UNITED STATES DISTRICT COURT
SOUTHERN DISTRICT OF NEW YORK

IN RE: ELECTRONIC BOOKS ANTITRUST LITIGATION	11 MD 2293 (DLC)

THE STATE OF TEXAS, et al.,
                              Plaintiffs
 	
                        v.
 	
 PENGUIN GROUP, USA, et al.,
                              Defendants
	12 Civ. 3394 (DLC)

REPLY DECLARATION OF ROGER G. NOLL

My name is Roger G. Noll, and I reside in Palo Alto California. Previously I submitted the *Declaration of Roger G. Noll* on October 11, 2013, and the *Corrected Declaration of Roger G. Noll* on October 18, 2013 (henceforth *Noll Report* refers to the corrected version) in this matter. The *Noll Report* includes my biographical information, including participation in other proceedings within the past five years.

ASSIGNMENT

Attorneys for the plaintiffs in this litigation have asked me to review the *Declaration of Joseph P. Kalt, Ph. D.* (henceforth *Kalt Report*) and the *Declaration of Jonathan Orszag* (henceforth *Orszag Report*) to determine whether any of the analysis and evidence in these reports causes me to alter the analysis and conclusions in the *Noll*

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Report. In undertaking this assignment I have been assisted by economists at Ashenfelter and Ashmore.

SUMMARY AND CONCLUSIONS

The *Kalt Report* and the *Orszag Report* make three general arguments. First, defendant's experts argue that prices for e-books do not exhibit sufficient price stability, either among books or for the same book over time, to permit estimation of a reliable econometric model of the effect of price collusion in e-books. Second, defendant's experts argue that the econometric model in the *Noll Report* is improperly specified (variables that affect price are missing), is estimated using data that ought to be excluded, is based on excessively broad average prices, and yields unreliable and impermissible average damages. Third, defendant's experts argue that even if the damages model were reliable in estimating the effects of collusion on e-book prices, the model overstates damages because it does not take into account other consequences of collusion, such as price cuts in e-readers, the entry of the iBookstore, the preservation of Barnes & Noble as a competitor, and increases in e-book prices that would have occurred anyway, regardless of the presence of price collusion.

Having reviewed the reports of defendant's economic experts, I conclude that their conclusions are not based on correct applications of economic analysis and that the data analysis that they present in support of their arguments contains so many errors that the results are incorrect and unreliable. The purpose of this report is to explain the basis for my rejection of the arguments by defendant's experts. Because the reports by defendant's experts are long and overlapping, I do not attempt to deal with every minute

detail of each report. Here I focus on broad issues concerning the applications of economic theory and the methods for analyzing data that the defendants' experts adopt. This section summarizes my evaluation of these reports, and the remaining sections provide a more detailed discussion of the basis for my conclusions.

Price Variability

The *Kalt Report* contains numerous exhibits that make two points about the behavior of e-book prices. The first is that e-book prices show "diversity" (e-books in the same category have different prices) and "churn" (the price for the same e-book varies through time). The second is that the prices of a large number of e-books either did not rise or fell after the implementation of collusive agency pricing.

Professor Kalt's empirical methods for demonstrating both points are riddled with methodological and calculation errors that cause the effects he is attempting to prove to be vastly overstated. First, much of Professor Kalt's analysis is based on tracking the behavior of modal prices, which are inherently less reliable as indicators of the trends in overall prices than other measures. Second, many of the price comparisons by Professor Kalt do not take into account the effects of changes in conditions that affect price and that are incorporated in my econometric model. Third, Professor Kalt's comparisons of prices before and after the adoption of the agency model misidentify the date on which the agency model began to apply to each e-retailer, and as a result a large fraction of his "before-after" price comparisons actually are comparisons of two "afters." Fourth, Professor Kalt's correlation analysis of prices within an e-book category overstates the extent to which prices exhibit independence by eliminating a large fraction of e-book

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titles from the data simply because their prices did not change. One cannot test whether prices of e-books diverge from each other on a data set that eliminates all of the observations with no divergence.

Econometric Modeling

Both the *Kalt Report* and the *Orszag Report* criticize the regression analysis in the *Noll Report*. One criticism is that the model contains too few explanatory variables and calculates average damages that mask substantial variation in damages among consumers who bought different books in the same category or the same book at different times. The other criticism is that the regression is based on data that should have been excluded.

Specification

Professor Kalt argues that the regression in the *Noll Report* is an average damages model that assumes commonality of damages and so represents the type of model that the American Bar Association has concluded is unreliable. Professor Kalt's discussion mischaracterizes both the content of the ABA report that he cites and the complexity of the regression model that was estimated in the *Noll Report*.

The regression model in the *Noll Report* contains variables that represent differences in market conditions among e-books, dividing these e-books into over 700 categories, of which the defendant publishers sell e-books in over 500. Professor Kalt criticizes the model for not including variables that could explain the popularity of a specific title, such as the author, the quality of reviews, and promotional expenses, but this criticism is misplaced because the model also includes an indicator variable for each

specific title that accounts for idiosyncratic features of each book.

The damages model does not calculate average damages for e-books within a category, but the average percentage mark-up for e-books in that category due to the adoption of the agency model. Damages for a particular title in one of the 500 categories are the price of the title multiplied by the damage percentage for that category.

Data Issues

Both Mr. Orszag and Professor Kalt criticize the data that were used in my regression analysis. Professor Kalt focuses on the use of average prices as the dependent variable, and Mr. Orszag focuses on transaction records that he believes ought to be excluded from the regression.

Average Prices

Professor Kalt criticizes my use of four-week average price for an e-book title as the unit of observation in the regression. The basis for this criticism is that the use of averages masks variation in prices during that four-week period. Whereas I agree that regression analysis is likely to produce better results if all of the data are used and not compressed into averages, great computational power is required to undertake a regression analysis of a data set like the e-book transaction records that contains an enormous number of observations. If after compression to averages the data set remains very large, the likely effects of such short-cuts are unlikely to be of great importance.

In the extra time since the *Noll Report* was submitted, two additional regressions have been estimated with the same independent variables but with different dependent

variables. One regression uses weekly average prices for each e-book title, and one uses prices from individual transaction records. The results of these regressions are similar but not identical. The regression based on individual transactions is preferred. The amount of total damages that is calculated from this model is $280,254,374 and the fraction of e-book sales for which the model finds no damages is 0.2%.

Data Exclusions

Mr. Orszag criticizes the *Noll Report* for including transactions records involving other commercial publishers, self-publishers, data from the first three months of 2010, and transactions data from the iBookstore and Barnes & Noble. In general, excluding data from an analysis is a questionable procedure, and in each of these cases Mr. Orszag provides no valid reason for these exclusions.

Mr. Orszag argues that other publishers should be excluded because they are small and specialized. There is no valid basis in economics for excluding firms from an analysis of competitive effects in a market on this basis. These firms account for more sales than any of the defendant publishers. The issue of whether, within one of the 500 e-book categories in which the defendant publishers offer e-books, independent publishers offer e-books that are competitive substitutes for e-books from the defendant publishers does not hinge on whether the independent publisher sells e-books in other categories.

Mr. Orszag and Professor Kalt argue that self-publishers and the iBookstore should be excluded from the analysis. Mr. Orszag also claims that data from Barnes & Noble should be excluded as well.

According to defendants' experts, the iBookstore would not have entered, and

Barnes & Noble would have gone out of business, had the defendants not forced all e-retailers to adopt the collusive agency model. Mr. Orszag and Professor Kalt also attribute the growth in self-publishing and the fall in the average price of self-published books in part to the higher prices for e-books from the publisher defendants (encouraging entry) and in part to Apple's support for self-publishers.

The facts show that the iBookstore is not remotely close to a leader in e-book sales or in the promotion of self-publishing. Moreover, Mr. Orszag's calculations that are intended to show that nearly all purchases from the iBookstore would not have switched to other vendors have no basis in fact and so are unreliable. Likewise, Mr. Orszag's assertion that Barnes & Noble would have left the industry is not based on facts or economic analysis, and in any case is not accompanied by evidence that sales by Barnes & Noble would not have switched to other e-retailers.

Offsets

Defendants' economic experts argue that the damages to consumers from price collusion by the defendants should be reduced to take into account two other factors. First, both Mr. Orszag and Professor Kalt claim that price collusion by the defendants caused a fall in the price of e-readers, and calculate that this amount for the Kindle alone is ███████████████ of Amazon's revenues from the sale of e-books. Second, Mr. Orszag claims that in the absence of price collusion by the defendants, Amazon would have raised e-book prices anyway.

These arguments are based on a narrow and incorrect application of the economic theory of complementary goods – i.e., goods that are used together and for which an

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increase in the price of one caused a reduction in the demand for both. Moreover, the explanations by defendants' experts for why these effects would occur are mutually inconsistent: collusion in e-books causes price declines in e-readers, but increased competition in e-readers causes price increases in e-books. In analyzing these issues, defendants' experts ignore all other factors that affect price in both markets, including the extent of competition in e-books if the market were unaffected by collusion and other factors that affect e-reader prices, such as advances in technology.

The facts about both markets do not satisfy the conditions that must be true for either of the price effects that are predicted by defendants' experts to be valid. Most importantly, despite the collusive price increases in e-books, the demand for e-books continued to grow extremely rapidly after the agency model was adopted, so there was no suppression of demand for e-readers to cause a price reduction. Moreover, the analysis by defendant's experts depends on the joint use of e-readers and e-books from the same vendor. The facts of both markets are inconsistent with that theory. Kindle, which is the focus of the analysis by defendant's experts, is not necessary for reading an e-book from Amazon on a portable device, and a Kindle owner is not tethered to Amazon as a source of e-books. Indeed, neither is an iPad owner tethered to the iBookstore. Thus, there is no basis in theory or fact for believing either of these arguments.

In the case of e-readers, even if the arguments of defendant's experts about the causality between collusion in e-books and prices in e-readers were true, this argument would not justify subtracting the decline in e-reader prices from damages in the e-book market. The fall in e-reader prices arises because e-readers are less valuable to consumers. In general, the reduction in price does not fully compensate consumers from

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the loss in welfare in e-readers arising from a price increase in a complementary product.

The rest of this declaration provides details about the basis for these conclusions.

COMMON PROOF OF HARM AND DAMAGES

The *Kalt Report* makes three related arguments regarding the methods that were used in the *Noll Report* to prove class-wide harm and to calculate damages. First, Professor Kalt argues that the prices that actually were paid for a particular e-book title are highly variable ("reveal pervasive dispersion and 'churning'" – *Kalt Report*, p. 3), which prevents calculating damages using a common formula instead of individual inquiry of each transaction. Second, Professor Kalt argues that my econometric model does not adequately explain variation in prices among titles in the same category, and so is unreliable, because it "relies on a modeling strategy of excessively aggregating thousands, if not tens of thousands, of titles" (*Kalt Report*, p. 4). Third, Professor Kalt argues that many sales of e-books have no damages because the actual sales price "did not rise following the onset of agency marketing" (*Kalt Report*, p. 4).

Professor Kalt presents numerous exhibits that claim to show that the variation in e-book prices is so large that individual variation swamps the average effect of collusion on prices. These exhibits suffer from numerous methodological and calculation errors. Among these mistakes are the following:

- Misidentifying the date at which the agency model was implemented by a particular publisher at a particular e-retailer, leading to misidentification of pre-collusion and post-collusion prices for that pair of publisher and e-retailer;

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- Using changes in the modal price (the most common price), rather than more reliable measures of the distribution of prices, as the indicator of price changes over time; and

- Comparing prices for e-books within the same genre category at different periods of time without taking into account changes in the composition of the titles within a category or other factors that are known from the econometric model to affect prices.

These mistakes are discussed in detail in subsequent sections of this report.

Notwithstanding the errors in Professor Kalt's analysis of the price data, I agree that e-book prices within a genre category differ substantially among titles and, for the same title, across time. But this variation does not imply, and Professor Kalt does not prove, that prices for the vast majority of e-books did not increase when price collusion began and that the percentage change in prices due to collusion is not approximately the same for all e-books within a category.

The Kalt Critique of the Damages Model

Before examining the details of Professor Kalt's calculations, a useful starting place is to show how Professor Kalt mischaracterized and misused the results of the damages model that was presented in the *Noll Report*. As reviewed in this section, the testimony of economic experts, including experts for the defendants, in the liability phase of this litigation proved conclusively that the vast majority of e-books experienced price increases when price collusion by the defendants was implemented. Professor Kalt has not produced any valid evidence to cause this conclusion to be called into question.

Professor Kalt characterizes the econometric damages model in the *Noll Report* as based on "gross averages of the many prices that consumers pay," reflecting a "focus on average prices and the calculation of average overcharges on the basis of clearly overbroad groupings" (*Kalt Report*, p. 5). According to Professor Kalt, these "groupings" are "based on four genre-like categorizations of titles, New York Times Bestseller status, whether the corresponding print book is available in hardcover and/or paperback, and time since release" (*Kalt Report*, p. 4).

Professor Kalt mischaracterizes the damages model in two ways: (1) The *Kalt Report* mischaracterizes the extent to which the variables in the model take into account differences among titles within the same genre; and (2) Professor Kalt mischaracterizes the role of average prices in estimating the model and calculating damages.

Model Specification

Professor Kalt argues that because books are a heterogeneous product and because the procedures for mapping book attributes into retail prices is not known, the regression model that I estimated is not reliable for demonstrating harm to all consumers and to produce a common formula for calculating damages. The *Kalt Report* (pp. 53-54) cites a report from the American Bar Association that "warns researchers about the use of reduced-form pricing equations when investigating common impact." After quoting two sentences from the ABA report, the *Kalt Report* (p. 54) then states: "This problem with reduced-form regression modeling is at the core of Prof. Noll's modeling and ultimate 'damage' calculations: Prof. Noll's approach (perhaps unwittingly) demonstrably assumes, rather than shows or tests, a fact of common class-wide injury." The

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implication is that the ABA report warns against the use of reduced-form hedonic price

regressions because such analysis assumes the fact to be proved, which is the presence of

class-wide anticompetitive effects. Such is not the case. The two sentences from the

report that Professor Kalt quotes are embedded in a longer discussion that is reproduced

in full here, with the sentences quoted by Professor Kalt in italics.

> "A typical model is a reduced-form pricing equation. In such an
> equation, the price paid by a given purchaser at a given time is a
> function of a vector of variables that affect demand and supply of the
> product, such as product features, input prices, or weather. The
> equation also includes a dummy variable with a value of one during the
> period of the conspiracy and zero at other times, and is assumed to be
> the same for all class members. The estimated coefficient on this
> dummy variable is generally taken to be the estimate of injury. If it is
> positive and significant, that result may support the plaintiff's view that
> the conspiracy increased prices.
>
> *The reduced-form pricing equation assumes that a conspiracy has the*
> *same effect on every purchaser and focuses on the average effect,*
> *which may hide variation among 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 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. This would be appropriate if members of the
> proposed class can be grouped using some observable structural
> characteristic that is believed to affect the price that they pay. For
> example, customers in one area may have seen smaller price increases
> because there was a small firm in the area that did not participate in the
> conspiracy.
>
> Assuming that one can support this type of structure, the first reduced-
> form regression model could be changed to include a dummy variable
> that has a value of one during the time period of the conspiracy only if
> a customer is in the area that is believed to have had a smaller price
> effect. This model would produce one estimate of the effects of the
> conspiracy for customers not in the affected area and another for
> customers in the affected area…

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It is possible to modify the preceding regression model to allow the effects of the conspiracy to vary among more than two groups of purchasers."[1]

As is apparent from the full quotation, the actual warning from the ABA is *not* against the use of reduced-form hedonic price regressions, but against equations that use a single indicator variable to measure the effect of anticompetitive conduct for all purchases. The recommended procedure is to group purchases according to structural features of the market that may have caused differential effects on prices. The regression model in the *Noll Report* engages in such groupings, some of which are recognized by Professor Kalt (genre, date of release, best seller). As noted in the *Kalt Report* (footnote 140, p. 60), the effect of all of the variables that were used to differentiate among e-books by factors that are likely to reflect differences in market conditions produces over 500 categories of e-books in which transactions are observed.[2]

The ABA report explicitly recommends taking into account the presence of sellers that did not participate in the conspiracy. The regression analysis in the *Noll Report* groups transactions by publisher (each of the six majors plus others), which enables the model to take into account differences in pricing strategies among publishers, and makes the conservative assumption that price collusion by the defendants had no effect on publishers that did not adopt the collusive agency model. The ABA report states that

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[1] ABA Section of Antitrust Law, *Econometrics*, American Bar Association, 2005, pp. 221-23.

