

IN THE UNITED STATES DISTRICT COURT
FOR THE 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,

v.

TYCO INTERNATIONAL, LTD.; TYCO
INTERNATIONAL (US) INC.; TYCO
HEALTHCARE GROUP LP; THE
KENDALL HEALTHCARE PRODUCTS
COMPANY,

Defendants.

§
§
§
§
§
§
§
§
§
§
§
§
§
§
§
§
§
§
§
§
§
§

Civil Action No. 05-12024 PBS

JURY TRIAL DEMANDED

**REPLY DECLARATION OF DANIEL L. MCFADDEN IN SUPPORT OF
REPLY BRIEF IN SUPPORT OF THE MOTION TO EXCLUDE THE
EXPERT REPORT AND OPINIONS OF PROFESSOR EINER ELHAUGE**

REPLY DECLARATION

OF DANIEL L. MCFADDEN

IN NATCHITOCHEs et al. vs. TYCO INTERNATIONAL et al.

Prepared on Behalf of Covidien

The Brattle Group, Inc.

353 Sacramento Street, Suite 1140

San Francisco, CA 94111-3657

v: (415) 217.1000

f: (415) 217.1099

November 26, 2008

D) INTRODUCTION

1. My name is Daniel L. McFadden. I have recently written a declaration in the *Natchitoches vs. Tyco International* matter in which I opined on the statistical and graphical analysis performed by Professor Elhauge to measure the impact of certain Covidien contract provisions with Group Purchasing Organizations (GPOs) and individual hospitals. Professor Elhauge has recently filed a declaration (hereinafter the “Elhauge Declaration”) that responds to my comments. I have been asked by counsel for Covidien to review Professor Elhauge’s amended analysis and to comment as appropriate. I refer to my previous declaration (hereafter the “McFadden Declaration”) in this matter for my qualifications and background of the case.
2. Professor Elhauge in paragraph 100 of his Declaration asserts that: “Tyco offers a declaration from Professor McFadden who has expertise in econometrics, but no apparent expertise in applying econometrics to antitrust issues, which probably explains why (as I explain above) he makes errors that reflect basic conceptual misunderstandings about antitrust economics and the issues raised in this case.” While my critique of Professor Elhauge’s expert report is limited to the scientific foundations of the graphical and statistical analysis he does in support of his arguments, antitrust economics is an element of this critique to the extent it identifies the purpose of his quantitative investigations and affects his interpretation of results. Critical questions in evaluating Professor Elhauge’s analysis econometrically are what the benchmark should be for market outcomes “but for” alleged anti-competitive conduct, and whether his economic analysis establishes harm relative to the appropriate benchmark. I believe that I have ample, relevant experience to opine on these questions. Since 1963, I have regularly taught the economic theory of markets at the graduate level, and given specialized courses on game theory and the theory of auctions and competitive bidding. I have 38 years of experience in offering expert opinion on antitrust matters, notably in the cases of *CBS v. ASCAP*, *DOJ v. AT&T*, *Netscape v. Microsoft*, and *Sun Microsystems v. Microsoft*. During 1995 and 1996, I served as a consultant to the Antitrust Division of the U.S. Department of Justice on the analysis of anti-competitive impacts of proposed mergers.

3. In the following sections, I summarize my findings on Professor Elhauge's amended analysis, discuss his specific comments and finally offer a brief conclusion.

