UNITED STATES DISTRICT COURT

DISTRICT OF MASSACHUSETTS

NATCHITOCHES PARISH HOSPITAL SERVICE DISTRICT and JM SMITH CORPORATION d/b/a SMITH DRUG COMPANY on behalf of themselves and all others similarly situated, Plaintiffs, vs. TYCO INTERNATIONAL, LTD., TYCO INTERNATIONAL (US) INC, TYCO HEALTHCARE GROUP, LP,; THE KENDALL HEALTHCARE PRODUCTS COMPANY, Defendants	)))))))))))))))))	Civil Action No. 05-12024 PBS **<u>SUR-REPLY DECLARATION OF PROFESSOR EINER ELHAUGE</u>**

INTRODUCTION

1. I file this sur-reply declaration to correct economic and factual errors made by Tyco and its experts in their reply filings supporting Tyco's motion to exclude my testimony. These reply filings continue to suffer from the same flaws as their original briefs and declarations did, and fail to meaningfully respond to the analysis contained in my original Daubert declaration.

2. I respond to the specific claims made by Tyco and its experts below. But first, it is important to emphasize a number of key, overarching issues with Tyco's filings.

3. ***First***, Tyco and its experts ignore the fact that, as the economic expert on liability, my opinions in this case have all been about the fact of anticompetitive impact from Tyco's exclusionary contracts, not the amount of damages. Even if one wrongly accepted the claims by the defense and its experts, all they would show is that applying alternative methods of analyzing impact would indicate a smaller, but still positive, amount of anticompetitive injury.[1] That would *confirm* all of my conclusions on anticompetitive impact, not undermine them. Nor do either Tyco or its experts even establish that the amount of anticompetitive injury would be smaller under their alternative "access" methodology, because they ignore the fact that using their preferred "access" method substantially increases the size of the burdened group of buyers. Thus, even if the Tyco "access" methodology shrinks the "gap" between the burdened and unburdened groups, it increases the size of the burdened group against which that gap is multiplied, and when both effects are considered it can produce a greater anticompetitive impact. Indeed, I understand that analysis

[1] *See infra* ¶9 (showing that Tyco and Professor Ordover's argument on economic rationality now concedes some anticompetitive impact but just disputes the size of it); *id.* ¶¶21-22 (showing that Tyco and Professor McFadden's linear and fixed regressions confirm an anticompetitive impact and just argues the size is smaller); *id.* ¶¶30-31 (showing that Tyco and Professor Ordover's access test confirms an anticompetitive impact and just argues that the size is smaller); *id.* ¶18 (showing that Tyco and Professor Ordover admit that sole-source contracts have the anticompetitive effect of forcing buyers to forego using GPO brokerage services to purchase from rival suppliers).

performed by Dr. Hal Singer using the numbers from Professor Ordover's own "access" approach to calculate damages shows that using Tyco's preferred "access" method leads to damages that are *even larger* than the damages he calculated using the figures produced by my original methodology. In short, even if they were valid, Tyco's arguments would not only confirm, but reinforce, my conclusions on anticompetitive impact.

4. ***Second***, nothing in the new filings alters the fact that the "access" approach advocated by Tyco and its experts is economically improper for the reasons I explained in my original Daubert declaration.[2] Instead, as I explained there, the proper economic method looks at buyers' actual contractual status because the anticompetitive effects flow from the exclusionary terms of those actual contracts.[3] This is precisely the method used in my reports.

5. ***Third***, Tyco and its experts continue to make the fundamental error of assuming that the alleged anticompetitive conduct is Tyco's pricing, when in fact the challenge is to the exclusionary commitments Tyco attached to its prices. Tyco's "coupon" hypothetical is one prominent illustration of this error, and thus is completely beside the point.[4] The issue is not whether discount coupons produced sales, and there is no challenge to Tyco offering any above-cost discounts it wants that are not tied to exclusionary commitments. Nor have Tyco or its experts offered any plausible procompetitive efficiency for its exclusionary commitments.