[2] Professor Kalt states that "Prof. Noll's regressions effectively drops the other 218 groups from his analysis..." This statement is misleading. The defendant publishers do not offer e-books that have not appeared in a print edition, but some other publishers do. Hence, comparisons cannot be made between titles from the defendants and titles from other publishers in categories in which the defendants sell no books. These categories have not been dropped, but are simply unpopulated by any observations.

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prices from non-conspirators can be expected to be affected less, not that they are

unaffected. In this circumstance, this conservative approach is justified because the

prices from publishers that did not adopt the collusive agency model do not appear to

have changed as a result of the conspiracy.

The *Kalt Report* understates the complexity of the econometric model in the *Noll*

Report, thereby creating the false impression that the model that I estimated is the type of

model that the ABA report criticizes. For example, the *Kalt Report* (p. 61) remarks that

Figures 25A and 25B show considerable variation in the prices of e-books within a

category, but that "according to Prof. Noll's modeling, these titles should have similar

prices by virtue of sharing the same characteristics as measured by his hedonic and

supply and demand variables." Professor Kalt also criticizes the model because it does

not include factors that are specific to a book title such as "Authors' growing or sinking

reputations; the appearance of good or bad reviews; events such as a movie release; so-

called 'buzz' or 'word of mouth' effects; celebrity, expert, or other endorsements; and

real-time and other marketing efforts" (*Kalt Report*, p. 4).

These criticisms are incorrect because the model does not predict that books in the

same genre will have similar prices and does not fail to take into account idiosyncratic

characteristics of an e-book title. Some factors that influence the supply and demand of

titles and demand for a specific title are common to groups of books (examples are genre,

the presence of a paperback edition, best-seller status, the identity of the publisher) and

are taken into account by variables that are included in the model. If an idiosyncratic

characteristic of a title, like bad reviews, affects its sales, the model takes these effects

into account because it includes indicator variables for each title. Likewise, a celebrity

endorsement or movie release that causes a title to be a bestseller will be taken into account by the best-seller component of the category.

Professor Kalt's claim that the model fails to account for differences among books in the same category is false because the title indicator variable does exactly that. The *Kalt Report* (pp. 133-34) mentions the indicator variables for titles, but mischaracterizes their role in the analysis. Professor Kalt offers no evidence that the variables that he lists would add additional explanatory power beyond the variation in prices that is picked up by the title indicators and the other variables in the model, including indicator variables for each publisher and each e-retailer.

Mischaracterization and Misuse of the Damages Model

Professor Kalt asserts that the econometric model in the *Noll Report* and the damages calculations that are based on that model "do not suffice to identify an economic basis for proving the fact, much less the magnitude, of antitrust injury on a predominantly common basis. This failing is exacerbated by Prof. Noll's use of and dependence on gross averages of the many prices that consumers pay" (*Kalt Report*, p. 5). The two "gross averages" that he cites are the use of average prices for a given e-book in each four-week period, and the use of percentage overcharges that are "derived from an average for the typically-very-large number of titles and transactions in his gross groupings." These characterizations of the nature and use of the econometric model in the *Noll Report* are inaccurate and misleading.

The damages model in the *Noll Report* estimates the percentage elevation in prices due to price collusion among each of the defendant publishers for each e-book,

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based on the values taken by all of the independent variables for that e-book. The percentage is calculated based on the differences in prices between each defendant publisher and publishers that did not adopt the agency model when price collusion began. These calculations take into account other characteristics of the transaction (best-seller status, publisher, physical copy editions, e-retailer, release date) and other factors taking place in the market.

The only sense in which the model is based on average prices is that, for each title, the price of an e-book that is used to estimate the regression is the average price over a four-week period. In the *Noll Report*, damages were calculated for each e-book title in each four-week period by multiplying the percentage overcharge by the actual average price in that period. To implement the model for each customer, the damage associated with that transaction would be the percentage overcharge multiplied by the actual transaction price for that customer. Thus, customers who paid an atypically low price for a given book on a given day would be awarded lower damages (the percentage overcharge for that book during that four-week period multiplied by the unusually low price that the customer paid). Because the departures above and below the average price sum to zero, the amount of total damages is exactly the same from these two procedures, so detailed calculations for every transaction are unnecessary when the goal is simply to produce a calculation of total damages.

Since submitting the *Noll Report*, economists at Ashenfelter and Ashmore, working under my direction, have estimated the same damages model using one-week average prices and individual transactions prices. The regression using one-week averages continues to use quantity weights, while the regression on individual

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transactions data does not use quantity weights because they are unnecessary (greater quantities are reflected in a larger number of transactions). The regression involving individual transactions required enormous computational power, and so was run on a super-computer for over 14 hours.

The results from these regressions are similar to the regression using four-week averages. The results of the transaction-level regressions are reported in Exhibits 1, 2 and 3. Exhibit 1 reports the percentage overcharge by e-book. Exhibit 2 reports total damages and the damages per defendant publisher. Exhibit 3 disaggregates total damages by state.

Compared to the regression that uses the four-week average prices for each title, aggregate damages are approximately the same in the regression that uses one-week averages and are about 9 percent lower in the regression that uses individual transactions records. The number of categories with negative average price effects and hence no damages is lower in both new regressions, accounting for 0.4% of sales in the one-week model and 0.2% of sales in the individual transactions regression. Because the regression using individual transactions avoids averaging and does not require quantity weighting, I prefer damages calculations from this regression. Total damages using this regression are $280,254,374.

The *Kalt Report* discusses some examples of books for which the price fell with the introduction of agency. As explained below, damages are calculated for some e-books that fell in price after collusion was adopted, based on the result from the regression that these e-book prices still were elevated – that is, these prices would have fallen even more in the absence of collusion. Notwithstanding Professor Kalt's criticism

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that the model predicts that some prices would have fallen less had the collusive agency model not been adopted, as explained elsewhere this result is perfectly consistent with the evidence in the liability phase of this litigation.

In addition, the price of some e-books fell with the introduction of collusive pricing even though no other independent variable in the regression model explains the price reduction and the average price within the category in which that book was placed increased. A property of the regression model is that the presence of an outlier that is unexplained by the model does not cause an overestimate of total damages. For books for which no other independent variable accounts for a price reduction, eliminating that book from the analysis would cause the estimated percentage price increase in its category to be higher, but total damages, calculated by multiplying this percentage by the actual prices of the books that remain, would be unchanged.

The econometric model in the *Noll Report* can be used to estimate the price of each title in each time period. This calculation produces the estimated average price of an e-book in a four-week period. Because the estimated price is not used to calculate damages, producing a formula for estimating the price of an e-book title is not a purpose of the model. Instead, the goal is to calculate the percentage elevation in prices due to price collusion for each e-book, based on the corresponding values of the independent variables for transactions of that e-book in the four-week period in which the transaction took place and the competitive benchmark prices for e-books that were not affected by collusion. What Professor Kalt calls "gross averages" are in fact the common effects on price of various structural characteristics of the market that plausibly could affect prices differently among e-books, as recommended in the ABA report.

The *Kalt Report* (pp. 69-76) criticizes the *Noll Report* for assigning damages to e-book transactions in which the predicted e-book price in the model was not above the pre-collusion level or differed from the actual transaction price. The basis for these criticisms is calculations that compare the predicted prices for specific transactions with the actual prices.[3] The procedure is misleading because it focuses on the level of prices, rather than changes in prices, and so misidentifies some transactions as being either unaffected by price collusion or affected in ways that benefit consumers.

To illustrate the problem with Professor Kalt's procedure, consider the following example. Assume that the standard posted price of an e-book from a specific e-retailer is $20 before price collusion and $24 after collusion, but that 1/3 of customers receive a 25% discount and 1/3 of customers receive a 50% discount in both periods. The resulting transactions prices are $10, $15 and $20 before collusion, and $12, $18 and $24 after the conspiracy begins (in every case an increase of 20%). The predicted price in the regression is the average price, which is $15 in the competitive benchmark period and $18 in the collusion period. Although in this example every consumer is harmed by 1/6 of the actual transactions price in the collusion period, Professor Kalt's procedure concludes that consumers who received the biggest discount and the lowest price were not harmed. The reason is that the average (predicted) price of $15 is above the actual price of $12 for the group that received the 50% discount during the collusion period. My procedure correctly would award damages of $2 to the consumer who paid $12, $3 to the consumer who paid $18, and $4 to the consumer who paid $24 dollars. Note that the only consumer who would pay "average damages" ($3) is the consumer who received the

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[3] Professor Kalt presents the results of this procedure in Figures 34B and 35 of the *Kalt Report*. See also *Deposition of Joseph P. Kalt, Ph.D.*, December 4, 2013, pp. 148, 152.

25% discount.

The Evidence on Price Changes

The *Kalt Report* contains numerous exhibits that were constructed to demonstrate two points: (1) Prices vary within the same genre ("price diversity") and for the same title ("churn"); and (2) Some e-book prices did not change or even fell at the time that collusive pricing was implemented. These exhibits suffer from numerous calculation errors and conceptual flaws, and so misrepresent the nature and extent of price variation, especially the effects of the implementation of collusion. But before turning to these issues, a useful starting place is the evidence about the effect of the conspiracy on prices that was presented in the liability phase of this litigation.

Liability Evidence

As discussed in the *Noll Report*, the liability evidence showed that the prices of almost all e-books rose substantially when the publisher defendants collectively imposed the agency model on e-retailers. The *Noll Report* describes the evidence that was presented in the *Direct Testimony of Orley Ashenfelter*, which I will not repeat here. The *Direct Testimony of Richard J. Gilbert, Ph.D.* (henceforth *Gilbert Report*), also contains numerous figures showing the effect of price collusion on e-book prices.

The *Gilbert Report* (p. 50-51) shows that the average price of e-books at Amazon rose during the first week of April for the four publisher defendants that implemented the collusive agency model at Amazon on April 3, 2010, while prices for e-books from other publishers, including Penguin before Amazon signed an agency agreement with that

publisher, stayed the same or fell. The *Gilbert Report* (pp. 51-52) then shows that when Penguin imposed the agency model on Amazon on May 28, 2010, its prices also rose, while the prices of e-books from Random House and other publishers did not.

The *Gilbert Report* (pp. 52-53) disaggregates e-book prices among new releases, backlist titles, and best-sellers for each defendant publisher plus Random House and other publishers, and shows that average prices in all categories for all defendant publishers rose at both Amazon and Barnes & Noble. Professor Gilbert also compared e-book prices in February 2010, and February 2011, for the six largest publishers and all others at both Amazon and Barnes & Noble, and found that average prices rose for all three types of books from all defendant publishers, but fell for Random House and other publishers (*Gilbert Report*, pp. 54-55).

While Professors Ashenfelter and Gilbert presented data on average prices, the *Gilbert Report* also contains data about the behavior of individual e-book prices. To address the issue of the share of e-books that experienced a price increase, Professor Gilbert examined the price of every e-book title that was sold immediately before and after implementation of the collusive agency model by the defendants. The *Gilbert Report* (pp. 56-61) presents the distribution of price changes before and after price collusion, and finds that among the defendant publishers the price of e-books was higher for titles accounting for 82.9% of e-book sales. Prices stayed the same for titles accounting for 4.9% of sales and fell for titles accounting for 12.2% of sales.

The fact that some prices fell does not prove that collusion caused these prices to fall and so benefited (or at least did not harm) some consumers. The distribution of price changes before and after the implementation of price collusion was affected by other

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factors taking place in the market and the stage of each e-book in its product life cycle.
For example, the introduction of a paperback edition of an e-book title causes a sharp
reduction in the price of the e-book edition, both before and after collusion. If a
paperback edition of a book was released around the time that price collusion was
implemented, the resulting reduction in the e-book price should not be counted as a
benefit of collusion.

To ascertain the distribution of price changes that would have occurred in the
absence of collusion, Professor Gilbert compared the price changes of the defendant
publishers with the price changes for Random House and other publishers. The *Gilbert
Report* (p. 60) references Appendix C of his February 8, 2013 expert report, which
contains many before-after price comparisons. The average price change for Random
House was 0.0%. About 11% of Random House sales were accounted for by titles for
which average price was unchanged, and nearly 50% of sales were accounted for by titles
for which average prices fell. For publishers other than the Big 6, average prices fell by
0.2%, with about 24% of sales at unchanged prices and 40% of sales at lower prices.

The *Gilbert Report* establishes that many more books experienced price increases,
and many fewer books experienced price reductions, among defendant publishers than
would have been expected on the basis of the distribution of price changes among
Random House and other publishers. These data do not indicate that *any* consumers
benefitted from price collusion by the defendants. To conclude that *all* price reductions
were a *benefit* from collusion is implicitly to assume that, in the absence of collusion, no
prices would have been cut. In fact, consumers who paid lower prices still were damaged
if the price reductions by the defendant publishers were less than otherwise would have

been adopted had the defendants not engaged in collusion. For example, about 7% of

Random House sales were at prices that were 10-20% lower, which is about the same

fraction of sales among the defendant publishers that were between zero and 10% lower.

If the effect of collusion was to cut the 10-20% price reductions in half and to eliminate

the 0-10% price reductions, which is consistent with the facts in the *Gilbert Report*, then

customers who received a 0-10% discount were still damaged by collusion.

The *Kalt Report* simply ignores this point. Professor Kalt chalks up every single

price reduction as a benefit of the agency model, and counts every transaction at the same

or a lower price as evidence that collusion had no class-wide impact. By failing to

consider the overall pattern of price reductions when the defendants were not engaged in

collusion and by other publishers who did not participate in the conspiracy, Professor

Kalt overlooks the evidence that prices for e-books from the defendants exhibited fewer

and smaller price cuts during the collusion period. Far from being benefited by collusion,

consumers who received less of a price reduction than otherwise would have occurred

were harmed by the conspiracy.

Data Analysis Mistakes

Many of the exhibits in the *Kalt Report* suffer from serious errors in the methods

that Professor Kalt used to make price comparisons. These methodological errors are the

use of modal prices, the misidentification of the start of the collusion period in measuring

the effect of collusion, and the elimination of a substantial fraction of the price data from

some of the statistical analysis. These errors bias Professor Kalt's data analysis in favor

of his conclusion that anticompetitive harm and damages cannot be established on a

class-wide basis.

Modal Prices

Several exhibits in the *Kalt Report* (Figures 12-17, 19-20) are based on movements of modal prices. The mode of a distribution is the price that occurs most frequently. Professor Kalt sometimes uses daily modal prices, sometimes used the highest or lowest daily modal prices during a week, sometimes uses the lowest daily modal price over two weeks, and sometime uses the lowest daily modal price over four weeks. Professor Kalt offers no explanation for why he uses the mode of price distributions or why different figures are based on modes over different time periods.

