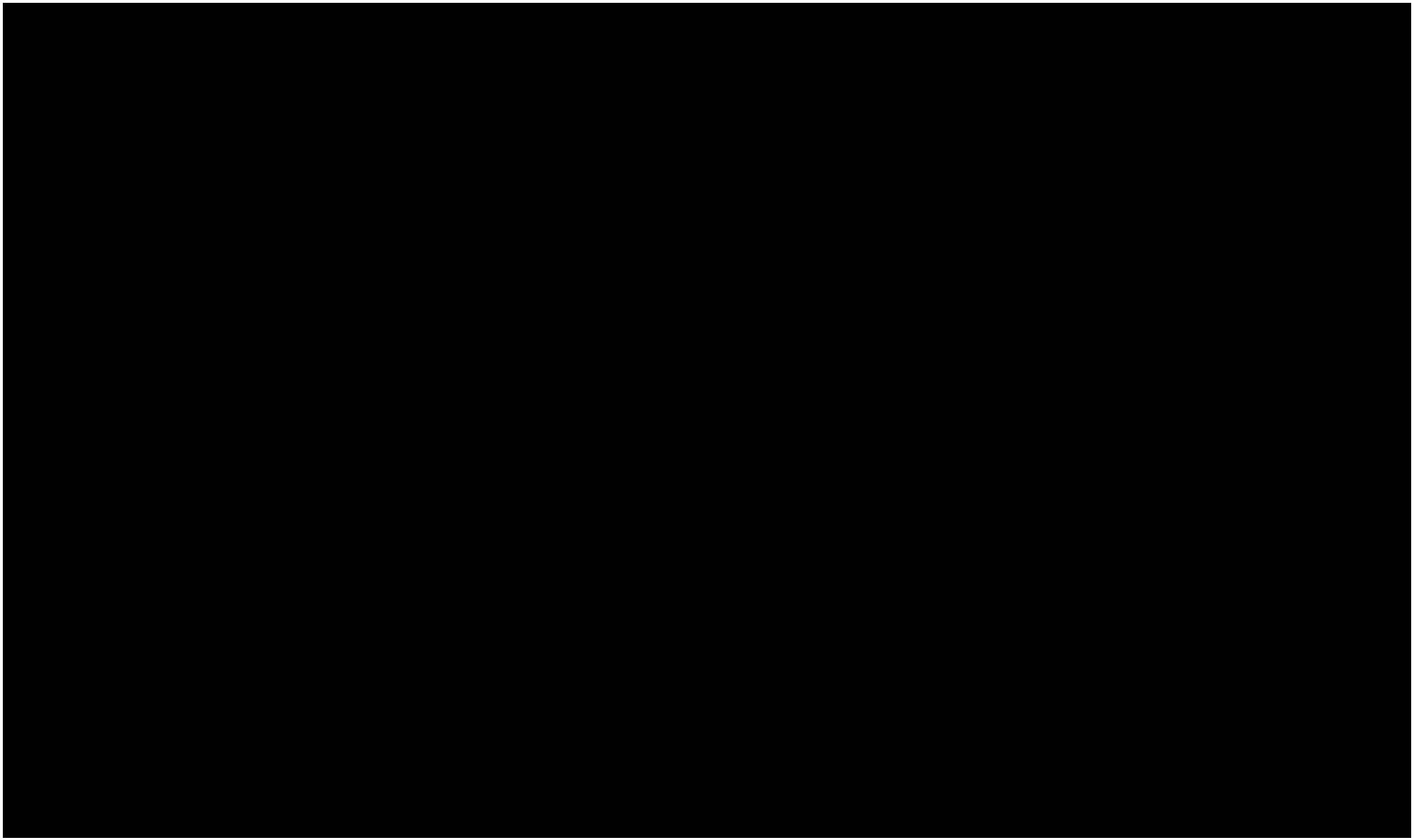
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 59, BL TOP SIRLOIN XT USDA SELECT



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 60, BEEF CHUCK–CHUCK ROLL 1"

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 61, 00011829 CH NXTG CHUCKEYE

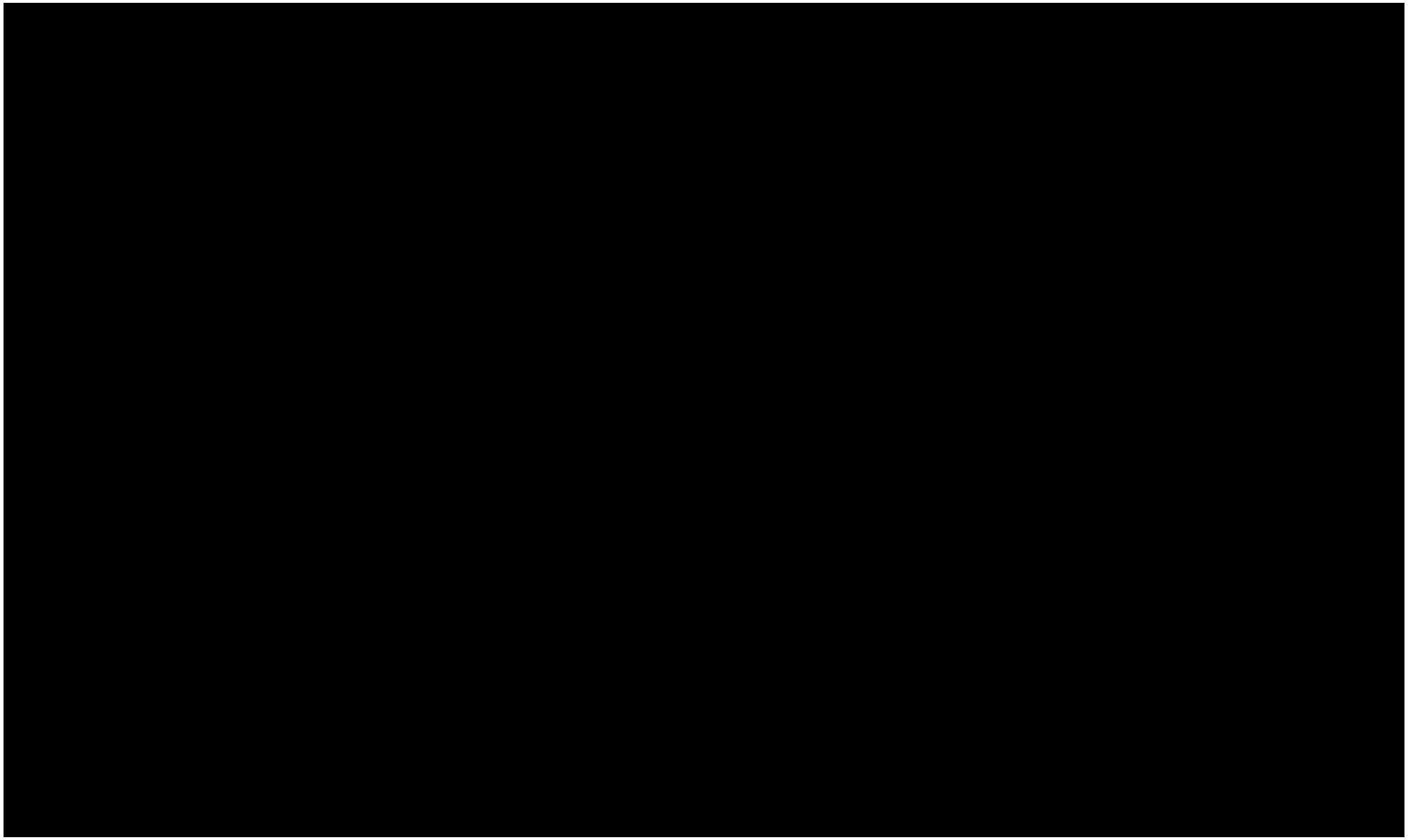
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 62, CAB CH/HI BNLS BEEF ROUND FLAT



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 63, CH/HI BNLS BEEF RIBEYE L/O 2X2 DN

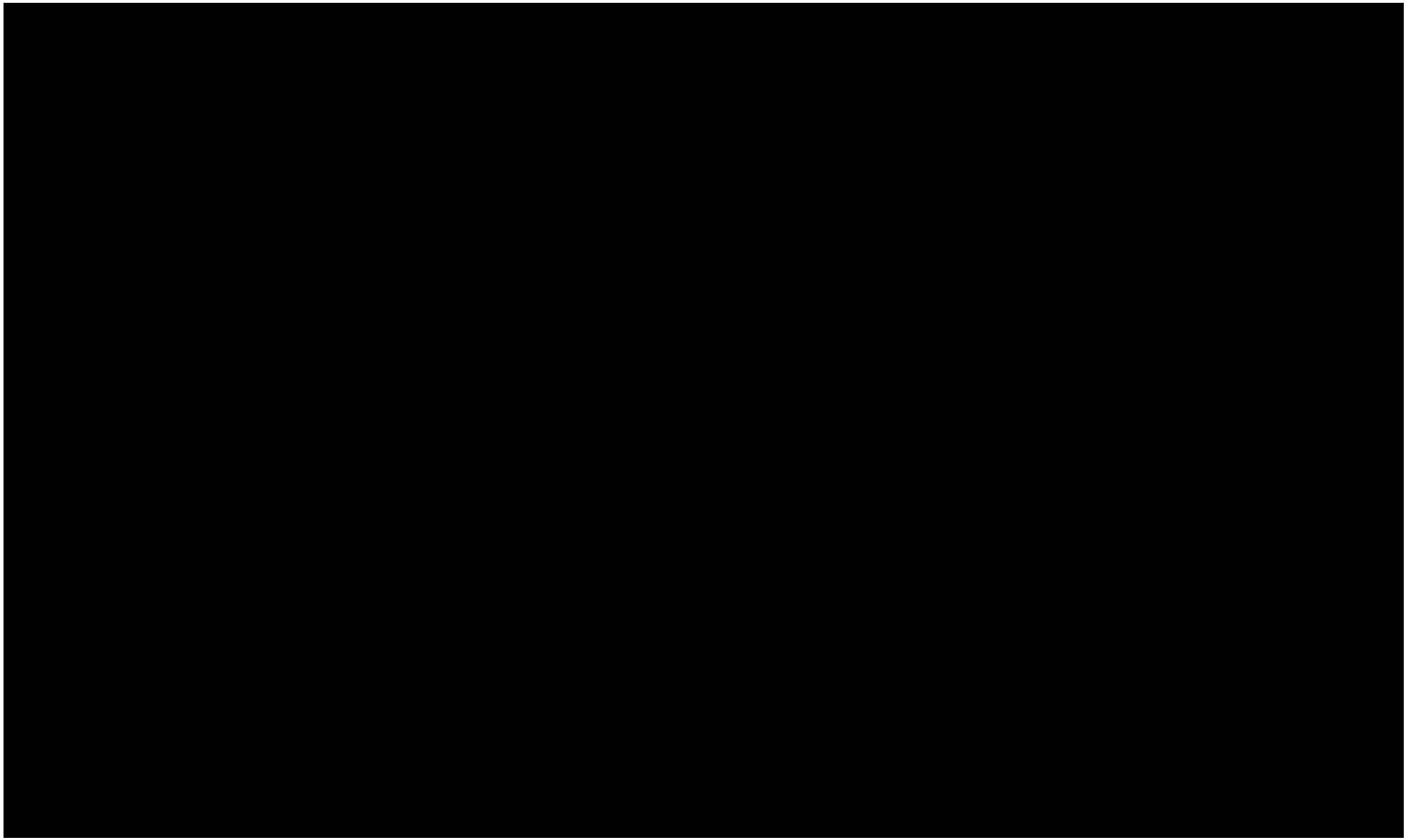
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 64, BF RND–INSIDE PREFD "1/4"

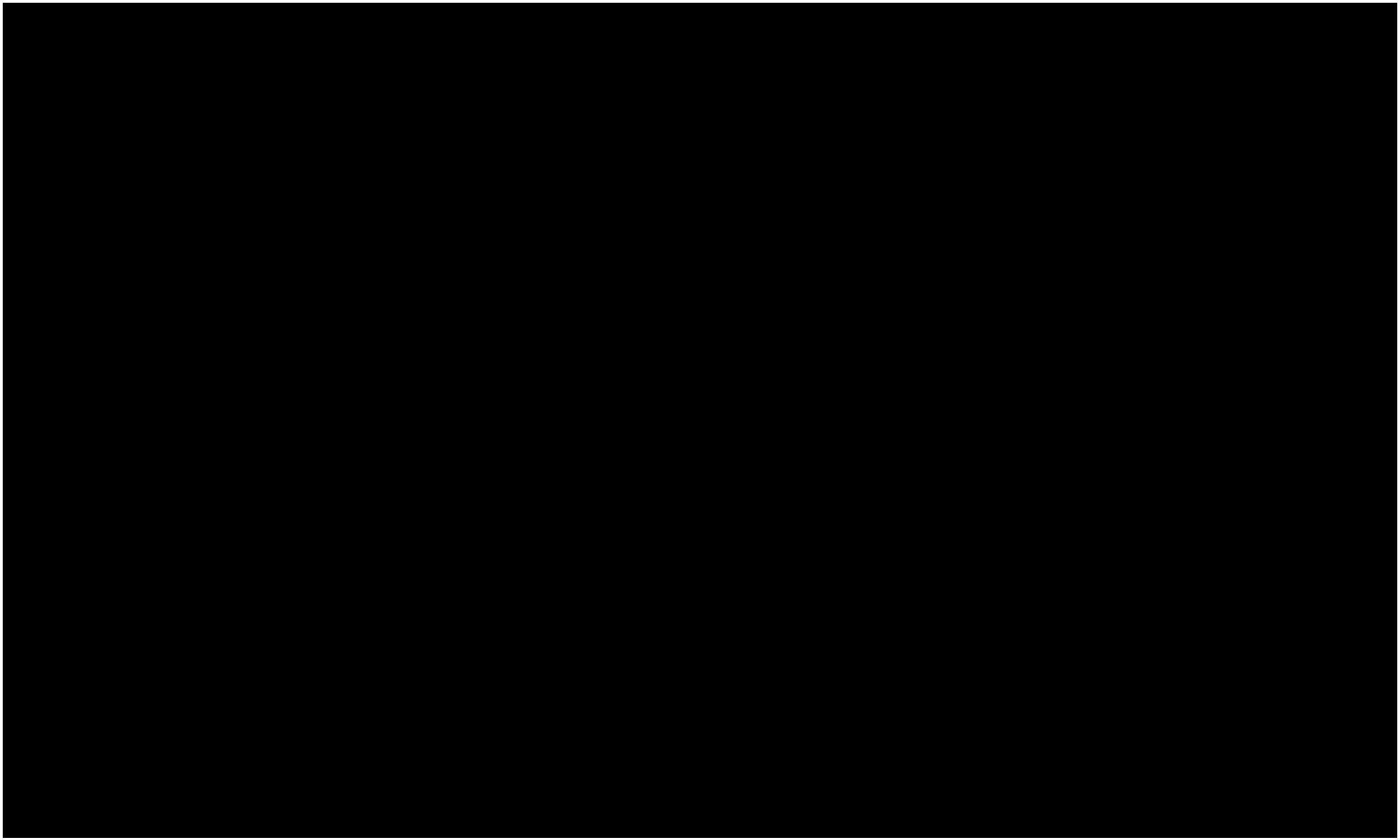


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 65, BEEF BOTTOM ROUND FLAT CRT

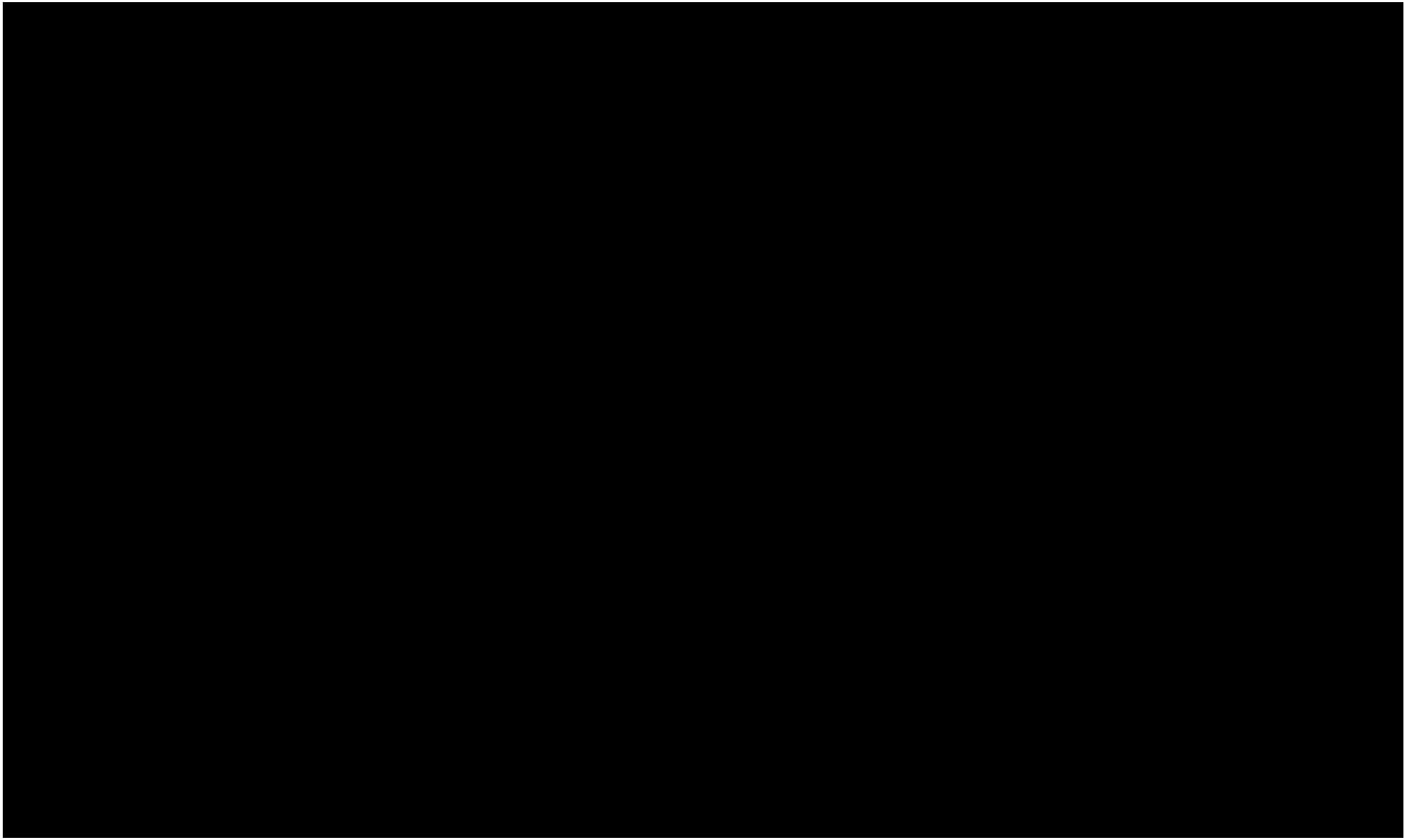


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 66, CH/HI BNLS BEEF RIBEYE L/O 2X2 UP



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 67, TOP SIRLOIN



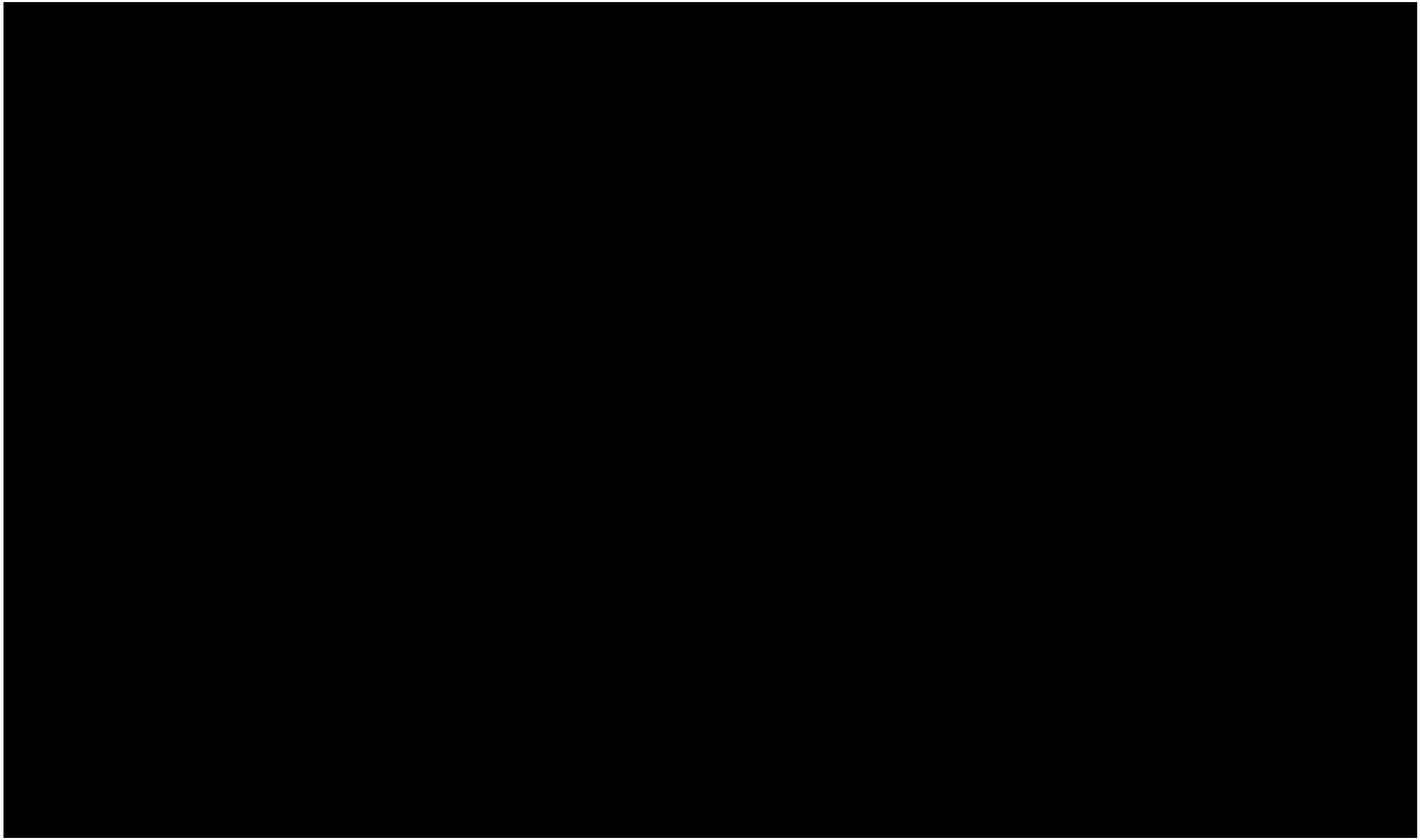
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 68, SHORT LOIN XT 0X1 CHOICE

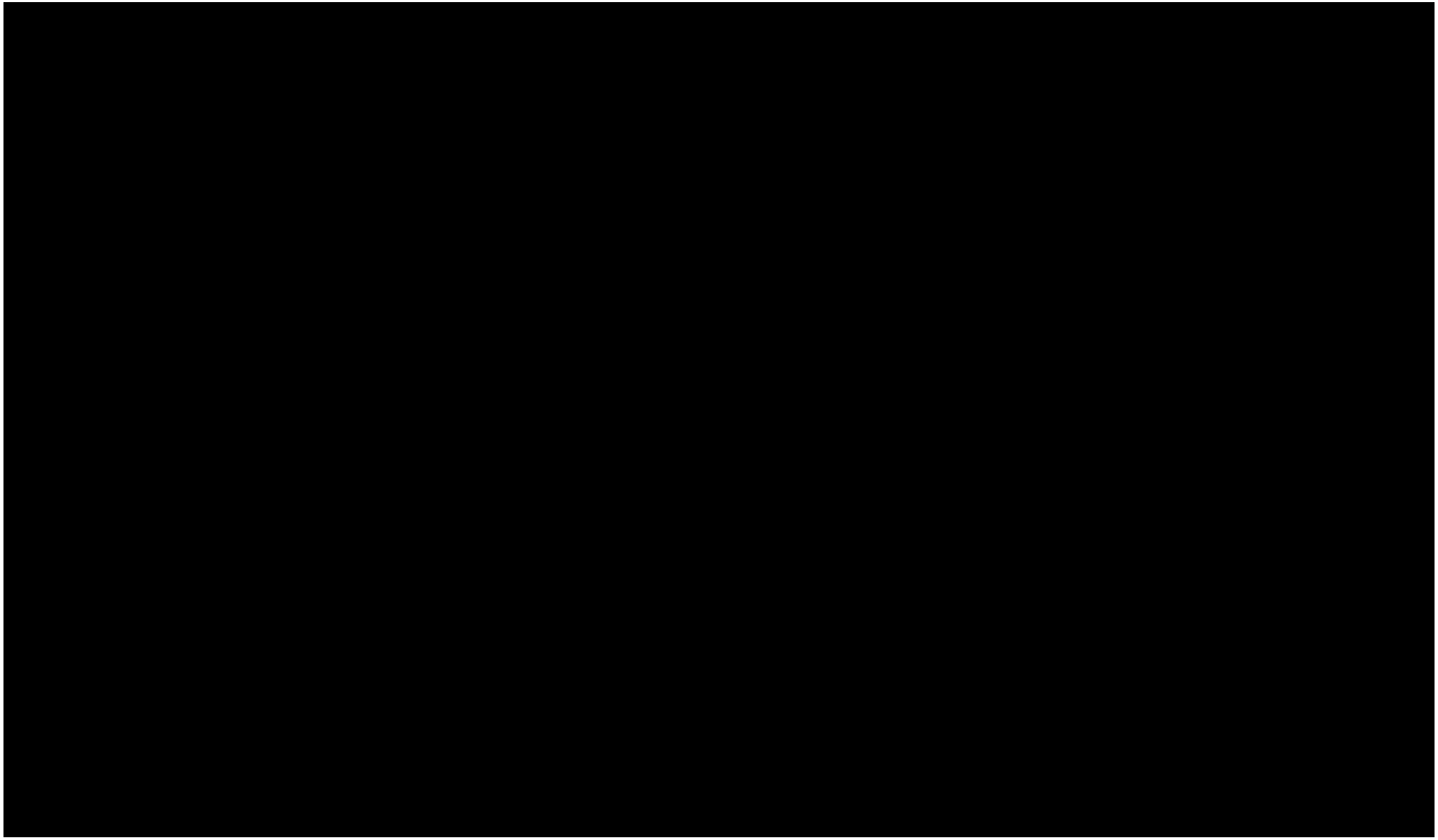
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 69, 00011422 CH SmtCh CLOD 1 Piece

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 70, CHUCK ROLL TRIMMED N/O XT

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 71, 00011425 CH PECTORAL MEAT