6. ***Fourth***, Tyco and its experts continue to ignore the fact that, because Tyco charged higher prices to buyers and GPOs that would not make exclusionary commitments, one cannot separate the effects of prices from the effects of commitments. They simply reiterate their claim that

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[2] Elhauge Dec ¶¶59-63.
[3] Elhauge Dec ¶60.
[4] Tyco Reply 10-11.

one should control for prices without remedying this fatal flaw with their argument.[5]

7. ***Fifth***, Tyco erroneously claims that I have conceded three of my comparisons and four of my eight regressions result from selection bias.[6] I did not. I pointed out that self-selection bias could not apply to the sole-source GPO comparison and four regressions involving buyers whose contract status changed over time. For the others, I did various cross-checks (including the ones to which no self-selection bias could apply) that *disproved* the claim that my conclusions resulted from selection bias.[7]

I. AFFIRMATIVE EXCLUSIONARY COMMITMENTS, NOT MERE DISCOUNTING

8. Nowhere in Tyco's reply briefing does it offer any response to my showing that this case does not involve, as Tyco claims, mere discounting, but rather involves affirmative exclusionary commitments.[8] Professor Ordover does claim that the share-based contracts were terminable despite their lack of a termination provision.[9] But his only support for this claim is (1) that the contracts do not specify a penalty for termination;[10] and (2) that 20% of buyers with Tyco share-based commitments switched their contract status between 2005-2007.[11] But his first point conflicts with the economic reality that most contracts do not specify the penalties for termination or breach, which hardly means that there are no penalties and that contractual commitments are meaningless. The second point says little about the terminability of the contracts because it is hardly

[5] Tyco Reply 11 n.12, 15-16, 19; McFadden Reply Dec ¶¶5, 15-16, 20; Ordover Reply Dec ¶50.
[6] Tyco Reply 5-6.
[7] *See* Elhauge Report ¶¶194-199; Elhauge Reply Report ¶¶81-85; Elhauge Dec ¶¶16-41.
[8] *See* Elhauge Dec Part I.
[9] Ordover Reply Dec ¶45.
[10] Ordover Reply Dec ¶45.
[11] Ordover Reply Dec ¶48 n.51.

surprising that 20% of contracts would end over a three year period, and even if some buyers were able to terminate their contracts and willing to suffer the associated penalties, that would not at all disprove the fact that Tyco's share-based contracts required commitments. In any event, even if Professor Ordover were right about these points, he acknowledges that breaching the exclusionary commitments would at least lead to price adjustments,[12] and as I already showed, these price adjustments were not mere discounts, but substantial penalties for noncompliance.[13]

II. Economic Rationality

9. Neither Tyco nor its experts deny that selection bias cannot rebut my showing of an anticompetitive impact on rival sales because it would be economically irrational for Tyco to give tens of millions of dollars to buyers who agreed to exclusionary terms if those terms did not affect buyer behavior.[14] To avoid claiming Tyco was economically irrational, Tyco and Professor Ordover now concede that Tyco's exclusionary contracts with GPOs and buyers *did* adversely affect rival sales.[15] But this concedes an anticompetitive impact (absent any redeeming efficiencies, which neither Tyco nor any of its experts have shown). Tyco and its experts are thus not really contesting my conclusion as a liability expert that there was an anticompetitive impact. Rather, they are arguing only that the size of that impact was smaller than Dr. Singer assumed when measuring the

[12] Ordover Reply Dec ¶45.

[13] *See* Elhauge Dec ¶11.

[14] Elhauge Dec ¶¶16, 21-23.

[15] *See* Tyco Reply 19-20; Ordover Reply Dec ¶¶6, 15-19. Although it now admits that its contracts had an adverse effect on rival sales, Tyco in a footnote nevertheless also claims that I offered "no meaningful rebuttal" to a hypothetical in Tyco's original briefing "in which a GPO contract was assumed to have absolutely no effect on purchasing." Tyco Reply 12 n.14. But I of course did respond to this hypothetical, by showing that it had no basis in the facts of this case and hinged on the uneconomical assumption that Tyco acted irrationally. Elhauge Dec ¶¶21-23. Tyco and its own expert now agree with me that its contracts did have an adverse effect on rivals, making it quite perplexing why Tyco continues to push this hypothetical that everyone now agrees has zero basis in reality.

degree of damages, which, even if true, does not undermine my conclusions on anticompetitive impact. Further, as noted above and discussed further below, the calculations in my reports to assess impact were in fact correct, and the alternative "access" approach advocated by Tyco and its experts makes the size of the anticompetitive impact even *greater*.[16]

III. THE NOVATION LONGITUDINAL STUDY

10. Neither Tyco nor its experts deny that the longitudinal study I performed of rival share at Novation is not infected by selection bias; instead, they make the factual claim that rival share growth did not increase at Novation after its sole-source contract ended. If Tyco's factual claim is incorrect, then its claim that my results can be explained by selection bias fails.[17]