The mode of a distribution is one of several ways to describe the "central value" (or typical observation) in a data sample. The more commonly used measures of a central value of a distribution are the median (the number of greater observations exactly equals the number of lesser observations) and the mean (the average of the sample). A perplexing feature of the *Kalt Report* is that it offers numerous criticisms of the *Noll Report* for using averages (the mean), but then bases so much analysis on the mode.

The mode of a distribution is the least useful measure of central tendency for reasons that have been widely recognized for decades. To quote a text from half a century ago:

> "It should be obvious that the mode will not always be a central value; in fact, it may often be an extreme value. Then, too, a sample may have more than one mode."[4]

The ABA report that Professor Kalt cites about the use of average values states:

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[4] Bernard Ostle, *Statistics in Research* (2nd), Iowa State University Press, 1963, p. 58.

"Economists use three different measures of central tendency: mean, median, and mode. The mean is the average for the data set. The median is the value that has an equal number of observations on either side. The mode is the most frequent value. It is common to use either the difference in means or medians. The choice depends on the circumstances, with the median being preferred if one is concerned that the mean may be significantly affected by outlying values in the data set."[5]

Thus, according to the ABA, averages are preferred unless the data set contains outliers, in which case the preferred measure is the median, not the mode.

The problems with the mode are easily demonstrated by a variation of the example above about discounts from a standard price. The previous example assumed that buyers were equally divided into three categories: those who paid the normal price of $20, those who received a 25% discount, and those who received a 50% discount. In this example, because each price occurs with equal frequency, all observations are the mode! This problem is likely to occur only if the number of buyers is small; however, given that the median number of units sold per title by the six largest publishers is only 111, a small number of buyers in a four-week period is a frequent occurrence.[6]

Another example illustrates a problem with the use of the mode that can arise even if the number of buyers is large, which is that basing an analysis of price volatility on the mode can lead to a misleading conclusion about whether prices actually do vary substantially over time. Suppose that an e-book retailer offers 1/3 of its customers a 25% discount and 1/3 of its customers a 50% discount, with the proviso that a customer can exercise the discount offer any time during the next three days. By random chance, the proportion of customers who pay each of the three prices is likely to vary from day to

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[5] ABA Section of Antitrust, *op. cit.*, p. 208, footnote 90.

[6] See *Orszag Report*, footnote 56.

day. For example, slightly more than 1/3 of the customers on Monday may pay the
normal price, slightly more than 1/3 of Tuesday's customers may receive the 50%
discount, and slightly more than 1/3 of Wednesday's customers may receive the 25%
discount. Professor Kalt's use of the modal price would lead to the conclusion that this e-
book exhibited enormous price churning – $20 on Monday, $10 on Tuesday, and $15 on
Wednesday. Yet on all days the median price would be $15 and the average (mean) price
would be within pennies of $15.

Of course, examples can be constructed in which the use of the mode leads to the
conclusion that prices never change when in fact they do. Suppose that the same e-
retailer set all e-book prices at $20 in the first period, and then in the second period
offered a 25% discount to 30% of its customers and a 50% discount to another 30% of its
customers. In both time periods, the modal price is $20, so Professor Kalt would
conclude that prices had not changed. But the mean price is $20 in the first period and
$15.50 in the second period, which is clearly a better measure of the effect of the change
in pricing policy on consumers.

The extreme value problem with the mode can be illustrated with another minor
variation of the same example. Suppose that the original policy of the e-retailer was to
sell the book at the normal price to 32% of customers, to give a 25% discount to 36% of
customers, and to give a 50% discount to 32% of customers. Both the mean and the
mode will be $15. Then suppose that the e-retailer adopts a new policy in which 35% are
charged the normal price, 31% receive the 25% discount, and 34% receive the 50%
discount. The mean price will rise by a tiny amount (from $15 to $15.05), but the mode
will jump from $15 to $20. (In all cases the median will remain at $15.)

An example from Professor Kalt's figures illustrates how the seeming instability of the mode can be misleading. Consider Figure 12 of the *Kalt Report*, which shows the time trend in modal prices for e-books in several categories. Figure 12E reports the results for the top 50 science fiction and fantasy titles. In March 2010, one line (colored magenta) seems to vary randomly between $4.15 and $6.39, which Professor Kalt interprets as exhibiting enormous short-term price "churn." Due to a coding error, this line actually follows the modal price for sales of two books in the *Song of Ice and Fire* series: *A Game of Thrones* (the first book in the series) and *A Clash of Kings* (the second book in the series), both published by Random House. Apparently Amazon assigned both books the same title identification code until the error seems to have been corrected on May 13, 2010. Initially both books were priced at $6.39, as were six other books, all of which are accounted for by a single line at that price point.[7] In January 2010, the price of *A Game of Thrones* was cut to $1.90 while the price of *A Clash of Kings* was unchanged. Because *A Game of Thrones* was more popular, its new price became the mode, so both books are now represented by a line at $1.90. In February Amazon increased the price of *A Game of Thrones* to $4.15, while the price of *A Clash of Kings* has remained at $6.39. At this point in Figure 12E, a rapid and seemingly random alternation between two prices begins because the sales of the two books had converged to very similar numbers. In this period, the mode varies between $4.15 and $6.39

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[7] Another misleading characteristic of figures that show trends in modal prices of a large number of books is that all books with the same modal price are represented by a single line. As a result, these figures have many fewer lines than the number of titles that they cover. Because only divergences in price within a group of books that has had the same price are depicted, the figures overstate the amount of price variation. For example, Figure 12E, which includes 50 titles, shows 10 distinct price lines on the date that the iBookstore was launched and 12 distinct price lines on the last date in the figure.

depending on which book outsold the other on a given day. Had the figure plotted the average price (about $5.27), the line representing these two books would have varied by much smaller amounts around the mean.

The preceding examples illustrate that changes in modal prices are unreliable indicators of changes in the general pattern of prices. That is, the mode can change a great deal when the entire distribution has experienced little change, or can be stable when the overall pattern of the distribution is shifting substantially. A more subtle but equally important problem is that the concept of statistical significance does not apply to the mode. That is, one cannot test whether a change in the mode is the result of a change in the underlying factors that affect all observations or is the result of random chance. Thus, one cannot test whether differences in modal prices in Professor Kalt's figures are statistically significant.

The preceding discussion illustrates an important property of using the mode of a distribution as a measure of its central value. The modal price is a poor indicator of price trends and is a less reliable indicator of price trends than the mean. As a result, statistical analysis almost never is based on an analysis of the mode.

Incorrect Dates for Implementing Agency Pricing

In the liability phase of this litigation, the evidence presented by Professors Ashenfelter and Gilbert, and by defendants' expert, Dr. Michelle Burtis, found that the prices of nearly all e-books from the defendant publishers rose when the collusive agency model was adopted. The *Kalt Report* (p. 37) asserts a quite different finding: "approximately [75]% of units sold within the four weeks after the shift to agency did not

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experience pricing above pre-agency levels." This statement summarizes the findings in Revised Figure 17 of the *Kalt Report* (updated to match the Revised Figure). Figures 19A through 19E show the number of titles from each of the five publisher defendants that were sold below the pre-agency price during the collusion period, and Figures 20A through 20E show the cumulative sales during the collusion period that were below the pre-agency price by each of the five publisher defendants. The *Kalt Report* (pp. 42-43) states: "Even after several weeks following the start of April 2010, a vast number of titles' prices stayed at or below their pre-agency price levels... These empirical findings are inconsistent with claims of a pricing 'structure' that moved up as a whole upon agency. They also belie the existence of a ubiquitous, everywhere-effective price fixing conspiracy that elevated all e-book prices upon the move to agency."

This strong conclusion is based on erroneous and unreliable calculations. Putting aside the fact that Professor Kalt's conclusions are unreliable because they are based on comparing modal prices, these calculations suffer from another massive error: Professor Kalt uses the wrong dates for the beginning of the collusion period, so that many of his supposed "pre-agency, post-agency" comparisons are actually "post-agency, post-agency" comparisons. Professor Kalt's core finding is that prices in the early days of agency are not much different from later prices under agency.

Based on the footnotes to Figure 17 and examination of the transaction data that Professor Kalt used to produce his results, Professor Kalt's procedure for comparing prices before and after the agency model is to calculate a pre-agency and post-agency modal price for each title at three e-retailers (Amazon, Apple and Sony). For each publisher, Professor Kalt defines the pre-agency period as the week before the last e-

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retailer agreed to use the agency model for that publisher, and the post-agency period as the four-week period that begins on the date that the last retailer switched to the agency model for that publisher. Thus, Professor Kalt's comparisons are between either the highest or the lowest daily modal price in the week before the last retailer adopted the agency model for each publisher and the highest daily modal price in the four weeks after adoption of the agency model by the last retailer for each publisher.

Hachette, HarperCollins and Macmillan all reached agency agreements with all e-retailers that commenced by April 3. For these publishers, Professor Kalt defines the pre-agency period as the week ending April 2, 2010. Simon & Schuster did not reach an agency agreement with Sony until April 19, 2010, and for the period from April 3 through April 18 Simon & Schuster e-books were effectively not available on the Sony site.[8] For Simon & Schuster, Professor Kalt defines the pre-agency period as the week ending on April 18, 2010, and the post-agency period as the four weeks beginning on April 19, 2010. Penguin did not reach agreement on an agency contract with Amazon until late May 2010 (the record is not clear about the precise date). During the period from April 1 through some date in late May, Amazon continued to sell old Penguin releases but was not permitted to sell e-books that were released after March 31, 2010.[9] Professor Kalt defines the pre-agency period for Penguin as the week ending on May 28, 2010, and the post-agency period as the four-week period beginning on May 29, 2010.

Professor Kalt's first error is to include the dates of April 1 and 2 in the pre-

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[8] Bates No. SEL-R-00014849 ████████████████████████████

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[9] Bates No. APLEBOOK00436944.

agency period of Hachette, HarperCollins and Macmillan. In reality, some e-retailers
were in the process of transitioning to agency prices before April 3, 2010.[10] For these e-
retailers the highest modal price for an e-book is likely to be the post-agency collusive
price. Moreover, although the official launch of the iBookstore was April 3, 2010,
Apple's data show transactions on April 1, 2010. Professor Kalt finds pre-agency
transactions for ▮▮▮ titles that were sold at the iBookstore on the first two days of April
2010. But the iBookstore never sold any books at prices other than the collusive prices
under the agency model, so all comparisons of iBookstore prices are between post-
agency and post-agency transactions. Not surprisingly, Professor Kalt finds that 94% of
Apple iBookstore titles were sold at prices that did not change with the introduction of
the agency model.

Professor Kalt's second error is to count all Simon & Schuster transactions for the
week ending April 18, 2010, as pre-agency transactions. For all e-retailers except Sony,
all transactions of Simon & Schuster e-books during the week ending April 18, 2010,
were under the agency agreement, so again Professor Kalt compares post-agency prices
with post-agency prices for these e-retailers. Moreover, nearly all Simon & Schuster
titles were unavailable through Sony during the week ending April 18, 2010. The Sony
transactions records show eight sales of two Simon & Schuster titles during the week
ending April 18, 2010, so all but two comparisons of the same title by Professor Kalt are

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[10] Bates No. MCMLN-LIT-00489772 ("As of this morning [April 1, 2010], agency
pricing for eBooks is live on B&N, Sony, Kobo, and Scrollmotion."); Bates No. HC-
DOJ-00065756 ("[S]ome of the Agency Five--now an accepted term of art--are switching
models tomorrow [April 1] while others are reportedly not doing so until Saturday, April
3."); SEL-R-00015726 ▮▮▮▮▮▮▮▮▮▮▮▮▮▮▮▮▮▮▮▮▮▮▮▮

of post-agency versus post-agency transaction prices.

Professor Kalt's third error is to count all Penguin prices during the week that ends May 28, 2010, as pre-agency prices. For e-retailers other than Amazon, all transactions during the week ending May 28, 2010, were under the agency model. Penguin withheld all newly released e-books from Amazon from April 1 through May 28, so that these books were only available only from other retailers under the collusive agency model. Thus, Professor Kalt's calculations for all Penguin new releases compare post-agency prices with post-agency prices. Older Penguin books (those published before April 1) were available at Amazon under the pre-agency wholesale model, so for Amazon, Professor Kalt does compare pre-agency prices with post-agency prices for older Penguin books; however, other e-retailers sold older Penguin books only at the collusive agency price during this period, so again Professor Kalt's calculations for these e-retailers compare post-agency prices with post-agency prices, as do his calculations for all transactions for Penguin new releases.[11]

Under my direction, economists at Ashenfelter and Ashmore have recalculated the number of titles for which prices rose, stayed the same, or declined under more accurate assumptions about the beginning of collusive pricing. In making these calculations, all of Professor Kalt's conventions were adopted with the following exceptions. For all publishers, the pre-agency week is defined as the week of March 25-31, 2010. For Hachette, HarperCollins, and Macmillan, the post-agency period is the four weeks beginning on April 3, 2010. For Simon & Schuster the post-agency period is

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[11] Professor Ashenfelter examined prices of Penguin e-books during April and May 2010, and found that over 95% of prices were higher at Apple and Barnes & Noble than at Amazon. The average price difference varied between $1.67 and $2.00. See Table A-6 of the *Ashenfelter Report*.

the four weeks beginning on April 19, 2010. For Penguin the post-agency period is the four weeks beginning on May 28, 2010.[12] Correctly defining the pre-agency period increases the fraction of e-books sold at a higher price as a result of collusion from 34.4% to 74.2%, and reduces the fraction of e-book sales at a price that did not change from 62.8% to 17.3%.

The effect of correctly identifying the date of each publisher's switch to the agency model is illustrated by recalculating Figure 20 using all of Professor Kalt's conventions except for defining the pre- and post-agency time periods. Professor Kalt found that approximately ████████ Simon & Schuster titles were sold at a price below their "pre-agency" price. Correctly identifying the beginning of agency reduces this number to approximately ██████—a nearly five-fold error. The difference is greater for Penguin. Professor Kalt reported that ████████ Penguin e-books were sold below their "pre-agency" price. Correctly identifying the beginning of agency reduces this number to approximately ████████—a difference approaching an order of magnitude. These comparisons do not attempt to correct Professor Kalt's other errors, including his use of an extreme daily modal price during a week or a month.

Correcting Professor Kalt's errors in identifying the pre-agency period eliminates most but not all of the differences between Professor Kalt's estimate of the number of e-books that were sold at the same or lower prices and the estimates by the experts in the liability phase of this trial. Because Professor Kalt's estimates are based on modal prices, the estimates by other experts are more reliable as measures of the effects of the adoption

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[12] To the extent that e-book prices typically decline over time, the gap between pre- and post-agency prices for Penguin (approximately two months) and Simon & Schuster (approximately three weeks) is conservative.

of the collusive agency model.

Eliminating Data

Figures 16A, 16B, and 26 of the *Kalt Report* show the distribution of the results of correlation analyses on pairs of e-books that were released within the same week. Figures 16A and 16B report separate distributions of correlations for the pre-agency period (January 1, 2009, to April 2, 2010) and the post-agency period (April 3, 2010, through April 2012), while Figure 26 reports the same results for the entire data period.

Professor Kalt's correlation analyses suffer from the problems discussed above: they are based on weekly modal prices and they do not control for the fact that, between April 1, 2010, and May 28, 2010, some e-books were sold under the agency model and some e-books were not, making the results unreliable for these reasons alone.

Here I focus on another issue that biases Professor Kalt's findings with respect to whether the prices of e-books exhibit substantial independent variability. Professor Kalt's correlation analyses do not include a large fraction of all possible pairs of e-books. Professor Kalt drops e-books from the analysis on the basis of when they were purchased and whether their prices changed.