II) SUMMARY OF FINDINGS

4. In the context of a claim of anticompetitive behavior, the econometric analysis is part of a scientific experiment to identify the existence or lack of impact. For the experiment to be meaningful, at least three criteria must be satisfied. First, there has to be a well-defined baseline against which impact is measured. Second, the model used to measure impact must control for relevant economic factors, such as price or consumer preferences, that would confound the measurement of impact with alleged anti-competitive behavior. Third, the execution of the econometric analysis must follow accepted econometric practice. An econometric analysis that fails on any of the three dimensions is of no probative value. Professor Elhauge's response to my declaration has focused on my comments regarding the failure of his econometric analysis to adhere to basic elements of proper econometric analysis. However, I should re-emphasize that Professor Elhauge's analysis fails on the first two criteria, because his analysis is not measured against a baseline of allowable competitive behavior and fails to account for confounding factors. Therefore, even if Professor Elhauge had followed proper econometric practice his analysis would possess no power to determine whether anti-competitive actions had occurred.
5. In terms of the first two criteria, Professor Elhauge's statistical and graphical analysis does not conform to the most elementary principles of the scientific method. His methodology fails to establish a workably competitive baseline against which allegedly anti-competitive conduct can be assessed, and is thus unable to distinguish between the effects of pro-competitive price competition and anti-competitive conduct. Professor Elhauge also violates a fundamental element of the scientific method, namely that the design and conduct of experiments be performed to control for confounding factors, such as prices offered by suppliers, whose impacts can mistakenly be attributed to the factor of interest. His analysis is consequently unable to address a key issue in this case, whether the alleged anti-competitive conduct by Covidien harmed consumers.

6. Regarding the third criteria, in my original Declaration I identified several serious deficiencies in Professor Elhauge's analysis that violate basic econometric standards. For instance, I found that Professor Elhauge's analysis of market shares discarded the majority of relevant observations, namely all observations where Covidien's rivals had zero market shares. Furthermore, I found that he failed to use statistical methods appropriate to analysis of the selected data that remained. I also found that his results were contaminated by selection bias, and that his analysis of whether economies of scale exist for sharps containers contained serious technical and statistical errors that nullified his conclusions. In short, I concluded that Professor Elhauge's methods failed to address the key issues on which he offered conclusions, and violated basic, sound econometric standards, which rendered his findings unreliable and incomplete. All of these deficiencies remain in Professor Elhauge's revised work.
7. Therefore I conclude that Professor Elhauge's statistical and graphical analysis remains flawed and unreliable. His analysis does not meet acceptable scientific standards, and does not support his conclusions regarding foreclosure in the market for sharps containers due to Covidien contracting practices.

III) A FUNDAMENTAL SCIENTIFIC FLAW IN PROFESSOR ELHAUGE'S METHODOLOGY

8. The methodology adopted by Professor Elhauge is biased toward finding an anti-competitive impact even if none is present. An example illustrates the logical point. Suppose GPOs follow the practice of soliciting bids for a product and awarding contracts to the sellers who are within a narrow band of the minimum bid. If there is only one seller in this band, that seller receives a sole-source contract; if there are two sellers in the band, they each receive a dual-source contract, and if there are more than two in the band, they receive a multi-source contract. For the example, suppose hospitals divide their purchases equally among the successful sellers, and buy nothing from the unsuccessful sellers. Now consider a situation in which the sellers all behave as pure competitors, simply making the lowest bids they can, given their costs. Then, observed market outcomes will be a 100 percent share for firms who receive a sole-source contract, a 50 percent share for firms who receive a dual-source contract, and a lower share for firms

who receive a multi-source contract. Applying Professor Elhauge's criterion to these data would lead to the conclusion that the winners of sole-source contracts in this situation are gaining market share by doing so, and thereby foreclosing the market to rivals. It is true that the market share of rivals is reduced when they fail to qualify in the band of low cost bidders, but in this situation this is a pro-competitive exclusion of rivals whose prices are too high, not the result of anti-competitive behavior by the winning bidders. Professor Elhauge's criterion in this case thus gives a "false positive" for anti-competitive conduct. His methodology is flawed because it cannot distinguish this pro-competitive situation from an alternative situation where there is anti-competitive conduct by a seller that excludes rivals who would have been successful absent the anti-competitive conduct. Contracting practices by GPOs may be considerably more complicated than the simple example, but the point of the example still applies that contracting outcomes such as sole-source contracts may be a consequence of pro-competitive behavior.

IV) PROFESSOR ELHAUGE'S REVISED ECONOMETRIC ANALYSIS

9. In my original declaration, I stated that by discarding the vast majority of the available data on market shares in hospitals, Professor Elhauge's analysis introduced a serious bias in his results.¹ His response to this criticism has been an invalid econometric justification for his original logarithmic model, upon which he still relies for his conclusions,² and several new analyses that purport to utilize all of the available data, but which still suffer from a number of severe methodological flaws. In this new analysis, Professor Elhauge includes not only hundreds of thousands of observations in which the rivals' shares are zero, but also tens of thousands of additional observations, not previously under consideration, in which the rivals' shares are 100 percent. As I discuss below, Professor Elhauge has not addressed the issue of data inclusion or exclusion in an acceptable econometric manner.