11. The basis for Tyco's factual challenge to the longitudinal Novation study is a regression analysis performed by Professor McFadden that purported to show that there was no statistically significant evidence that the absolute growth in rival share was greater after the Novation contract became multi-source than when it was sole-source. But my original Daubert declaration showed that Professor McFadden failed to perform his regressions using a start date of October 2003 and an end date of October 2006 – which is the proper time period to use given the available data – and that a regression performed using this proper time frame would find statistically significant evidence that rival share growth was greater after Novation became multi-source.[18]

12. Tyco and Professor McFadden never deny that they failed to run this regression. Instead, Professor McFadden offers the irrelevant response that he ran one regression from October

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[16] *See supra* ¶3; *infra* ¶32.
[17] Elhauge Dec ¶25.
[18] Elhauge Dec ¶¶26-33.

2001-October 2006 and one regression from October 2003-May 2007.[19] But Tyco and Professor McFadden offer no explanation why the McFadden regression that started in October 2003 ended seven months after October 2006, even though data on Tyco's largest rival, BD, was clearly not available after that date.[20] Nor do they offer any explanation for how it could possibly be reliable to perform a regression on changes in rival share that includes a post-change period when no data existed about the largest rival. Nor does Professor McFadden offer any explanation why the regression he ran that did properly end in October 2006 started in October 2001, [redacted][21] Nor do they offer any explanation for how it could possibly be reliable to perform a regression on changes in rival share that includes a pre-change period when the data from the largest rival had serious accounting problems. Nor do either Tyco or Professor McFadden dispute that nothing in McFadden's original declaration revealed that his regression that started in October 2003 failed to use the October 2006 end date that his declaration otherwise indicated was the appropriate end date to use.

13. Although Tyco's experts do not challenge the fact that October 2003 to October 2006 is a correct time period for such a regression, Tyco does claim that I was wrong to use this time period, arguing that I should have included data from October 2001 through December 2002, [redacted][22] This argument is misguided because one cannot meaningfully do a growth study with a 9-month gap in the middle of the growth period. In any event, even if I did include these months, it would confirm – indeed strengthen – all of my

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[19] McFadden Reply Dec ¶18.
[20] McFadden Reply Dec ¶18.
[21] McFadden Reply Dec ¶18.
[22] Tyco Reply 18.

results. Performing the regression with the dates Tyco suggests, I find that the average absolute monthly rival share growth was 0.92% higher after the Novation contract switched to multi-source than when the contract was sole-source, and that this difference is statistically significant at the 97.2% confidence level. If I instead calculate rival share by using moving averages, the difference is 0.68% and is significant at the 99.9% confidence level. These are even larger effects with even greater statistical significance than I found originally. Thus, the regression period that Tyco claims is right *disproves* Tyco's arguments and *reinforces* my prior analysis showing that Novation's sole-source contract had an anticompetitive impact.

14. Professor McFadden does not deny that a regression using the proper period of October 2003 to October 2006 finds statistical significance at the 90% confidence level.[23] Instead, Professor McFadden dismisses this regression because it does not find significance at the 95% confidence level.[24] But Professor McFadden never explains why he demands a 95% confidence level here when he did not demand it in his own academic work.[25] He just conclusorily asserts that the "circumstances" were different, without explaining what difference in circumstances he claims justified demanding a different level of statistical confidence. Nor does he explain what criteria he used to determine what confidence level to demand here.[26]

15. Professor McFadden also does not deny that a regression performed on the absolute growth in rival shares when rival shares are calculated using a three-month moving average finds

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[23] Elhauge Dec ¶28.

[24] McFadden Reply Dec ¶18.

[25] Elhauge Dec ¶88 n.114 (citing article by Professor McFadden using the 90% confidence level multiple times).

[26] McFadden Reply Dec ¶18 n.16. Professor McFadden also claims that Table 9 in my report used a 95% confidence level, but in fact all this table did was accurately report that its results were significant at the 95% confidence level. Nowhere did I state that I demanded a 95% confidence level or that a 90% confidence level could not be significant or economically meaningful.

statistical significance at even a 99% confidence level.[27] Instead, he just asserts that this conclusion violates the Gauss-Markov theorem because, if the "original least squares regression is best estimated by ordinary least squares, no linear transformation ... can improve the precision of estimates."[28] But Professor McFadden offers no analysis whatsoever for his premise that a regression on monthly figures without using moving averages satisfies the Gauss-Markov theorem and provides the best estimate. Instead, he simply assumes it is correct to measure rival monthly shares without using moving averages. His assumption completely ignores the fact that the variability in the absolute month-to-month change of rival share without using moving averages is far greater than the variability in the month-to-month change of rival shares when moving averages are used. Using moving averages thus eliminates much of the random noise in the month-to-month absolute growth in rival shares, providing a better picture of rival share growth over time.