The main purpose of this section is to show how Professor Kalt's correlation analysis is misleading because it excludes a large proportion of the e-books from the analysis. The *Kalt Report* does not explain how correlation analysis works or why correlation analysis makes sense as a method for measuring the extent to which the prices of two e-books exhibit the same general pattern over time. Thus, before discussing the data elimination issue, it is useful to explain correlation analysis.

The correlation coefficient between two variables is a measure of the extent to which two variables move together. That is, correlation is designed to analyze data that exhibit variation over time and to determine the extent to which variations in two variables tend to be in the same direction (positive or negative changes). The correlation coefficient is the covariance between two variables divided by the product of the standard deviations of the two variables. For two variables, X and Y with mean (average) values of x and y, the covariance of X and Y is the expected value of $(X - x)(Y - y)$, and the standard deviations of X and Y are the square roots of the expected values of $(X - x)^2$ and $(Y - y)^2$. This mathematical formula has two important properties.

The first property of the correlation coefficient is that two variables that almost never change can have a zero or negative correlation coefficient. For example, suppose e-books A and B have the same price in every week except one during a 100 week period, but in that one week the price of A goes up by one cent and the price of B goes down by one cent. In this case the correlation coefficient between X and Y is -1.0 – that is, changes in the two prices are perfectly negatively correlated. The reason is that correlation analysis determines *only* whether changes are in the same direction, and in this example the one change in prices that did occur involved an increase in X and a decrease in Y. Alternatively, suppose that both X and Y changed exactly once by one cent during the 100 week period, but in different weeks. In this case, the correlation coefficient between X and Y would be very close to zero. In both examples, the fact that for nearly all weeks the two prices were the same and that the changes that did occur were trivial is not taken into account by the correlation coefficient.

The second important property of the correlation coefficient is that if a variable

exhibits no variation, its correlation coefficient with other variables is undefined. The reason is that the denominator of the formula for the correlation coefficient includes the standard deviation of each variable. If a variable exhibits no variation, its standard deviation is zero, in which case the denominator of the formula for the correlation coefficient also is zero. Because one cannot divide by zero, a correlation analysis of e-book prices cannot include any e-book title for which price did not vary. The implication is that if e-books A and B were always sold at the same price, the correlation coefficient between them is undefined, even though the price of either is a perfect predictor of the price of the other – that is, for some constant k, $X = kY$ in every week.

These properties of the correlation coefficient render its use inappropriate if the relevant data set contains many variables that do not change or that change in trivial ways. Correlation can tell the analyst whether two variables depart from their mean values in the same direction at the same time. Correlation analysis cannot tell the analyst whether these departures are important or unimportant, and has nothing to say about time periods in which neither variable exhibits any change.

Given this background, Professor Kalt was forced to eliminate from his correlation analysis all e-books for which the modal price did not change during the entire data period. Of course, the point of Professor Kalt's analysis was to add evidence to demonstrate his claim that e-books do not have a stable price structure. But e-books for which the price never changes clearly do have a stable price structure. Eliminating these books causes an understatement of the extent to which the prices of any random pair of e-books actually move together, since no change by one is a perfect predictor of no change in the other.

Professor Kalt's analysis excludes e-book titles on other grounds. Figure 26 is limited to e-books in the ten genre/publisher categories that had the greatest sales. In each of Figures 16A, 16B and 26, Professor Kalt includes a title pair only if they were both sold on the same day for at least 36 days. In addition, Figure 16A includes only e-books that were first released between January 1, 2009, and April 2, 2010. This procedure eliminates the following titles: (1) All titles that were released before January 1, 2009, and so most titles that were sold in early 2009; (2) For Figure 16A, all e-books that were released between January 1, 2009, and April 2, 2010, for which both were sold on the same day for fewer than 36 days; and (3) For Figures 16B and 26, all e-books released between January 1, 2009, and April 2012 that were sold on the same day for fewer than 36 days.

Due to these restrictions on the titles considered, the correlation analysis does not include e-books that were on the market for fewer than 36 days or that were on the market for longer but had enough days of zero sales so that the number of days for which they had positive sales was less than 36. Professor Kalt's procedure also excludes pairs of e-books for which each was sold on more than 36 days, but for which each book had a sufficient number of days with zero sales that the two books had fewer than 36 days on which they both were sold. The effect of this restriction is to under-represent new releases that do not sell well and exit the market quickly. Another effect of these restrictions is that in the pre-agency period the data covers primarily new releases, while the correlation analysis in the post-agency period includes a much larger proportion of backlist titles. The *Kalt Report* does not provide any explanation for why e-book titles were dropped from the sample on the basis of when they were released and why e-book

pairs were dropped if they both were sold on the same day for fewer than 36 days.[13]

Professor Kalt does not report how many e-book price pairs were eliminated from his analysis. But the footnotes to Figures 16A, 16B and 26 do report how many e-book titles that satisfy his other criteria were excluded because their prices did not vary. The number of e-books that had to be eliminated for this reason is substantial. The fraction of all e-book title pairs that were eliminated is 46.59% for Figure 16A, 50.31% for Figure 16B, and 27.23% for Figure 26. These fractions imply that about 27% of titles were eliminated for this reason in the pre-agency period in Figure 16A, about 30% of titles were eliminated in the post-agency period in Figure 16B, and about 15% were eliminated from the full period in Figure 26.[14]

Excluding titles for which price did not change causes an underestimate of the extent to which prices are stable and predictable. Obviously all e-books for which price did not change exhibited price stability. Any one of these titles could be used to predict perfectly the prices of all other e-books for which price did not change. Although these titles must be excluded from a correlation analysis because of the mathematical properties of the calculation, the fact that so many titles were excluded on this basis means that the correlation analysis understates the extent of price stability among all e-book prices.

Professor Kalt's correlation analyses in Figures 16A and 16B fails to take into

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[13] The *Kalt Report* does not explain why titles were excluded on the basis of their release date. The decision to consider new releases within the same week causes all title-pairs in the analysis to enter back-list status at the same time, so would capture the effect on prices of moving from new release to backlist status, but this could have been taken into account without eliminating titles by considering only title-pairs with the same release status (new or backlist).

[14] Suppose that f is the fraction of e-books for which the price changes at least once during the period being examined by Professor Kalt. Then the fraction of all pairs of e-books for which both e-books have at least one price change is approximately f^2.

account uncorrelated price changes that are explained by other variables in the regression equation. For example, the release of a paperback edition normally leads to a reduction in the e-book price. Two titles that are released in the same week subsequently may have paperback editions released during different weeks, or one to a pair of titles may never be released in paperback. Such different paperback release decisions will lead to a reduction in the correlation of the prices of an e-book pair for a reason that is taken into account in the econometric model. Likewise, titles released in the same week vary in terms of their best-seller status and genre. Thus, Professor Kalt's conclusions on the extent to which prices are uncorrelated does not account for uncorrelated price changes that are explained by the econometric model.

Implications for Common Proof of Injury

Professor Kalt's extensive discussion of price dispersion in e-books is only indirectly related to the issue of whether the harm to consumers from collusive pricing can be demonstrated using common methods. Two facts show that injury was predominantly common to class members. The first is the liability evidence that shows that the entire distribution of prices increased with the adoption of the collusive agency model, without even taking into account factors that changed at roughly the same time that would cause prices to fall. The second is the result from the econometric model that virtually all categories of e-books experience an increase in the average price of e-books, taking into account these other factors.

The regression results permit another test of the commonality of injury. The transactions records from the iBookstore include customer identifiers that permit tracking

all of the e-book purchases by the same customer to determine the fraction of iBookstore

customers whose total payments for e-books were higher than would have been the case

had the agency model not been adopted. The median number of e-books purchased by

iBookstore customers was one, and the mean number of books purchased was about 3.25.

The average damages per customer were ██████ for the median and ██████ at the mean. Of

the ████████ Apple e-book customers who bought at least one e-book from a publisher

defendant, ██████ customers (approximately 0.2%) bought only e-books in categories for

which the overcharge is zero or negative. Thus, 99.8% of iBookstore customers who

purchased at least one book from the publisher defendants were damaged. Of the

customers who only purchased e-books from categories with zero or negative overcharge,

about 98% of these customers purchased only one book from the defendant publishers.

ECONOMETRIC MODELING

        While both the *Kalt Report* and the *Orszag Report* argue that the damages model

in the *Noll Report* is unreliable for calculating damages, this section focuses mainly on

the criticisms in the *Orszag Report* because the *Kalt Report* is less comprehensive and,

for the issues that it does discuss, largely repeats the arguments in the *Orszag Report*.

Mr. Orszag's criticisms fall into two categories. The first is that in estimating my

econometric model I used data that should have been excluded, and in fact was not used

in a broadly similar model that was estimated by Professor Orley Ashenfelter. The

second is that had collusion not taken place, e-book prices would have increased anyway,

and prices of e-readers would have fallen. This section first deals with the issue of the

data that were used to estimate the econometric model, and then turns to whether

damages ought to be adjusted for either the fall in device prices or Mr. Orszag's

hypothetical increase in e-book prices in the absence of collusion.

Data Exclusion

The *Orszag Report* (p. 13) lists five differences between my analysis and the analysis by Professor Ashenfelter. (1) I use data from all publishers, while Professor Ashenfelter used data only from the five publisher defendants plus Random House. (2) I used data from June 8, 2008, through April 8, 2012, while Professor Ashenfelter used data for two 24-week periods before and after the adoption of the agency model. (3) I used data from more retailers than did Professor Ashenfelter, who analyzed only data from Amazon, Apple and Barnes & Noble. (4) I weighted the observations on e-book prices by the quantity of e-books sold, while Professor Ashenfelter did not use weighted regressions. (5) My analysis grouped e-books into many categories and estimated the overcharge due to the adoption of the agency model for each category, whereas Professor Ashenfelter did not group e-books into categories and estimated an average overcharge for all types of books. Mr. Orszag observes (*Orszag Report*, p. 13): "Some of these differences derive from the fact that Professor Noll is addressing damages issues while Professor Ashenfelter was not." The *Orszag Report* (p. 14) then criticizes the first two of the five differences between my econometric model and the model that was estimated by Professor Ashenfelter.

The criticism that I used too much data in estimating the price regression is very strange. A core principle of econometric analysis is that using more data normally increases the reliability of a regression model and excluding some data normally amounts to throwing away valuable information. As explained in this section, Mr. Orszag does not

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have valid reasons for excluding the data that I used but Professor Ashenfelter did not.

According to the *Orszag Report* (p. 14), I use "an inappropriate set of publishers" as the control group for making comparisons with the prices of the defendant publishers, and the second claim is that I use "data from time periods that are non-representative" of the agency period, which "can contaminate" the regression results. These complaints are inconsistent with the testimony of Dr. Burtis on behalf of Apple in the liability phase of this litigation. Dr. Burtis testified that to test properly the effect of the collusive agency agreement on prices, data for all publishers should be examined, and the regression should include the entire time period when data were available.[15] Thus, I find myself in the position of being criticized by one Apple expert for doing precisely what another Apple expert recommended.

Mr. Orszag re-estimates my damages model by eliminating the transactions data for all publishers other than the Big 6 (the five publisher defendants plus Random House) and from outside the 24-week period after the agency model was adopted that was analyzed by Professor Ashenfelter. In addition, Mr. Orszag eliminates all transactions before April 1, 2009, and all transactions during the first three months of 2010. Because Professor Ashenfelter included these data in his analysis, Mr. Orszag presumably believes that Professor Ashenfelter also should have removed these data from his analysis. Finally, Mr. Orszag argues that some portion of the transactions of the iBookstore and Barnes & Noble also should be eliminated because, in the absence of collusion, Apple would not have launched the iBookstore and Barnes & Noble would have exited the market.

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[15] Trial transcript, pp. 2263-64.

Except for the elimination of transactions from the iBookstore and Barnes & Noble, the effect of Mr. Orszag's procedures is to exclude from his analysis competitive benchmark transactions that had lower average prices than the benchmark prices that he does not eliminate. Eliminating these data causes Mr. Orszag's estimate of damages to be 14.2% of e-book sales revenue, compared to 19.9% from the model that I estimated.

All of the differences between my analysis and the analysis that was undertaken by Professor Ashenfelter, including the two differences that Mr. Orszag criticizes, arise from differences in the purposes of our analysis. Mr. Orszag's criticisms of my inclusion of more data are unfounded, in part because he fails to take into account the differences in the purposes of my and Professor Ashenfelter's analysis, and in part because his criticisms have no basis in economic analysis or econometrics.

The goals of Professor Ashenfelter's analysis were to show that the introduction of the agency model caused e-book prices to be higher and to estimate the elasticity of demand for the purpose of showing that price collusion reduced sales volumes. For these purposes it made sense to focus the analysis on prices in the few months before and after the agency model was adopted because in this period a high proportion of transactions was accounted for by e-books that were sold both before and after the adoption of the agency model. Moreover, because Professor Ashenfelter was interested in estimating sales quantities and the elasticity of demand as well as prices, quantity weighting would be inappropriate in his model.

My extensions and refinements of Professor Ashenfelter's model serve the purpose of constructing a reliable damages model. The goal of calculating damages from collusion in pricing e-books requires examining data from as much of the period in which

collusion took place as possible. Terminating the analysis 24 weeks into the damages period is unwarranted because it requires the implicit assumption that transactions in the first 24 weeks after the agency model was adopted are representative of transactions from the entire damages period. One obvious effect of using a shorter data period is to alter the relative quantity weights among e-book titles, giving too much weight to e-books with low sales in the latter part of the damages period and too little weight to e-books with large sales in the latter part of the damages period, such as best sellers that were not released until after the 24-week period that was analyzed by Professor Ashenfelter. Likewise, the goal of calculating damages for the entire collusive period affects the choice of competitive benchmarks. Random House transactions cannot serve as valid competitive benchmarks after Random House adopted the agency model. For this later period, the only available competitive benchmarks are prices charged by other publishers.

Exclusion of Other Publishers

The *Orszag Report* (pp. 15-18) asserts that publishers other than the Big 6 (the defendant publishers plus Random House) are "an inappropriate control group" for measuring the effect of collusion among the defendants on e-book prices. Mr. Orszag then observes that the "other" category of publishers differs from the Big 6 publishers in that the former are smaller, charge lower prices, are more specialized, and include a rising number of self-publishers. The *Orszag Report* then states (p. 17): "Although all of these books are in the same relevant market for trade e-books, there is no reason to expect, as Professor Noll implicitly assumes, that books in the 'other' category would respond in the same way to changes in the competitive conditions facing e-books." If this

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vague statement is a reference to how prices are affected by changes in market conditions, then products that are in the same relevant market necessarily do respond in the same way to changes in market condition.

A relevant market is defined on the basis of the extent to which other products (here, e-books from other publishers) constrain the price of reference products (here, e-books from the publisher defendants). In reality, there is no basis in economics to conclude that the differences between the Big 6 and the other publishers that Mr. Orszag cites could have a different effect on the intensity of competition among types of publishers before and after the adoption of the agency model.

Size of the Publisher

Mr. Orszag's first reason for excluding other publishers is that they are small. By definition, the Big 6 publishers are larger than all other publishers, but Mr. Orszag does not explain why this fact bears any relation to whether the books from the two groups of publishers are close competitive substitutes. There simply is no basis in economics for ignoring firms simply because they are smaller than other firms.

The *Orszag Report* (Table V-1) lists the top 19 publishers by revenue,[16] and the 13 small publishers on his list have a combined market share of 14.7%, which exceeds the market share of all but one of the Big 6 publishers. The sum of the market shares of the Big 6 publishers is 65%, leaving 35% for smaller publishers. Thus, publishers that are not listed by Mr. Orszag account for over 20% of the market, which is more than the market share of the largest Big 6 firm. There is no basis in economics for excluding on

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[16] Mr. Orszag's top 20 list includes "Unidentified," leaving only 19 publishers on the list.

the basis of size alone a group of competitors in the same relevant market that have such a large collective market share.