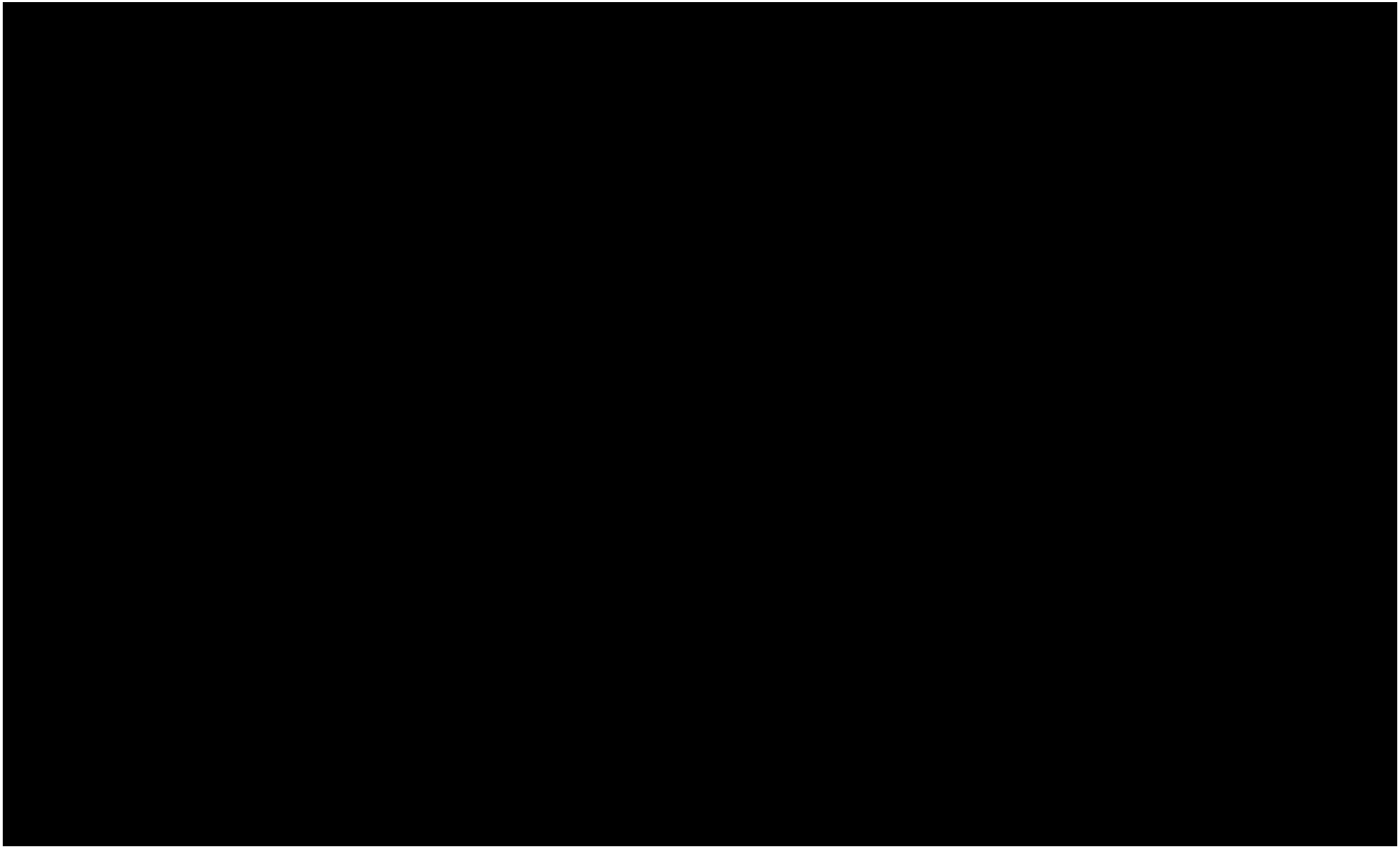


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 72, CH/HI B/I BEEF SHORT LOIN 1X0 S/T TFR



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 73, STR CK ROLL N/O CHOICE

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 74, CHUCK MOCK TENDER CHOICE



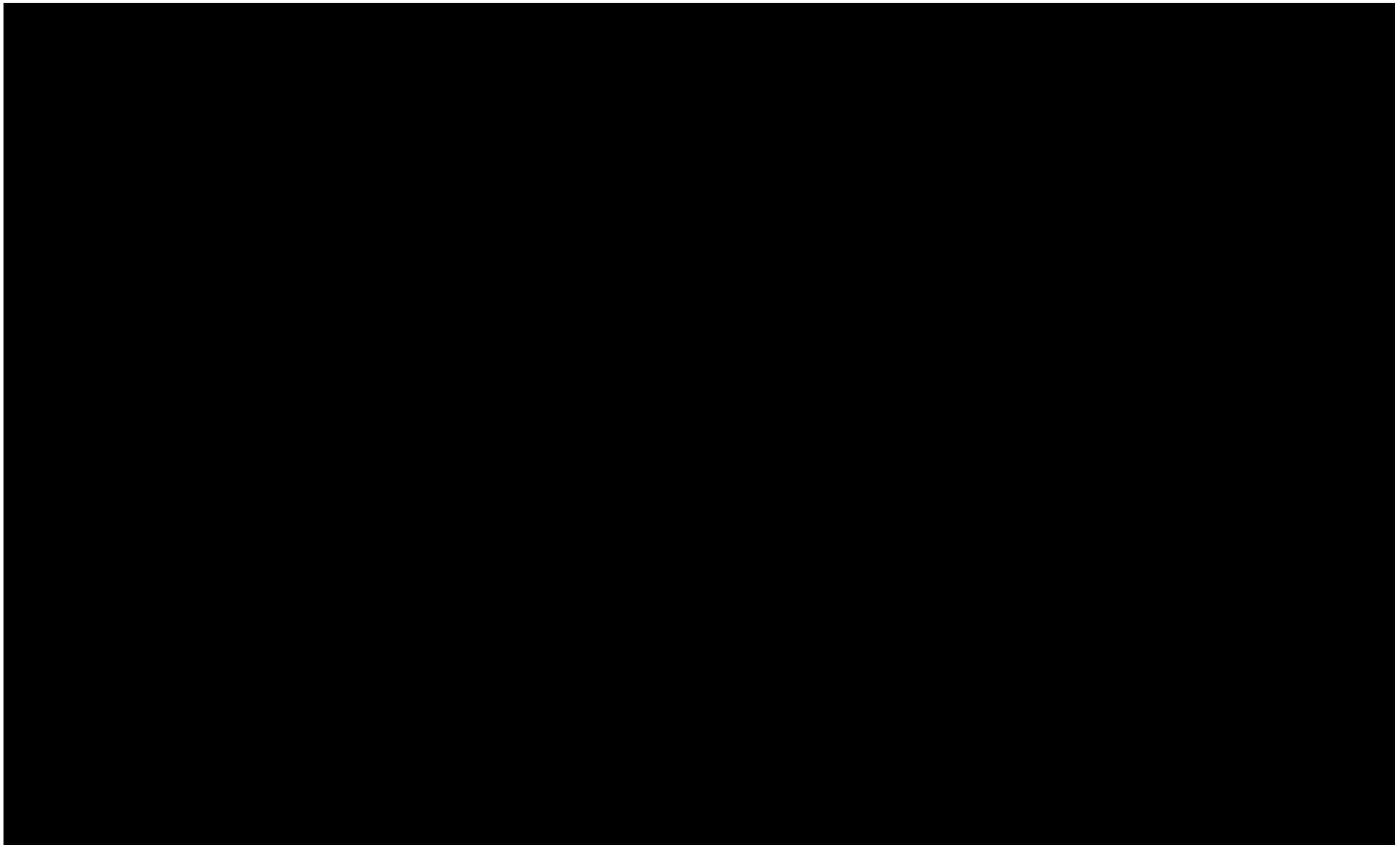
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 75, 00016620 CH SmtCh KNUCKLE Peeled

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 76, 00018924 CH TENDERLOIN Pld NWR

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 77, 00018720 CH FLAP MEAT



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 78, CH/HI BNLS BEEF BOTTOM ROUND FLAT B/R

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 79, BF CHK=CHK ROLL N/O PRED CH/HL

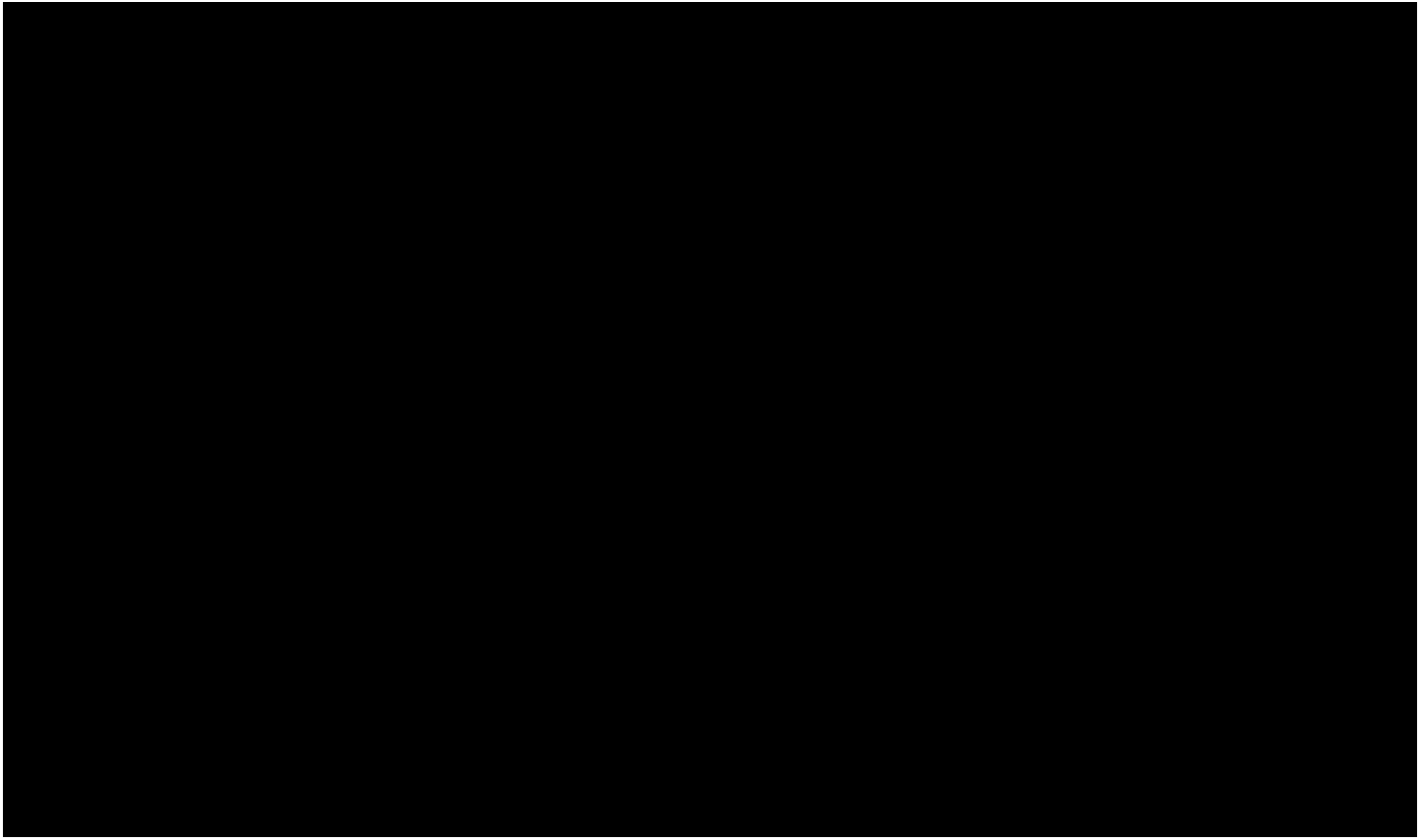
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

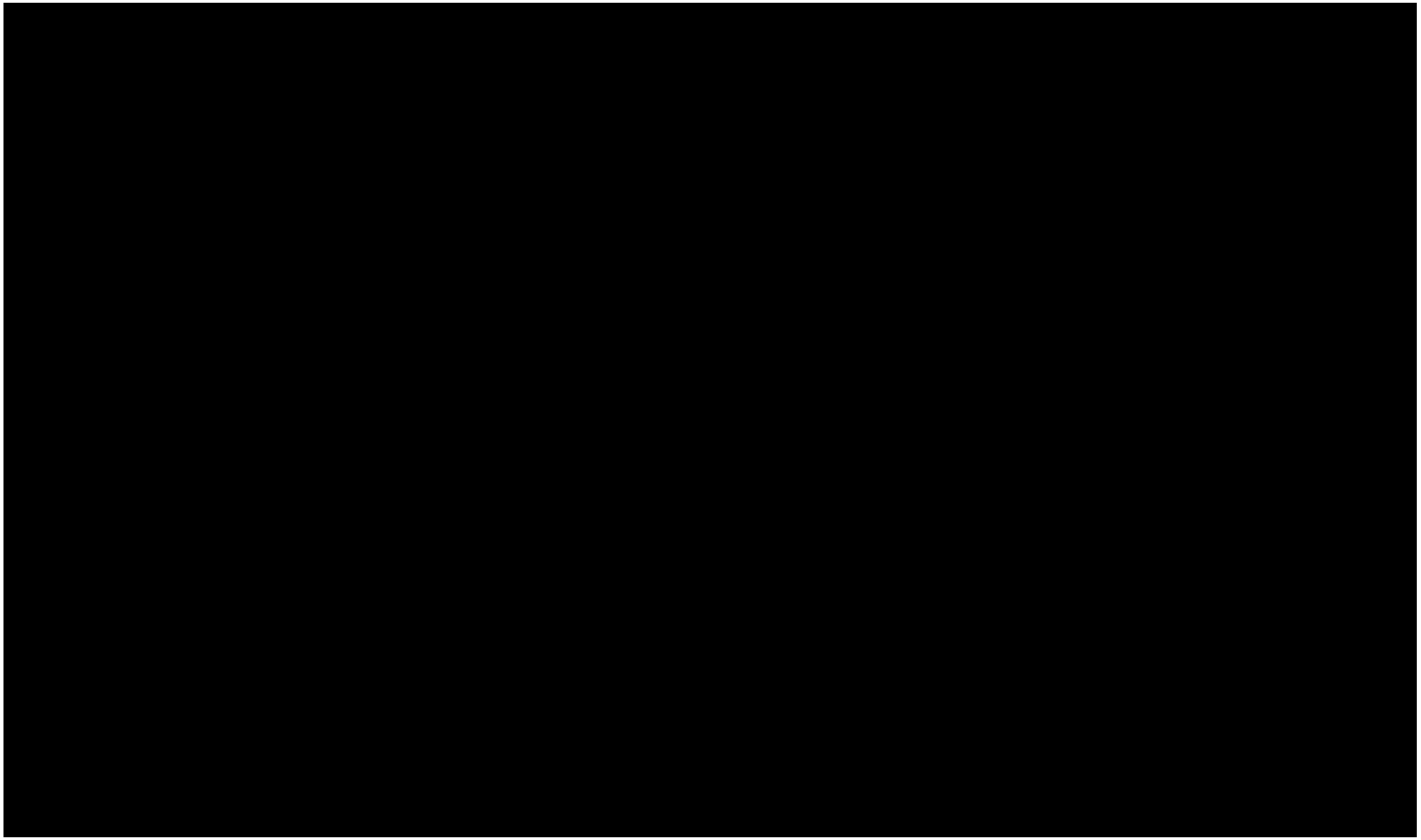
Product Rank 81, BNLS BEEF LOIN TAILS

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 82, BF ROUND–EYE OF ROUND CH/HI

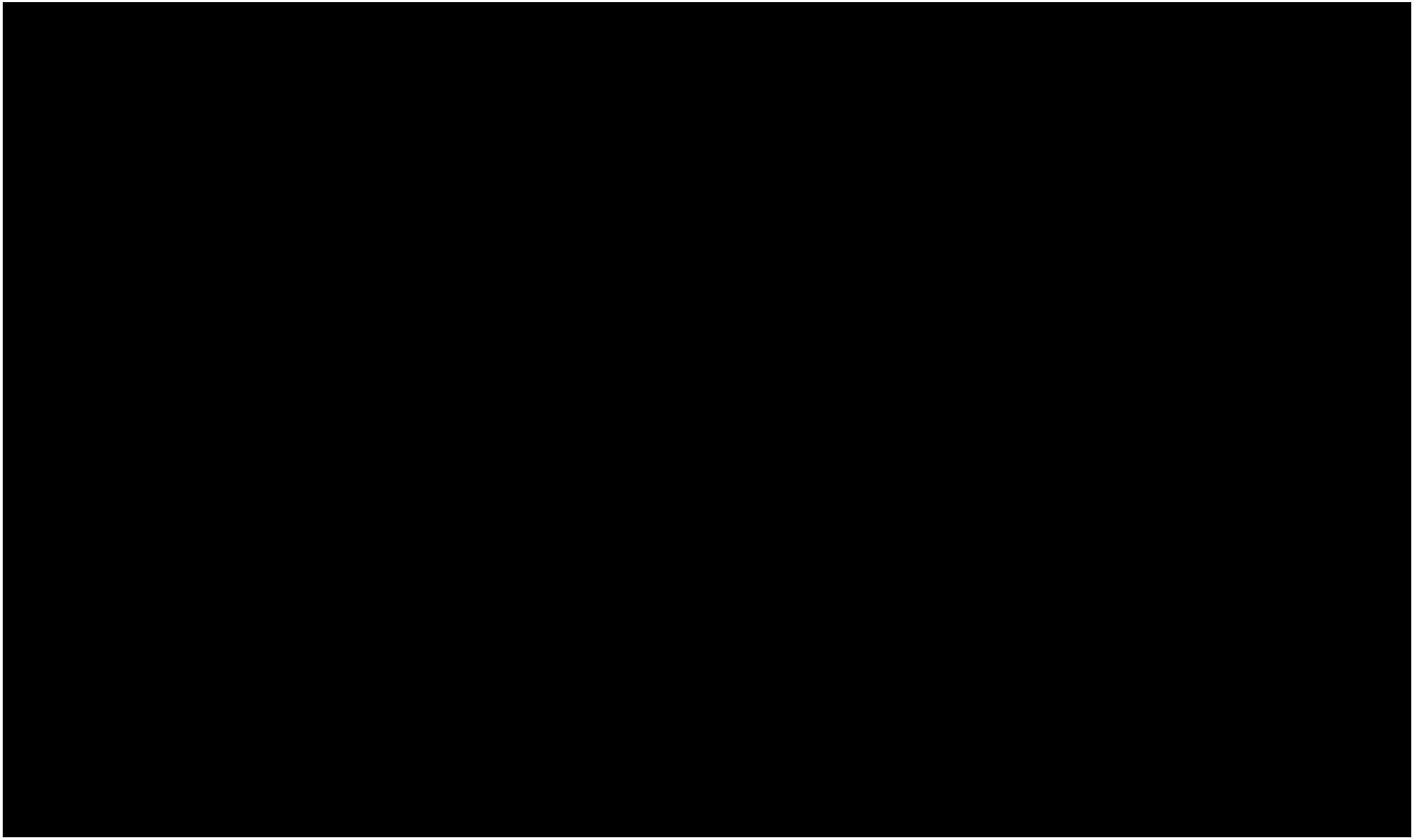


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 83, BF CHUCK–SHOULDER CLOD CH/HI



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 84, CH/HI BNLS BEEF PECTORAL MUSCLE



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank #5: CAB BEEF KNUCKLES PEELED

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 86, BONELESS TOP SRLN XT CHOICE

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 87, SE BNLS BEEF TOP SIRLOIN BUTT S/T

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 88, BF CHUCK–B/I SHORT RIBS CH\HI

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

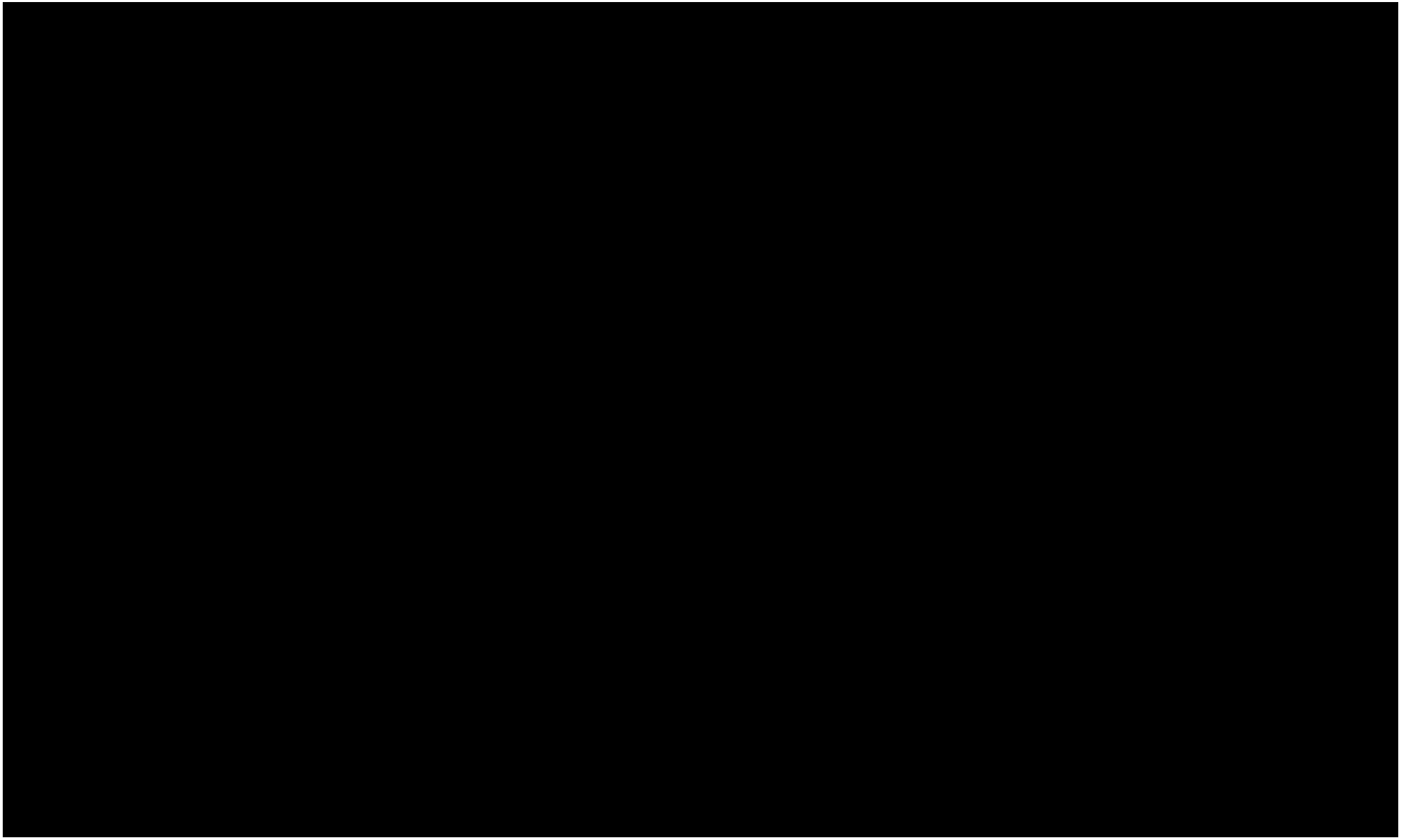
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 90, LOIN STRIP 0X1 USDA SELECT

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 91, 00017125 CH SmtCh EYE of Round

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 92, 00017826 CH SmtCh STRIPLOIN 0x1



Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 93, PEELED KNUCKLES USDA SELECT

[redacted]

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 94, BF RND=OUTSIDE FLAT USDA SELCT

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 95, BUTT BALL TIPS 1.5 SELECT

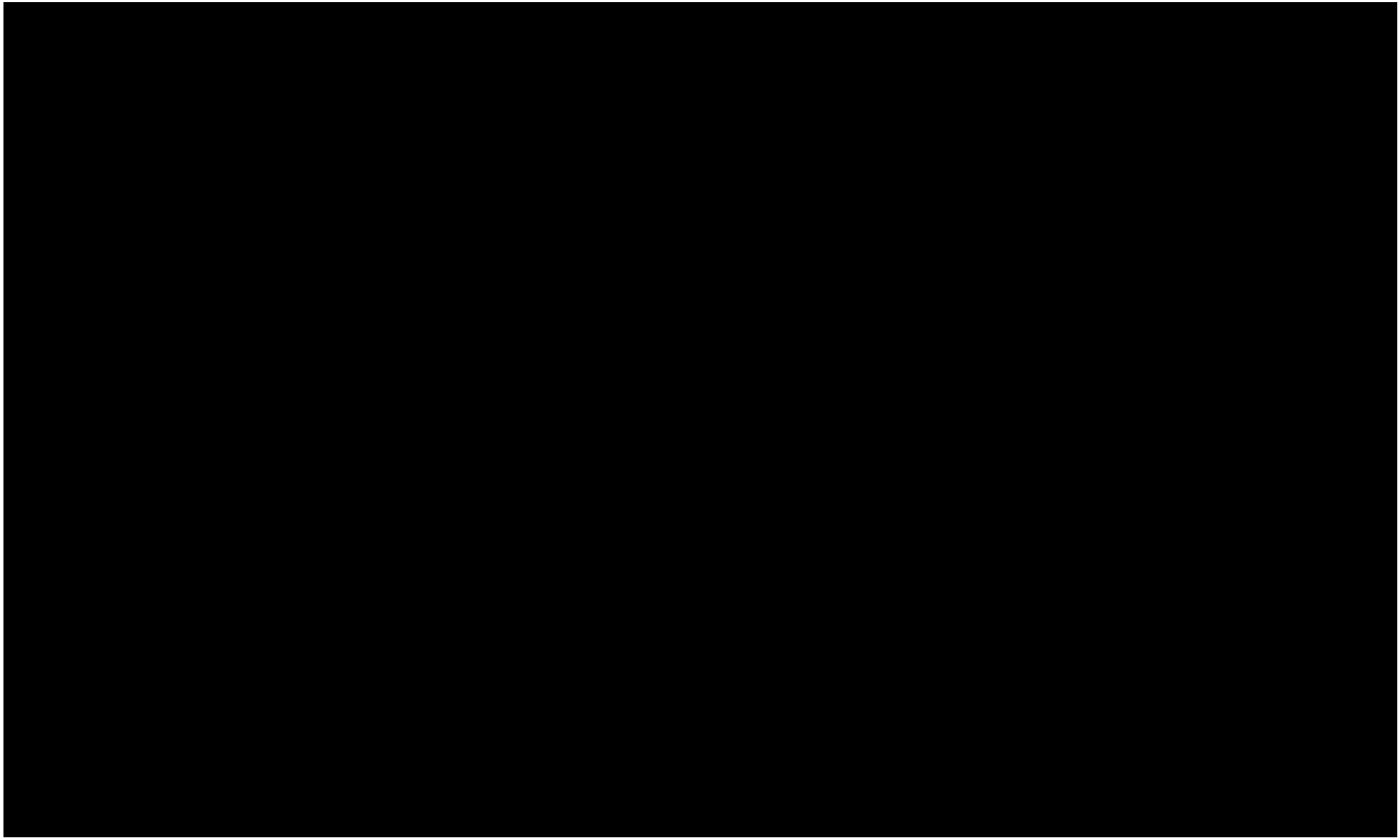
Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 96, STR LOIN STRIP 0X1 CHOICE ANG