16. In fact, one can show that, contrary to Professor McFadden's unsupported assertions, a regression using the absolute growth in monthly rival shares without moving averages would *not* satisfy the Gauss-Markov theorem. The reason is that one requirement for the Gauss-Markov theorem to apply is that the error terms of all the individual observations (here, the growth rates) need to have a constant variance.[29] But one can reject at the 95% confidence level the hypothesis that the error terms in the regression without moving averages have a constant variance.[30] In contrast, for the alternative regression with moving averages, this hypothesis cannot be rejected at

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[27] McFadden Reply Dec ¶19.

[28] McFadden Reply Dec ¶19.

[29] *See, e.g.*, ROBERT S. PINDYCK & DANIEL L. RUBINFELD, ECONOMETRIC MODELS AND ECONOMIC FORECASTS 58-59, 62 (4td ed. 1998) (stating that one of the five assumptions of the "classical linear regression model" that underlie the Gauss-Markov Theorem is that "the error term has constant variance for all observations").

[30] I perform this analysis in Stata by using the command "estat hettest" to test for heteroskedasticity, i.e., differing variance.

that level.[31]

17. Professor Ordover's reply declaration performs a new regression that starts in September 2004 and ends in October 2006.[32] The beginning and end dates of his new regression do not match *any* of the four regressions performed by Professor McFadden, *nor* the period that Tyco's briefing advocates, and thus represent the *sixth* separate attempt by Tyco and its experts to choose different beginning and end dates that will yield results favorable to Tyco. But this particular manipulation of the start dates is no more reliable than the others, because there is no reason for the analysis to exclude the period starting in October 2003, when there was indisputably reliable data on rival shares and which even Professor McFadden used as a start date. Professor Ordover also misstates the econometric meaning of his results when he states that he finds "with 95% confidence that the average share growth in these two periods were not different."[33] In fact, what Professor Ordover found is a lack of 95% confidence that such a difference does exist; this finding of a more than 5% likelihood of no difference does not at all make it over 95% likely there is no difference.

IV. THE SIMULTANEOUS SOLE-SOURCE GPO COMPARISON

18. Tyco concedes that a sole-source GPO contract "necessarily *excludes* every customer" who wanted to buy from rivals.[34] But as my original Daubert declaration pointed out, this sort of concession *confirms* the relevant anticompetitive effect: that rivals were foreclosed from

[31] I cannot reject the constant variance hypothesis when using moving averages at the 95% confidence level for a regression starting in any month from June 2003-December 2003.

[32] Ordover Reply Dec ¶26. In a footnote, Professor Ordover says that the same result holds if he instead uses a 14 month period before the contract change and starts his analysis in July 2004. *Id.* at ¶26 n.25. But this regression is invalid for the exact same reason as his other regression: it ignores reliable data from October 2003 forward.

[33] Ordover Reply Dec ¶26 n.25.

[34] Tyco Reply 7; *see also* Ordover Reply Dec 57 ("All hospitals that ... decide not to purchase [Tyco] containers and instead buy from other firms must necessarily purchase entirely off contract.").

access to these GPO brokerage services and customers had to forego using GPOs to buy from rivals.[35] Tyco here is thus again admitting that the challenged conduct had an anticompetitive impact.

19. Further, Tyco's claim (that buyers who bought zero containers through a Tyco sole-source GPO contract should be included in the group burdened by such sole-source GPO contracts) ignores the issue of whether such buyers are in the market for using GPO brokerage services. In any event, my original Daubert declaration did not exclude buyers who did not buy through a Tyco sole-source GPO contract when I performed Tyco's preferred "access" analysis, and I continued to find a significant anticompetitive effect from Tyco's sole-source contracts.[36] Tyco's argument thus has no bearing on the existence of an anticompetitive impact in this case.