¹ McFadden Declaration ¶ 24.

² Elhauge Declaration Table 1B ¶ 56.

A. PROFESSOR ELHAUGE OFFERS NO VALID RATIONALE FOR DISCARDING THE GREAT MAJORITY OF AVAILABLE DATA

10. Professor Elhauge does not refute the criticism that his original logarithmic regression ignores the majority of the available data. Instead, he seeks to defend the use of the natural logarithm in his regression analysis by citing two of its supposed virtues: the logarithmic regression “plausibly assumes that the rivals’ market share changes multiplicatively” and, by not including observations from those hospitals with zero share for Covidien, the logarithmic regression focuses on those buyers able and willing to buy from both Covidien and rivals thus limiting the impact of selection bias.³ From an econometric standpoint, neither of these reasons justifies ignoring the statistical consequences of dropping relevant observations.
11. At the outset, Professor Elhauge’s assumption of a logarithmic specification does not make economic sense and is not consistent with the data. For example, the logarithmic form is unrealistic in that it will predict more than a 100 percent share for some hospitals⁴ and implies that a hospital that buys only Covidien products under a restricted contract will always buy only Covidien products. Moreover contrary to Professor Elhauge’s claim,⁵ I have not opined on the correct functional relationship between changes in rivals’ shares and restricted contract status. In my Declaration, I state explicitly that I introduce the linear alternative solely to demonstrate the sensitivity of regression analysis to the omission of the zero rival share observations. I do not claim that the linear model is proper for the econometric analysis at issue in this case.
12. Professor Elhauge’s logarithmic model excludes all observations from purchasers who only ever bought from Covidien rivals and excludes observations in each month in which a purchaser had a zero share for Covidien rivals.⁶ He justifies this practice by saying it is

³ Elhauge Declaration, ¶ 49.

⁴ In Professor Elhauge’s Table 1B up to 2.2 percent of the unrestricted hospitals would be forecast to have shares in excess of 100 percent.

⁵ Elhauge Declaration, ¶ 48 .

⁶ Professor Elhauge in his original analysis does, however, collapse all of the observations from hospitals that only ever purchased from Covidien rivals into a single observation with a 100 percent rival share and he assumes that observation has an unrestricted contract. He makes no statistical adjustment to account for the affect of aggregation on the precision of this observation. In his reply Declaration, Professor Elhauge reverses this observation and includes the tens of thousands of 100 percent share observations.

“evenhandedly focused on those buyers that purchased some containers from both Tyco and rivals.”⁷ He asserts that this reduces the effects of selection bias. There are two distinct problems with Professor Elhauge’s justification for excluding these observations. First, there are statistical reasons to expect that excluding the zero and 100 percent share observations will bias the regression results and lead to erroneous conclusions. Second, there is no reason to expect that selection bias is either limited to or pervasive throughout the hospital population that purchases either 100 percent Covidien or rivals’ products.⁸

B. PROFESSOR ELHAUGE’S NEW REGRESSIONS CONTINUE TO SUFFER FROM SEVERE METHODOLOGICAL ERRORS

13. In his Declaration, Professor Elhauge reports new regression analyses of rivals’ market shares in which he no longer excludes data from hospitals that have zero rivals’ share and *includes* new observations from hospitals that never purchased Covidien sharps containers.⁹ The results from these new regressions confirm, as shown in Table 1 below, the sensitivity of Professor Elhauge’s models to the inclusion of all relevant data. By including all of the data, Professor Elhauge himself has calculated a series of linear regressions that yield impact results that vary considerably from the logarithmic regressions. In five of the eight linear regressions without fixed effects, the alleged foreclosure falls by over 40 percent. These results demonstrate that the coefficients and percentage impacts that Professor Elhauge reports are highly sensitive to the model specification and to the inclusion or exclusion of data. His revised findings continue to be unsound and unreliable.