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 97, BEEF ROUND EYE USDA SELECT

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products Product Rank 98, 00017526 CH SmtCh STRIPLOIN BI 1x1

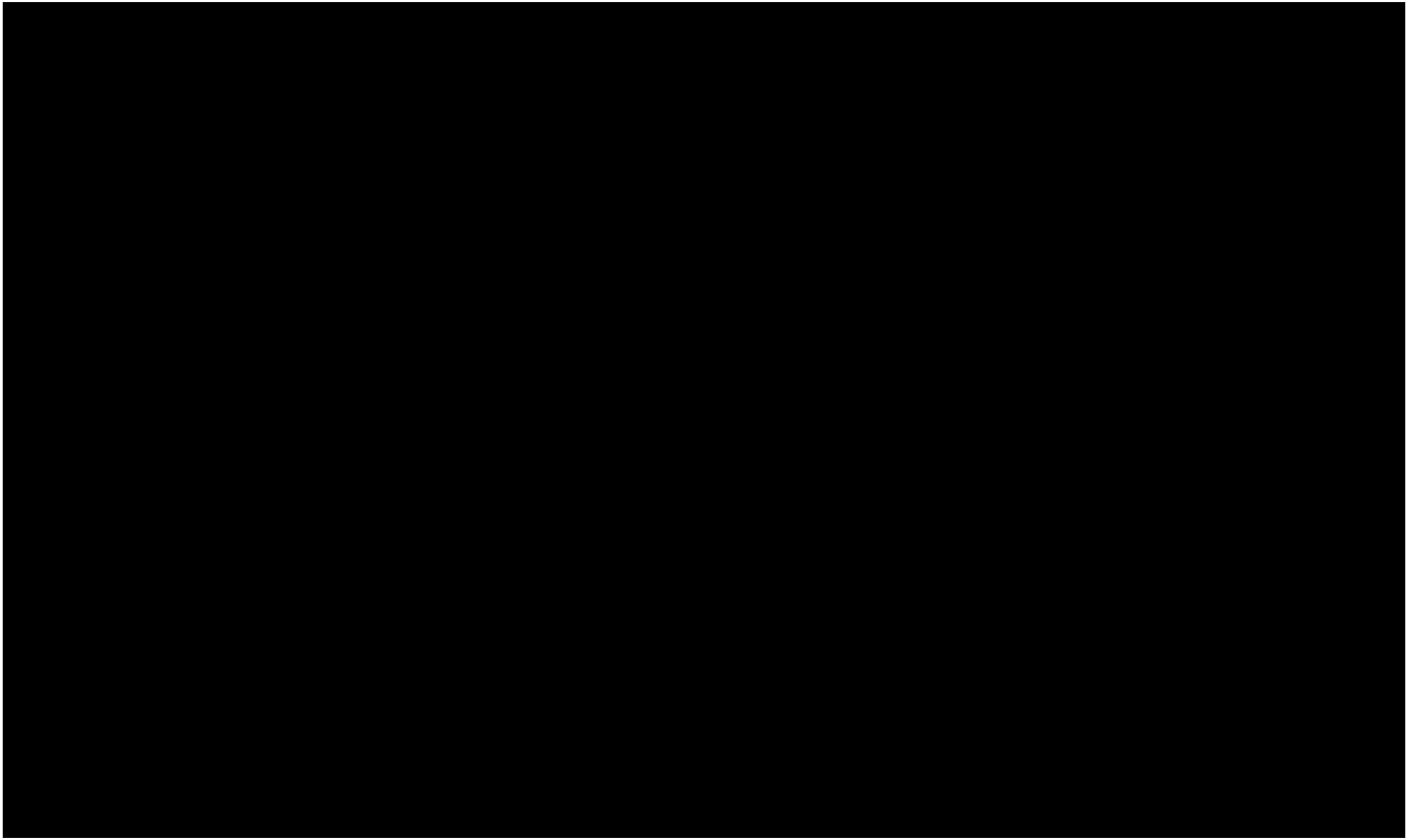


Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 99, 2 PC BNLS CHOICE COMBO

Appendix C. Dr. Stiroh's Figure 2.4 Modified to Show Top Selling Beef Products

Product Rank 100, BF LOIN–FLAP MEAT CH/HI

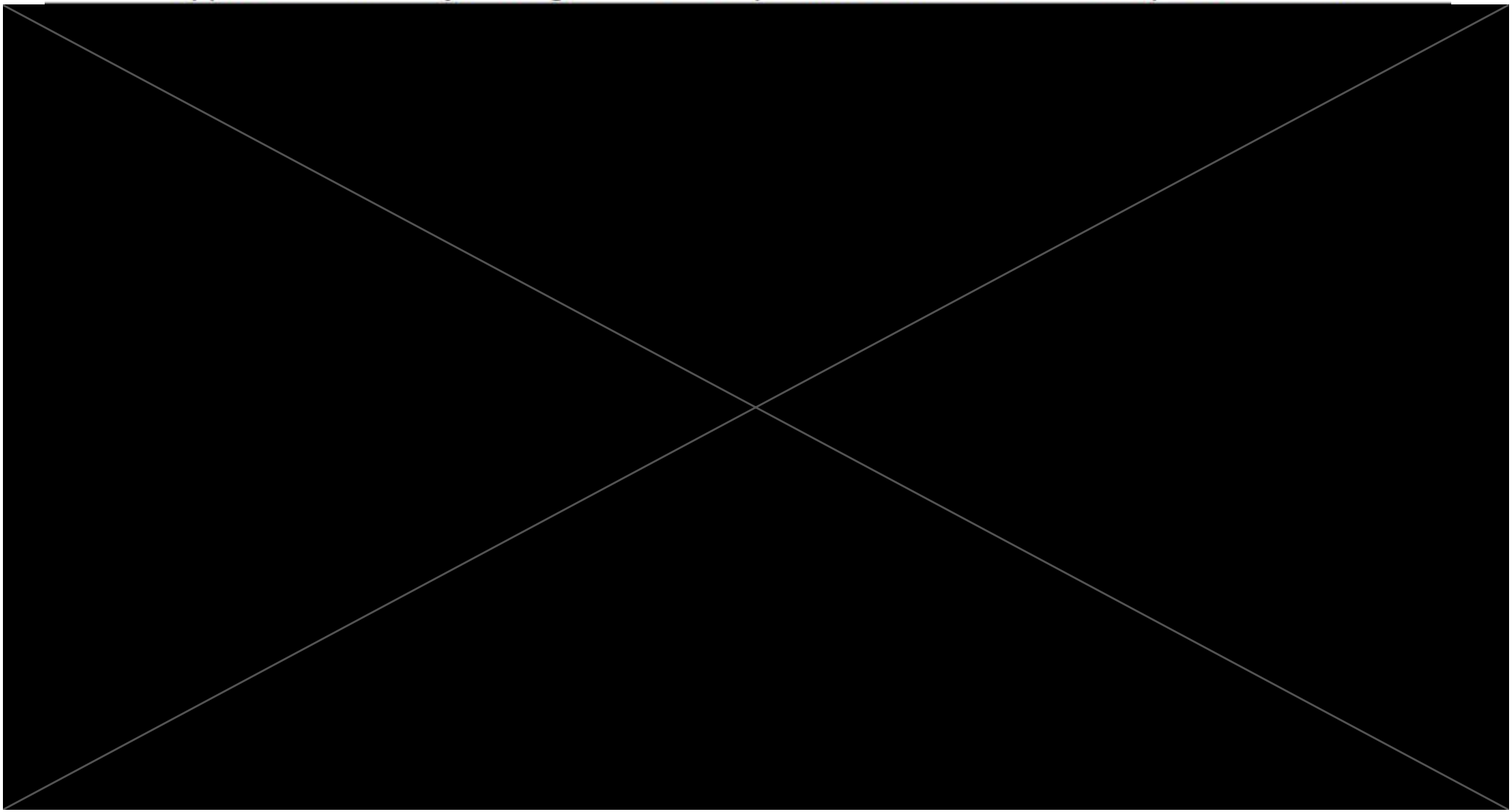


Appendix D

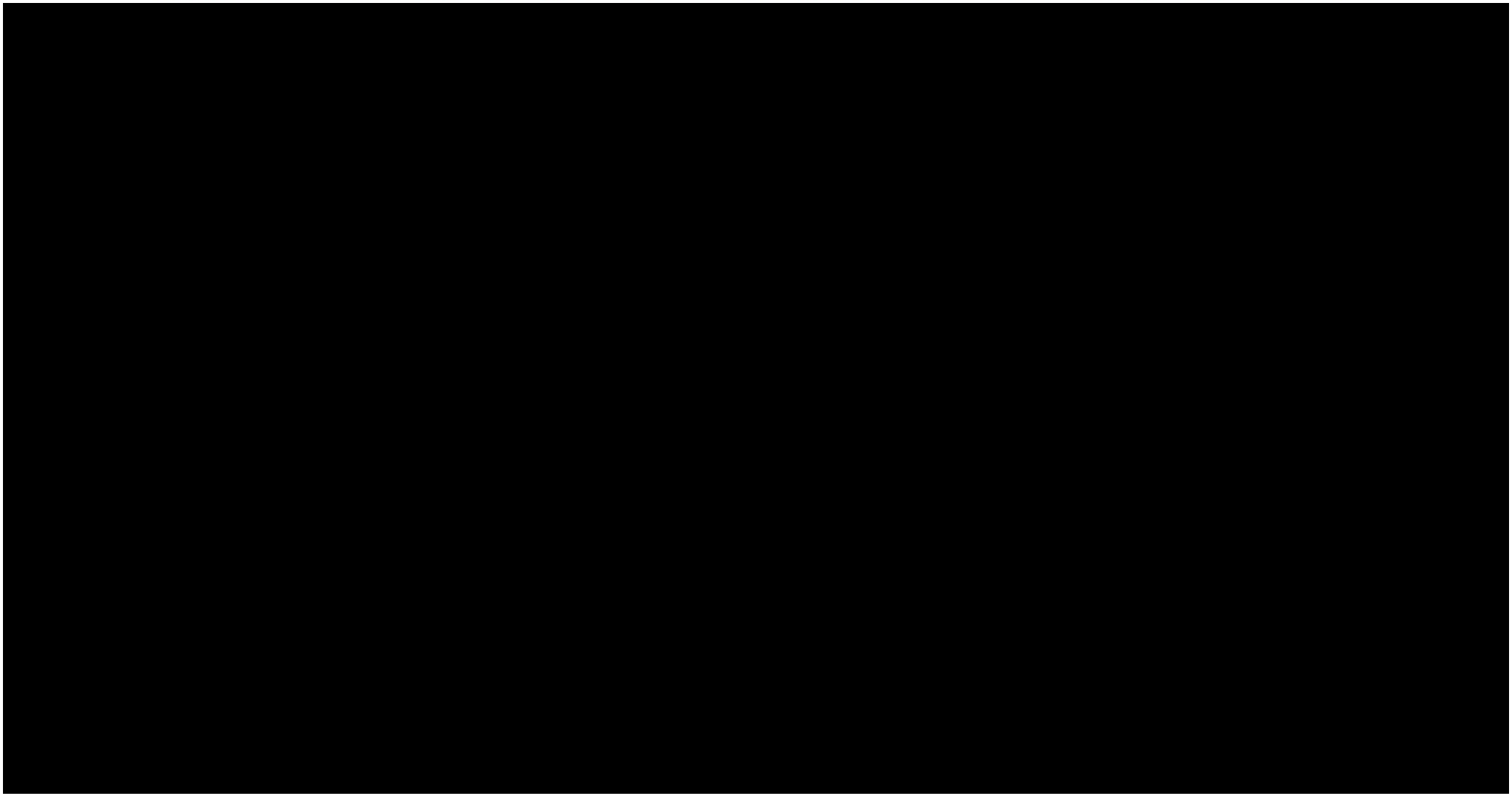
Appendix D. Purchase Type Direct Overcharge Sensitivity

Appendix E

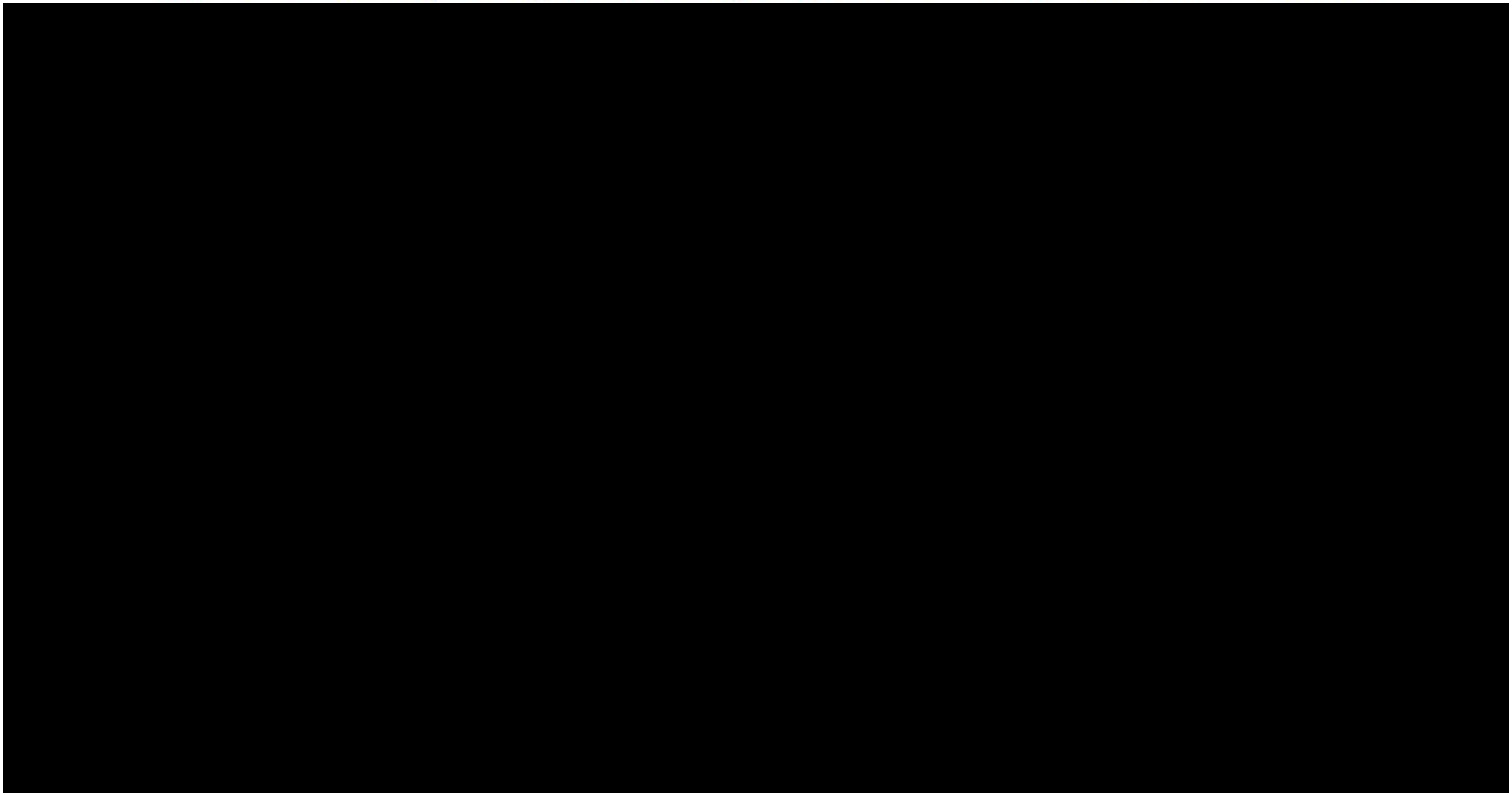
Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers



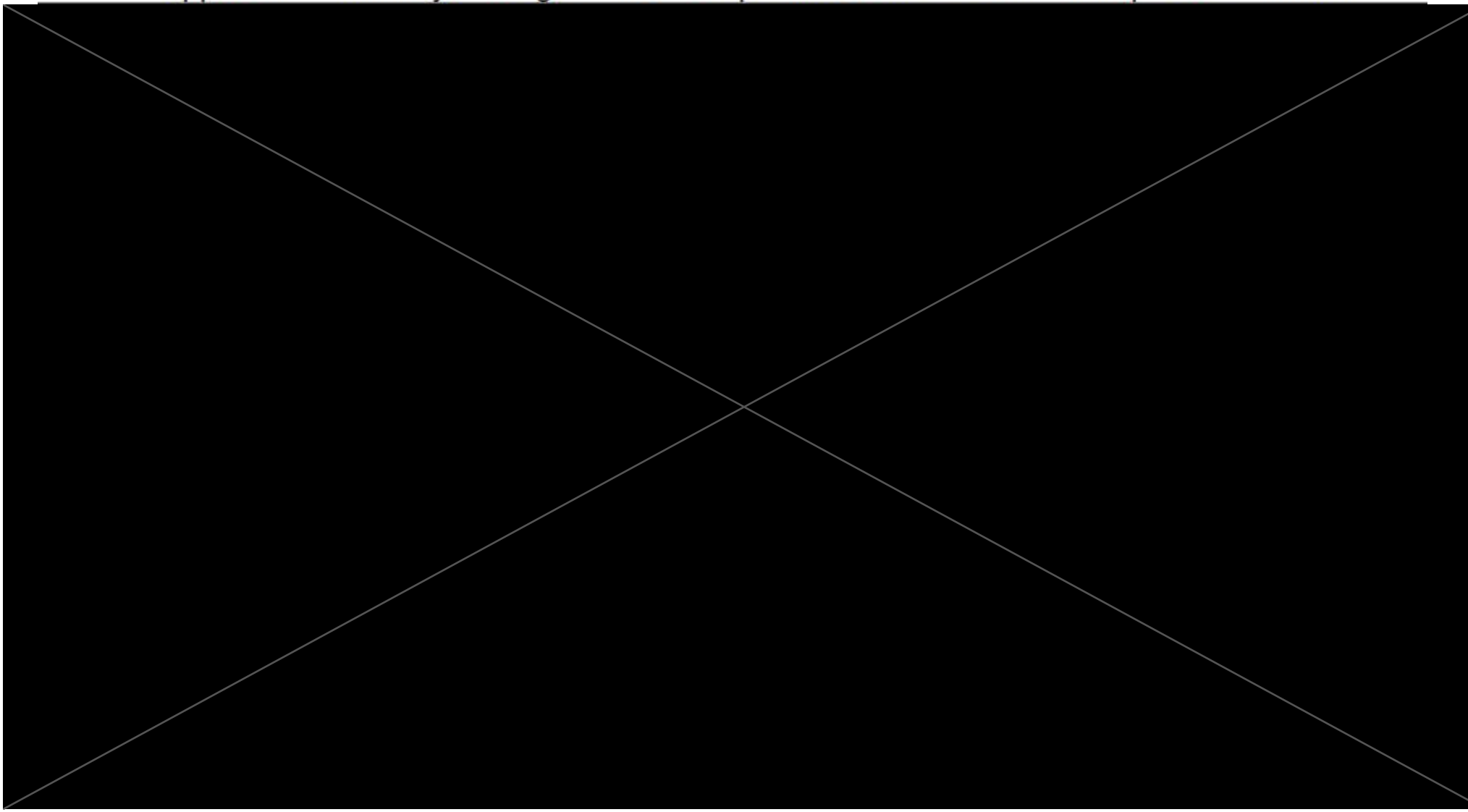
Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers



Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers



Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers



Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

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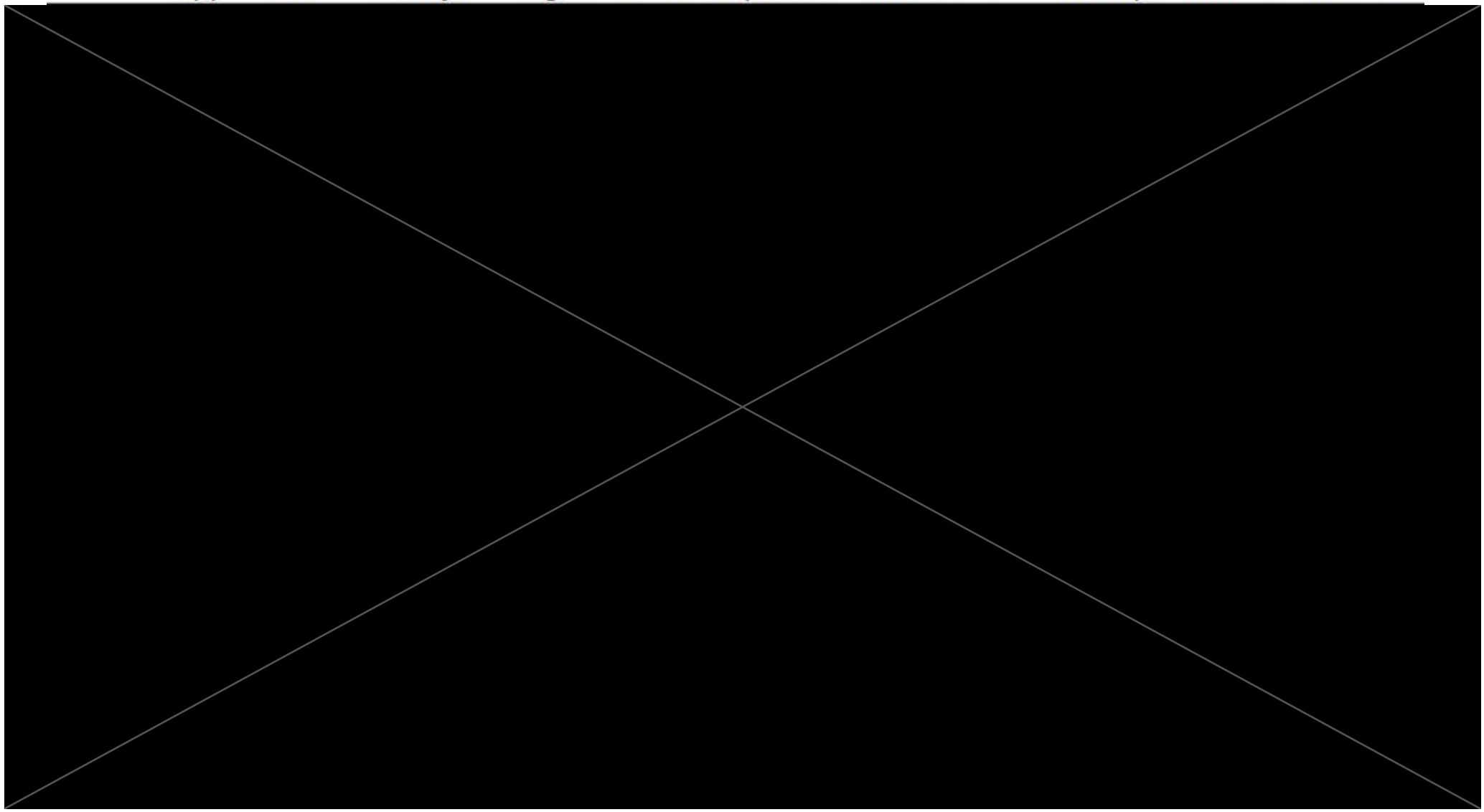
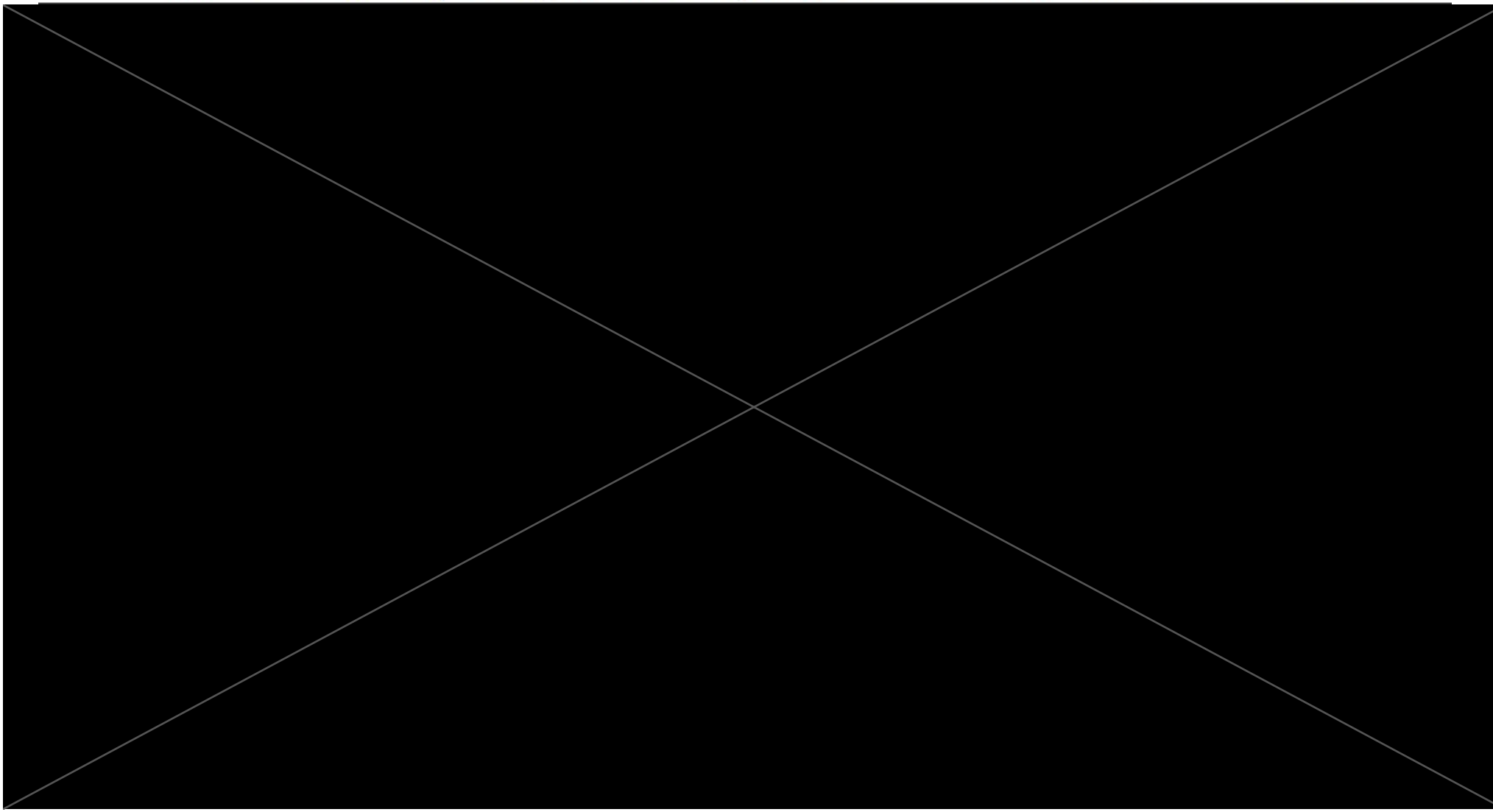
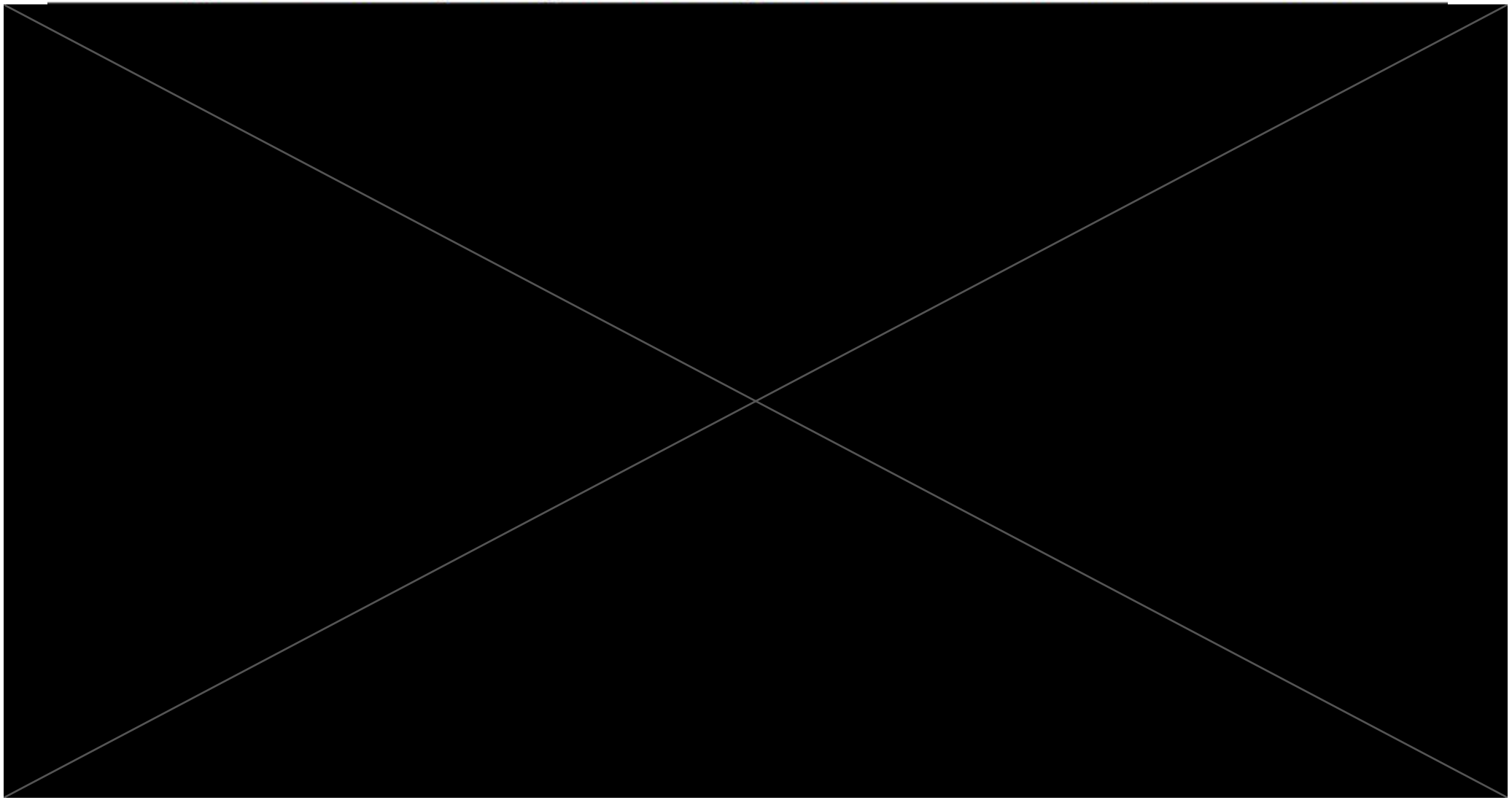


Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

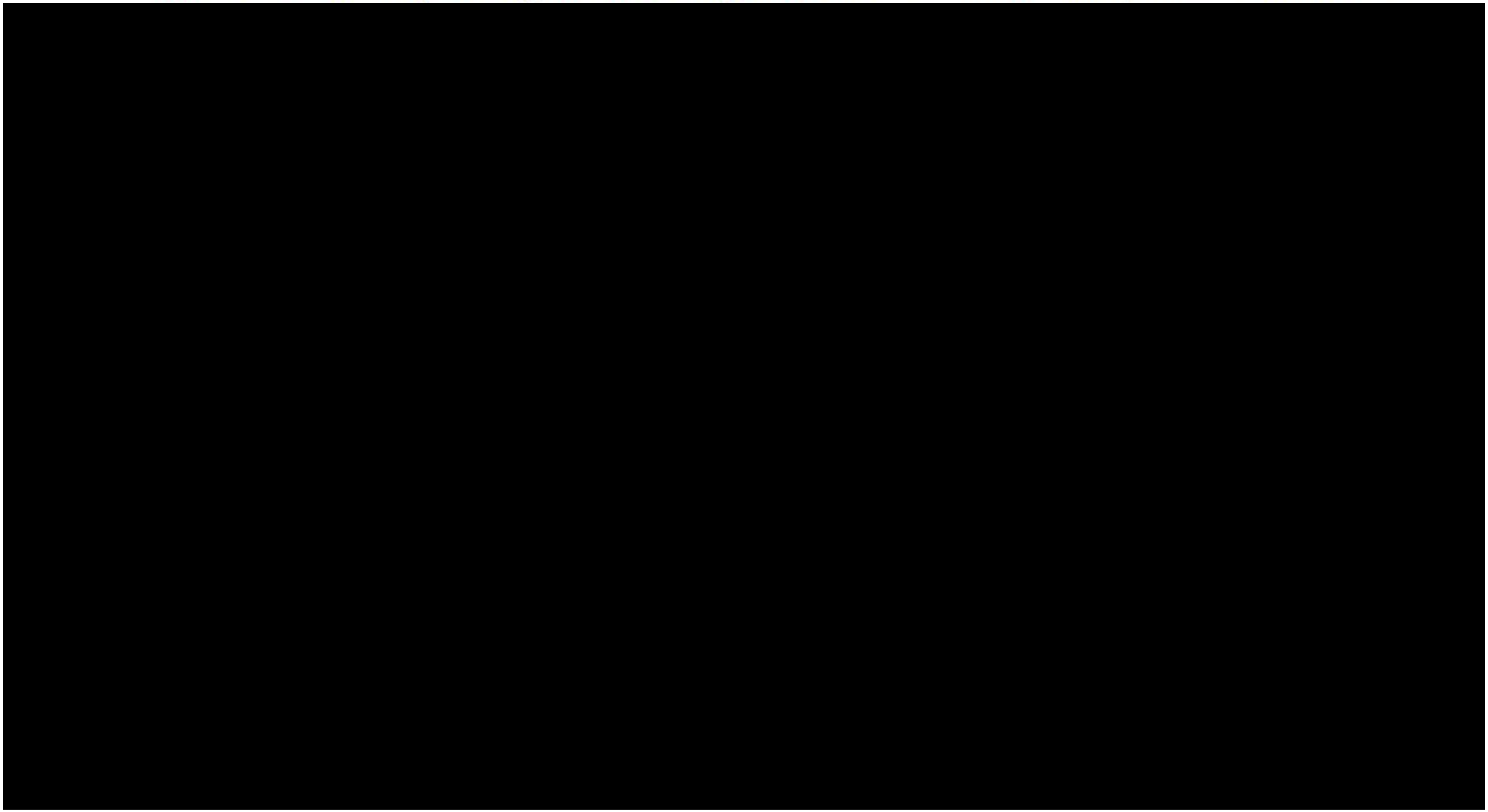


Appendix F. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers



Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

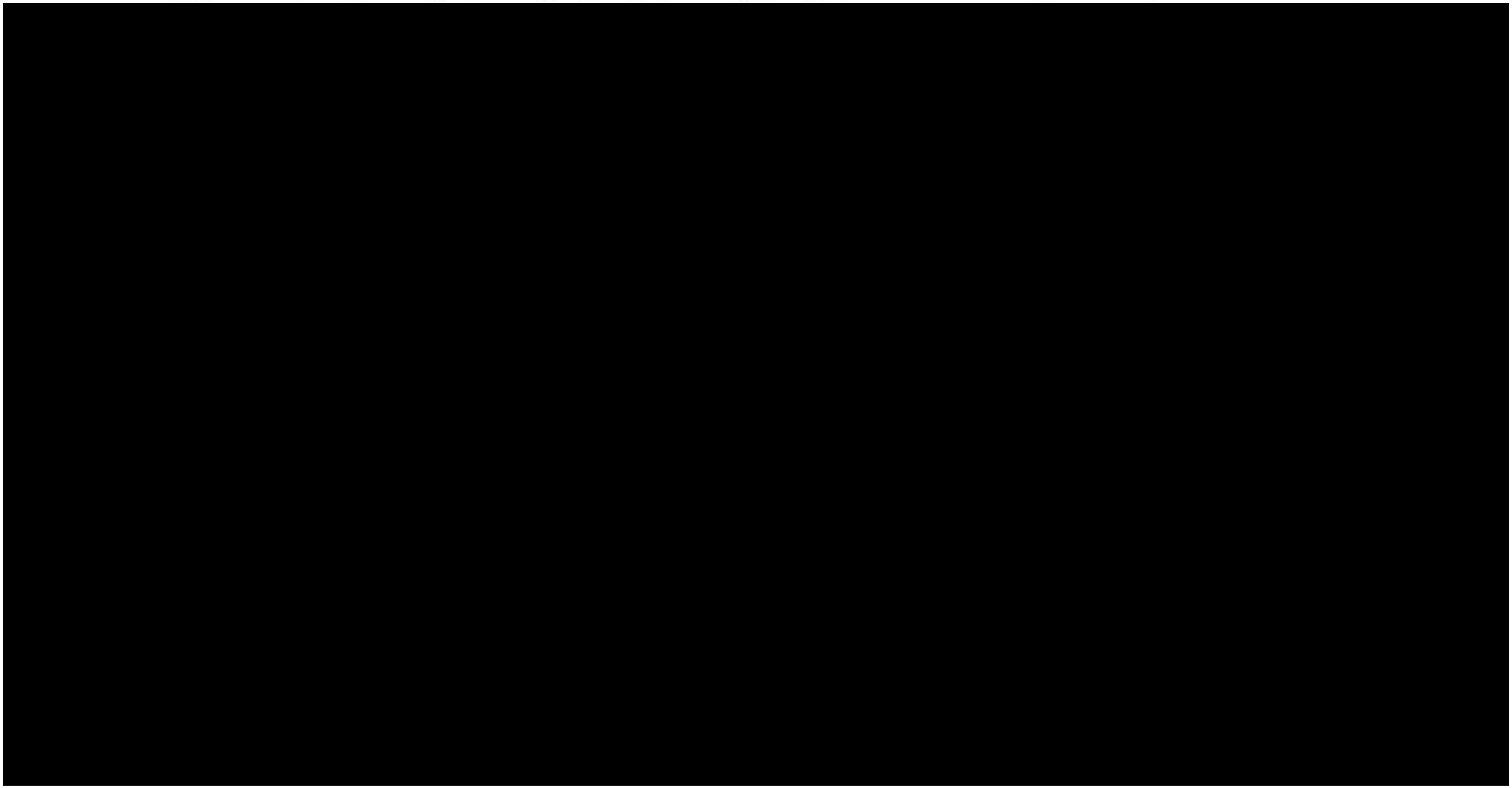


Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

[redacted]

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers



Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

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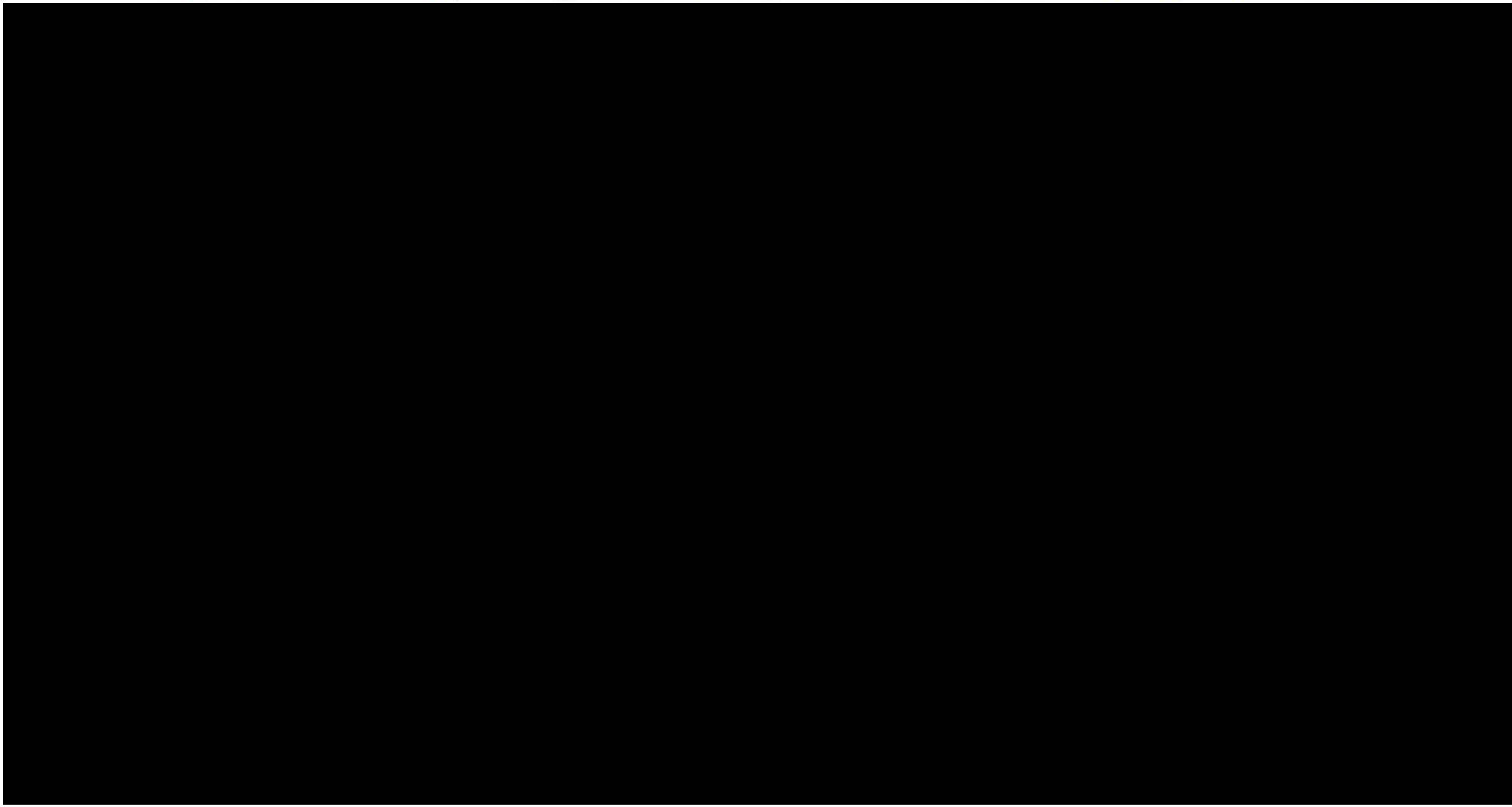
Appendix F. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers



Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

[redacted]

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

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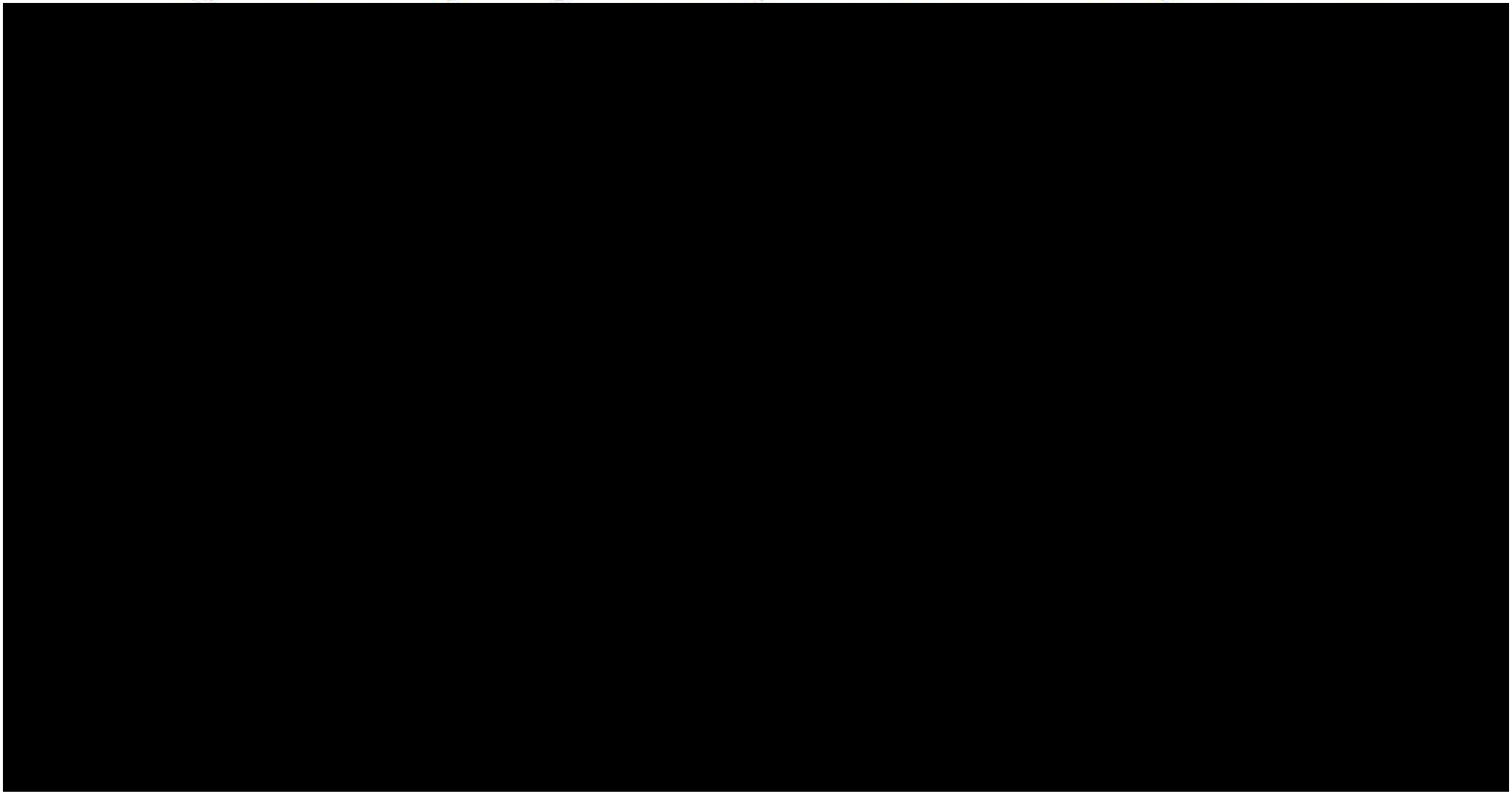
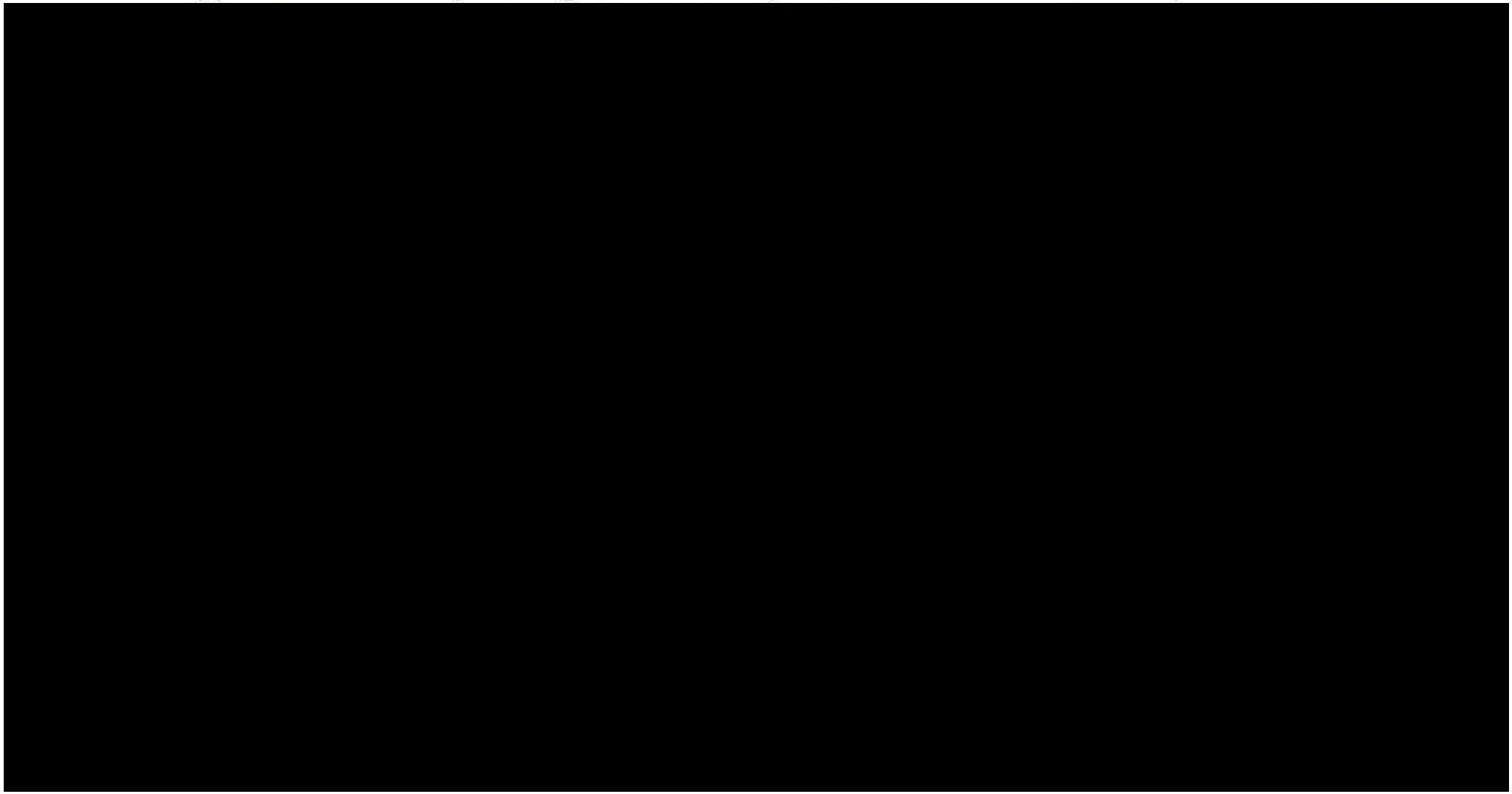


Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers



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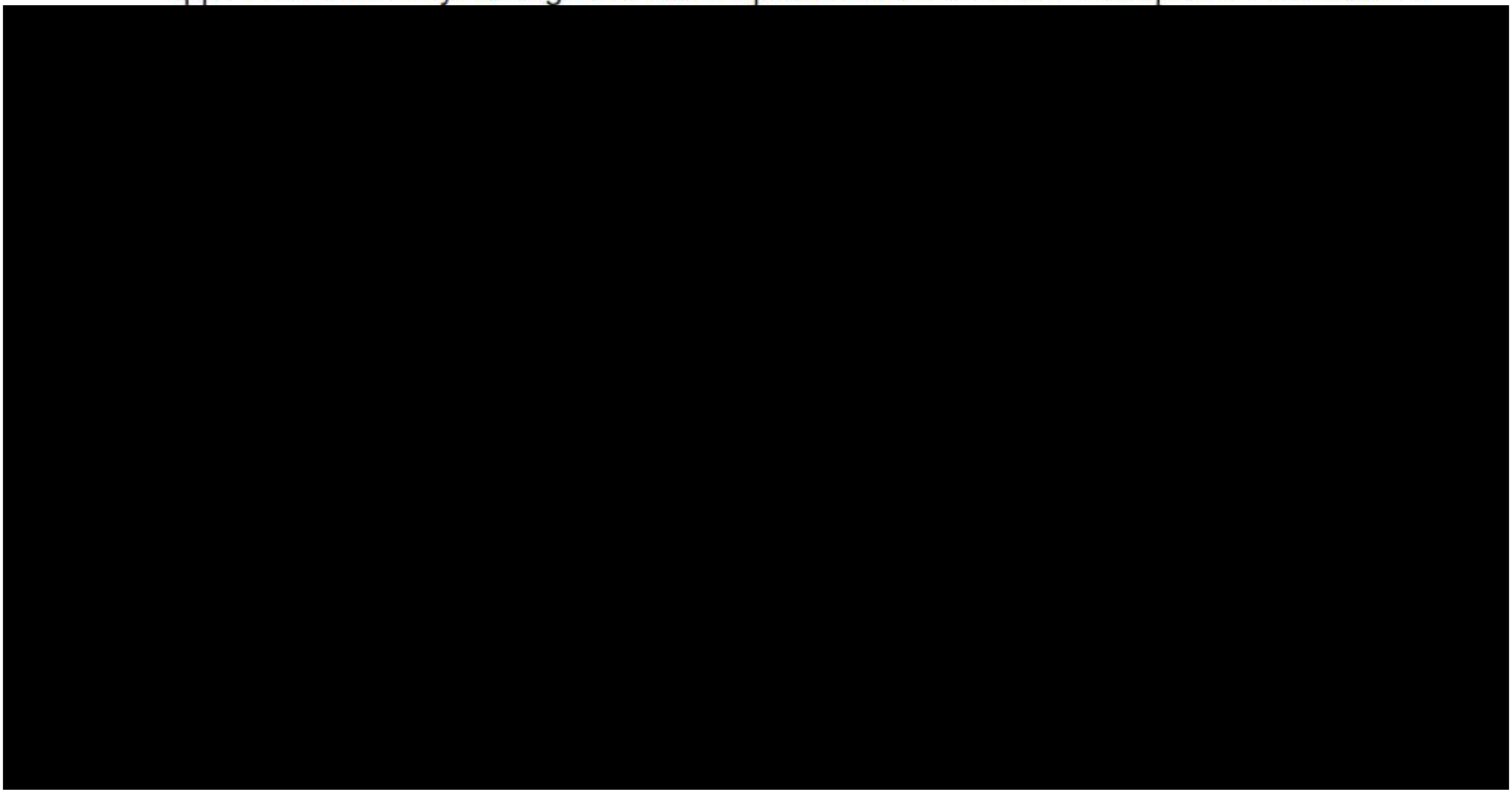