20. Tyco and Professor Ordover now concede that my comparison did not reassign buyers from the burdened group unless a buyer failed to buy a single container through a sole-source contract, and admit that they were wrong to assert that such buyers were re-assigned to the unburdened group.[37] Tyco and Professor Ordover nonetheless continue to claim that the few cases where there were reassignments demonstrate selection bias, but they offer no response at all to my showing that even with zero reassignments, all my conclusions would continue to hold.[38] Indeed,

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[35] *See* Elhauge Dec ¶35.

[36] Elhauge Dec Table 2A; Ordover Reply Dec ¶63 ("[Professor Elhauge] includes in his access analysis hospitals that purchased sharps containers without utilizing a GPO contract.").

[37] Tyco Reply 8-9; Ordover Reply Dec ¶28 n27. Professor Ordover does offer a tortured defense of his demonstrabl[redacted]

[38] *See* Elhauge Dec ¶37, ¶65 n.89.

the analysis in my original Daubert declaration showed that the predicted anticompetitive effects from the sole-source GPO comparisons would be 9% *higher* without *any* reassignment.[39] Tyco and Professor Ordover simply ignore this fatal factual flaw with their claim that reassignment creates selection bias.

V. THE LONGITUDINAL REGRESSIONS

21. Tyco and Professor McFadden do not deny that a linear regression (with or without using fixed effects) that includes all the relevant data would show anticompetitive impact; they just claim that the amount of impact shown by the regressions would be smaller.[40] Their argument thus (once again) simply *confirms* my conclusion that Tyco's contracts had an anticompetitive impact. Nor is it even true that the linear regressions that Professor McFadden prefers always lead to a smaller impact than calculated in my original reports; in 3 of the 8 linear regressions without fixed effects, Professor McFadden's own calculations show that his linear regression leads to a *larger* impact.[41]

22. Tyco and Professor Ordover also complain that my longitudinal regressions are not limited to buyers whose GPOs changed their offerings.[42] This is true, but that is because the

[39] *See* Elhauge Dec ¶65 n.89.

[40] Tyco Reply 17; McFadden Reply Dec ¶¶13, 17.

[41] McFadden Reply Dec Table 1 (showing that, compared to figures in my reports, Professor McFadden's linear regressions without fixed effects show a larger impact in the restricted and sole-source regression for all hospitals and the sole-source regressions for all hospitals and for those whose contract status changed). Professor McFadden also argues that a logarithmic form is "unrealistic" because it predicts a rival share of over 100% at up to 2.2% of unrestricted hospitals. McFadden Reply Dec ¶11 & n.4. This criticism is misplaced because it applies equally to linear regressions. A linear regression predicts a rival share of less than 0% between 1.6% and 8.1% of the time without fixed effects, and up to 1.5% of the time with fixed effects. Such occasional outlier predictions merely reflect the fact that a regression generates its coefficients by performing calculations across all observations, meaning that the coefficients in some sense represent the "average" effect that a certain independent variable has on the dependent variable.

[42] *See* Tyco Reply 16; Ordover Reply Dec ¶¶43-53.

alternative approach favored by Tyco and Professor Ordover would wrongly use the Tyco "access" approach, which is incorrect for the reasons I discussed in my declaration. Even if I implemented the Tyco "access" approach, that would not alter my conclusions in the regressions.[43] Nor would the access approach change the conclusions in my simultaneous comparisons, as I discuss next.

VI. TYCO'S PROPOSED "ACCESS" ALTERNATIVE

23. Tyco does not deny that applying its proposed "access" method would produce the nonsensical result that there could never be any provable anticompetitive effect if a monopolist offered foreclosing contracts to all buyers.[44] Given that offering foreclosing contracts to all buyers increases the likely anticompetitive effect under standard antitrust economics, a method that produces such a nonsensical result cannot reliably ascertain anticompetitive effects.

24. For this reason and others detailed in my original Daubert declaration, Tyco's "access" approach is incorrect.[45] Instead, as I have explained previously, the correct economic approach is to look at buyer's actual contracts, because it is buyers who are governed by exclusionary contractual terms that are anticompetitively foreclosed, not all buyers who have "access" to a chance to agree to such exclusionary contracts.[46] Looking at buyers' actual contracts

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[43] When I re-run the regressions using the "access" approach, I find that every single variation of the regressions – both logarithmic and linear, both with and without fixed effects, both for all buyers and for only buyers whose contract status changed, and for all four classifications of buyers into burdened and unburdened groups – shows an anticompetitive effect from Tyco's challenged contracts. In short, all 32 different iterations of the regressions, including all of the regressions using the modified forms preferred by Tyco and its experts, continue to show an anticompetitive impact under the access approach. All 32 regressions are statistically significant even at the 99.9% confidence level.