Specialization

The second reason that Mr. Orszag gives for excluding these publishers is that most small publishing houses are specialized. Here Mr. Orszag does not take into account the fact that in the econometric model books are allocated to categories so that the competitive effects of small, specialized publishers are limited to the categories in which they operate. For example, Mr. Orszag includes in his list e-books from specialized children's publishers like Disney and Scholastic. But e-books from these publishers affect the calculation of the overcharge only for children's books. Mr. Orszag does not offer any reason to believe that children's books from Disney and Scholastic are not competitive substitutes for children's books from the Big 6 publishers.

Likewise, Mr. Orszag observes that the largest other publisher is Harlequin, which publishes romance novels that are targeted at women. But Harlequin has published over 50 books by Janet Dailey, a best-selling fiction writer who in recent years has been published by Kensington, a small general-interest publishing house that ranks tenth on Mr. Orszag's list. Mr. Orszag offers no explanation for why a novel by Janet Dailey somehow is not a competitive substitute for a novel by, say, Danielle Steele, another best-selling author of romance fiction who is published by Random House.

In a footnote, the *Orszag Report* (p. 15, footnote 56) notes that the average and median sales of an e-book title from the Big 6 is substantially larger than the average and median sales of a title from other publishers. Again, there is no basis for excluding e-

books from other publishers because many have low sales. Mr. Orszag does not exclude books from the Big 6 with low sales, of which there are many because the median number of sales of a given e-book title for even the Big 6 is only 111. In any case, the point of weighting transactions by sales quantities is to avoid giving undue influence to transactions of e-books with low sales. Thus, the econometric method that is used in the *Noll Report* already takes into account any effect that might arise from the differences in average sales between the Big 6 and the other publishers as well as considerable sales variation among e-book titles that are sold by the Big 6 publishers.

Lower Prices

The next evidence that Mr. Orszag cites to support the assertion that books from other publishers are an inappropriate control group is the fact that the average price of e-books from other publishers shows "no obvious downward trend… in the pre-agency period" but fell "from around $7 in the pre-agency period to approximately $5 by the end of the sample period" (*Orszag Report*, pp. 17-18).

Mr. Orszag's statement does not accurately describe the path of average prices that is shown in Figure V-1 of the *Orszag Report*. The accurate characterization of the price trends is that the average price of e-books from other publishers was lower at the end of the data period than at the beginning, but the decline began several months before the collusion began, initially was arrested by the implementation of the collusive agency model, and occurred only intermittently thereafter.

According to Figure V-1, the average price of e-books from other publishers peaked in October 2009, six months before the adoption of the agency model, then fell

for the next few months through March 2010. This decline occurred after Barnes &

Noble introduced the Nook in November 2009, thereby allowing Barnes & Noble to

become a more effective competitor against Amazon. The average price for e-books

from other publishers then stabilized for the first six months after the agency model was

adopted, and actually was higher in August 2010 than in March 2010, the last month

before the agency model was adopted. Collusion among the defendants apparently

arrested the price declines among e-books from other publishers that had emerged after

the introduction of the Nook. After August 2010, the average price of e-books from other

publishers then fell by about $1 over a period of about five months, but then stabilized at

this new, lower level for the rest of the data period.

The two major changes in market conditions during the period that is covered by

the transactions data are that demand for e-books was growing rapidly and that price

competition in e-books intensified in the early period but was arrested in the later period

when the defendants engaged in collusion. The actual pricing pattern of e-books from

other publishers reflects the effects of these changes in market conditions.

If the number of e-book titles is fixed, economic theory predicts that growth in

demand will cause an increase in both average prices and average sales per e-book. The

cost of creating a new e-book title is almost entirely a fixed cost (aside from billing cost,

an e-book has virtually no variable cost), so that demand growth in the absence of entry

will cause a substantial increase in profits per title. This short-term increase in profits per

title will attract entry of more e-books. Effective price collusion by the defendants

further increases the expected sales from a competitively priced e-book title, so the long-

run effect of collusion is to induce even more entry from other publishers than would

otherwise occur. This additional entry will arrest the effect of collusion on the prices of other publishers, so that once entry increases in response to collusion, prices for e-books from other publishers will fall.

Eventually the fall in e-book prices will end as prices reach the competitive level that is necessary for authors and publishers to expect, on average, to recover the fixed cost of entry. Thus, the fall in e-book prices from other publishers six months after collusion began and its subsequent stabilization has a plausible theoretical explanation that justifies including these prices in the econometric model as valid competitive benchmarks. Because e-book prices from other publishers in the months after the Nook was introduced and after price collusion induced more entry of e-books from other publishers are the results that would be expected in a competitive market, they constitute a valid competitive benchmark for measuring the effect of collusion. Consequently, excluding these prices from the regression analysis is unwarranted.

The Rise of Self-Publishing

Mr. Orszag makes an additional argument about why self-publishers should be excluded from the analysis. According to Mr. Orszag, the growth in self-publishing led to greater competition among self-publishers, which caused prices of self-published e-books to fall. According to Mr. Orszag, the growth in self-publishing was due to the entry of the iBookstore and its plan for paying higher royalties to self-publishers, which would not have occurred in the absence of price collusion among the defendants. "The entry of Apple induced Amazon to change its royalty model for self-published titles. Amazon began offering much more generous royalties to self-publishing authors in June

2010, shortly after the move to agency" (*Orszag Report*, p. 18; see also pp. 42-47).

Professor Kalt makes the same claim (*Kalt Report*, pp. 15-17).[17]

The claim that Apple is responsible for higher royalties for self-published authors and the growth of self-publishing is not even consistent with the reference that Mr. Orszag cites to support his assertion. This document is an announcement by Amazon on January 20, 2010, about its new royalty plan for self- publishers. When Amazon made this announcement, other e-retailers already were offering royalties for self-publishing authors that were even higher than Amazon's proposed rates.[18] Jeff Bezos, the Amazon CEO, proposed a royalty rate of 70% for self-publishers in December 2009.[19] Amazon adopted this proposal and scheduled the announcement of the new policy before the publisher defendants proposed the agency model to Amazon.[20] As Mr. Orszag notes (p. 43), Apple announced its plan to pay a 70% royalty in May 2010, long after these other events transpired.

Mr. Orszag's explanation also is inconsistent with other facts regarding the rise of

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[17] The backup material to the *Kalt Report* includes a regression of weekly sales of self-published books on a time trend, an indicator variable for the entry of the iBookstore, and an interaction between these two variables. I assume that Professor Kalt did not include this regression in his report because it sheds no light at all on whether Apple played any significant role in promoting self-published books. To separate the effects of the iBookstore from other factors that affected sales of self-published books, such as the launch of each improved model of e-reader and tablet computer and the fact that self-published books experienced greater sales because price collusion raised the prices of other e-books, would require a much more complex regression analysis. Professor Kalt's regression is exactly the type of simplistic analysis that is warned against in the ABA report that he cites.

[18] Lulu offered a royalty of 80% in 2008 (see http://lulupresscenter.com/uploads/assets// Press_Kit_908.pdf). Smashwords paid a royalty rate of 85% in 2009 (see http://www. idealog.com/blog/ideas-triggered-by-amazon-buying-lexcycle/).

[19] Bates No. AMZN-MDL-00044064.

[20] Bates No. AMZN-MDL-00058718 (dated January 11, 2010).

self-publishing. Before the entry of the iBookstore, Barnes & Noble planned to make opportunities for self-publishing an element of its entry strategy.[21] And before Apple announced its self-publishing plan, publishers feared that self-publishing would become a mechanism for authors – including famous authors – to avoid publishers completely.[22]

Mr. Orszag also claims that Apple was more important than Barnes & Noble in the promotion of self-publishing. The *Orszag Report* (pp. 43-44) asserts that Barnes & Noble "did not become a significant competitor to Amazon's self-publishing platform. Competition for self-publishing was centered around Amazon's and Apple's platforms." The basis for this claim is Figure VII-1 of the *Orszag Report* (p. 45), which shows the shares of e-book sales at Amazon, Apple and Barnes & Noble that were accounted for by self-published e-books. Mr. Orszag's assertion mischaracterizes the facts in two ways: by overstating the importance of these three firms in the growth of self-published books, and by overstating the importance of the iBookstore in relation to Amazon and Barnes & Noble in sales of self-published books.

Mr. Orszag's analysis focuses exclusively on sales of self-published books by Amazon, Apple and Barnes & Noble, but these firms were not the pioneers, let alone the only firms, in self-published books. As mentioned in the *Kalt Report* (pp. 15-16), the first seller of self-published e-books was Lulu, which entered in 2002. Other vendors are AuthorSolutions (Booktango), BookBaby, FastPencil, Kobo, Scribd, Vook, and Smashwords. The last has been called the "largest distributor of indie e-books in the world now carries more than 180,000 titles in its catalog."[23] Many of these firms,

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[21] *Deposition of Anthony Astarita*, February 27, 2013, p. 20.

[22] *Deposition of Carolyn Reidy*, January 17, 2013, p. 133 and Exhibit 11.

[23] Alex Palmer, "DIY: How to Self-Publish an e-book," *Publisher's Weekly*, October 7,

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including Booktango, BookBaby, Vook and Smashwords sell self-published e-books through other e-retailers, including Amazon, Barnes & Noble and the iBookstore. Mr. Orszag does not acknowledge that these vendors played any role at all in the growth of self-publishing. His failure to mention Smashwords – the largest distributor of self-published e-books and a distributor to all major online retailers of e-books, including Apple – is sufficient to dismiss his analysis of self-publishing as uninformed.

Mr. Orszag's use of self-published sales as a fraction of e-book sales at each e-retailer also creates a misleading impression of the importance of the iBookstore in relation to Amazon and Barnes & Noble. Whereas the proportion of iBookstore sales that are accounted for by self-published titles was nearly as large as the share at Amazon for about six months in 2010, total sales at Amazon were about five times total sales of self-published books at the iBookstore. Since 2010, Amazon's e-book sales have grown faster than sales by the iBookstore, so that by early 2012 Amazon's e-book sales were nearly ten times e-book sales at the iBookstore. Thus Amazon's roughly 11% share of self-published e-books, compared to Apple's 7% share, actually means that Amazon sold roughly 15 times as many self-published e-books as were sold by the iBookstore. Likewise, while in early 2012 7% of e-book sales at the iBookstore were self-published books compared to about 3% at Barnes & Noble, total e-book sales at Barnes & Noble were roughly three times the sales at the iBookstore. Hence, Barnes & Noble actually sold more self-published titles than did Apple.

These facts show that Mr. Orszag vastly overstates the importance of the entry of the iBookstore in the growth of self-published e-books. The iBookstore is simply too

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2013, at http://publishersweekly.com/pw/by-topic/authors/pw-select/article/59367-diy-how-to-self-publish-an-e-book.html.

small and entered self-publishing too late to be the driving force in encouraging self-published e-books.

Eliminating the First Quarter of 2010

Mr. Orszag eliminates transactions data from the first quarter of 2010 on the grounds that prices during this period "may be non-representative" because of "the iPad's imminent arrival and Apple's negotiations with publishers," arguing that "Apple's negotiations with publishers and subsequent entry led to a competitive response by Amazon and that response would be absent in the but-for world where Apple does not enter in the absence of the agency contracts" (*Orszag Report*, p. 21).

This passage is vague, but if the intended meaning is that the iPad would not have been introduced unless the publisher defendants agreed to fix e-book prices, this assertion is not based on any evidence and is facetious on its face. The iPad is far more successful than the iBookstore, so Apple would have no rational reason to abandon an important and highly successful product because it could not get its way on the terms for launching a much less important product.

Mr. Orszag eliminates the data from the first three months of 2010 because Amazon's e-book prices fell during this period. Mr. Orszag attributes this fall in prices to the pending launch of the iPad and iBookstore. Mr. Orszag does not explain why greater demand for e-books due to the introduction of the iPad plus expectations of collusive prices in the future created an incentive for Amazon to cut prices. Economic theory predicts exactly the opposite.

The introduction of the iPad increased the number of consumers who had a device

that could be used as an e-reader.[24] Amazon could and did sell e-books to these new

potential customers. According to the *Kalt Report*, only ▮ of iPad owners who bought

books from the iBookstore did not buy e-books from other e-retailers, and both Professor

Kalt and Mr. Orszag report that about 40% of iPad owners also own a Kindle. Figure C-

1 of the *Orszag Report* shows that e-book sales increased dramatically after the iPad was

launched in April, 2010, and that most of this growth was enjoyed by Amazon. Indeed,

the figure shows that Amazon sales have increased by many times more than sales at the

iBookstore since the introduction of the iPad. An increase in demand for the dominant

firm in a concentrated market usually leads to higher, not lower, prices.[25]

Mr. Orszag's attribution of a large competitive response to the entry of the

iBookstore is inconsistent with the actual effect of the iBookstore on Amazon's sales.

Entry by an effective new competitor into a market that was not highly competitive

normally would cause prices to fall. But the iBookstore was not a normal competitive

entrant. Instead, it was an entrant that was helping to organize collusion to raise prices,

not to lower them. Thus, in expectation of a future increase in demand as well as higher

prices, Amazon would have no rational reason to cut prices before the occurrence of

these events. As shown in Figure C-1 of the *Orszag Report*, the iBookstore accounted for

a small fraction of e-book sales for the first few months after it was introduced, and

remained below ten percent of e-book sales through the entire period that is covered by

the transactions data.

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[24] See *Deposition of Keith Moerer*, December 13, 2012, pp. 60-65; *Deposition of Robert McDonald*, December 11, 2012, pp. 81-82.

[25] An increase in demand can lead to price reductions if the new demand causes demand to be substantially more price elastic. The rapid growth in e-book sales immediately after collusion among the defendants began demonstrates that this was not the case.

Meanwhile, Barnes & Noble, which entered e-book retailing in 2009, had a much greater share of the e-book retail market than the iBookstore throughout the collusive period. To the extent that Amazon was responding to competition, the competitor that posed the biggest threat to Amazon's e-book sales was Barnes & Noble, not Apple. Moreover, the competition between Amazon and Barnes & Noble intensified in November 2009, when Barnes & Noble introduced the Nook e-reader. By eliminating the transactions data during the first quarter of 2010, Mr. Orszag throws away most of the data that captures the effect of greater competition from Barnes & Noble after the introduction of the Nook. The competition between Barnes & Noble and Amazon during early 2010 makes these data more, not less, representative of the but-for world.

Elimination of Sales from the iBookstore and Barnes & Noble

Mr. Orszag argues that damages should not be collected for some transactions that were accounted for by the iBookstore and Barnes & Noble. The *Orszag Report* (pp. 51-53) argues that some iPad owners who bought e-books from the iBookstore would not have bought any e-books had the iBookstore not been launched. The *Orszag Report* (pp. 53) also argues that "[t]he evidence in the record is clear that Barnes & Noble would not have been profitable and likely would have reduced its operations at the but-for prices proposed by Professor Noll." According to Mr. Orszag, price collusion by the defendants "allowed Barnes & Noble to stay in business and continue to invest in new devices and other services."

The iBookstore

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To reach the conclusion that significant iBookstore sales would not have switched to another e-retailer, Mr. Orszag must make several *ad hoc* assumptions that have no basis in either the factual evidence or economic theory. One assumption is that a significant number of iPad users who buy e-books are not sophisticated enough to download or to be able to use the software that is necessary to load an e-book from an e-retailer other than the iBookstore. There is simply no reason to believe that a user who is sophisticated enough to use the other features of an iPad is too unsophisticated to use software from Amazon or Barnes & Noble to read their e-books. Indeed, Mr. Orszag's argument is inconsistent with the statement in the *Kalt Report* (p. 24) that the early adopters of e-books were "tech savvy" consumers who were atypically willing to try new, uncertain technologies.