14. Professor Elhauge asserts that observations for hospitals that only ever bought from

⁷ Elhauge Declaration, ¶ 48.

⁸ Accordingly, to the extent that Professor Elhauge quantitatively and qualitatively determined that the logarithmic regression was the better econometric method, he still should not have ignored the consequence of discarding economically relevant data his regression required, as there are several econometric methods for dealing with and overcoming such data problems, such as Tobin’s and Heckman’s methods for truncated and censored data. Professor Elhauge has not used any of these textbook methods to control for the effects of omitting data, and his Declaration indicates no awareness that data truncation is a substantial statistical issue in his analysis.

⁹ Previously Professor Elhauge had collapsed data from all hospitals without Covidien customer identification numbers -- which numbered in the thousands -- into a single observation. Hence they were technically included in his prior analysis, but in a manner that failed to take into account the impact of this aggregation on the accuracy of the observation and the weight it should receive in the regression. He now includes separate observations for each hospital by separating them into separate observations instead of

Covidien rivals should not be omitted, and criticizes the linear regressions in my declaration for excluding these data.¹⁰ Generally, I agree that all relevant data should be included in a regression. However, Professor Elhauge categorizes all of these observations as associated with unrestricted contracts. Professor Ordoover has indicated that a number of observations that Professor Elhauge categorizes as restricted were for hospitals that were members of GPOs that, in fact, had access to Covidien sole-source contracts.¹¹ In that event, a number of these observations would be misclassified. This misclassification would create a bias toward finding an impact from restricted contracts.

15. Professor Elhauge similarly does not control for basic supply and demand factors that govern economic outcomes. This is a fundamental flaw. An analysis that ignores relevant supply and demand factors is not suitable for valid inferences on the effects of Covidien contracts. Professor Elhauge states that because Covidien's prices are part of the allegation of anti-competitive conduct, relative prices are irrelevant to the analysis.¹² Professor Elhauge, in effect assumes, without a test, that the entire price differential is due to anti-competitive conduct. Accepting that price determination is part of the challenged conduct, it is still necessary to estimate what prices *would have been* under permissible conduct in the bidding for GPO contracts. The inability of Professor Elhauge's analysis to distinguish between competitive conduct and conduct that is anti-competitive, rather than a careful allocation of effects to variations in buyer tastes, legitimate price competition, and allegedly anti-competitive conduct, renders his conclusions of no probative value.
16. Professor Elhauge has access to price data for sharps containers and could have undertaken an analysis of price formation in the sharps container market. This analysis is necessary because all economists recognize that prices are a key determinant of buyers' demands. In Table 1 of my original declaration, I demonstrated the feasibility of developing useful price data by calculating prices for restricted versus unrestricted sales

collapsing them into a single observation.

¹⁰ Professor Elhauge's original analysis aggregated these observations into a single monthly observation. I used Professor Elhauge's data as it appeared in his working files, and had not previously retrieved the individual hospital observations with 100 percent rival share that were behind his aggregate observation.

¹¹ Ordoover Declaration, ¶ 54.

¹² Elhauge Declaration, ¶ 75.

of four Covidien products that span much of the size spectrum of Covidien's containers. Given such data, it should be possible econometrically to estimate buyer response to relative prices. Professor Elhauge has not undertaken such an analysis, and as a result his study of the sharps container market fails to separate the effects of price differences that would appear under competitive conditions from the effects of the activities that he alleges are exclusionary.