[44] Tyco Reply 12.

[45] Elhauge Dec ¶¶58-64.

[46] Elhauge Dec ¶¶59-62. Tyco's claim in a footnote that my analogy to cigarette smoking is "strikingly misleading" simply misses the point that what is relevant is the contracts under which buyers purchase, not the contracts to which buyers have access. *See* Tyco Reply 12 n.13. In its footnote, Tyco claims that I "should test the effect of belonging to a GPO with the challenged contract." *Id.* But that is completely wrong – the relevant anticompetitive effect is on buyers who actually buy through the challenged contracts and thus are burdened by the contracts' exclusionary terms. My analogy to smoking studies was thus entirely apt. Just as how in a study on the effects of smoking the

is precisely the approach that I used in my reports, and nothing in Tyco's briefing or its expert reports has provided any convincing response to this basic reason why a proper economic analysis should classify buyers based on their actual contractual terms. Tyco is thus incorrect to insinuate that I have "walk[ed] away" from the analysis performed in my reports.[47] To the contrary, my original Daubert declaration explained why my reports were economically correct, why Tyco's proposed "access" alternative is economically flawed, and then why, on top of that, even using Tyco's "access' approach would confirm all my conclusions.

25. Tyco and Professor Ordover argue that, to implement their proposed "access" approach, I should have used membership rosters rather than actual buyer purchases through GPOs to determine when buyers had "access" to a GPO. But as Professor Ordover acknowledges, such membership rosters are unavailable, so this amounts to making the claim impossible to prove by insisting on unavailable data.[48] Given this lack of membership rosters, whether a buyer was ever, in any prior month, deemed a member of a GPO in any firm's transactional sales data is clearly the best available indication of whether that buyer belonged to that GPO. Indeed, whether purchasers ever actually use GPO contracts by making purchases through them is actually a better indication of actual "access" than membership rosters. All buyers who purchase through a GPO contract have had "access" to the contract, while if one were to rely on a membership roster, that roster might erroneously (1) fail to list buyers that do have access or (2) list buyers as being members who actually are not and thus do not have access. In other words, relying on a membership roster would make the accuracy of the analysis turn on the accuracy of the membership roster, and there is no

relevant issue is how smoking affects people who smoke, here the issue is the effects of having an exclusionary contract, so the relevant issue is how purchasing through an exclusionary contract affects purchasing behavior.

[47] Tyco Reply 12-13.

[48] Ordover Reply Dec ¶¶12, ¶54 n.58, ¶56 & n.59.

reason to believe that a membership roster will provide a systematically more accurate indication of which buyers have access to a GPO than the actual purchasing data does.

26. Tyco's argument for using membership lists rather than membership information in the sales data is that hospitals can join new GPOs even while they continue purchasing under a different GPO contract.[49] But if buyers can join one GPO and buy through another, that would simply show that *all* hospitals have "access" to sole-source GPOs and to GPOs offering exclusionary terms, which makes applying the "access" approach nonsensical.[50] Tyco and Professor Ordover also argue that my analysis will wrongly treat as unburdened a hospital that, say, belongs to both a multi-source GPO and a sole-source GPO but who buys only through the multi-source GPO, while they claim that such a buyer should be treated as in the burdened group.[51] But my analysis would have assigned such buyers to the burdened group if there was any indication in the sales data that the buyer purchased through the sole-source GPO at any prior point in time.[52] Further, their argument is contrary to the approach Professor Ordover used in the *Masimo* case, because he there treated a buyer who belonged to one GPO with a sole-source contract and another GPO without one "as being an unrestricted hospital."[53]

27. Tyco and Professor Ordover also argue that, even if one accepts using the sales data to identify "access," the gaps in rival performance between the burdened and unburdened groups using their preferred "access" approach are smaller than those reported in my original Daubert declaration and in my comparisons that do not use the "access" approach. They cite two reasons.

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[49] Tyco Reply 13-14.
[50] Elhauge Dec ¶63.
[51] Tyco Reply at 14 n.16; Ordover Reply Dec ¶¶56-59.
[52] Elhauge Dec ¶66.
[53] Elhauge Dec ¶78 (citing testimony from Professor Ordover from the *Masimo* case on this point).