To take into account the effects on damages if the iBookstore had not been launched, Mr. Orszag assumes that iPad users who have purchased few e-books from the iBookstore and who do not also own a Kindle would be unlikely to have bothered to download the software necessary to read e-books from another source. Mr. Orszag then multiplies the fraction of iBookstore sales accounted for by low sales per user by the fraction of iPad owners who do not own a Kindle to estimate the fraction of iBookstore sales that would be unlikely to switch to another e-retailer if the iBookstore did not exist. The estimate that arises from this calculation is ▮ of iBookstore sales, which is then the reduction in damages arising from these assumptions.

In making these calculations, Mr. Orszag implicitly assumes that no iPad owners possess another e-reader besides a Kindle, such as a Nook. Mr. Orszag also ignores the fact that only ▮ of iPad owners who bought e-books from the iBookstore did not buy

e-books from any other source (*Kalt Report*, p. 24). Thus, his estimate that ▮ of

iBookstore sales would not switch implies that ▮ of the customers who purchased only

from the iBookstore would not switch if the iBookstore did not exist. Mr. Orszag also

assumes that the likelihood that an iPad owner owns another e-reader and/or buys e-

books from another e-retailer is unrelated to the number of e-books that the customer

buys from the iBookstore. Instead, if iPad owners who buy e-books from other sources

buy few e-books from Apple, the sales that Mr. Orszag identifies as least likely to switch

to another vendor would, in fact, be the sales that are most likely to switch. Mr. Orszag

also ignores the fact that had the defendants not engaged in price collusion, e-book prices

would have been lower, in which case iPad owners would have purchased more e-books

and had more incentive to download the software that would enable them to use e-books

from Amazon, Barnes & Noble, and other e-retailers.

In summary, Mr. Orszag has no basis for the assumptions that are necessary to

support the conclusion that damages for iBookstore customers are overstated by 15%, or

any other proportion. Beyond these issues, a major problem with Mr. Orszag's argument

is that it is based on the assumption that in a competitive market the presence or absence

of a single firm affects prices and quantities in the market. Whereas books and e-readers

are differentiated products, an e-book is the same regardless of which e-retailer sells it.

For this reason, customers are likely to be largely indifferent about the identity of the e-

retailer that sells them an e-book. As Professor Kalt recognizes but Mr. Orszag does not,

consumers who are among the first adopters of e-readers and tablet computers are

unlikely to have their willingness to purchase e-books affected by which e-retailers are in

the e-book market.

Barnes & Noble

Mr. Orszag's assertion that Barnes & Noble would have gone out of business had the defendants not engaged in price collusion is based on two types of evidence. The first is statements in depositions by Barnes & Noble executives, and the second is the fact that Barnes & Noble lost money in the months after it launched its e-book web site.

One problem with this evidence is that the price-fixing agreement did increase the profitability of the Barnes & Noble e-book business, so current executives from Barnes & Noble had an incentive to defend this agreement. Even so, a former Barnes & Noble executive testified that the company's plans for its e-book and e-reader business did not depend on the adoption of the agency model.[26] Another problem with Mr. Orszag's argument is that in software and e-retailing, entrants expect to lose money for a while after a product is launched. Profitability is achieved from growth in sales, which allows fixed costs to be spread over increasing sales volume.[27] But the biggest problem with Mr. Orszag's confident prediction that the agency model was necessary to the survival of Barnes & Noble is that the business still survives, after the collusive agency agreements have come to an end.

To calculate the reduction in e-book sales that would have been caused by the exit of Barnes & Noble from the market, Mr. Orszag performs the same type of calculation that he undertakes to estimate the effect of the failure to launch the iBookstore. As before, these calculations are based on *ad hoc* assumptions about the behavior of

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[26] *Astarita Deposition, op. cit.*, pp. 132, 139.

[27] This business model is explained in Sony documents, which contemplate ████████ See Bates Nos. SEL-R-3353, 3358, 3360, and 5277.

consumers who, unlike iPad customers, have demonstrated their interest in e-readers and

their ability to use e-reader software. The *Orszag Report* (p. 56) argues that because the

Nook is a closed system (a Nook cannot be used to read e-books from another vendor),

Nook users would be less likely to switch to another e-retailer than customers of the

iBookstore. The problem with this claim is that if Barnes & Noble did exit the retail

book industry, as Mr. Orszag argues, in bankruptcy they would have had a valuable asset

to sell – the proprietary software for storing e-books that can be read by a Nook. It is

implausible that no e-retailer of would be willing to pay a positive price to inherit all

consumers who own a Nook. Notwithstanding that there is no evidence to support the

conclusion that Barnes & Noble would have exited the market in the absence of price-

fixing, there is also no reason to believe that the customers of Barnes & Noble would not

have switched to another source of e-books.

OFFSETTING EFFECTS

The *Orszag Report* (pp. 22-42) argues that the net harm to consumers from price-

fixing among the defendants is negligible because of two offsetting effects. The first is

that the increase in e-book prices intensified competition in the market for e-readers,

thereby causing lower prices for devices. The second is that Amazon would have

changed its business model by raising retail e-book prices regardless of whether the

defendants had engaged in collusion.

These claims by defendants' economic experts about changes in Amazon's

pricing strategies incorrectly apply the economic theory of markets for complementary

products. Two products are complements if they are used together. Mr. Orszag (*Orszag*

Report, p. 22), Professor Kalt (*Kalt Report*, p. 47) and the *Noll Report* (p. 13) all start their analysis with the same theoretical observation: an increase in the price of one product causes a reduction in the demand for complementary products. Mr. Orszag and Professor Kalt then make the following claims. First, because e-book prices rose due to price collusion among the defendants, Amazon cut the price of Kindles. Second, the entry of Apple's iPad caused Kindle prices to fall, which would have caused Amazon to increase e-book prices anyway. Each of these arguments is incomplete and inconsistent with the other. The first ignores the effect on the price of Kindles from increased competition in the device market from not only the iPad but also the Nook and other tablet computers. The second switches the cause of changes in pricing strategy from price collusion to increased competition in devices, claiming that collusion was not a cause of increased e-book prices. And both ignore the effects on prices of changes in competition for e-books and technological progress.

The claims of defendants' economic experts are based on an incorrect economic analysis and are not supported by the factual record. There is no evidence that Amazon changed its pricing strategy for Kindles on the basis of events in the e-book market, as opposed to changes in competition and technology in e-readers, and no evidence that Amazon would have raised e-book prices in the absence of the adoption of the collusive agency model in response to the entry of the iPad. This section explains why these claims by defendants' economic experts are incorrect.

Prices, Profits and Sales in e-Readers

The economic theory of complementary products indicates that the increase in the

prices of e-books that arose from collusion among the defendants caused the demand for

e-readers to be less than otherwise would be the case. Indeed, one purpose of price

collusion in e-books by the publisher defendants was to reduce the pace at which e-books

were replacing physical copies, which would reduce sales of both e-books and e-

readers.[28] To this point, the plaintiffs and the defendants do not differ about either the

goal of collusion in e-book prices or its effect on the demand for e-readers.

After describing the nature of the demand relationship between complementary

products, both Mr. Orszag and Professor Kalt incorrectly apply this theory to the market

for e-readers in two ways. First, the *Orszag Report* (pp. 22-23) and the *Kalt Report* (p.

47) argue that higher prices for e-books created an incentive to Amazon to cut the price

of the Kindle and cite data showing that the price of Kindle and Nook e-readers fell after

the date of adoption of price collusion by the defendants. Second, the *Orszag Report* (pp.

37-44), with approving reference in the *Kalt Report* (p. 48), uses a summary financial

report from Amazon to calculate that Amazon lost ███ on each Kindle device after price

collusion was implemented, and concludes that the fall in revenues per Kindle due to the

price reduction was a benefit of the adoption of price collusion on e-books and so should

be subtracted from the damages due to e-book price collusion. According to the *Orszag*

Report (p. 40): ███████████████████████████████████████

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[28] See the *Opinion and Order* in *United States v. Apple* and *State of Texas v. Penguin Group (USA), Inc.*, p. 47.

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Mr. Orszag's arguments are based on an incomplete characterization of the economic theory of complementary products and of the facts about events in the e-reader markets. The collusive increase in the price of e-books, while harmful to e-book consumers, was not the most important factor affecting the growth in demand for e-books or the price of e-readers. Instead, technological progress and increased competition in e-readers had a much greater effect on e-reader prices and on the growth in demand for e-books. Moreover, the claim that price reductions in e-readers can be attributed to the introduction of price collusion in e-books is factually incorrect, but even if it were true, the decline in e-reader prices would not be an offsetting benefit to consumers, but instead is another indicator of the anticompetitive harm from collusion.

The Economics of Complementary Products

A good starting point for correctly applying the economic theory of complements is a *precise* statement of what the theory says about the link between the price of one product and sales of a complementary product, which is contained in all three expert reports. If Products A and B are complements, an increase in the price of A causes a reduction in sales of B, holding constant all other factors that affect demand.

In the case of e-readers, other factors were not constant but were changing at the time that the defendants implemented price collusion. First, the number of e-book titles that were available was increasing, in part due to the rapid growth of self-publishing.

Second, the technology of devices that can be used to read e-books was improving rapidly, causing both a fall in the cost of a device with a given set of features and an increase in the features that were incorporated into a device, such as the quality of the e-book image and the other uses for the device. Reflecting advances in technology during the period of price collusion, e-readers were largely replaced by tablet computers, as exemplified by the introduction of the iPad and the transformation of both the Kindle and the Nook from e-readers to tablets.

The theory on complementary products is broader than simply the effect of the price of one product on the demand for the other. The markets for a product (here, e-books) and its complement (here e-readers) are interconnected by more than simply the price in each market. Any factor (not just price) that changes the value of one product to consumers will cause a change in demand for the other product. For example, technical improvements in Product A will increase its demand and also will increase the demand for Product B. In order to construct a valid theoretical forecast of the effect of changes in the e-book market on the demand for e-readers, one must take into account all of the factors that affect demand for e-books and e-readers, not just prices.

Sales Growth of e-Books and e-Readers

The sales data for both e-books and e-readers show that, despite their best efforts, the defendants did not succeed in slowing the growth of e-book sales. That is, the increased availability of e-books and the improved quality of e-reader devices caused sales of e-books to increase dramatically during the period of price collusion, in spite of the increase in e-book prices. This is not to say that collusive prices did not have a

negative effect on sales, but that the effects of price collusion were overwhelmed by other factors that had a positive effect on e-book sales.

The document that Mr. Orszag cites as the basis for his calculations about the fall in the price of the Kindle also reports Amazon's analysis of its e-book sales before and after the collusion period.[29] Notwithstanding that higher e-book prices, by themselves, had a negative effect on e-book sales, this document reports that the number of e-books sold by Amazon was ███████ in 2009, was estimated as ███████ in 2010 (after price collusion began and the iPad and iBookstore were launched), and was expected to be ███████ in 2011. Other data on e-book sales in the *Orszag Report* (p. 8) show that Amazon sold fewer than ██████ e-books per four-week period (an annual rate of less than ██████) in March 2010. By December 2010 sales per four-week period had ████████████ (an annual rate of about ███████ By early 2012, sales per four-week period had jumped to about ██████ (an annual rate of about ███████

These data show that, despite the collusive e-book price increase, other factors caused e-book sales to increase rapidly. From a theoretical perspective, rapid growth in the demand for e-books is expected to cause an increase in the value of e-readers to consumers and, thereby, an increase in e-reader sales. Mr. Orszag does not mention that the document he cites about the profitability of the Kindle also reports sales of Kindles, which were ██████ in 2009, were estimated to be ██████ in 2010 (in spite of greater competition from both the Nook, which was launched in November 2009, and the iPad, which was launched on April 1, 2010), and were expected to be ██████ in 2011. The document also reports that Amazon sold ████████████

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[29] "Kindle 2011 OP1," October 1, 2010, Bates Nos. AMZN-DOJ-000023-38 at 24.

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██████████ in 2010. ███████████████████████████████████

████████████████████████[30] In short, Amazon's Kindle business was not

exactly in shambles in 2010 and 2011.

The facts about e-book and e-reader sales are not consistent with the story told by

Mr. Orszag and Professor Kalt. The rise in e-book prices was only one factor affecting

the demand for Kindles, and the negative effect of higher e-book prices on e-reader

demand was swamped by the other changes in these markets. The evidence indicates

that, ██

██, even had price collusion in e-books never occurred. Hence, the collusive price

increase of e-books could not possibly have caused a price cut in Kindles in 2010.

The error by Mr. Orszag and Professor Kalt is that they misapply the economics

of complementary products by adopting a single-effect, single-cause theory of the

demand for e-readers. Defendants' experts ignore all of the other changes in the market

for e-books and focus exclusively on the effect of one factor (price) to the exclusion of

other factors. The facts show that the price of e-books was not the main driver of e-

reader demand during the conspiracy period.

Other Factors Influencing e-Reader Prices

The analysis of e-reader prices by Mr. Orszag and Professor Kalt ignores all

events in the e-reader market other than the introduction of the iPad. The prices of e-

readers did not begin falling with the implementation of collusion and the introduction of

the iPad in April 2010. As shown in Figure 5 of the *Kalt Report*, the price of a Kindle

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[30] *Ibid.* at 25.

fell $140 from $399 in January 2008 (really November 2007) to $259 in November

2009.[31] Moreover, the drop in the price of the Kindle 2 to $189 took place on June 21,

2010, immediately after Barnes & Noble cut the price of the Nook. At the time this price

reduction was attributed primarily to competition between Amazon and Barnes & Noble,

which is a more plausible cause than the increase in e-book prices that had taken place

nearly three months earlier.[32] Thus, I conclude that the facts presented by Mr. Orszag

and Professor Kalt do not support their conclusion that the implementation of collusive

pricing in e-books had any effect on the prices of e-readers.

Notwithstanding that the data do not support the conclusions of defendants'

experts about Kindle prices, the procedure that they use to reach this conclusion is

unreliable. The proper test for whether a change in market conditions caused a change in

prices requires estimating a reliable econometric model of e-reader prices. As the

passage of the ABA Report that is quoted by Professor Kalt states, such a model would

include measures of the cost and technical attributes of each model of e-reader as well as

the extent of competition in the market. The latter would reflect the entry of other

portable devices that can be used for reading e-books, including tablet computers and

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[31] Figure 5 in the *Kalt Report* is misleading. The Kindle was introduced in November 2007, but sold out in 5.5 hours. Amazon did not sell Kindles until the Kindle 2 arrived in late April 2008. The price of the November 2007 Kindle was $399 (as shown in Figure 5), but in reality no transactions occurred at that price at any time in Professor Kalt's figure. Even though the first Kindle was a huge success and was not available for five months, Amazon priced the Kindle 2 $140 below the original Kindle. For the history of Kindle models and prices, see http://www.thekindlechronicles.com/2012/11/19/kindle-history-it-all-started-five-years-ago-today/.

[32] See Julianne Pepitone, "e-Reader Price War Breaks out: Kindle, Nook Cuts," *CNNMoney*, June 21, 2010, at http://money.cnn.com/2010/06/21/technology/nook_price_cut/; Brad Stone, "Amazon Drops Kindle Price to $189," *New York Times*, June 21, 2010, at http://bits.blogs.nytimes.com/ 2010/06/21/amazon-drops-kindle-price-to-189/?_r=0.

smart-phones, and would include manufacturers other than Amazon, Apple and Barnes &
Noble. Notably, Samsung, the leading seller of tablet computers and smart-phones, is
excluded from Professor Kalt's Figure 5. Obviously an analysis of price competition in
e-reading devices that excludes the largest seller is unreliable.