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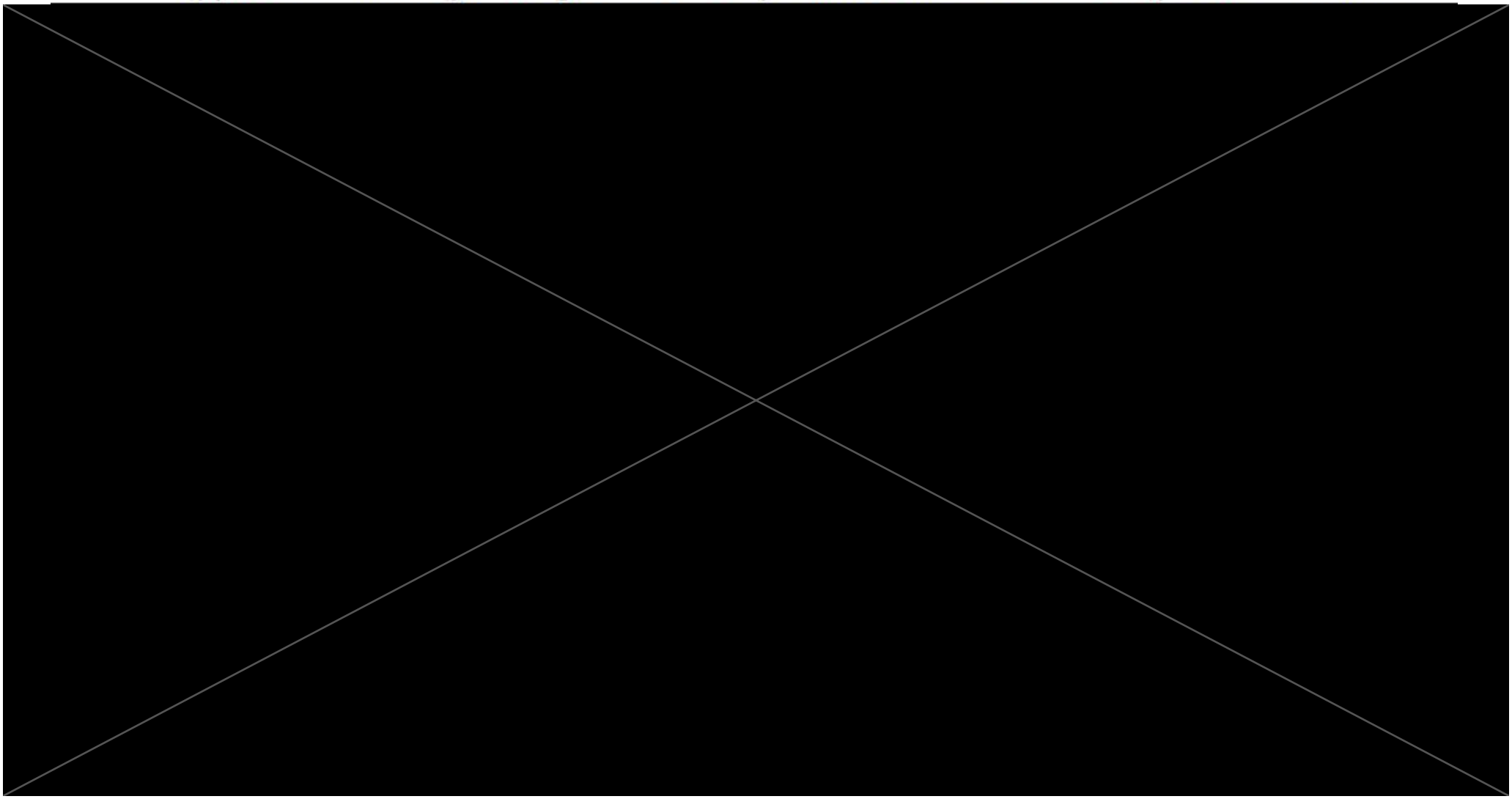

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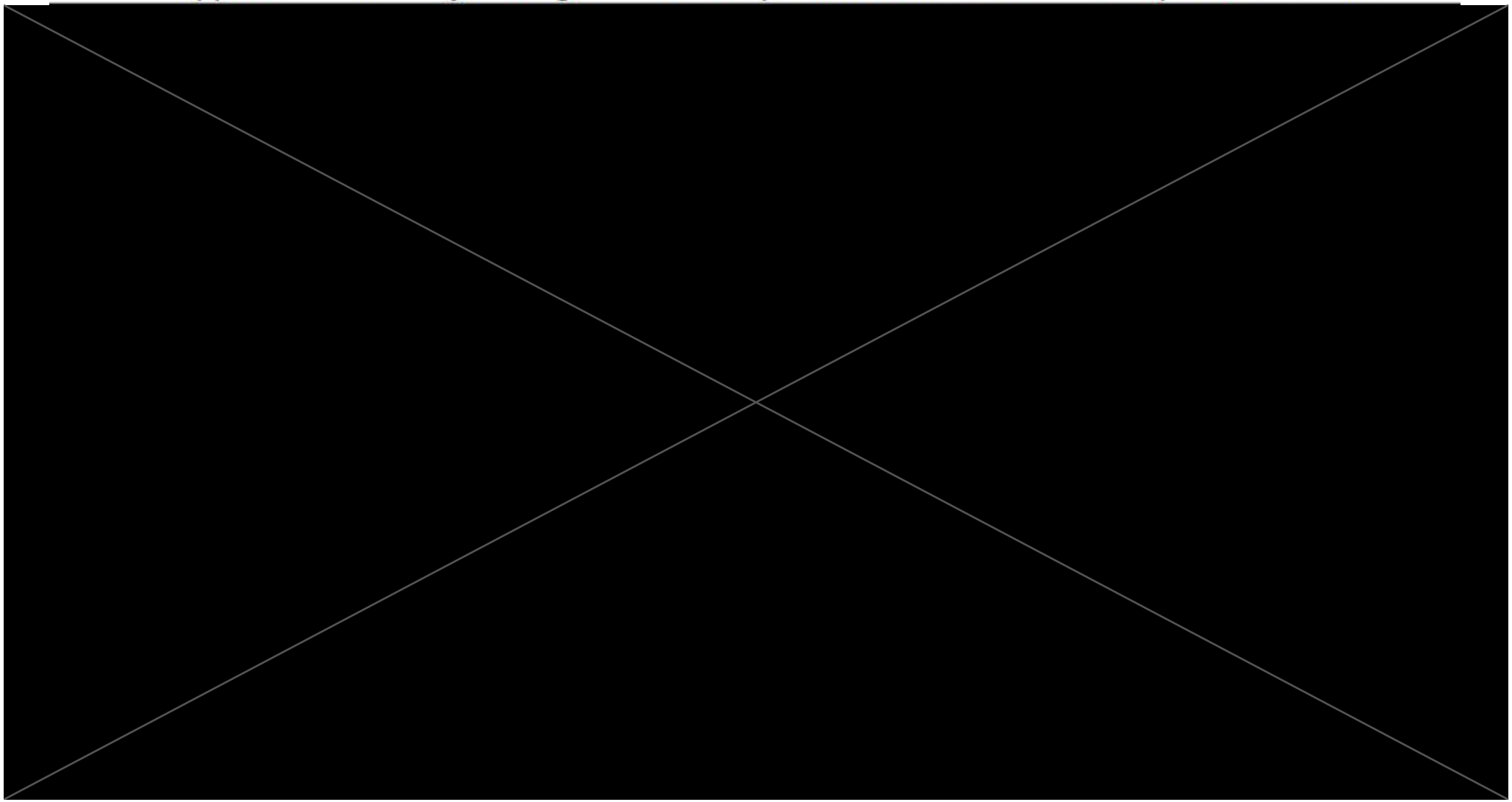

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

[redacted]

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

[redacted]

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

[redacted]

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Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

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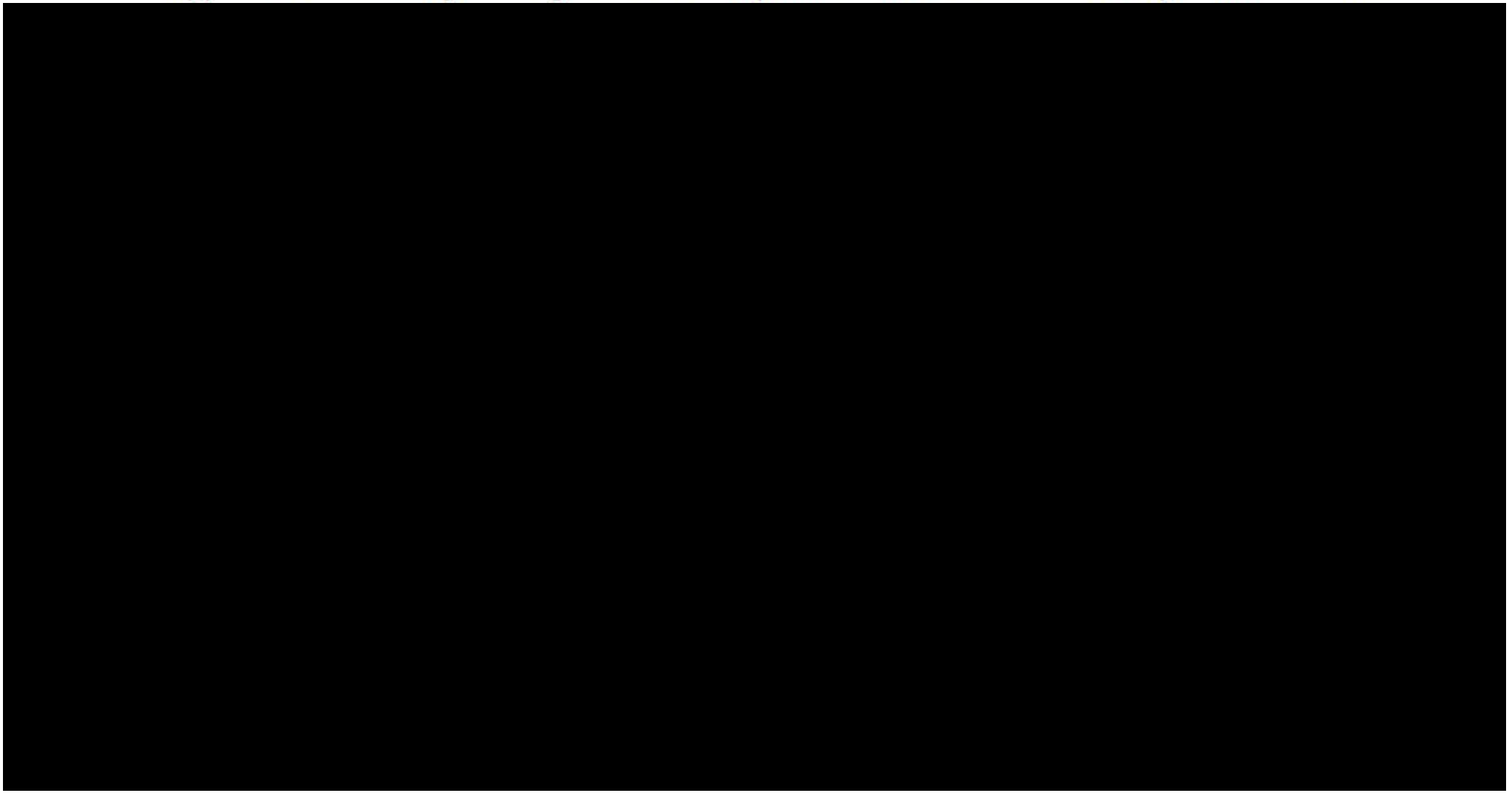

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

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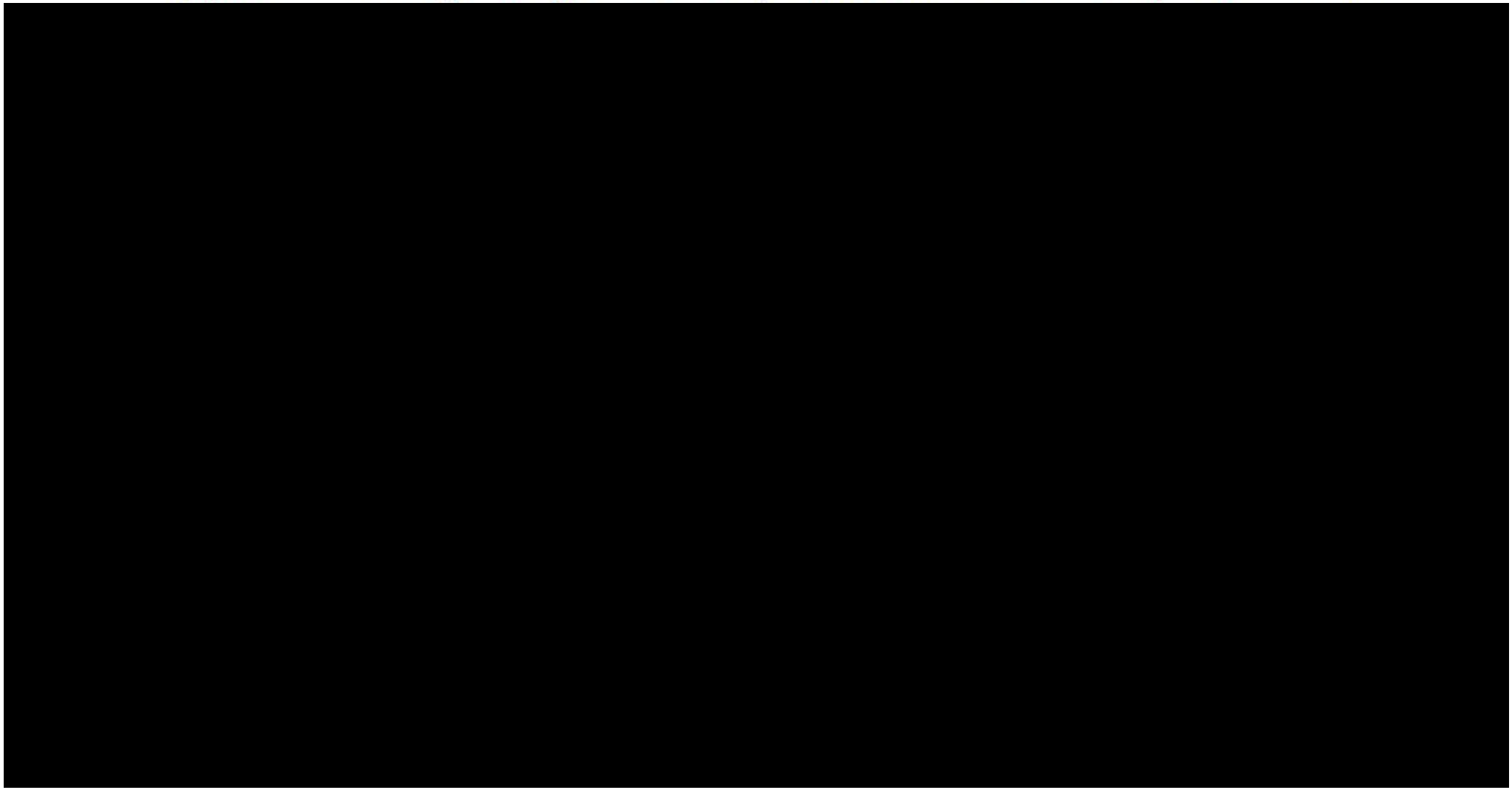

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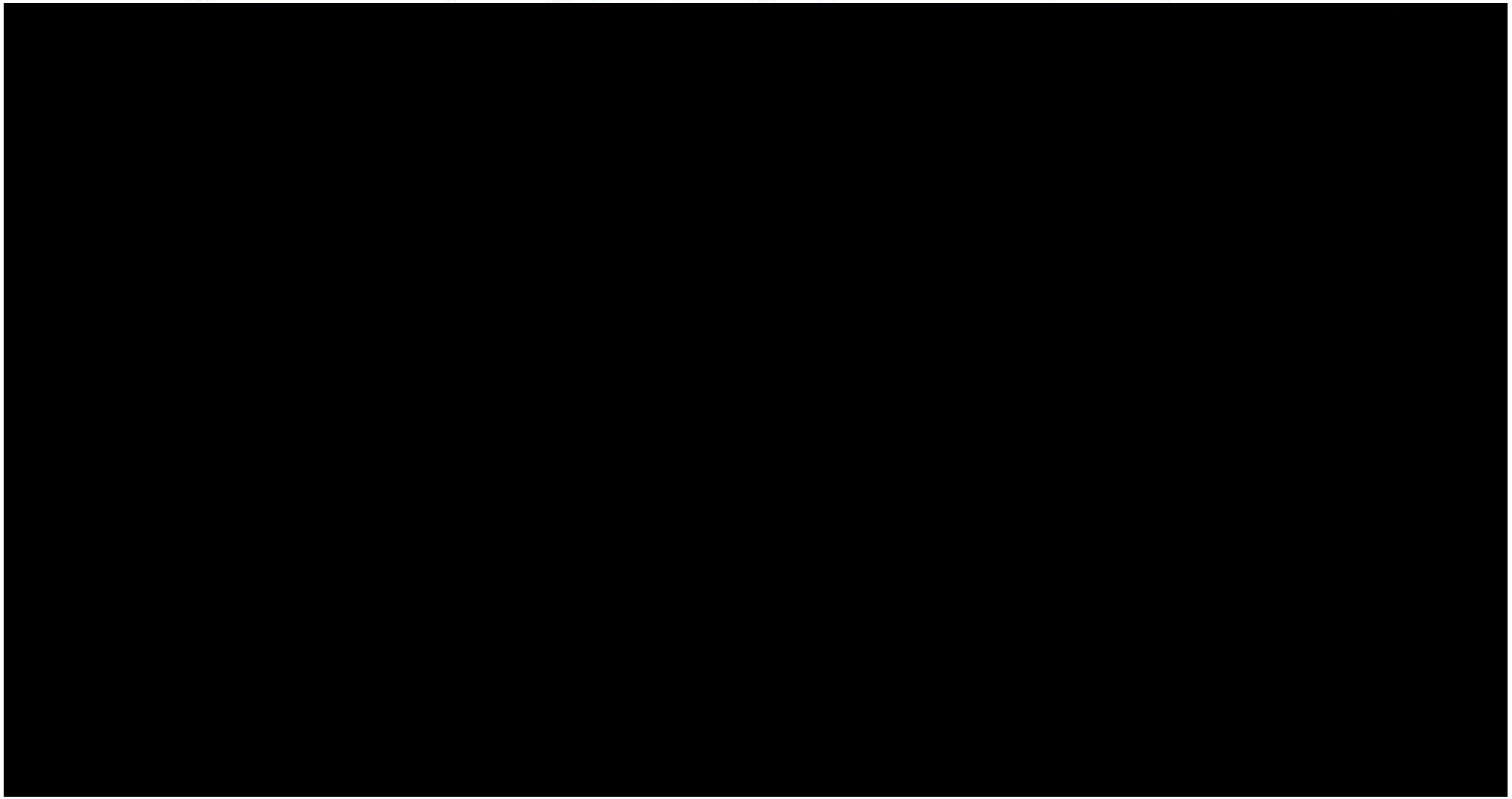

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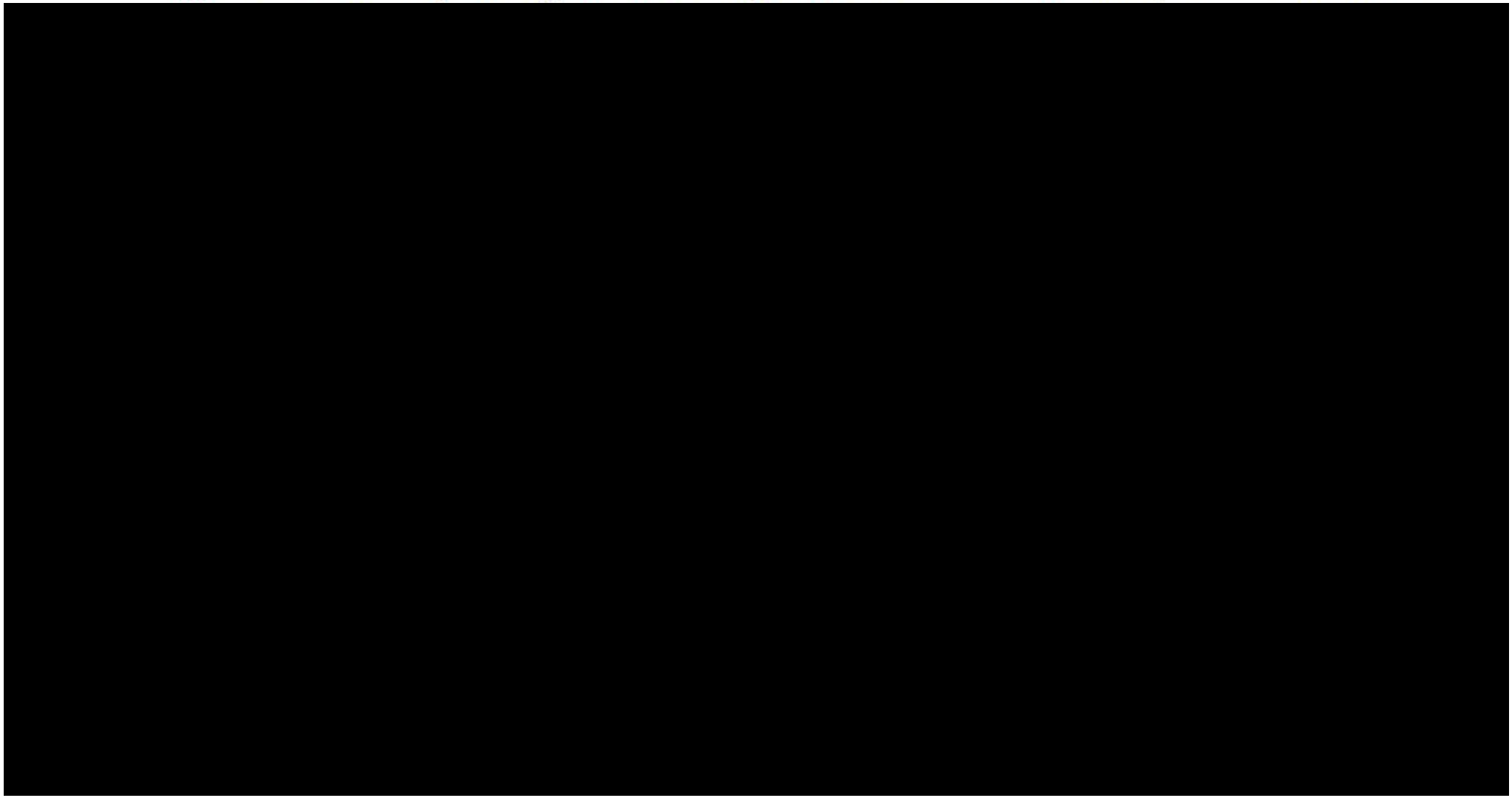

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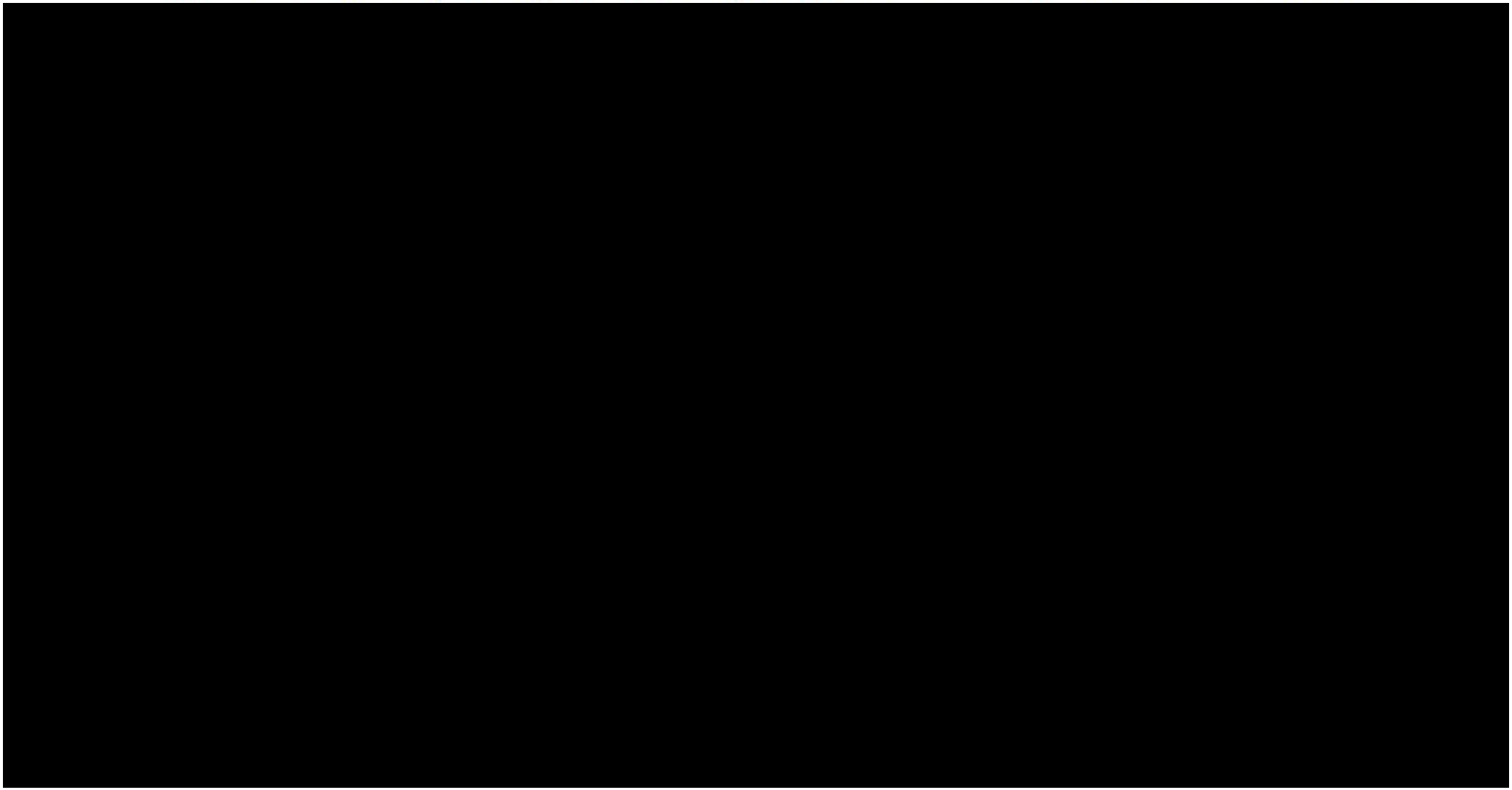


Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

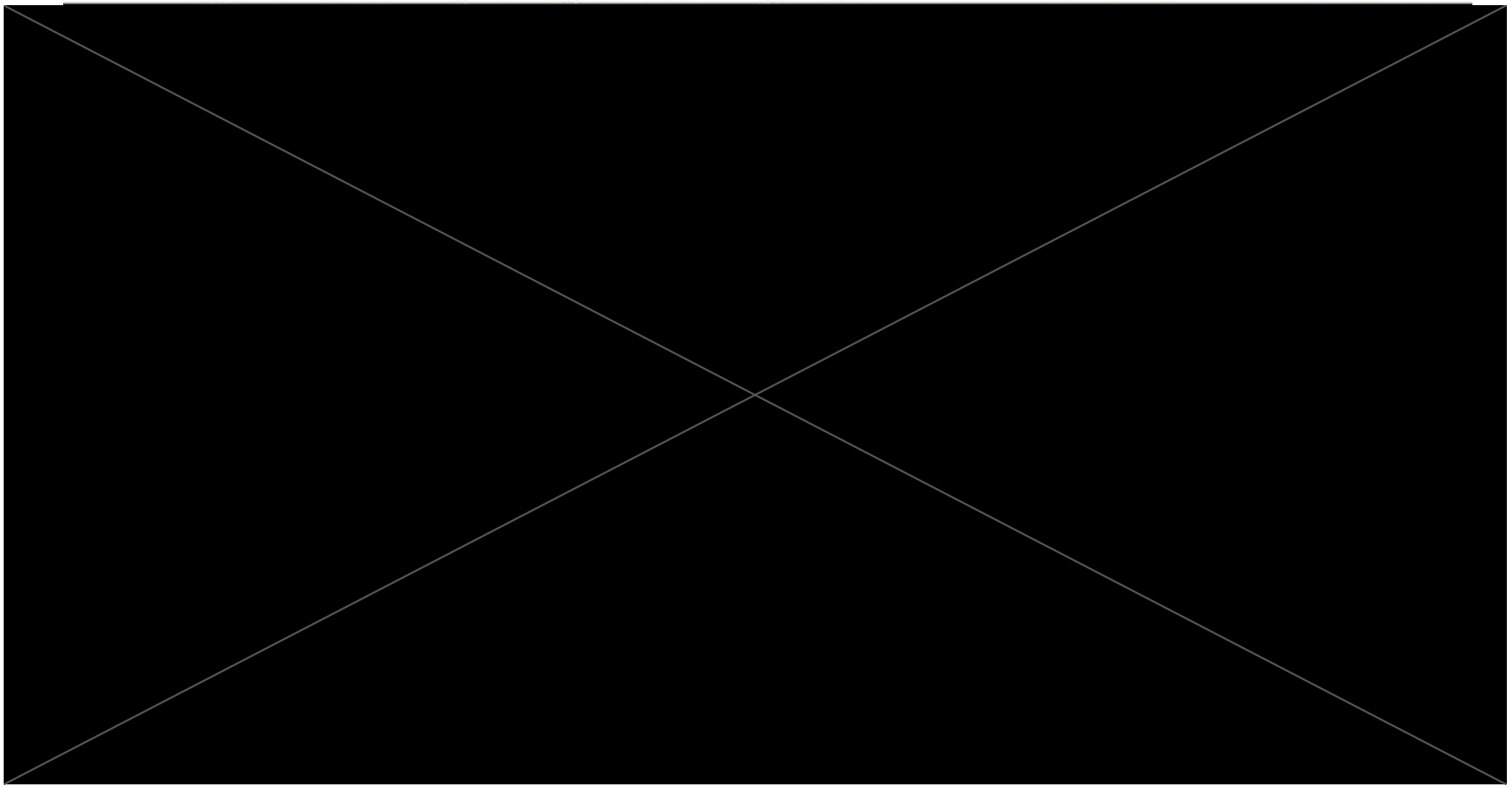
Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

[redacted]

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

[redacted]

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers



Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

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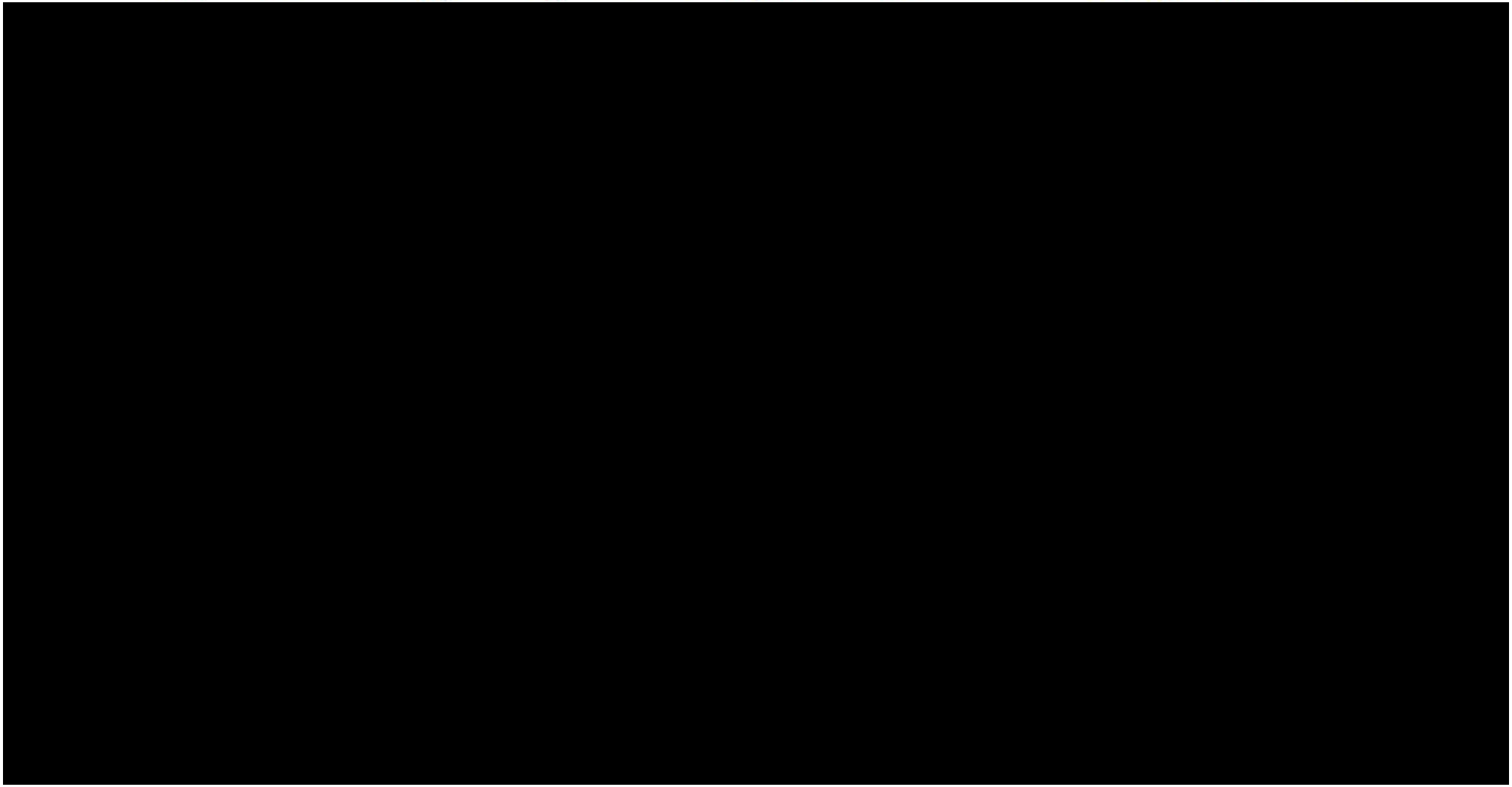

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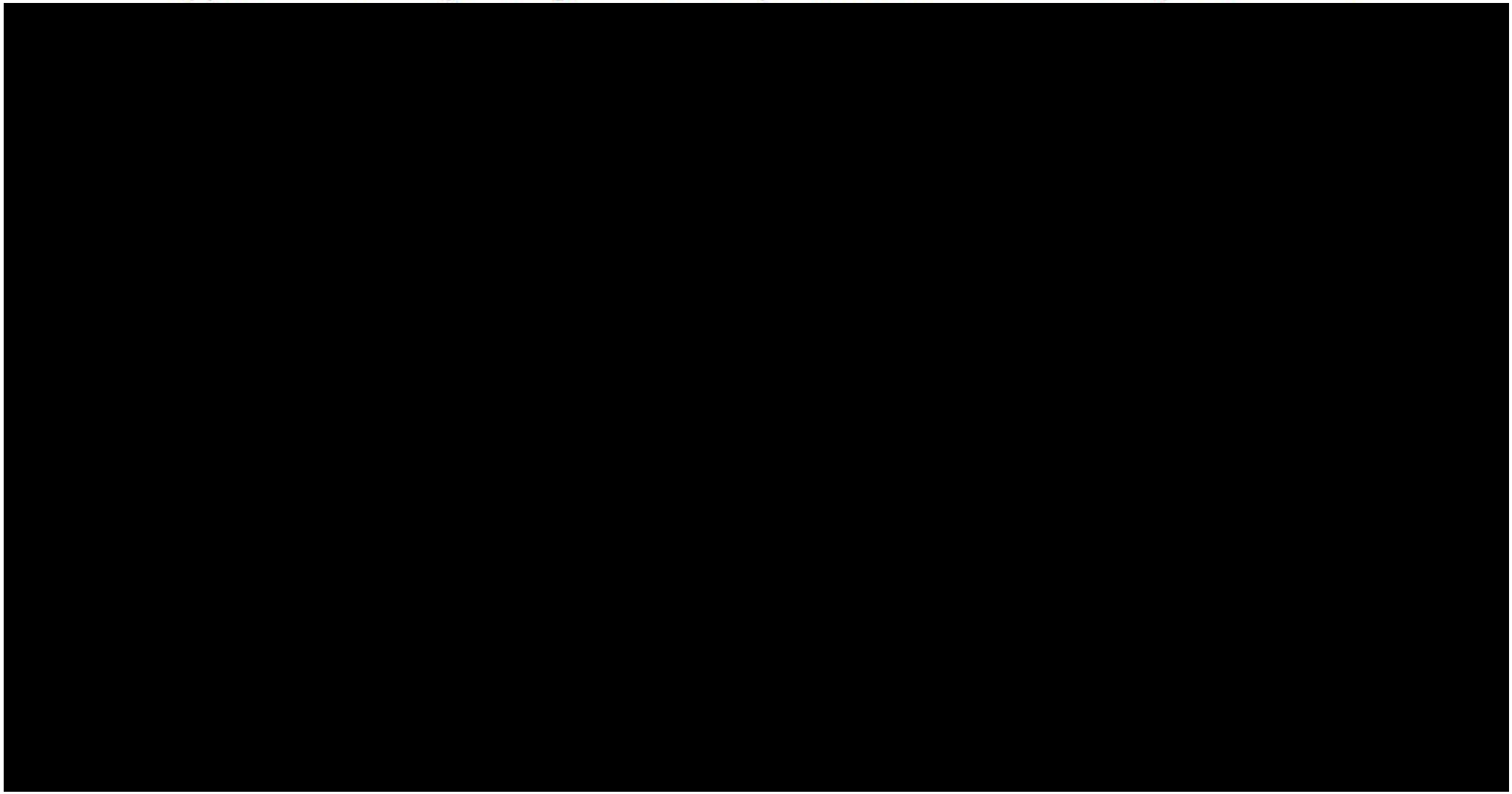

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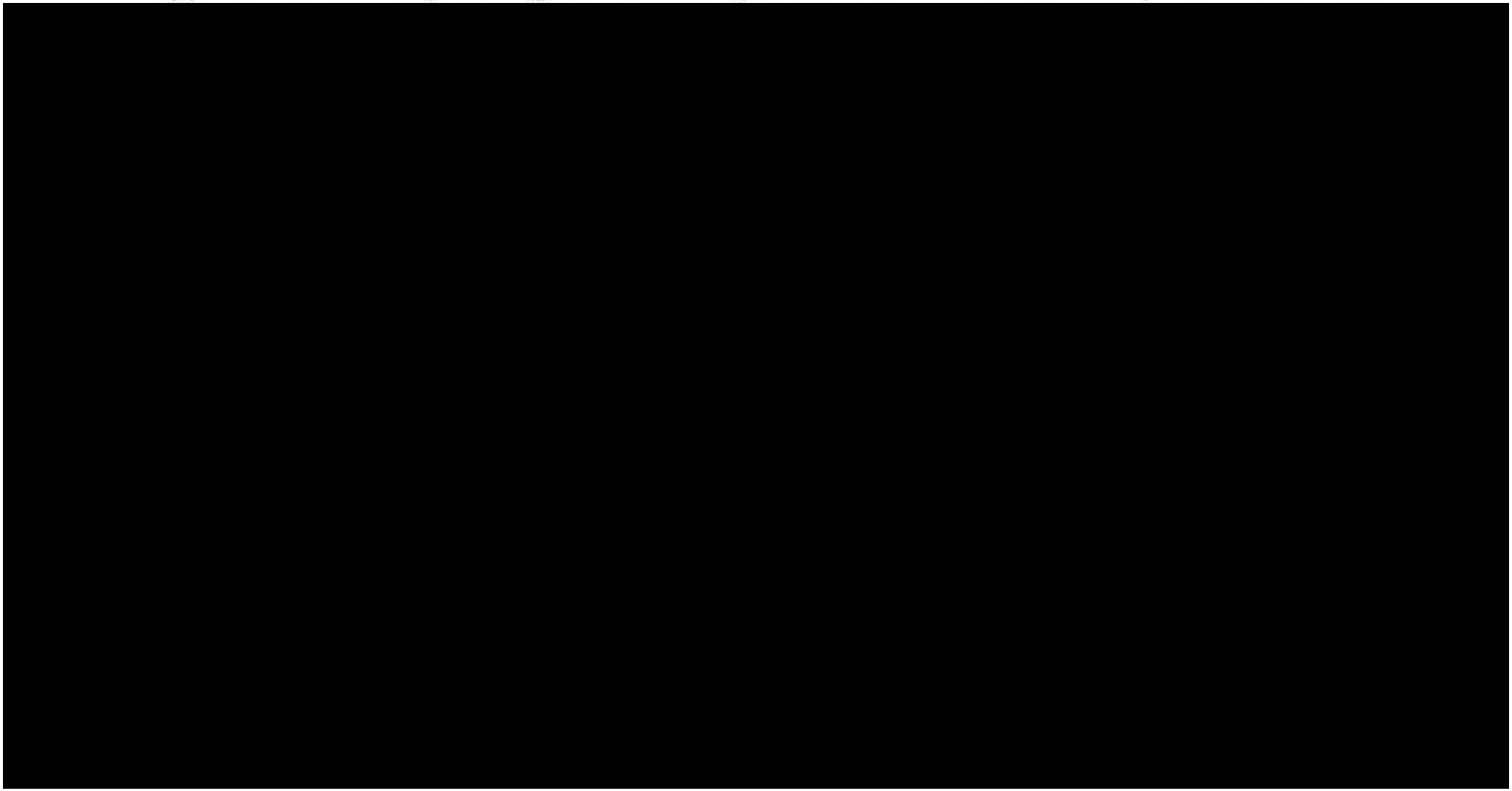

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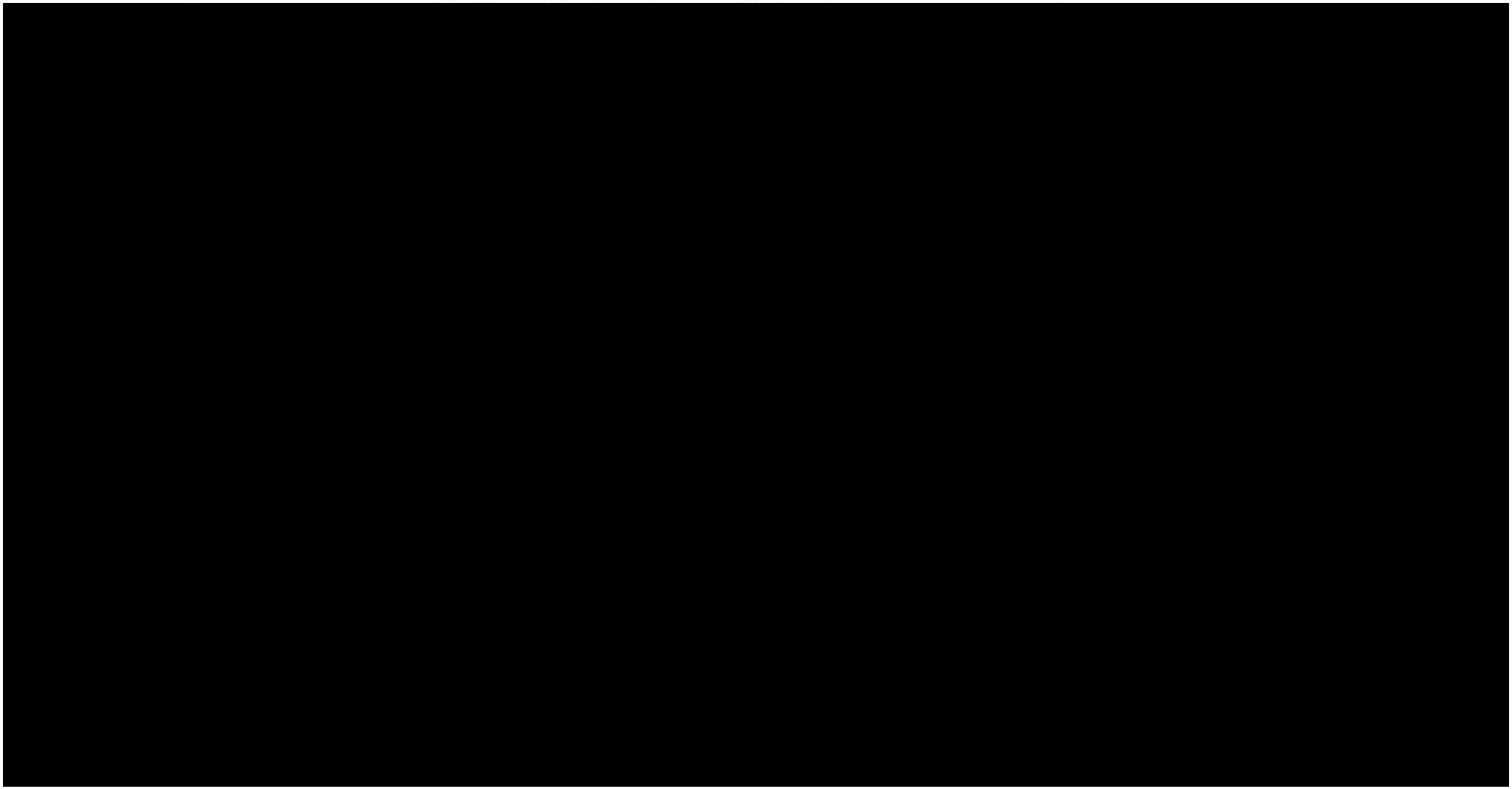

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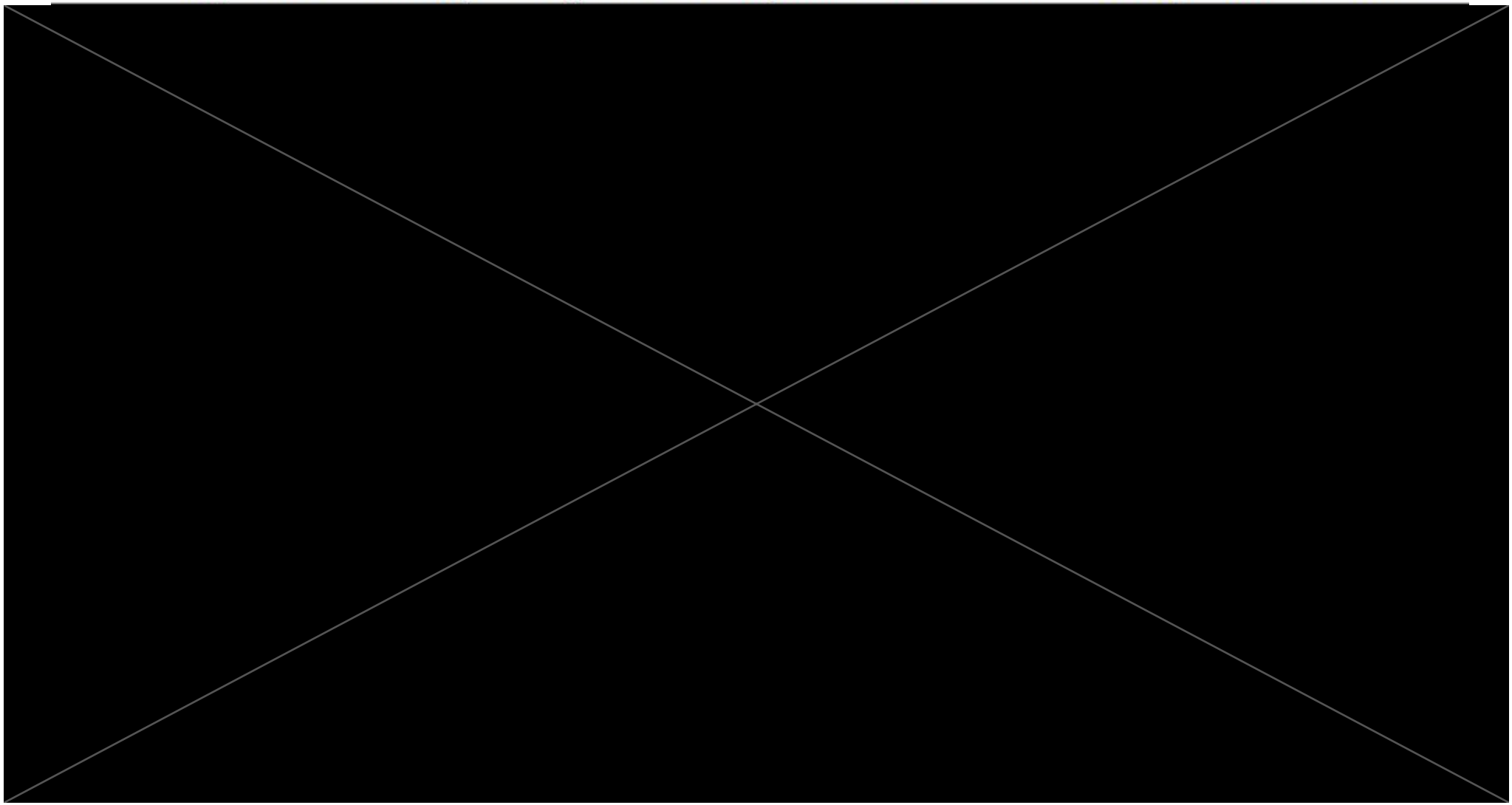

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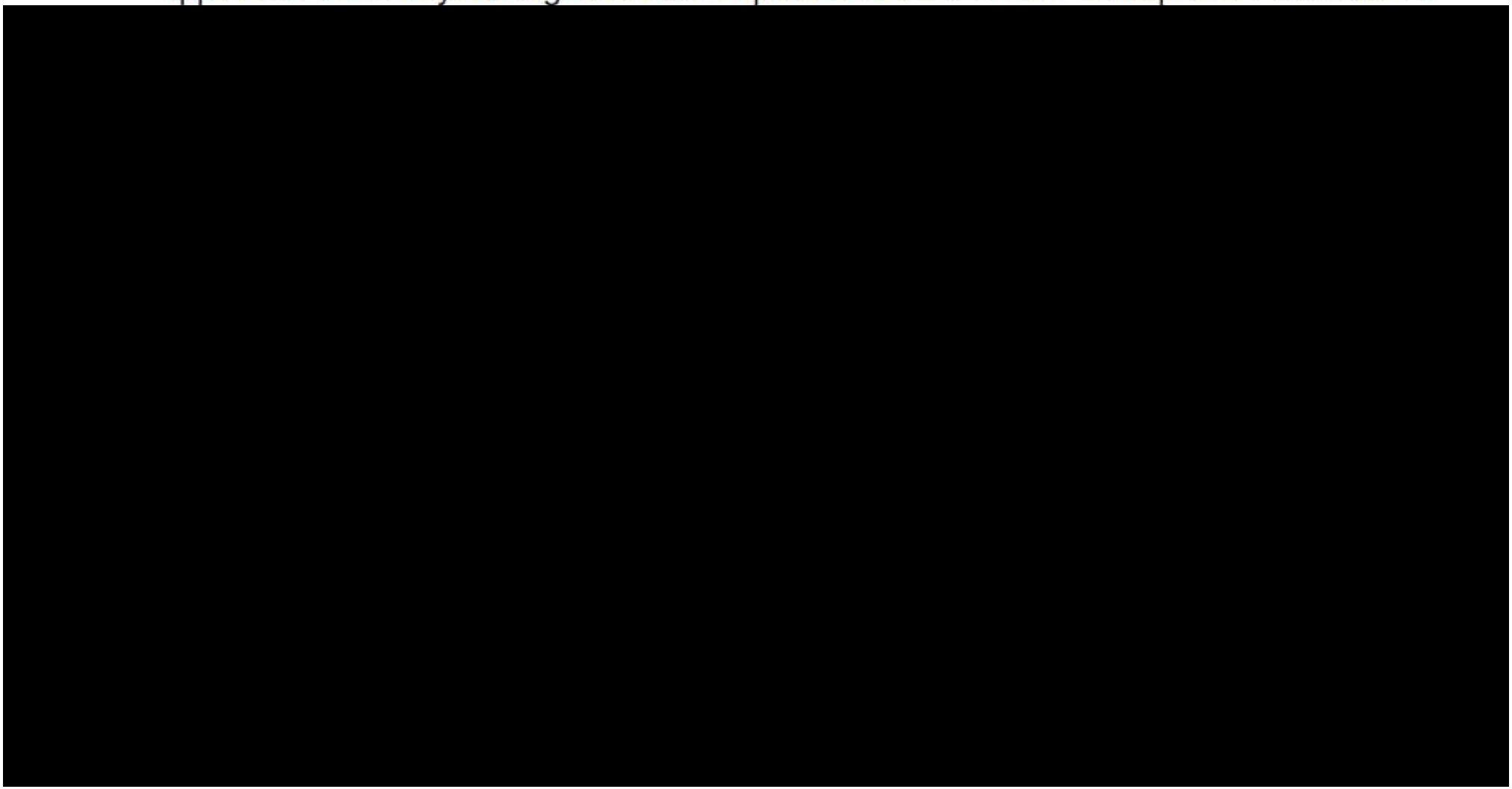