28. First, Professor Ordover says that the gap in the sole-source "access" comparisons in my original Daubert declaration would be smaller if I excluded from the analysis buyers who never utilized a GPO contract at all.[54] But although there are economic reasons to exclude these buyers when performing an analysis on how a buyer's actual contract status affects its purchasing behavior – as I did in my reports – it is incorrect to exclude these buyers when performing the alternative "access" analysis that Tyco and Professor Ordover prefer. Tyco and Professor Ordover's analysis is based on buyer "access" to contracts, not on actual contract utilization. Given that there is no evidence these direct buyers ever had "access" to a GPO contract in the sense that they mean, these buyers should be placed in the group that lacks "access" for purposes of running their "access" comparison.

29. Second, Professor Ordover and Tyco argue that the "access" gap is smaller than my original Daubert declaration reported because the "access" approach that I described in my declaration was not fully implemented due to an error in the computer programming that mistakenly classified customers as lacking access to Tyco share-based contracts if they bought exclusively from a rival through a multi-source GPO that offered Tyco share-based contracts.[55] However, re-running the analysis with this programming error corrected continues to show a significant gap in rival performance between the groups that do and do not have "access." Although smaller, the gap found with these corrections implemented is still similar to the gap in my non-"access" comparisons.

30. Indeed, neither Tyco nor Professor Ordover deny that if one performs the "access" analysis and incorporates all of Professor Ordover's modifications, a significant gap in rival

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[54] Ordover Reply Dec ¶¶63-65 & Table 2.

[55] Tyco Reply at 15; Ordover Reply Dec ¶¶66-74; Ordover Reply Dec 42-45.

performance continues to exist between the groups that do and do not have "access" to exclusionary contracts.[56] This *confirms* all my conclusions on anticompetitive impact. The resulting gap in the sole-source "access" comparisons is only 2% lower than the gap in my sole-source comparisons in most years, and the gaps in the share-based comparisons are as little as 1% different.[57]

31. Nor does the existence of a smaller gap indicate that the anticompetitive impact will be smaller under the "access" approach, because the "access" approach necessarily increases the size of the burdened group.[58] In assessing the anticompetitive impact, one must consider *both* the size of the gap and the size of the burdened group, meaning that even if the gap is smaller using the "access" method, the overall anticompetitive effect could be larger if the size of the burdened group increased sufficiently. To illustrate using hypothetical numbers, if switching to the "access" approach reduced the gap in rival performance between the unburdened and burdened segments from 50% to 40%, but increased the market share of the burdened group from 40% to 60%, then the total anticompetitive impact on rival sales with the non-access approach would be 20% (50% gap times 40% burdened share), but with the "access" approach would be 24% (40% gap times 60% burdened share). Tyco and Professor Ordover both completely ignore the fact that the size of the burdened group will increase using the "access" methodology, and thus their analysis cannot and does not establish that the anticompetitive impact from Tyco's contracts is smaller under the

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[56] *See* Ordover Reply Dec Table 2 (sole-source comparison showing gaps of 18-42%), Table 3 (share-based comparison showing gaps of 27-46%), Table 4 (sole-source and share-based comparison showing gaps of 37-60%), and Table 5 (sole-source or share-based comparison showing gaps of 26-53%).

[57] *See* Ordover Reply Dec Table 2 ("access" sole-source comparisons using his modifications compared to my original calculations show a difference of only 2% in four out of the six years); Table 3 ("access" share-based comparisons using his modifications compared to my original calculations show a difference of only 1% in 2003).

[58] The size of the burdened group necessarily increases when shifting to an "access" approach from an approach that looks at the actual contract status of buyers because using the latter method, only buyers that actually purchase under an exclusionary contract are placed in the burdened group, while in the "access" method, all of these same buyers who actually buy through the exclusionary contracts are placed in the burdened group, along with any other buyers who do not actually buy through the exclusionary contracts but still are deemed to have "access" to the contracts.

"access" approach.

32. Although as noted my opinions are limited to the fact of anticompetitive impact and I do not opine on the precise amount of impact, I understand that analysis performed by Dr. Singer using the figures from Professor Ordover's own "access" methodology to calculate damages shows that the amount of damages is in fact *greater* using the "access" methodology than it was using my methodology of assigning buyers to the burdened and unburdened groups based on their actual contract status.[59] This finding completely undermines Tyco and Professor Ordover's claim that implementing the "access" approach indicates a smaller anticompetitive impact because of the smaller gaps in rival performance. Instead, the data shows exactly the opposite: due to the larger size of the burdened group, applying Tyco's own "access" approach leads to a *larger* anticompetitive impact.