Implications for Damages of the Fall in e-Reader Prices

Mr. Orszag and Professor Kalt argue that the fall in the price of the Kindle after
the implementation of price collusion is a benefit to consumers that ought to be
subtracted from the amount of damages arising from collusive prices on e-books. An
essential element of this argument is that price collusion in e-books was the cause of the
fall in the price of a Kindle, which clearly is not true. But even if it were true, the
conclusion that the price reduction is a benefit to consumers that ought to be attributed to
collusion in the price of e-books is incorrect for two reasons: (1) If the price of a Kindle
had fallen solely due to the implementation of collusion in e-book prices, the cause would
have been a loss in the value of e-readers to consumers, only some of which was
compensated by the price reduction; and (2) to the extent that the fall in the price of a
Kindle was caused by the introduction of the iPad and other devices, the price reduction
did benefit consumers, but was a benefit that would have occurred anyway regardless of
whether the defendants engaged in price collusion for e-books.

The Effect of Rising e-Book Prices on Consumers

The value of an e-reader is derived from its uses, one of which is to provide a
convenient portable device for reading an e-book. While e-books can be read on devices

other than e-readers and tablet computers, the latter come in sizes that are similar to that of a physical copy of a book and that are easy to carry around. Thus, qualitatively, a reduction in the net value of e-books to consumers, such as arose due to price collusion among the defendants, reduces the value of a complementary product such as an e-reader.

As a theoretical matter, a rise in e-book prices can be expected, holding other things constant, to reduce the demand for e-readers, but the effect on the price of e-readers can be either positive or negative. For example, if the production of e-readers exhibits economies of scale, or causes some firms to stop producing e-readers and the market to become less competitive, the price of e-readers actually can increase as a result of a fall in demand. Here I examine a case that is favorable to the defendants in that I assume that the cost of making e-readers can be represented as a fixed cost, F, and an incremental cost of 100 so that the total cost, C, of making Q units is $C(Q) = F + 100Q$. I also assume that before the price increase the quantity sold can be represented as a linear equation, $Q(P) = 900 - P$. Implicit in this formulation is the assumption that the firm possesses unilateral market power. Thus, a profit-maximizing e-reader firm will maximize revenues minus costs, $PQ - C(Q) = P(900-P) - (F + 100(900-P))$. The solution to this profit-maximization problem is to set $P = 500$ and to sell 400 units.

The standard measure of the economic welfare that consumers derive from a product, called "consumers' surplus," is the difference between the maximum that consumers would have been willing to pay for the units that were purchased and the amount that they actually paid. The maximum that consumers would have been willing to pay for 500 units is the area under the demand curve between zero units sold and 400 units sold, while the amount that they actually paid is 400 multiplied by 500. Thus,

consumers' surplus is the area under the triangle defined by the demand curve and marginal cost, which here is ½(400)(400) = 80,000.

Next assume that higher prices for a complementary product cause the demand curve to shift, causing the quantity sold to be (800 – P) instead of (900 – P). Now the firm seeks to maximize P(800-P) – (F – 100(800 – P)). The solution to this problem is to set P equal to 450 and to sell 350 units. Notice that both price and quantity have fallen by 50. Consumers' surplus in the market falls to ½(350)(350) = 61,250, or a net loss of 18,750. But Mr. Orszag and Professor Kalt argue that the damages to be paid to these consumers from the cause of this loss of surplus should be reduced by the amount of the price cut – here 50(350) = 17,500. Thus, the effect of subtracting the reduction in prices from the damages arising from collusion is to *increase* the harm to consumers from 18,750 to 36,250.

The fallacy in the argument by defendants' experts is that while consumers are paying less for the complementary product, they also are deriving less net benefit from this product even though price has fallen. The price reduction in the complementary product determines how the loss of welfare due to reduced demand is shared between buyers and sellers, but in all cases both buyers and sellers are harmed by the reduction in the value of the complementary product. Making consumers pay extra for the harm they suffered in the market for the complementary product has no basis in economic analysis.

The Magnitude of the Price Reduction

Notwithstanding that price collusion did not cause the fall in the prices of e-readers and that, even if it did, the price reduction, had it occurred, would have only

partially offset the loss to consumers in the e-reader market arising from the price increase, Mr. Orszag's procedure for calculating what this price reduction should be also has no basis in fact or economic analysis.

The data that Mr. Orszag uses is taken from a planning document from Amazon for its e-reader and e-book business for fiscal year 2011 that was issued on October 1, 2010.[33] The document contains actual costs and revenue data for 2009, preliminary data for 2010 (Amazon's fiscal year ends December 31, so not all results for 2010 were in when the document was prepared), and estimates for 2011 and 2012. Figure VI-1 in the *Orszag Report* reproduces the 2009 and 2010 data for revenues and contribution profits from this document for devices, accessories and content, but not the estimates for 2011 and 2012.[34]

Mr. Orszag derives his conclusion about the price reduction in Kindles that was due to the increased price of e-books by making two calculations. The first calculation (*Orszag Report*, p. 38) simply divides the ███████████████ in 2010 ████ ██████ by ████████████████ which is ██████████ Mr. Orszag then argues that this amount should be subtracted from the damages that any consumer suffered from buying e-books at an elevated price. Figure 21 of the *Kalt Report* carries out this calculation for the named plaintiffs.

Mr. Orszag's second calculation computes ██████████████████████ for devices and accessories in 2010 ██████████ by ████████████████ in that year

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[33] Bates Nos. AMZN-DOJ-000023-38.

[34] Footnote 144 of the *Orszag Report* states that "Amazon has not produced data of profit margins for later periods." The table that Mr. Orszag cites contains data through 2012. Mr. Orszag may mean that the data for 2011 and 2012 are forecasts, but so are the data for 2010, so it is not clear why he finds the 2010 data sufficiently reliable to use but not the data for 2011 and 2012.

██ which yields the

result that Kindl████████████████████████████ He then concludes that these

losses should be subtracted from damages.

The Amazon document that Mr. Orszag uses for these calculations also contains

the gross profit from devices, accessories and content, and shipments of Kindles, the

Kindle application for other devices, and e-books, none of which is included in Mr.

Orszag's Table VI-1. Exhibit 4 reproduces all of the data on the same page of the

Amazon document that Mr. Orszag cites. I also have added an entry for average revenue

(or average price) for both devices and content, which is the revenue figure divided by

the number of units sold in the same table.

The first issue concerning the reliability of Mr. Orszag's calculation is the

accuracy of the estimate that ████████████████████████████████

████████████ Amazon's *Annual Report* for 2012 states that the company expects "a

disproportionate amount of our net sales to occur during our fourth quarter" and explains

that either over-stocking or under-stocking for the holiday season can substantially affect

its annual financial performance.[35] The implication is that the data in this table are

forecasts of holiday sales in a year (2010) when the economy is emerging from the worst

recession since the Great Depression. Given the statement in the *Annual Report* about

over-stocking and under-stocking, I infer that Amazon's forecasts are subject to

considerable uncertainty, in which case the ████████████ estimates also are uncertain.

The second issue concerning these calculations is why Mr. Orszag and Professor

Kalt chose to use the ████████████████████ and not the ████████████ on both

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[35] *Form 10-K: Amazon.com Annual Report, Fiscal Year 2012*, U.S. Securities and Exchange Commission, January 29, 2013, p. 8.

the Kindle and accessories, in calculating the ██████████████ but Mr. Orszag then

used the combined ████████████ in calculating that the ██████████████ of revenues.

The combined ██████████████████████████████████████.

The third issue concerning the reliability of Mr. Orszag's calculation is the

unexplained decision to use "contribution profit" rather than "gross profit" as the basis

for estimating the ██████████████ Mr. Orszag is silent about the appropriate concept

of profitability for calculating ██████████████ and why he believes that contribution

profit is a more accurate measure of economic profit than gross profit.

The appropriate concept of profitability in determining whether a supplier benefits

from continuing to produce a product is whether operating revenues are sufficient to

cover the incremental costs of producing the product. The concept of economic profit

differs in important ways from the methods that accountants use to calculate profits.[36]

The document that Mr. Orszag uses does not explain the difference between gross profits

and contribution profits.[37]   One plausible interpretation is that gross profits are closer

than contribution profits to the difference between revenues and operating costs, while

the latter include other types of costs, such as allocations of fixed costs (such as interest,

general administration, and legal costs) and product research and development (which is

an investment and not an operating cost and so, in economics, is not properly included in

estimating the current profits from the sale of a product). If Mr. Orszag had used the

gross profit on devices for his ██████████████████████████ would have been

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[36] The difference between economic profit and accounting profit is explained in Franklin
Fisher and John J. McGowan, "On the Misuse of Accounting Rates of Return to Infer
Monopoly Profits," *American Economic Review* Vol. 7, No. 1 (Mar. 1983), pp. 82-97.

[37]   I do not know the meaning that Amazon attaches to contribution profit, but in financial
reports of other companies the term "contribution" sometimes refers to the allocation of
fixed costs to each product line.

████ and had Mr. Orszag used the gross profit on devices plus accessories, ████████

████████████████████████████████████ As a fraction of revenue, the

██████ on devices plus accessories would have been ████████████ Thus, the

choice of contribution profit instead of gross profit has a very large effect on the results

of Mr. Orszag's calculations.

Fourth, 2011 and part of 2012 also were affected by price collusion on e-books.

Regardless of their accuracy, these data reflect ████████████████████

████████████████████ While Amazon expects ████████████

████████████████████████████████████

████ . If instead these calculations are based on the ████████████████

████████████████████████████████████

████████ If the gross profit on devices is used instead of contribution profit, the

loss is $6 per device in 2011 and a gain of $7 per device in 2012. And, if the gross profit

on both devices and accessories is used as the measure of profit, ████████████

████████████ Mr. Orszag does not explain why he ignored these expectations,

which are inconsistent with his argument that Amazon had adopted a policy of losing

money on Kindles to encourage customers to buy more e-books. The entire document

reveals that ████████████████████████████████

████████████

Fifth, the premise of Mr. Orszag's argument is that higher prices for e-books

would cause Amazon to cut prices on devices. The data on revenue and unit sales permit

a calculation of average prices, which is shown at the bottom of Exhibit 4. Note that

Amazon's financial forecasts are based on ████████████████████

████ between 2009 and 2010 and again between 2010 and 2011, ████████ While

████████ are surprising, given that collusion was known to increase average e-book

prices substantially, they do raise questions about Mr. Orszag's assumption that e-book

prices overall (as opposed to just those of the publisher defendants) were increasing.

To summarize, in calculating the value of the price cut to be subtracted from

damages, Mr. Orszag picked numbers from a summary financial projection that

maximize the amount to be deducted, without offering an explanation for why he chose

to use contribution profits on devices in 2010, as opposed to data from other years and for

gross profits and without actually knowing what either contribution profits (the number

he used) or gross profits (the number he elected not to use) mean. Thus, I find his

calculations to be arbitrary and unreliable in addition to being irrelevant.

Amazon's Business Plan for e-Book Prices

Both Mr. Orszag and Professor Kalt argue that Amazon would have raised the

prices of e-books regardless of whether the collusive agency model was imposed upon

them by the publisher defendants. The *Orszag Report* (pp. 30-32) and the *Kalt Report*

(pp. 47-48) state that the foundation for their analysis is the economic theory of pricing of

complementary goods. The essence of their argument is that increased competition e-

readers due to the entry of the iPad caused a fall in e-reader prices and profits. Professor

Kalt states that competition from the iPad ████████████████████████

████████████████████████████████████ (*Kalt

Report*, p. 48), while Mr. Orszag states that "a change in business strategy would have

been necessitated by the increased competition among devices" (*Orszag Report*, p. 32).

Mr. Orszag and Professor Kalt have no valid basis for concluding that Amazon would have increased e-book prices even if the defendants had not engaged in price collusion that raised retail e-book prices. Mr. Orszag cites a textbook in industrial organization by Jean Tirole as the basis for his analysis, but his analysis and the analysis by Professor Kalt is based on a simplistic characterization of the economic theory of the pricing of complementary products. Their conclusion about how Amazon would price e-books after the entry of the iPad is valid only under very specific market conditions facing Amazon. Mr. Orszag and Professor Kalt do not attempt to prove that these conditions existed, and some of the assumptions that are necessary to predict these conclusions clearly are not correct.

Mr. Orszag begins with a valid statement of economic theory of a firm that sells complementary products, but then incorrectly applies that theory to the markets for e-readers and e-books. The *Orszag Report* (p. 31) states (emphasis added):

> "**Depending on the business environment** (i.e., consumer demand, competition, and production costs), a company **may find it profitable** to sell the device or the complementary good at or below cost. **When consumers are homogeneous** in their demands, a profit-maximizing monopolist **would tend** to sell the content at cost and collect profits through the sale of the device ▮▮ "

The first two sentences of Mr. Orszag accurately characterize the theory of complementary goods. The theory in fact does not actually predict the price structure of complementary goods, but instead identifies how pricing depends on market conditions. Here I present a few examples of the relationship between market conditions and the predictions about prices from the theory. These examples do not cover all possible

circumstances, and are intended only to illustrate that proper application of the theory requires detailed empirical examination of the supply and demand conditions in the market – a detailed examination that is not undertaken by defendants' experts.

1. One monopolist serves both markets and consumers are homogeneous. The second sentence in the quotation from Mr. Orszag asserts that a monopolist in both markets facing homogeneous consumers will sell content "at cost" (which means at the average unit cost of production, including a competitive return on investment) and will earn monopoly profits on the device. The stated premises of this statement are that a seller of both devices and content is a monopolist and that all consumers have exactly the same demand for content. The unstated premise of this statement is that the *only* way to use content is through a device. In this context, if the monopolist prices the content at cost, the net value (consumers' surplus) to consumers in the content market is maximized. Because all consumers have the same demand curve, the monopolist can extract all of this value to consumers in the monopoly price for the device.

If any one of these conditions is not true, the profit-maximizing pricing strategy will be different. As the fourth sentence in the quotation states, the premise that the monopolized device (the Kindle) is the only way to read e-books clearly is false. Thus, one must consider the pricing behavior of firms when the assumptions in the first two sentences are not true. The following discussion examines a few of these cases for the purpose of showing that under other plausible conditions the conclusions of defendants' experts are not correct.

2. One monopolist serves both markets and consumers are heterogeneous. Heterogeneity among consumers means that the pattern of demand differs among

consumers. That is, different consumers have a different willingness-to-pay for each title

and differ in their responsiveness to price changes (i.e., their price-elasticity of demand).

If some titles have a lower price-elasticity than other titles, a monopolist in content can

increase profits by engaging in price discrimination, thereby departing from the strategy

of earning all profits on devices and no profits on content. Here, price discrimination

means charging different mark-ups over the incremental unit cost of a product for

different consumers. Likewise, price-elasticity may differ for the same title at different

times, in which case the optimal pricing strategy is to price above cost when demand is

less price-elastic then revert to lower prices when demand is more price-elastic.

3. *One market is competitive and the other is monopolized.* In this case, price in

the competitive market equals average cost and the other product is sold at the monopoly

profit-maximizing price. A monopolist in one product may also participate in the

competitive market and under some conditions has an incentive to monopolize the second

market as well. For example, a monopolist in content may be able to earn greater profits

by becoming a monopolist in devices by lowering content prices and increasing device

prices. Or, a device monopolist may be able to earn additional profits from monopolizing

the market for content for the purpose of engaging in price discrimination, charging

higher prices for consumers with more intense demand. One mechanism for achieving

this goal is for the firm that sells the monopolized product to make that product

compatible only with its product in the competitive market.