17. In my Declaration, I also suggested that Professor Elhauge needed to control for preferences for sharps containers that could vary across hospitals. Econometrically these differences are controlled for by what are called fixed effects. Professor Elhauge states that controlling for fixed effects would cause him essentially to lose the information from hospitals that do not change contract status during the period covered by the data.¹³ In other words, one cannot determine whether the Covidien share of those hospitals' purchases is due to the contract status or the particular preferences of that hospital for sharps containers. He prefers to include the information from those hospitals without controlling for fixed effects, essentially assuming that there are no differences in hospital preferences. In reality, when a buyer buys Covidien products through a challenged contract throughout the observation period, we cannot know whether it is because of the contract or because the buyer simply prefers Covidien.¹⁴ Professor Elhauge should not simply assume this fact away. His model that does so is plainly improper. Professor Elhauge's results in which he includes hospital fixed effects suggests they are being confounded with his measure of foreclosure. Results from Professor Elhauge's Report and Declaration are shown in Table 1. The results show that Professor Elhauge's estimates of impact are highly sensitive to the inclusion of variables that control for variations in buyer preferences and functional form. Again, I do not endorse any of the models in Table 1 as complete or correct, but use them to emphasize that Professor Elhauge's results are highly sensitive to variations in buyer preferences and to the omission of data.

¹³ Ibid., ¶ 55.

¹⁴ Although such a hospital's data will not contribute to the identification of the contract coefficient, it will help identify the monthly fixed effects and should therefore not be dropped.

Table 1: Various Estimates of Foreclosure from Professor Elhaug's Regressions

	Percentage Impact of "Affected" Contracts on Rivals' Share			
	Elhaug Report	Elhaug Declaration		
Functional Form	Log	Linear	Log (fixed effects)	Linear (fixed effects)
Zero Percent Rival Shares Omitted?	Yes	No	Yes	No
Hospitals with No Covidien Number Omitted?	Yes	No	No	No
Regressions Including All Hospitals				
Sole-Source vs. Not Sole-Source	-41%	-57%	-33%	-11%
Restricted vs. Unrestricted	-78%	-37%	-36%	-11%
Restricted and Sole-Source vs. Neither	-48%	-51%	-27%	-7%
Restricted or Sole-Source vs. Neither	-72%	-38%	-41%	-14%
Regressions Including Only Hospitals Which Changed Status				
Sole-Source vs. Not Sole-Source	-21%	-22%	-33%	-13%
Restricted vs. Unrestricted	-60%	-22%	-37%	-14%
Restricted and Sole-Source vs. Neither	-31%	-16%	-28%	-9%
Restricted or Sole-Source vs. Neither	-41%	-24%	-43%	-18%

Note: Linear coefficients were converted to share impacts by calculating a sales-weighted average of the ratio of the coefficient on contract status to rivals' share at each hospital in the unburdened category. Log share impacts are from Elhaug Report and Elhaug Declaration.

C. PROFESSOR ELHAUGE'S GRAPHICAL AND REGRESSION ANALYSIS OF MARKET SHARE IS FLAWED AND INCOMPLETE

18. In my previous Declaration, I discussed two tests I performed to determine whether there was a statistically significant increase in the growth of rivals' market share after the change in Novation's contract from sole-source to multi-source in August 2005.¹⁵ The first of these tests was based on data for the period October 2001 through October 2006. The second was based on data from October 2003 through May 2007. In both of these tests I did not find a statistically significant increase in the growth of rivals' share at a 95 percent confidence level. In paragraph 26 of his Declaration, Professor Elhaug claims I do not perform the regression I described in my discussion of the Novation contract change analysis, and he claims that, if I had run the regression I described, I would find a statistically significant change in rivals' growth rates after the change in the Novation contract. In fact the text of my Declaration was based on the two tests I performed. When either Professor Elhaug's time period or the periods I used are examined, the change in rivals' share is not significant at the 95 percent confidence level

¹⁵ McFadden Declaration, ¶ 21.

most typically used in econometric analysis.¹⁶

19. When Professor Elhauge applies a three-month moving average approach to the analysis of rivals' market share at Novation he obtains much higher levels of statistical significance.¹⁷ However, his conclusion here violates the Gauss-Markov theorem, the foundation of regression analysis. A consequence of the Gauss-Markov theorem is that when an original least squares regression is best estimated by ordinary least squares, no linear transformation of the observations, such as forming moving averages, can improve the precision of estimates. Professor Elhauge's specious moving averages regression should be excluded from an examination of his Novation claims.
20. Moreover, as I stated in my original Declaration, this analysis fails to control for relative prices and other relevant factors that may also affect rivals' market share at Novation member hospitals. In particular if the switch by Novation from sole-source to multiple-source contracting came from the product preferences of its hospital members, reflecting increasing tastes for the products of Covidien's rivals, then in the terminology of econometrics, the contract status indicator is endogenous, and ordinary least squares estimation is no longer valid. I have made no formal test of this possibility. However, examination of Professor Elhauge's revised Exhibit 17 from his deposition shows that rival shares grew substantially after October 2003 and before the change to multi-source contracting, raising the possibility that it was this penetration of the market by rivals that triggered the change in contracting, rather than the other way around. I note that Covidien prices increased for the ten most popular products after the change in contract status;¹⁸ other things equal, this would cause a loss in Covidien market share that should be accounted for in the regression.