33. To summarize, Tyco and Professor Ordover's "access" analysis does not undermine my conclusions for three independent reasons. First, Tyco's "access" approach is economically incorrect; instead, my approach of looking at the actual contract status of buyers is the right approach. Second, even if one believed the "access" approach were the correct approach, the "access" approach implemented with all of Professor Ordover's modifications continues to find a significant gap in rival performance, which *confirms* my conclusion that Tyco's contracts had an anticompetitive impact. And third, even if one thought that a smaller but still significant gap somehow undermined the evidence of impact, Tyco and Professor Ordover ignore the fact that the size of the burdened group increases with the "access" approach, and that when this is taken into account, the marketwide anticompetitive impact is *even larger* using Professor Ordover's own

[59] Specifically, comparing Dr. Singer's results using the "access" method and his results performed in March 2008 using output from my methodology, the aggregate damages increased by $0.5 million. *See* Singer Dec ¶5.

calculations of the gaps under the "access" approach than using my original approach, again *confirming* my conclusions.

VII. ECONOMIES OF SCALE

34. Professor McFadden now admits that his own economies of scale regression would have confirmed economies of scale at a 99% confidence level if he had included Tyco's top ten selling models.[60] But he now claims that he made a "deliberate" choice to state his conclusion as being that there is no "consistent" statistical evidence of Tyco economies of scale at "a conventional level of statistical confidence" because when he excluded the top ten selling products he found economies of scale at a 90% confidence level, not the 95% confidence level he had in mind.[61] But a 90% confidence level is clearly a conventional level of statistical confidence – indeed, it is one that Professor McFadden himself has used in his academic work – so his statement in his original declaration remains inaccurate. It is also deceptive that his declaration did not reveal either (1) that he excluded the top selling products in an economies of scale study, or (2) what confidence level he had in mind.[62] Nor is there any support for Professor McFadden's claim that his analysis was based on running "the test both including and excluding the top ten selling products."[63] There is no mention in his declaration of either including or excluding the top ten selling products, and the computer programs Professor McFadden produced as backup to his report did *not* run this regression

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[60] McFadden Reply Dec ¶22.
[61] McFadden Reply Dec ¶22.
[62] As already noted above, *see infra* ¶14, Professor McFadden nowhere offers any explanation for why he demands a 95% confidence level here even though he has used a 90% confidence level in his academic work.
[63] McFadden Reply Dec ¶22.

including the top ten selling products.[64] Professor McFadden is also wrong in his assertion that it is proper to exclude the biggest selling products in an economies of scale study, because those products are precisely the ones over which a researcher would most expect to find economies of scale.

35. Professor McFadden's reply declaration also never responds to my other observations on why his model-by-model regression was biased against finding statistical significance and how alternative regression methods would confirm the existence of economies of scale.[65] Nor do Tyco or its experts dispute the fact that Professor Ordover and I actually agree on economies of scale, and Professor McFadden offers no affirmative position to the contrary.[66] Nor do they dispute that all the other evidence supports my conclusions on economies of scale.[67] There thus appears to be no actual dispute over any of the conclusions I reached about economies of scale.

VIII. MY QUALIFICATIONS AND ECONOMIC THEORIES

36. Tyco and its experts offer no response to (a) the point that all my conclusions would continue to hold without any regressions,[68] and (b) my showing that my theories on loyalty discounts comport with the peer-reviewed economics scholarship.[69] For reasons previously explained, Tyco's claims that my only expertise and experience are legal remain erroneous,[70] as does Tyco's

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[64] See the Stata program called Paragraph 32.do produced as part of Professor McFadden's backup. As noted in my original Daubert declaration, *see* Elhauge Dec ¶88 n.115, the code that performs his regression in this program excluded the top ten selling products. No other line of code performs this regression including the top ten products.

[65] Elhauge Dec ¶¶85-87, 89-90.

[66] Elhauge Dec ¶83.

[67] Elhauge Dec ¶91.

[68] Elhauge Dec ¶¶101-106.

[69] Elhauge Dec ¶¶107-120.

[70] Elhauge Dec ¶¶93, 98.

characterization of prior court decisions, which in fact all *admitted* my testimony.[71]

* * *

I declare under penalty of perjury that the foregoing is true and correct. Executed December 9, 2008.

Einer Elhauge

[71] Elhauge Dec ¶¶94-95.