4. *Both markets are competitive and products in one market can be used with*

many products in the other market. If both markets are competitive, under normal

conditions both complementary goods (say, tea and sugar) will be sold at the competitive

price. In this circumstance a firm that sells both products (say, pre-sugared tea) will not price differently than firms that sell only one product. This outcome may not occur if one product is a durable good that is used over a long period in connection with multiple units of the other product (e.g., tea and tea cups). Competitive pricing in the market with multiple purchases will not occur if the act of buying the device ties the consumer to buying both products from the same source.

5. *Both markets are competitive but a product in one market must be used with a specific product in the other market.* If e-books from all e-retailers offer identical products but every device can display e-books from only one e-retailer, then price in the competitive device market (called in economics the *ex ante* market) may be below average cost, while price in the content market (in economics called the *after-market*) price will be above cost. Even though the content market appears to be structurally competitive (i.e., many firms selling the same product), consumers become "locked-in" to one content vendor when they buy a device. As a result, a content supplier can enjoy monopoly power over customers who have bought the device for accessing content from that supplier.[38] A necessary condition for this result to occur is that integrated suppliers offer closed systems (sometimes called "walled gardens") that require buying a device and content from the same supplier. In incorrectly applying the theory of complementary products to the device market, defendants' experts reached the conclusion that Amazon's reduction in device prices after price collusion in e-books began was due to increased profits in content and so should be subtracted from damages. One reason that this argument does not apply to the Kindle is that Amazon's e-book retail operation is not a

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[38] Severin Borenstein, Jeffrey K. Mackie-Mason and Janet S. Netz, "Antitrust Policy in Aftermarkets," *Antitrust Law Journal* Vol. 63, No. 2 (1994-95), pp. 455-82.

walled garden – that is, it is not restricted to consumers who use a Kindle. Likewise, consumers who buy iPads are not tethered to the iBookstore for buying e-books.

   6. *Firms have market power in both markets and some firms operate in both.* In this case, a firm with unilateral market power that operates in both markets and that increases price in one market will experience a measurable reduction in sales in the other market. Consequently, a firm with unilateral market power that operates in only one market, all else equal, will have a greater incentive to raise price than a firm that operates in both markets. As a result, price will be lower in each market if some firms operate in both than the corresponding prices that firms would set if all firms operated in only one market. The latter effect (a form of what economists call "double marginalization") leads to the conclusion that if both markets are imperfectly competitive, consumers benefit if some firms operate in both markets.

   The third sentence in the quotation from Mr. Orszag asserts that Amazon faced conditions like those of an integrated monopolist facing homogeneous consumers in the "pre-conduct period," or before April 2010. For Mr. Orszag correctly to have applied the theory of pricing complementary goods to the period before collusion in pricing e-books began, it must be true that prior to price collusion all consumers bought roughly the same number and titles of e-books, no e-readers other than the Kindle were available for reading e-books from Amazon, and Amazon's Kindle enjoyed monopoly power in e-readers. For Mr. Orszag to be correct that greater competition in e-readers would have caused Amazon to charge higher prices for e-books, it must be true that Amazon enjoyed unilateral market power in e-books but not in e-readers at the time collusion began.

   Neither of defendants' experts has undertaken any economic analysis at all to

prove that the market conditions before and after the introduction of collusion in e-book prices are consistent with their predictions about e-book prices. To do so requires showing that Amazon enjoyed unilateral market power in both e-readers and e-books before collusion commenced and continued to enjoy unilateral market power in e-books after the collusive agency model was imposed upon them. Indeed, some of these conditions clearly are not true.

First, consumers do not have homogeneous demand. The variation in prices of the same title through time, such as when a book goes on the backlist or a paperback edition is introduced, cannot occur if consumer demand is homogeneous. Thus, there has always been an incentive for a firm with unilateral market power in e-books to engage in price discrimination in e-books, rather than to price all e-books at average cost.

Second, Amazon never was a monopolist in devices that can read e-books. Except for 5.5 hours in November 2007, Amazon did not sell Kindles until April 2008, long after it began to sell e-books. Other portable devices that could be used as e-readers for Amazon's e-books, including laptop and notebook computers and smart-phones, were in the market before April 2008. For the Kindle to succeed, it had to compete effectively against these devices. Moreover, because the Kindle is not tethered to Amazon, but can be used to read e-books from other vendors, sales of Kindles may encourage growth in e-book sales at Amazon without necessarily increasing Amazon's market power in e-books. Indeed, by increasing the overall demand for e-books, increased sales of Kindles may intensify competition by increasing the incentive to enter the e-book retail market.

Third, because the iPad and nearly all other tablet computers are not tethered to another e-retailer, Amazon benefits from the sale of any e-reading device that is not part

of a walled garden. As shown in Exhibit 4, Amazon's plans foresaw the same number of

Kindle sales as installations of the Kindle app on other devices. Amazon also anticipated

that sales of devices plus accessories would be a profitable business. Thus, Amazon's

own plans do not reveal that its e-book business had a sufficiently large stake in the

market to justify setting the price of Kindles below cost.

Fourth, whether Amazon's profit-maximizing prices after the adoption of the

collusive agency model were higher than before hinges on the amount of unilateral

market power that Amazon possessed in e-books in the two periods. Amazon is the

largest seller of e-books, but the complaint by the defendant publishers was that

Amazon's prices prior to collusion were too low, not too high. In order for Amazon to

raise prices unilaterally requires that Amazon could have done so without losing so much

business to other e-retailers that a price increase was unprofitable. To prove that the

unilateral profit-maximizing price of e-books was higher after the introduction of the iPad

requires showing, among other things, that other e-retailers faced significant barriers to

entry in the retail e-books market. Defendants' experts present no evidence that existing

e-book retailers were unprepared to take business away from Amazon and that other e-

retailers that do not sell e-books would have been unable to enter effectively had Amazon

attempted to raise its e-book prices. The evidence in the liability phase of this litigation

shows only that Apple did not want to engage in price competition with Amazon. If

Apple had agreed with its experts that Amazon would have increased its e-book prices

anyway after the launch of the iPad, Apple would have had no need to assist in creating a

retail price-fixing agreement.

Fifth, Amazon's pricing behavior for books that were not sold under the collusive

agency model is not consistent with the conclusion that Amazon's strategy changed to charging higher e-book prices. The econometric model that is used to calculate damages is based on the difference between collusive prices and prices that were not affected by collusion over the entire period that the collusive agency model was in effect. If Amazon had changed its strategy to increase e-book prices generally, there would be no gap between prices of the defendant publishers and other prices, and my model would calculate damages to be zero. Likewise, the planning document that Mr. Orszag cites shows Amazon expected an increase in the profitability of e-books, but did not expect that these profits would come from higher prices.

Aside from the absence of proof that Amazon's optimal pricing strategy in the face of the entry of the iPad was to raise prices, the testimony of participants in the industry is not consistent with the conclusions offered by defendants' experts. Executives from both Apple and the publisher defendants testified that they expected that Amazon would force publishers to lower wholesale prices as opposed to increasing retail prices.[39] While the accuracy of these expectations is debatable, they do reveal that the argument put forth by defendant's experts does not correspond to the expectations of the participants in the price-fixing conspiracy. Finally, Russell Grandinetti, who is quoted to the contrary by Mr. Orszag, testified:

> "Some of the publishers argued with us that our pricing for ebooks wasn't sustainable, or that we must have a plan to gain control of the market and then raise prices. None of these claims were true, and we told them that repeatedly. There never has been any plan or assumption

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[39]   See Bates No. APLEBOOK-00013539 (statement of Steve Jobs to James Murdoch of HarperCollins that Amazon would demand lower wholesale prices); *Deposition of Eddy Cue* (Apple), pp. 81-82; *Declaration of Carolyn Reidy* (Simon and Schuster), p. 3; *Declaration of John Sargent* (Macmillan), p. 5; *Declaration of David Young* (Hachette), p. 7.

that at some point in the future consumer prices would or should be higher."[40]

Thus, apparently no one other than defendants' experts expected Amazon to raise prices for e-books anyway after Apple launched the iPad.

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[40] *Declaration of Russell Grandinetti*, p. 9.

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I declare that the foregoing is true to the best of my knowledge and belief.

Roger G. Noll

ON BEHALF OF : Ben

Executed in Palo Alto, California, on December 18, 2013.

Exhibit 1

(A)

	Hachette		Harper-Collins		Macmillan		Penguin		Simon & Schuster	
	Log Points	Percent	Log Points	Percent	Log Points	Percent	Log Points	Percent	Log Points	Percent
NYT bestseller - hardcover fiction	0.25	27.9	0.26	29.4	0.19	21.0	0.26	29.4	0.29	33.8
NYT bestseller - hardcover nonfiction	0.29	33.7	0.36	43.3	0.13	13.7	0.36	43.3	0.34	40.3
NYT bestseller - hardcover advice	0.38	45.7	0.34	40.8	-0.95	-61.5	0.34	40.8	0.33	39.5
NYT bestseller - trade paperback Fiction	0.32	37.8	0.32	37.1	0.14	15.0	0.32	37.1	0.22	24.8
NYT bestseller - paperback nonfiction	0.18	19.1	0.36	42.6	0.07	7.7	0.36	42.6	0.23	26.3
NYT bestseller - mass-market paperback fiction	0.30	34.8	0.17	18.9	0.11	11.5	0.17	18.9	0.24	27.1
Non-NYT bestseller - fiction	0.25	28.0	0.25	28.3	0.08	8.1	0.25	28.3	0.28	31.7
Non-NYT bestseller - nonfiction	0.17	18.0	0.27	30.6	0.06	6.6	0.27	30.6	0.30	35.6
Non-NYT bestseller - advice	0.18	20.1	0.28	32.5	0.04	4.1	0.28	32.5	0.30	34.4
Non-NYT bestseller - teen's and children's	0.23	25.5	0.20	22.3	0.10	10.6	0.20	22.3	0.22	24.0
Non-NYT bestseller - other	0.10	10.7	0.05	5.5	0.15	15.8	0.05	5.5	0.27	31.4
Non-NYT bestseller - unidentified genre	0.23	26.0	0.22	24.0	0.26	30.1	0.22	24.0	0.29	33.9

(B)

	Log Points
Hardcover version available	0.00
Paperback version available	-0.01
Frontlist, non-new release (91-365 days)	-0.03
New release (under 90 days)	-0.05

(Notes over)

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Exhibit 1

Notes to Exhibit 1

The dependent variable is the average price (in natural logarithms) for a given e-book title, retailer, and month.

Panel A presents the mean difference in e-book price (expressed in logarithms) from the period prior to agency pricing to the period following agency pricing for e-books of the specified type published by the specified publisher, given that the e-book is older than one year and has no print versions available. Panel B presents adjustments to the agency price effect for newer titles and titles with hardcover or paperback versions available.

The regression analysis adjusts for factors specific to each title, the effect of becoming a NYT bestseller of each type, whether a hardcover version of the title was available, whether a paperback version of the title was available, whether the title was less than a year old, whether the title was less than 90 days old, whether the observation was affected by the 'buy button' incident, non-durable consumption in the month of the observation, and a month time trend. The regression was calcuated using transaction level data.

Percentage differences are expressed as a percentage of the price in the absence of agency.

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Revenue and Damages, by Publisher

	Revenue[1]	Damages Percentage[2]	Damages
Hachette	$ 280,240,961	19.3%	$ 54,042,969
HarperCollins	$ 279,092,733	17.9%	$ 50,074,521
Macmillan	$ 212,153,598	6.7%	$ 14,192,907
Penguin	$ 481,215,001	19.9%	$ 95,849,492
Simon & Schuster	$ 294,900,771	22.4%	$ 66,094,485
Total	**$1,547,603,064**	18.1%	**$ 280,254,374**

1. The period for which I have been asked to calculate damages extends beyond the period included in my data. As a result, I have projected the revenue for the missing weeks based on data from March 4, 2012 to March 31, 2012, the last complete four-week period in my data.

2. Weighted average of the effects of collusion on e-book prices expressed as a percentage of actual prices.

Exhibit 3
Damages Attributable to Each State

State/Territory	Overall	Damages
Alabama	1.077	$3,017,430.82
Alaska	0.464	$1,300,701.44
American Samoa	0.001	$2,744.58
Arizona	2.051	$5,749,191.56
Arkansas	0.680	$1,905,787.57
California	10.815	$30,310,382.71
Colorado	2.051	$5,748,387.95
Connecticut	1.498	$4,199,366.27
Delaware	0.291	$814,380.76
District of Columbia	0.365	$1,023,106.92
Florida	5.781	$16,201,871.02
Georgia	2.702	$7,571,507.87
Guam	0.021	$59,897.14
Hawaii	0.408	$1,143,694.66
Idaho	0.466	$1,305,637.36
Illinois	3.736	$10,469,244.66
Indiana	1.525	$4,273,931.91
Iowa	0.838	$2,348,718.22
Kansas	0.822	$2,303,823.96
Kentucky	0.921	$2,580,646.83
Louisiana	1.074	$3,010,514.38
Maine	0.423	$1,184,649.08
Maryland	2.160	$6,052,495.91
Massachusetts	2.742	$7,683,950.24
Michigan	2.399	$6,722,141.12
Minnesota	1.725	$4,833,743.75
Mississippi	0.536	$1,503,015.16
Missouri	1.513	$4,241,660.36
Montana	0.339	$950,414.38
Nebraska	0.470	$1,318,099.40
Nevada	0.828	$2,319,652.66
New Hampshire	0.585	$1,639,208.86
New Jersey	3.171	$8,885,563.18
New Mexico	0.590	$1,653,511.16
New York	6.438	$18,042,587.49
North Carolina	2.519	$7,060,274.48
North Dakota	0.208	$582,739.59
North Mariana Islands	0.002	$5,663.16

State/Territory	Overall	Damages
Ohio	2.658	$7,449,008.77
Oklahoma	1.029	$2,884,761.77
Oregon	1.272	$3,565,543.40
Pennsylvania	3.759	$10,533,656.77
Puerto Rico	0.076	$214,002.13
Rhode Island	0.336	$941,157.07
South Carolina	1.261	$3,534,974.56
South Dakota	0.216	$605,219.43
Tennessee	1.568	$4,393,133.81
Texas	7.550	$21,160,400.17
U.S. Virgin Islands	0.025	$68,803.26
Utah	0.848	$2,377,099.14
Vermont	0.226	$634,140.36
Virginia	3.252	$9,112,887.06
Washington	2.981	$8,353,113.67
West Virginia	0.408	$1,142,814.70
Wisconsin	1.406	$3,939,560.81
Wyoming	0.235	$658,454.68
Armed Forces Americas[1]	0.007	$19,527.68
Armed Forces Africa, Canada, Europe, Middle East[1]	0.210	$588,999.22
Armed Forces Pacific[1]	0.116	$326,326.90
Federated States of Micronesia[2]	0.000	$650.68
Marshall Islands[2]	0.001	$1,496.36
Palau[2]	0.000	$821.10
Unidentified[3]	6.326	$17,730,449.98
Subtotal for Plaintiff States	55.424	$155,329,332.21
Subtotal for Class States	37.915	$106,259,737.93
Subtotal for Armed Forces	0.334	$934,853.79
Subtotal for Unidentified	6.326	$17,730,449.98
Excluded Territories	0.001	$2,968.13
Total after exclusion	99.999	$280,254,373.91

1. The Armed Forces zip codes represent military members whose state of residence is unknown
2. Federated States of Micronesia, Marshall Islands, and Palau have valid USPS zip code data, but are not included in either the Class or Plaintiff States, and damages are not being claimed for these consumers
3. This represents tranactions which were not identified as being into a U.S. state or territory, but which were included in the U.S. sales databases provided by retailers

Exhibit 4:
Amazon Performance Data
(in millions)

MATERIALS CONSIDERED

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