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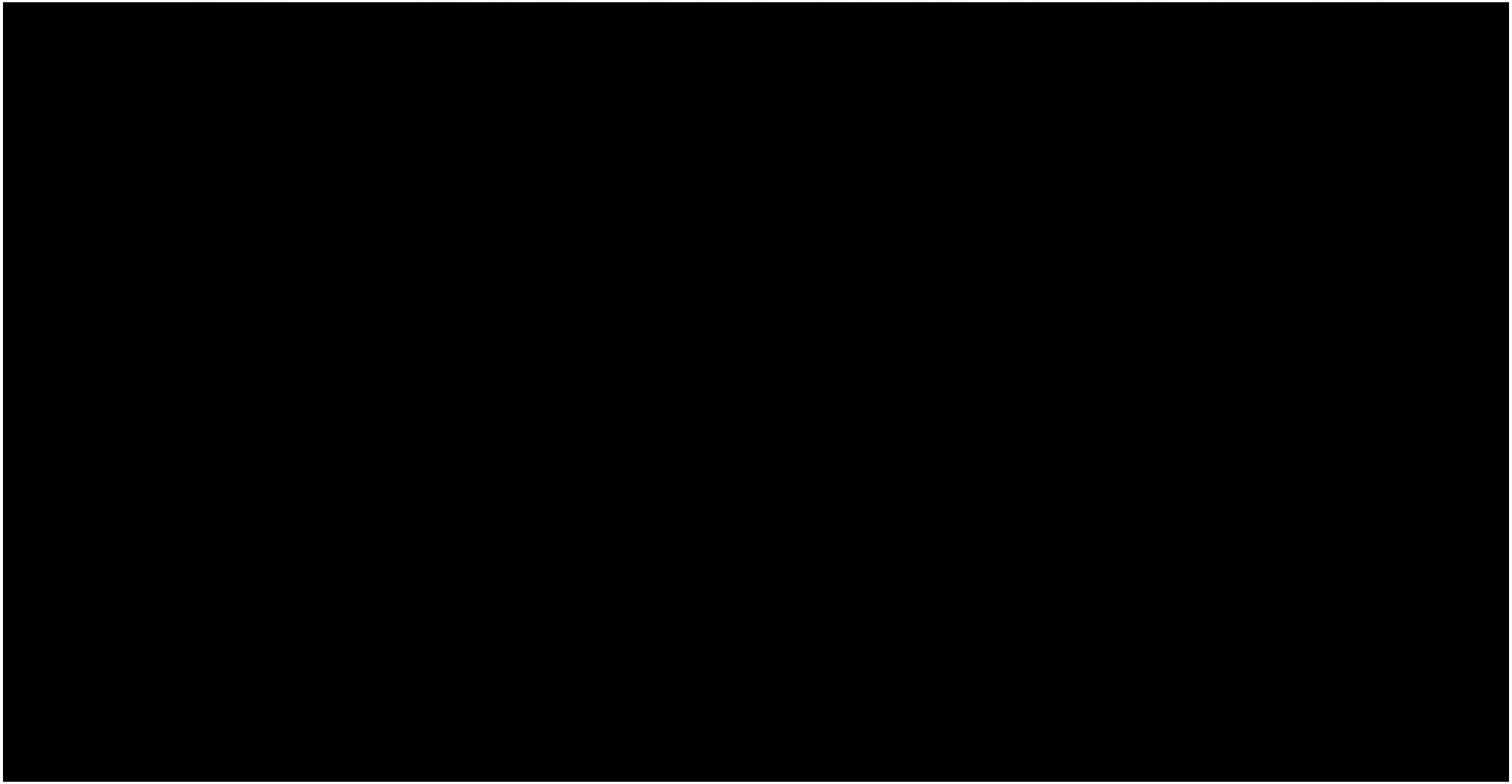

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

Appendix F. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

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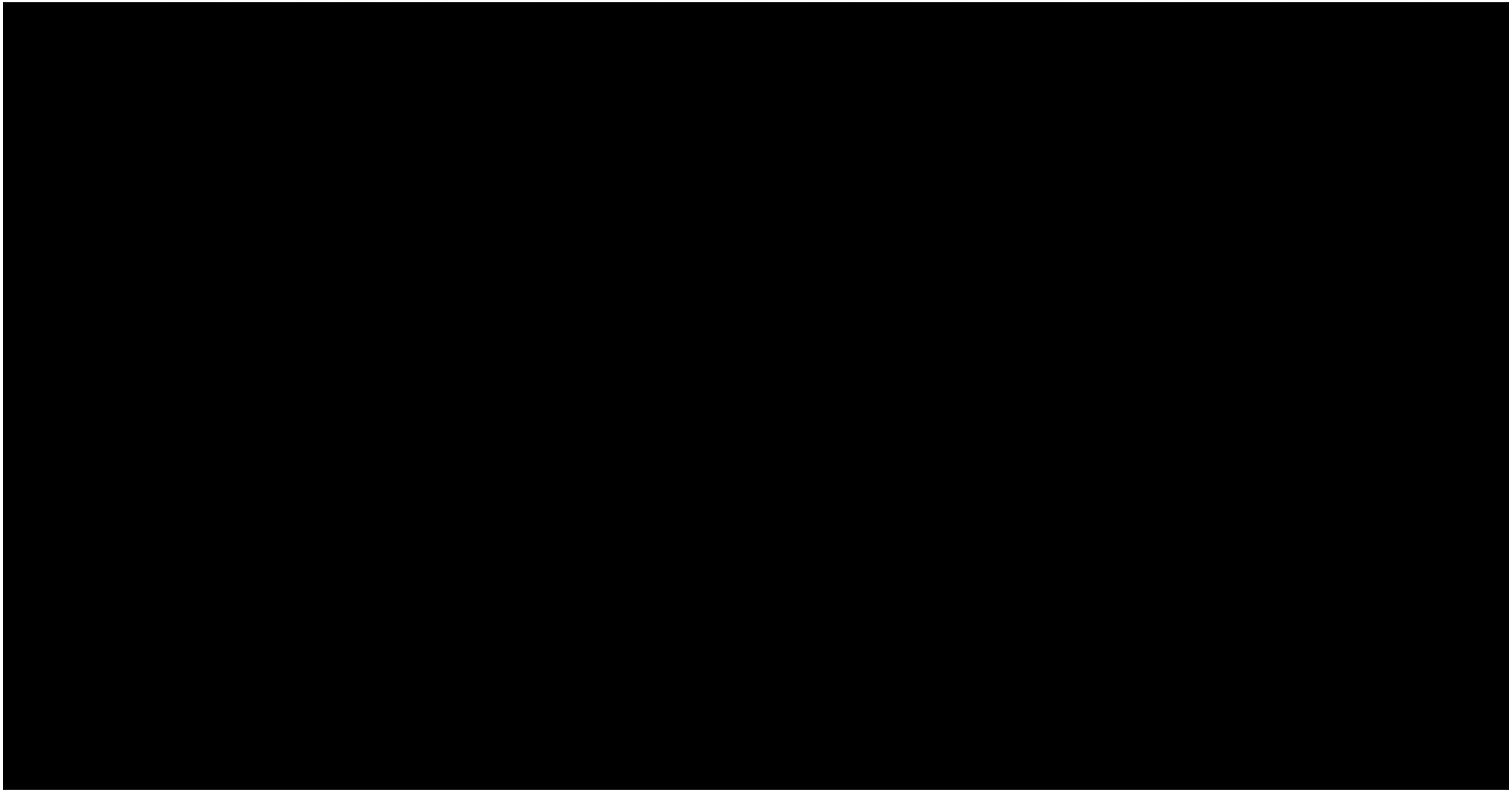

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

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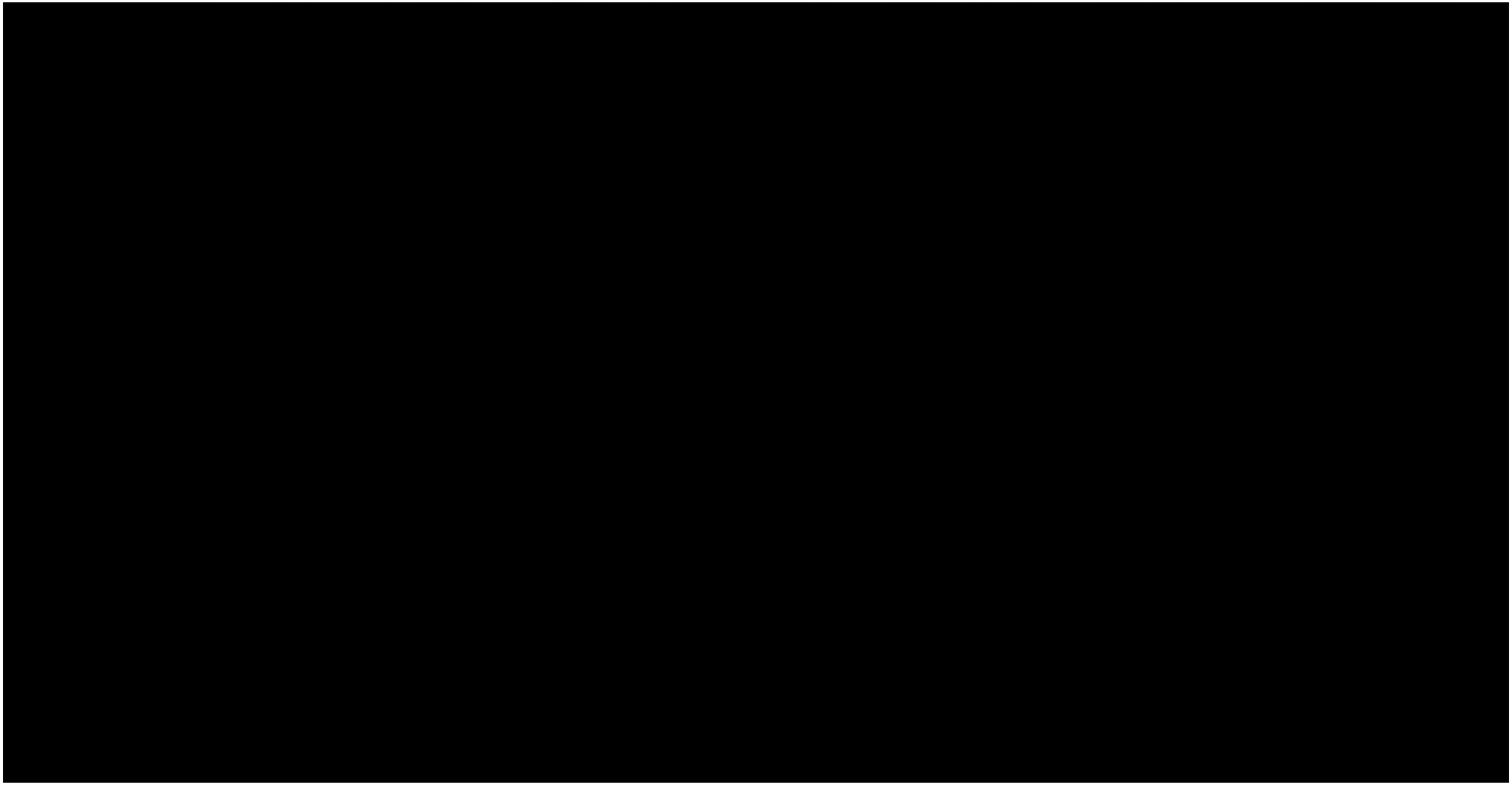

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

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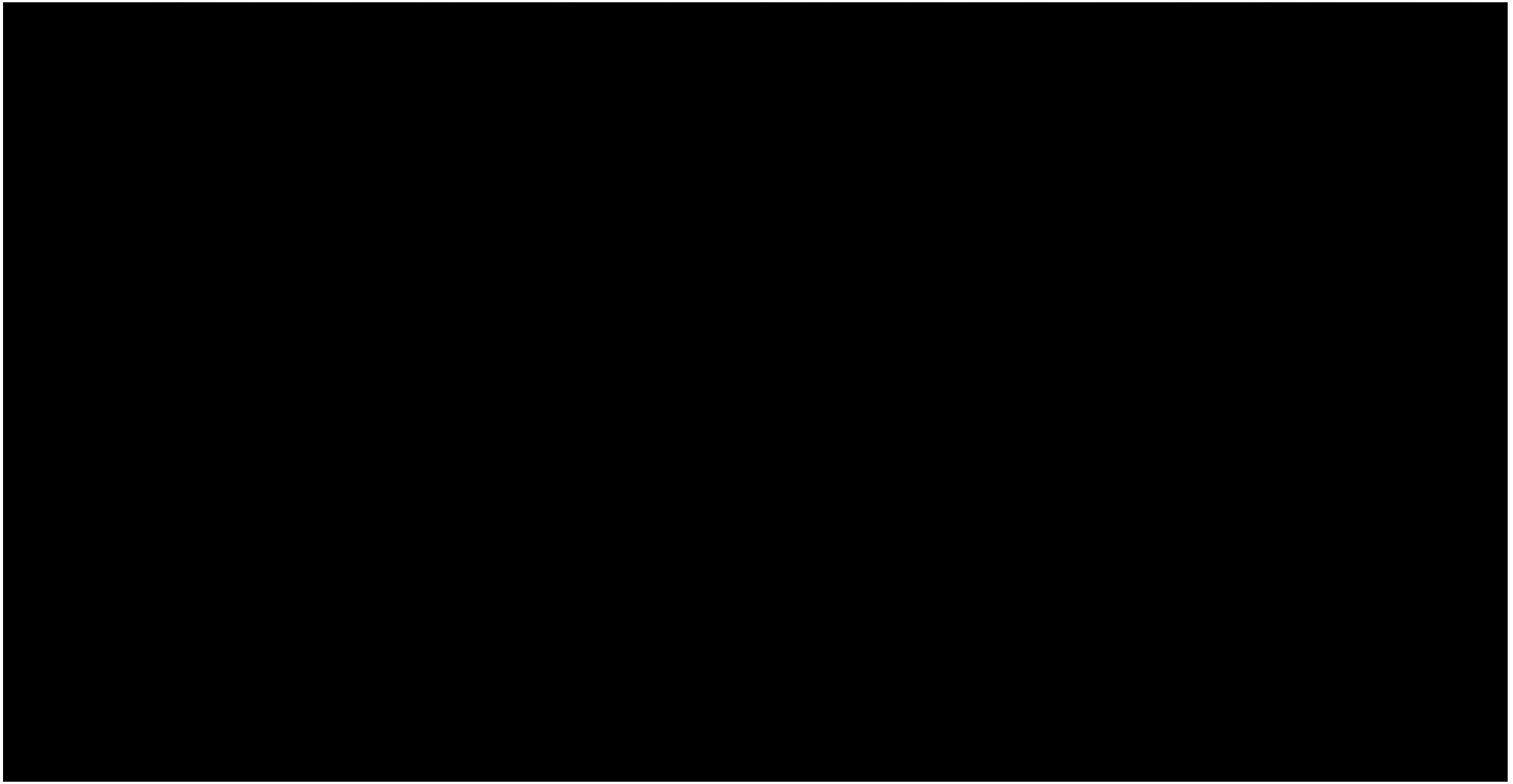

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

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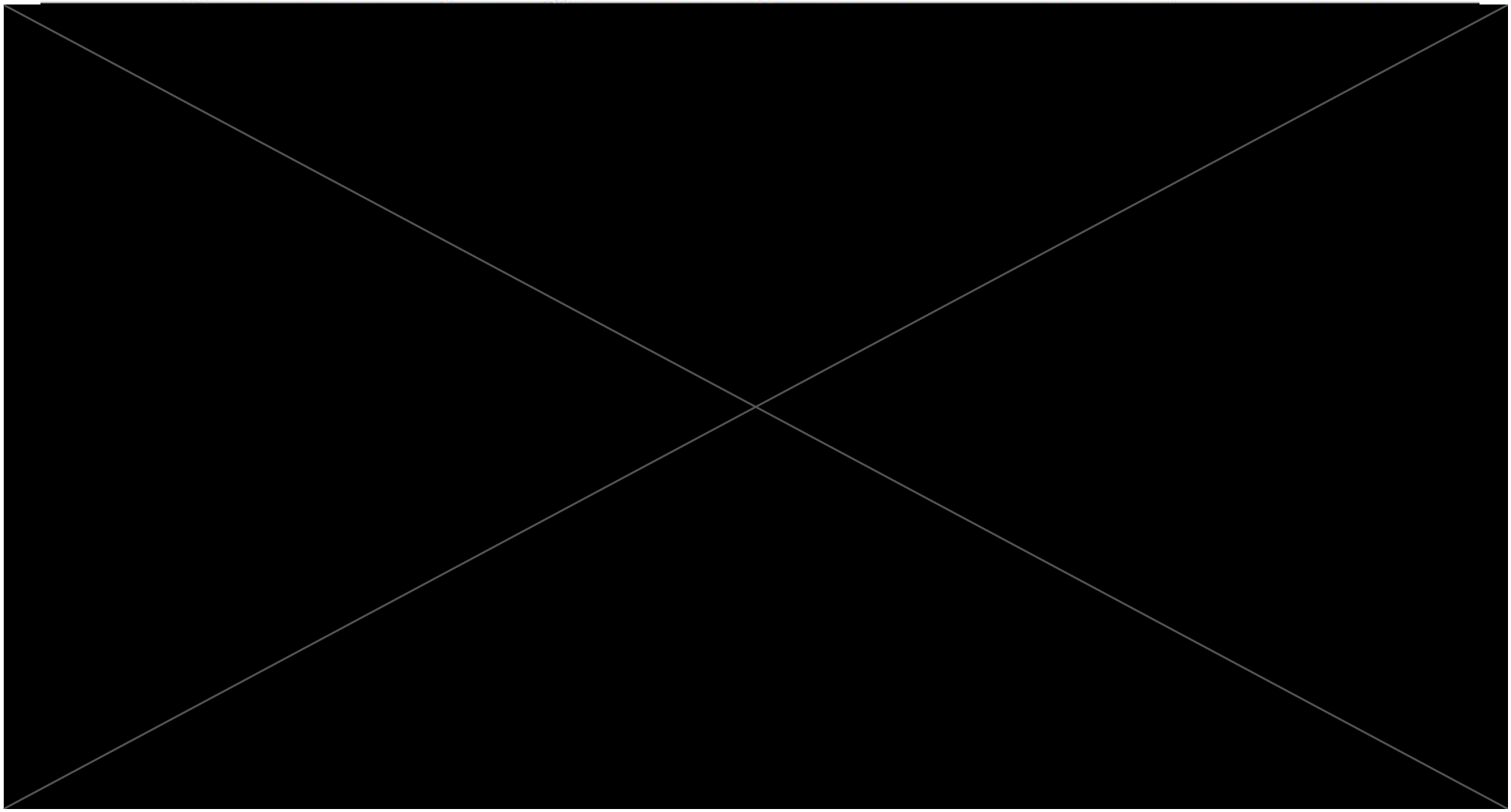

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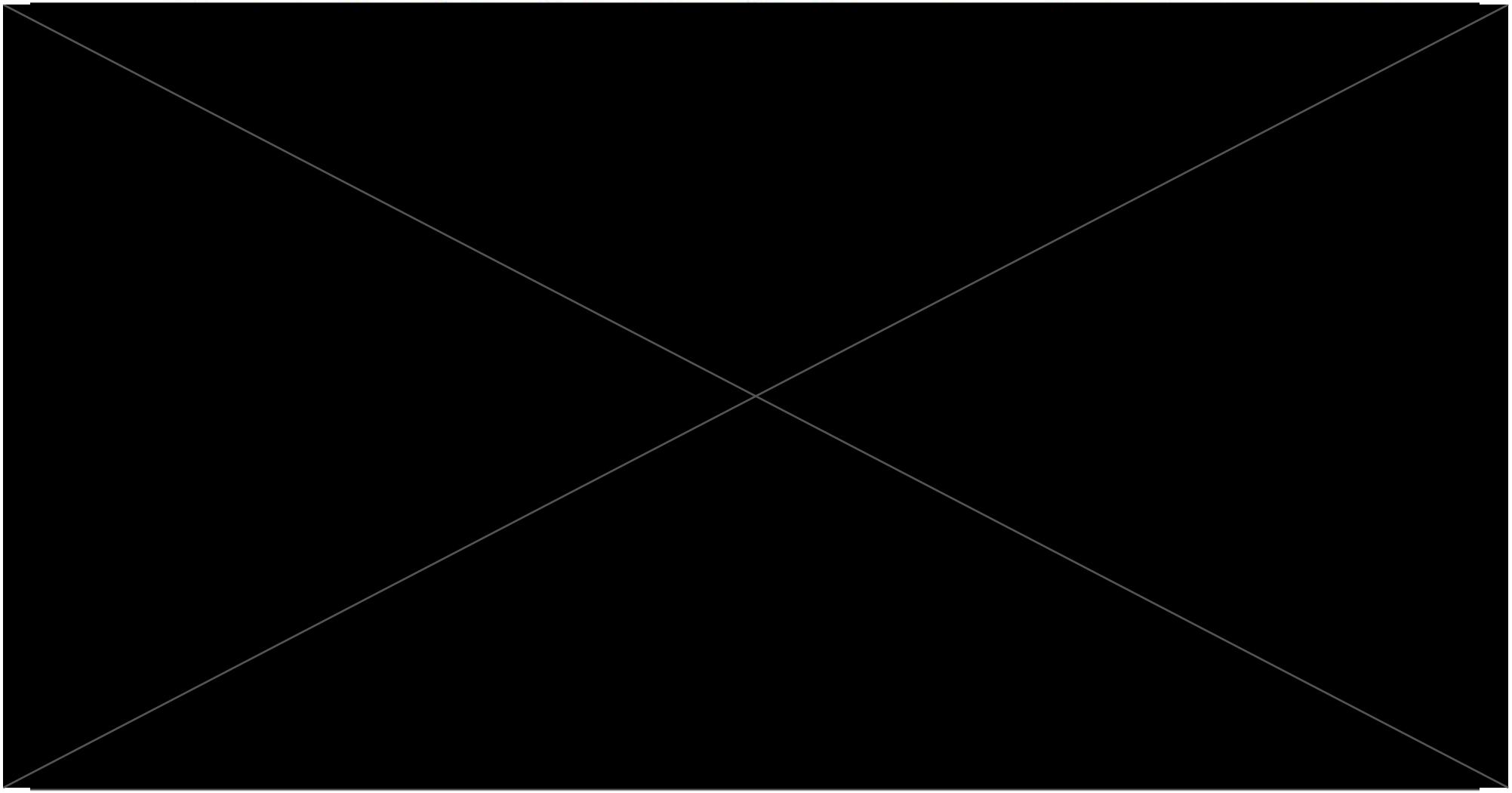

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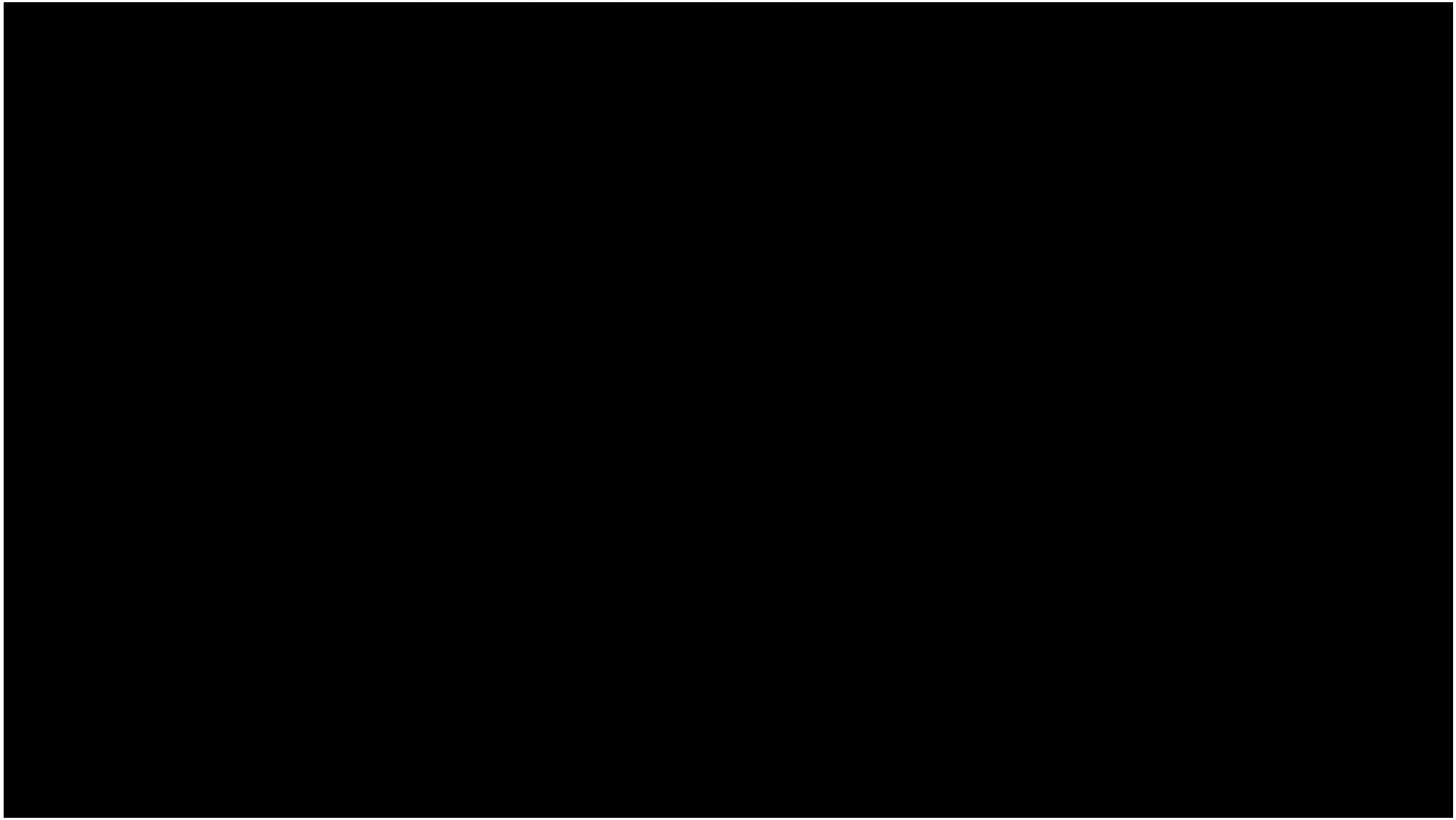