¹⁶ Professor Elhauge has made reference to my work in which I use a 90 percent significance level. In that paper, I use 90 percent level for circumstances that do not apply here. Also in that paper, I use the 95 and 99 percent confidence levels for circumstances that do apply here. I believe the 95 percent significance level to be appropriate in this case. I note that Professor Elhauge used the 95 percent level in Table 9 of his expert report.

¹⁷ Elhauge Declaration, ¶ 28.

¹⁸ See Ordover Expert Report, ¶ 121.

D. PROFESSOR ELHAUGE CONTINUES TO IGNORE SELECTION BIAS

21. Professor Elhauge has failed to address my comments relating to selection bias. The core of Professor Elhauge’s analysis is that, for his definition of the group of hospitals that were restricted by Covidien contracts, rivals’ market share was lower than for the group of hospitals that were unrestricted. Professor Elhauge assigns hospitals to the restricted class if they buy sharps containers from Covidien through a challenged GPO contract and assigns them to the unrestricted class if they do not. In essence, Professor Elhauge has demonstrated that Covidien has a larger market share at hospitals where Covidien makes sales. Because the assignment is based on hospital behavior, Professor Elhauge’s analysis is corrupted by selection bias. No reliable inferences on the effects of the challenged contracts can be drawn from his experimental design. None of Professor Elhauge’s adjustments address this fundamental shortcoming.

E. PROFESSOR ELHAUGE CONTINUES TO MAKE ERRORS IN HIS ANALYSIS OF ECONOMIES OF SCALE

22. My comments from my original Declaration regarding economies of scale address Professor Elhauge’s methodology. I did not and am not drawing any conclusions on the actual returns to scale of Covidien production, contrary to Professor Elhauge’s claims in his Declaration. He claims that a regression of costs that include product-specific scale effects is biased against finding “statistical significance.”¹⁹ As reported in my declaration, the hypothesis of a common scale factor for all products is decisively rejected. Given that result, it is statistically inconsistent to proceed as Professor Elhauge does to test that a common coefficient is zero. Professor Elhauge states my declaration “deceptively mischaracterizes”²⁰ the results of tests I performed on the aggregate scale of Covidien production because my backup material shows the test would be significant at the 90 percent level and the fact that test is significant at the 99 percent level when the test is run for all products. The language of my Declaration, that there is “no consistent

¹⁹ Elhauge Declaration, ¶ 84.

²⁰ Ibid.

statistical evidence for returns to scale,” was deliberate. I run the test both including and excluding the top ten selling products. If there are returns to scale that are exhausted at sufficiently large scale, as Professor Elhauge suggests and I consider economically reasonable, then including the ten largest selling products should weigh against finding returns to scale. In fact the opposite is true. The regression that omits Covidien’s ten largest selling products fails to reject the hypothesis of no returns to scale for the overall manufacturing of sharps containers at the 95 percent confidence level, while the regression that includes the ten largest selling products *does* reject the hypothesis of no returns to scale at 99 percent confidence. I consider this to be evidence that some factors other than returns to scale are influencing these data, and these results are not giving consistent statistical evidence for or against returns to scale.

V) CONCLUSION

23. I continue to find that Professor Elhauge’s empirical analysis contains serious technical and statistical errors that invalidate his conclusions and render them of no probative value in support of his claims that certain contractual forms have an exclusionary impact.

I hereby declare under penalty of perjury under the laws of the United States that the foregoing is true and correct.

Executed on this day November 26, 2008 in Berkeley, California.

Daniel McFadden