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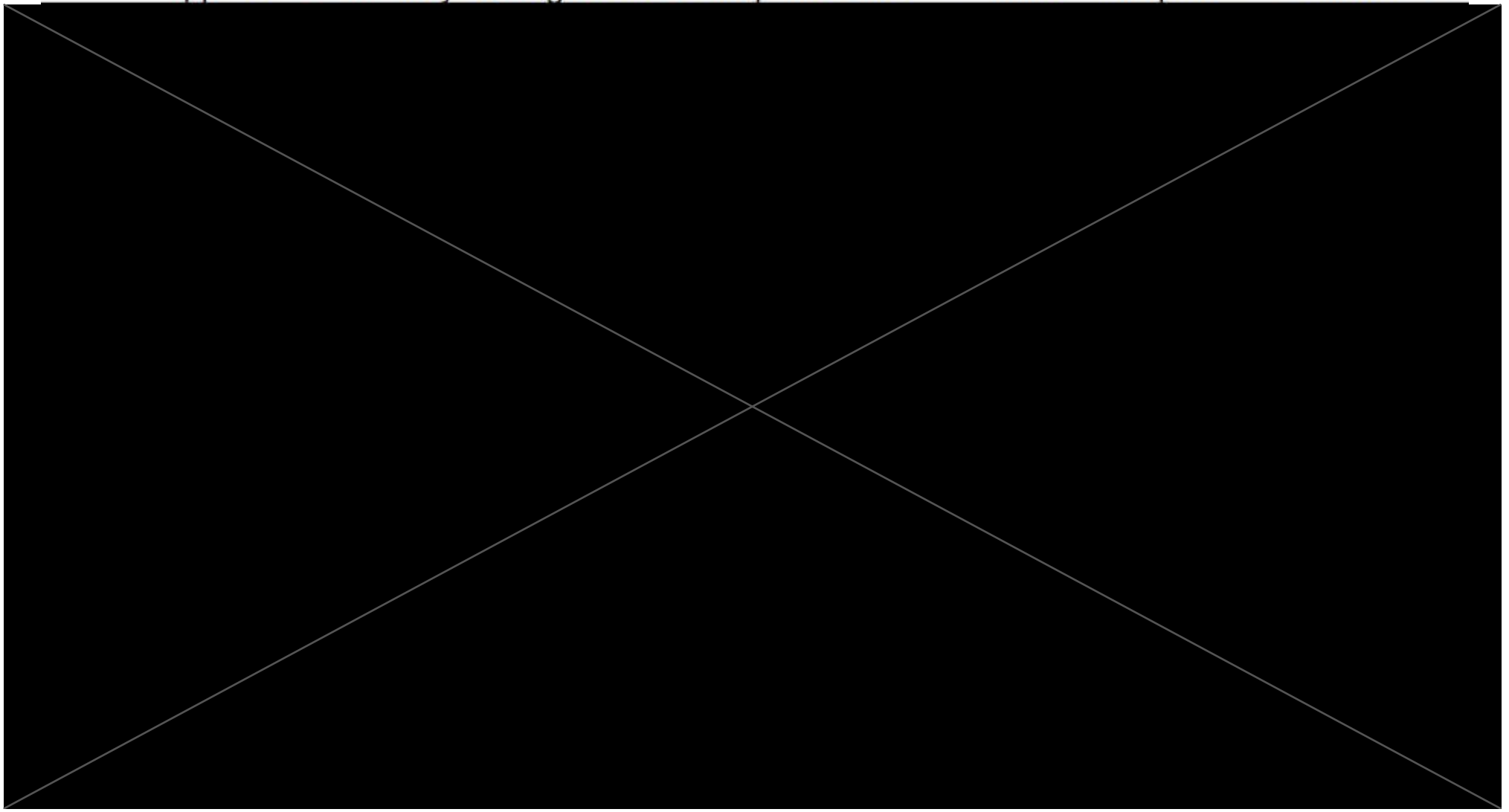

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

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Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

[redacted]

Appendix E. Monthly Average Price for Impacted and So-Called "Unimpacted" Customers

Appendix F

Appendix F. Dr. Stiroh's Customer Subregressions Across all Expert Models

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]	1	43,612	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	2	37,632	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	3	86,258	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	4	67,307	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	5	27,788	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	6	73,938	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	7	45,327	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	8	78,920	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	9	35,908	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	10	12,375	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	11	24,729	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	12	47,657	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	13	7,603	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	14	11,844	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	15	15,041	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	16	30,883	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	17	28,146	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	18	14,310	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	19	61,092	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	20	11,747	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	21	47,145	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	22	14,181	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	23	24,755	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	24	27,887	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	25	11,351	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	26	7,792	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	27	8,343	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	28	21,669	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	29	5,344	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	30	6,733	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	31	1,201	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	32	13,016	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	33	10,932	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	34	1,031	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	35	2,557	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	36	16,117	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	37	5,163	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	38	3,091	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	39	20,760	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	40	3,612	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	41	6,162	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	42	11,464	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	43	3,480	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	44	18,644	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	45	3,711	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	46	16,431	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	47	5,742	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	48	5,855	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	49	7,222	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	50	25,383	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	51	5,223	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	52	13,098	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	53	1,553	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	54	349	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	55	19,766	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	56	7,515	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	57	2,662	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	58	5,278	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	59	1,642	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	60	22,016	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	61	7,820	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	62	12,984	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	63	11,988	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	64	4,050	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	65	19,171	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	66	18,659	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	67	3,815	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	68	6,543	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	69	2,394	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	70	13,962	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	71	1,478	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	72	5,017	1. Impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	73	6,016	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	74	4,946	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	75	10,420	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	76	9,560	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	77	2,561	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	78	9,149	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	79	4,934	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	80	3,889	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	81	541	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	82	8,162	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	83	4,141	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	84	3,262	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	85	20,889	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	86	2,637	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	87	4,239	1. Impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	88	8,998	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	89	4,574	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	90	4,176	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	91	7,284	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	92	7,317	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]	93	1,784	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	94	2,303	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	95	2,435	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	96	2,125	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	97	2,279	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	98	2,749	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	99	4,977	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	100	7,421	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	101	1,887	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	102	1,708	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	103	5,590	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	104	4,667	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	105	16,188	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	106	3,664	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	107	1,133	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	YES
	108	7,866	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	109	11,894	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	110	2,111	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	111	4,364	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	112	932	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	113	1,036	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	114	2,668	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	115	4,113	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	116	4,569	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	117	811	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	118	1,301	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	119	3,518	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	120	5,063	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	121	8,419	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	122	9,107	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	123	15,527	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	124	1,994	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	125	2,336	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	126	352	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	127	6,540	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	128	4,147	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	129	6,483	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	130	2,425	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	131	10,888	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	132	799	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	133	7,868	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	134	3,811	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	135	3,896	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	YES
	136	8,118	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	137	6,220	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	138	1,212	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	139	2,599	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	140	10,626	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	141	1,448	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	142	3,098	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	143	1,438	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	YES
	144	9,687	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	145	2,811	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	146	3,046	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	147	843	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	148	7,315	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	149	1,015	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	150	3,924	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	151	16,182	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	152	889	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	153	2,596	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	154	12,366	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	155	2,182	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	156	5,200	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	157	6,724	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	158	1,333	1. Impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	159	2,104	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	160	3,652	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	161	986	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	162	2,741	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	163	1,725	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	164	523	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	165	403	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	166	6,177	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	167	4,570	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	168	2,213	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	169	5,964	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	170	480	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	171	2,981	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	172	12,010	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	173	2,878	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	174	4,894	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	175	4,206	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	176	7,407	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	177	1,069	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	178	9,265	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	179	1,980	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	180	1,216	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	181	4,297	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	182	3,337	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	183	2,133	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	184	2,799	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	185	1,537	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	186	2,093	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	187	1,849	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]	188	4,597	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	2. No impact	YES
	189	5,002	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	190	5,155	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	191	7,715	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	192	3,354	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	193	1,643	2. No impact	2. No impact	1. Impact	2. No impact	2. No impact	2. No impact	YES
	194	363	1. Impact	1. Impact	2. No impact	2. No impact	1. Impact	1. Impact	YES
	195	4,105	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	196	259	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	197	3,722	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	198	281	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	199	3,640	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	200	2,343	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	YES
	201	963	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	202	1,766	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	203	1,600	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	204	8,001	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	205	946	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	206	1,948	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	207	1,646	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	208	2,420	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	209	202	2. No impact	1. Impact	2. No impact	2. No impact	2. No impact	2. No impact	YES
	210	5,972	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	211	4,804	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	212	418	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	213	5,308	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	214	3,345	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	215	5,708	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	216	3,451	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	217	4,188	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	218	461	1. Impact	2. No impact	1. Impact	2. No impact	2. No impact	2. No impact	YES
	219	2,128	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	220	8,382	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	221	860	1. Impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	222	2,682	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	223	4,065	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	224	6,503	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	225	3,418	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	226	4,951	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	227	5,158	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	228	701	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	229	4,482	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	230	3,375	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	231	2,899	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	232	1,149	2. No impact	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	YES
	233	20,383	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	234	3,740	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	235	596	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	236	407	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	YES
	237	1,344	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	238	1,888	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	239	4,243	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	240	2,255	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	241	3,561	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	242	17,396	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	243	4,887	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	244	3,969	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	245	556	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	2. No impact	YES
	246	2,653	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	247	4,026	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	248	422	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	249	2,896	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	250	853	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	251	347	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	252	392	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	253	524	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	254	853	1. Impact	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	YES
	255	507	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	256	1,216	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	257	987	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	258	319	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	259	340	1. Impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	260	983	1. Impact	2. No impact	2. No impact	2. No impact	2. No impact	2. No impact	YES
	261	3,473	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	262	3,100	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	263	570	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	264	4,925	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	265	1,635	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	266	4,594	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	267	2,857	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	268	1,188	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	269	3,535	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	270	1,844	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	271	2,942	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	272	1,227	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	273	2,489	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	274	5,156	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	275	3,546	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	276	464	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	2. No impact	YES
	277	8,282	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	278	3,293	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	279	2,053	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	280	221	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	281	6,318	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	282	3,293	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]	283	2,451	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	284	4,154	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	285	886	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	2. No impact	YES
	286	3,938	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	287	1,433	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	288	972	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	289	1,097	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	290	833	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	291	2,517	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	292	3,708	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	293	367	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	294	1,499	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	295	233	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	296	180	1. Impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	297	156	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	298	2,089	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	299	2,017	2. No impact	1. Impact	1. Impact	2. No impact	2. No impact	1. Impact	YES
	300	901	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	301	1,044	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	302	2,510	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	303	3,544	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	304	2,947	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	305	882	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	306	2,121	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	307	198	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	308	2,219	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	309	4,966	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	310	793	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	311	1,912	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	312	3,718	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	313	1,620	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	314	935	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	315	6,115	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	316	2,873	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	317	696	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	318	3,308	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	319	1,633	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	320	5,141	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	321	487	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	322	217	2. No impact	2. No impact	2. No impact	2. No impact	2. No impact	2. No impact	NO
	323	684	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	324	5,080	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	325	4,286	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	326	251	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	327	2,884	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	328	3,135	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	329	121	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	330	286	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	331	493	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	332	319	2. No impact	2. No impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	NO
	333	3,373	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	334	1,321	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	335	2,052	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	336	414	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	337	1,537	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	338	304	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	339	733	1. Impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	340	3,528	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	341	2,151	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	342	2,870	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	343	968	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	344	154	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	345	5,328	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	346	1,598	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	347	3,924	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	348	768	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	349	665	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	350	2,329	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	351	1,260	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	352	409	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	353	243	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	354	344	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	355	1,270	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	356	2,097	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	357	4,455	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	358	4,167	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	359	480	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	YES
	360	2,016	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	361	1,455	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	362	1,639	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	363	4,752	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	364	2,681	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	365	1,049	1. Impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	366	841	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	367	3,926	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	368	2,900	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	369	3,304	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	370	2,673	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	371	996	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	372	645	2. No impact	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	YES
	373	341	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	374	2,218	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	375	467	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	376	212	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	YES
	377	2,538	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]	378	3,846	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	379	729	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	380	4,235	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	381	1,637	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	382	556	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	383	3,279	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	384	3,573	1. Impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	385	258	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	386	1,655	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	387	1,105	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	388	4,525	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	389	2,202	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	390	2,917	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	391	4,939	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	392	732	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	393	1,197	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	394	1,557	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	395	2,480	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	396	1,708	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	397	4,088	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	398	1,084	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	399	1,229	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	400	3,267	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	401	2,798	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	YES
	402	1,061	1. Impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	403	3,680	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	404	2,178	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	405	1,485	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	406	205	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	407	2,515	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
	408	1,053	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	409	1,914	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	410	2,511	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	411	247	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
	412	2,719	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	413	1,506	2. No impact	2. No impact	2. No impact	2. No impact	2. No impact	2. No impact	NO
	414	1,837	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	415	2,286	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	416	468	1. Impact	1. Impact	2. No impact	2. No impact	1. Impact	1. Impact	YES
	417	1,356	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	418	1,352	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	419	263	2. No impact	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	YES
	420	3,387	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	421	329	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	422	2,868	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	423	2,828	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	424	1,682	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	425	2,566	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	426	1,292	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	427	974	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	428	1,966	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	429	1,604	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	430	582	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	431	1,165	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	432	1,529	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	433	131	2. No impact	1. Impact	2. No impact	2. No impact	2. No impact	2. No impact	YES
	434	967	1. Impact	1. Impact	2. No impact	2. No impact	2. No impact	1. Impact	YES
	435	249	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	436	166	2. No impact	1. Impact	3. Not tested	3. Not tested	4. Not a customer	4. Not a customer	YES
	437	1,099	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	438	2,075	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	439	2,465	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	440	583	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	441	188	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	442	1,739	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	443	1,374	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	444	834	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	445	1,811	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	446	473	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	447	1,683	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	448	969	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	449	553	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	450	516	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	451	180	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	2. No impact	YES
	452	457	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	453	1,664	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	454	1,798	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	455	847	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	456	851	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	457	687	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	458	2,053	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	459	295	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	460	707	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	461	1,546	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	462	4,429	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	463	969	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	464	1,177	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	465	793	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	466	1,602	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	467	1,202	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	468	1,011	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	469	457	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	470	2,394	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	471	388	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	472	113	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]	473	702	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	474	2,173	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	475	1,919	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	476	566	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	477	2,228	1. Impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	478	2,070	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	479	1,482	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	480	2,050	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	481	643	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	482	1,266	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	483	2,284	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	484	806	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	485	1,682	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	486	578	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	487	1,339	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	488	179	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	489	2,158	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	490	2,406	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	YES
	491	470	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	492	922	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	493	1,264	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	494	268	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	495	257	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	YES
	496	416	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	YES
	497	264	1. Impact	2. No impact	1. Impact	1. Impact	2. No impact	2. No impact	YES
	498	1,674	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	499	1,087	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	500	236	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	501	1,489	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	502	687	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	2. No impact	YES
	503	469	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	504	1,546	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	505	1,371	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	506	267	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	507	222	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	YES
	508	1,141	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	YES
	509	2,829	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	510	1,554	1. Impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	511	140	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	512	1,129	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	513	915	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	514	1,035	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	515	808	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	2. No impact	YES
	516	494	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	517	877	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	518	1,759	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	519	1,080	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	520	1,463	1. Impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	521	597	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	522	1,808	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	YES
	523	323	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	524	491	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	525	3,514	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	2. No impact	YES
	526	1,339	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	527	460	1. Impact	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	528	919	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	529	2,092	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	530	173	1. Impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	531	1,382	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	532	278	1. Impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	533	1,230	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	534	479	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	535	194	1. Impact	2. No impact	2. No impact	2. No impact	2. No impact	2. No impact	YES
	536	424	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	537	1,965	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	538	1,045	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	539	507	2. No impact	2. No impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	NO
	540	2,729	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	541	1,065	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	542	736	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	543	244	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	544	3,535	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	545	167	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	546	262	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	547	802	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	548	881	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	549	1,475	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	550	3,849	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	551	1,500	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	552	542	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	553	291	1. Impact	2. No impact	2. No impact	1. Impact	2. No impact	2. No impact	YES
	554	215	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	555	2,036	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	556	2,121	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	557	864	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	558	3,204	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	559	533	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	560	275	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	561	1,080	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	562	2,836	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	563	1,342	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	564	1,800	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	565	357	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	YES
	566	182	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	567	1,247	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]	568	171	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	569	551	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	570	1,046	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	571	720	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	572	1,428	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	573	2,308	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	574	198	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	575	537	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	576	529	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	577	2,199	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	578	438	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
	579	216	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	580	357	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	2. No impact	YES
	581	1,349	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	582	1,517	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	583	2,200	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	584	902	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	2. No impact	YES
	585	1,094	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	586	384	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	587	336	1. Impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	588	409	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	589	260	2. No impact	1. Impact	2. No impact	1. Impact	4. Not a customer	4. Not a customer	YES
	590	1,705	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	591	1,346	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	592	484	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	593	158	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	594	1,352	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	595	926	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	596	3,032	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	597	541	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	598	1,011	2. No impact	2. No impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	NO
	599	523	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	600	664	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	601	879	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	602	1,003	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	603	264	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	604	1,334	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
	605	476	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	606	1,208	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	YES
	607	1,182	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	608	535	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	609	316	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	610	2,049	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	611	375	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	612	281	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	613	359	1. Impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	614	1,354	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	615	1,082	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	616	190	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	617	376	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	618	6,825	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	619	1,345	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	YES
	620	2,533	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	621	1,383	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	622	1,326	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	623	2,097	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	624	1,158	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	625	231	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	626	614	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	627	112	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	628	120	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	629	872	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	630	1,827	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	631	103	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	2. No impact	YES
	632	1,284	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	633	303	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	634	169	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	635	542	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	636	502	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	637	396	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	638	474	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	639	325	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	640	521	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	641	1,052	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	642	931	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	643	268	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	644	903	2. No impact	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	YES
	645	370	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	646	294	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	647	199	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	648	140	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	649	630	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	650	1,053	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	651	265	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	652	752	1. Impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	653	1,294	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	654	486	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	655	1,874	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	656	1,106	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	657	1,297	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	658	2,391	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	659	174	1. Impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	660	320	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	661	225	1. Impact	2. No impact	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
	662	157	2. No impact	2. No impact	2. No impact	2. No impact	2. No impact	2. No impact	NO

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]	663	1,383	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	664	849	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	665	1,238	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	666	392	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	667	1,328	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	668	1,517	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	669	351	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	670	165	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
	671	1,310	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	672	1,008	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	673	712	1. Impact	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	YES
	674	469	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	675	1,037	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	676	779	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	677	139	2. No impact	2. No impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	NO
	678	787	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	679	954	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	680	738	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	681	938	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	682	218	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	683	1,034	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	684	1,112	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	685	2,446	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	686	427	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	687	1,373	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	688	546	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	2. No impact	YES
	689	693	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	690	286	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	691	957	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	692	132	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	693	733	1. Impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	694	2,686	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	695	436	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	696	671	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	697	773	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	698	383	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	699	179	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	700	312	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	701	811	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	702	829	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	703	251	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	704	1,298	1. Impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	705	1,261	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	706	3,651	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	707	1,499	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	708	448	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	709	1,213	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	710	113	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	711	989	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	712	1,275	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	713	449	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	714	180	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	715	424	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	716	1,132	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	717	499	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	718	463	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	719	603	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	720	115	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	721	324	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	722	629	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	723	195	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	724	105	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	725	737	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	726	295	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	727	1,668	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	728	636	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	729	612	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	730	1,064	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	731	711	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	732	428	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	733	133	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	734	912	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	735	1,621	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	736	951	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	737	372	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	738	308	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	739	222	2. No impact	2. No impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	NO
	740	868	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	741	1,581	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	742	538	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	2. No impact	YES
	743	929	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	744	1,184	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	745	352	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	746	353	2. No impact	2. No impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	NO
	747	582	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	748	143	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	749	1,224	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	750	832	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	751	154	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	752	918	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	753	144	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	754	308	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	755	1,695	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	756	607	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	757	1,727	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]	758	149	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	759	542	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	760	882	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	761	1,200	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	762	662	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	763	722	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	764	359	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	765	380	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	766	1,305	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	767	350	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	768	1,025	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	769	2,448	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	770	1,133	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	771	1,225	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	772	620	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	773	1,963	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	774	610	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	775	554	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	776	805	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	777	1,017	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	778	138	1. Impact	1. Impact	4. Not a customer	4. Not a customer	4. Not a customer	4. Not a customer	YES
	779	262	1. Impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	780	644	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	781	123	1. Impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	782	1,183	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	783	204	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
	784	175	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	2. No impact	YES
	785	495	1. Impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	786	261	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	787	1,328	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	788	747	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	789	333	1. Impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	790	308	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	791	464	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	792	482	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	793	174	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
	794	243	1. Impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	YES
	795	1,093	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	796	1,224	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	797	925	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	798	525	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	799	223	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	800	194	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	801	753	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	802	458	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	2. No impact	YES
	803	320	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	804	433	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	805	583	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	806	560	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	807	441	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	808	1,309	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	809	284	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	810	958	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	811	1,106	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	812	219	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	813	236	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	YES
	814	125	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	815	192	2. No impact	2. No impact	2. No impact	2. No impact	2. No impact	1. Impact	YES
	816	343	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	817	797	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	818	605	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	819	433	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	820	715	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	821	313	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	822	822	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	823	2,383	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	824	718	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	825	690	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	826	1,153	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	827	329	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	828	681	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	829	1,894	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	830	1,168	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	831	605	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	832	366	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	833	904	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	834	1,299	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	835	385	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	836	1,247	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	837	832	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	838	501	1. Impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	839	262	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	840	349	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	841	138	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	842	931	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	843	513	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	844	1,203	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	845	764	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	846	1,006	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	847	884	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	848	1,461	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	849	163	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	2. No impact	YES
	850	1,001	2. No impact	2. No impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	851	1,011	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	852	252	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]	853	639	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	854	666	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	855	1,167	1. Impact	2. No impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	856	603	1. Impact	1. Impact	4. Not a customer	4. Not a customer	1. Impact	1. Impact	YES
	857	935	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	858	1,124	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	859	1,154	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	860	417	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	861	192	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	862	694	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	863	1,055	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	864	591	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	865	1,904	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	866	1,136	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	867	959	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	868	117	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	869	428	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	870	588	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	871	1,626	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	872	1,230	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	873	1,795	2. No impact	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	874	481	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	875	469	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	876	2,322	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	877	182	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	878	735	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	879	417	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	880	1,098	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	881	1,054	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	882	339	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	883	1,192	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	884	544	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	885	948	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	886	566	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	887	162	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	888	163	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	889	202	2. No impact	1. Impact	2. No impact	1. Impact	4. Not a customer	4. Not a customer	YES
	890	2,070	2. No impact	2. No impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	891	1,707	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	892	596	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	893	268	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	894	231	1. Impact	2. No impact	2. No impact	2. No impact	1. Impact	1. Impact	YES
	895	1,053	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	896	676	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	897	281	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	898	756	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	899	147	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	900	582	2. No impact	1. Impact	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
	901	535	1. Impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	902	1,367	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	903	323	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	904	231	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	905	417	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	906	510	2. No impact	1. Impact	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
	907	481	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	908	1,564	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	909	912	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	910	401	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	911	283	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	912	415	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	913	394	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	914	436	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	915	190	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	916	431	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	917	464	2. No impact	1. Impact	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
	918	1,517	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	919	1,137	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	920	674	2. No impact	1. Impact	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
	921	565	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	922	813	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	923	2,191	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	924	452	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	925	1,108	1. Impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	926	464	2. No impact	2. No impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	927	168	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	928	786	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	929	159	2. No impact	2. No impact	1. Impact	2. No impact	3. Not tested	3. Not tested	YES
	930	149	2. No impact	1. Impact	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
	931	940	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	932	475	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	933	277	2. No impact	1. Impact	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
	934	242	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	935	126	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	936	201	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	937	535	1. Impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	938	1,676	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	939	533	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	940	203	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	941	418	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	942	535	2. No impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	943	1,297	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	944	325	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	945	464	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	946	989	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	947	133	1. Impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]	948	374	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	949	538	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	950	701	1. Impact	1. Impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	951	836	2. No impact	2. No impact	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
	952	1,023	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	953	808	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	954	437	2. No impact	1. Impact	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
	955	650	2. No impact	1. Impact	2. No impact	1. Impact	1. Impact	1. Impact	YES
	956	264	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	957	1,122	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	958	1,036	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	959	937	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	960	945	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	961	378	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	962	202	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	963	1,355	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	964	376	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	965	740	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	966	739	1. Impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	967	449	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	968	258	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	969	1,099	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	970	274	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	971	764	2. No impact	2. No impact	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
	972	935	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	973	344	2. No impact	2. No impact	2. No impact	1. Impact	2. No impact	1. Impact	YES
	974	182	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	2. No impact	YES
	975	1,177	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	976	1,472	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	977	284	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	978	587	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	979	1,091	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	980	678	2. No impact	1. Impact	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
	981	186	2. No impact	2. No impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	982	892	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	983	666	1. Impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	984	959	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	985	695	2. No impact	1. Impact	1. Impact	1. Impact	2. No impact	1. Impact	YES
	986	123	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	987	749	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	988	137	2. No impact	2. No impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	NO
	989	296	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	990	490	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	991	582	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	992	273	2. No impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	993	884	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	994	278	2. No impact	2. No impact	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
	995	927	2. No impact	1. Impact	1. Impact	1. Impact	1. Impact	1. Impact	YES
	996	533	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
	997	751	2. No impact	1. Impact	3. Not tested	3. Not tested	3. Not tested	3. Not tested	YES
	998	418	1. Impact	1. Impact	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
	999	590	2. No impact	1. Impact	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
	1000	396	2. No impact	1. Impact	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	2. No impact	2. No impact	3. Not tested	3. Not tested	NO
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	2. No impact	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	2. No impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	2. No impact	1. Impact	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	2. No impact	3. Not tested	3. Not tested	NO
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	2. No impact	NO
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	2. No impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	2. No impact	3. Not tested	3. Not tested	NO
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	2. No impact	4. Not a customer	4. Not a customer	NO
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES

Customer Name	Mangum Rank	Mangum Observation Count	Mangum Individual Customer Result	Mangum Interaction Customer Result	Sunding Individual Customer Result	Sunding Interaction Customer Result	Williams Individual Customer Result	Williams Interaction Customer Result	Impacted in at least one customer specific subregression
[redacted]			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	2. No impact	1. Impact	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	1. Impact	1. Impact	1. Impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			3. Not tested	3. Not tested	3. Not tested	3. Not tested	1. Impact	1. Impact	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	2. No impact	1. Impact	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	2. No impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	4. Not a customer	4. Not a customer	1. Impact	1. Impact	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	3. Not tested	3. Not tested	YES
			4. Not a customer	4. Not a customer	2. No impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	4. Not a customer	4. Not a customer	YES
			4. Not a customer	4. Not a customer	3. Not tested	3. Not tested	2. No impact	1. Impact	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	1. Impact	1. Impact	YES
			4. Not a customer	4. Not a customer	1. Impact	1. Impact	1. Impact	1. Impact	YES

Tested Customers	**1291**
Customers Impacted In At Least One Customer-Specific Subregression	**1248**
	96.7%

Source: See backup production. Customer subregressions are based on Dr. Stiroh Report Exhibits 7A, 7B, 7C, and Appendix B.

Appendix G

Appendix G. Third Party Data Summary

For an explanation of how third party data is processed for my pass-through analysis, see Appendix C of the Mangum Class Report. I've included a high-level summary of each new or updated third party's data, after limiting to relevant primal cuts, below.[1]

[redacted]

[redacted]

[redacted]

[redacted]

[redacted]

[1] For all 4 third parties added or updated for this report [redacted] the cost measure included in the sales data was used to calculate the monthly weighted average cost in the pass-through regression.

Appendix H

Appendix H. Consumer IPP Damages, August 2014 - December 2019
By State

Consumer IPP State	Damages
Arizona	[redacted]
California	
District of Columbia	
Florida	
Illinois	
Iowa	
Kansas	
Maine	
Massachusetts	
Michigan	
Minnesota	
Missouri	
Montana	
Nebraska	
Nevada	
New Hampshire	
New Mexico	
New York	
North Carolina	
North Dakota	
Oregon	
Rhode Island	
South Dakota	
Tennessee	
Utah	
West Virginia	
Wisconsin	
IPP State Total	

Source: Defendant Transaction Data.

Note: Damages are calculated as relevant nationwide Consumer IPP purchases from Defendants apportioned to relevant IPP states based on population. This relevant state commerce is then multiplied by the overcharge rate [redacted] from Mangum Class Report Figure 50 and the pass through rate of 100.