Ayres on Markovits and Markets: A Reply

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The article I contributed to this Symposium\(^1\) reached two basic conclusions: (1) a primary conclusion that no matter how markets are defined it can never be cost-effective to use market-aggregated data (such as market-share, market-concentration, or HHI figures) to predict the competitive impact of horizontal mergers; and (2) a secondary conclusion that, although the primary conclusion does not depend on the effective presence of international competitors, such international competition does strengthen the primary conclusion by decreasing the absolute and relative cost-effectiveness of market-oriented approaches.

Professor Ayres' Comment\(^2\) questions my article's primary conclusion (though it says several complimentary things about my own non-market-oriented approach to predicting the competitive impact of horizontal mergers as well as about the conceptual systems and theories that underlie it). I decided to accept the Editor's invitation to reply to Professor Ayres' Comment because I feared that his Comment might unintentionally deter those who have not plowed their way through my exhaustive (and, regrettably, exhausting) article from reading it by mis-characterizing the premises and nature of the argument it made against market-oriented approaches.

The risk that Professor Ayres' Comment might mislead and deter my potential readers is substantial for two reasons. The first relates to Professor Ayres' Comment. Although the Comment gives the impression of addressing my argument against the cost-effectiveness of market-oriented approaches, Professor Ayres not only (1) fails to articulate or respond to the argument in question but also (2) implies or indicates that it is based on various premises I explicitly rejected \textit{ad arguendo} and (3) incorrectly claims that it requires empirical confirmation.

The second reason why Professor Ayres' Comment may deter its readers from reading my article relates to the character of my article. My article (and the body of work to which it adds) is based on a recon-


ceptualization of micro-economic and competition theory. It employs three new conceptual systems (in Professor Ayres’ words, an “idiosyncratic terminology”) which respectively break down the gap between price and marginal cost into two major and seven minor components, distinguish price competition from quality-or-variety-increasing-investment (QV-investment) competition, and define over ten intermediate determinants of the intensity of QV-investment competition. The “revolutionary” character of my article is relevant in this context because proponents of new conceptual schemes should be required to shoulder a substantial burden of proof. This “fact” is important because it implies that readers are and should be skeptical of the value of such innovations. Since my potential readers will be reluctant to incur the substantial costs of reading, comprehending, and assessing the kind of article I wrote, they will be understandably prone to use other’s criticisms of my article as excuses or justifications for not bothering to do so.

I believe that I have carried the burden of proof that conceptual innovators should be required to bear. Both my contribution to this Symposium and a large number of other articles in which I have employed the conceptual schemes it uses have demonstrated their capacity to help us formulate new, important questions and to answer both these new questions and old questions that can be posed without them. Nothing in Professor Ayres’ Comment undermines this claim: indeed, his piece confirms the value of my conceptual schemes in that some of the arguments he makes suffer from the same deficiencies that led me to develop them.

This Reply contains five brief parts. Part I summarizes the argument that led me to conclude that no market-oriented approach to predicting the competitive impact of horizontal mergers can be cost-effective—i.e., to reach the conclusion Professor Ayres questions. Parts II-V then address Professor Ayres’ parts I-II and Conclusion.

3. Id. at 871.
4. The word “revolutionary” is Professor Ayres’. See id. at 861 (title), 871.
I. MARKOVITS ON MARKETS: A SUMMARY

Although my contribution to this Symposium deals with a large number of other issues as well, Professor Ayres' Comment focuses on my primary conclusion. My argument for this conclusion was that all market-oriented approaches to predicting the competitive impact of horizontal mergers consume substantial amounts of resources to define markets in order to calculate market-aggregated figures that have less predictive power than the non-aggregated data the analyst collected in order to define the relevant markets in the first place.

In a moment, I will outline the argument I made for my primary conclusion as well as for my conclusion that pure market-oriented approaches inevitably give very inaccurate predictions of the competitive impact of horizontal mergers. But before doing so, I want to emphasize the a priori character of my argument for my primary conclusion. Thus, although my article took pains to explain why predictions based on market-aggregated data would be extremely inaccurate (a contention that could be empirically investigated), its primary conclusion did not depend on the absolute inaccuracy of market-oriented approaches. It argued that even if these approaches were far more accurate than I suppose, they would be cost-ineffective relative to the kind of non-market-oriented approach I proposed because "the traditional approach spends a considerable amount of resources to make a market-definition decision that succeeds only in reducing the predictive value of the data on which it is based" or, more generally, because "markets cannot be acceptably defined without collecting refined data that would have more predictive power than any market-aggregated figures that can be based on the market definitions the refined data were used to generate." More specifically, I argued that the non-aggregated data market-oriented analysts do or should collect on the competitive-position distribution of the merger

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6. For example, the article also delineates and attempts to justify a particular non-market-oriented approach to predicting the competitive impact of horizontal mergers and argues that the more or less refined version of this approach that turns out to be optimal will almost certainly be both more accurate and cheaper than any market-oriented approach one could devise.

7. Markovits, supra note 1, at 797.

8. Id. at 848.

9. In my terminology, the competitive position of a seller refers to his position when dealing for the patronage of an individual buyer. A seller is said to be best-placed to obtain an individual buyer's patronage if he can profit by making a sale to that customer on terms that no one else can profit from matching (from the buyer's perspective)—oligopolistic and predatory strategic considerations aside. The best-placed seller's closest rival for the relevant buyer's patronage is the second-best-placed seller for that buyer's patronage, etc. In my terminology, the amount by which a seller is best-placed in his relations with a given buyer is referred to as his "overall competitive advantage." That advantage itself equals the sum of his basic competitive advantage (BCA) and his contextual cost advantage (CCA). For simplicity, I will ignore contextual costs and contextual cost advantages.
partners and their various product-rivals in the course of defining markets have more predictive power than the market-aggregated data they eventually use to predict the competitive impact of horizontal mergers.

In any event, my argument against the cost-effectiveness of market-oriented approaches was developed in four stages. First, I listed the various non-market-aggregated factors that determine the competitive impact of a horizontal merger—e.g., (1) the various factors that influence the amount by which the merger would reduce competition by increasing "basic competitive advantages" (such as the frequency and amount by which each merger partner was the other's closest competitor for the patronage of individual buyers and the frequency and amount by which any static efficiencies the merger would generate would exceed the amount by which the better-placed merger partner was worse-than-second-best-placed [up to the size of the relevant merger partner's basic competitive disadvantage]), (2) the various factors that influence the pre-merger profitability of contrived (and natural) oligopolistic pricing both for the merger partners and for their product-rivals, (3) the various factors that influence the extent to which the merger would increase the profitability of contrived (and natural) oligopolistic pricing for these sellers, and (4) the various factors that influence the merger's impact on QV-investment competition.

Second, I delineated the various non-aggregated data one would in this reply. (For a discussion of these terms, see id. at 761-62 and the accompanying footnotes 35-37.)

10. A seller's pricing is said to be "oligopolistic" if his pricing decision is influenced by his belief that his rivals' responses to his price will be influenced by their belief that he will react to their responses. Oligopolistic pricing is said to be "contrived" if the seller has induced his rivals to anticipate his reacting in a way that would make undercutting unprofitable for them despite the fact that it would not be in his interest to react in this way were it not for the tendency of such a reaction to deter future undercutting. Oligopolistic pricing is said to be natural when the anticipated reaction is anticipated because it would be profitable even if it did not yield the oligopolistic pricer such a strategic oligopolistic advantage.

11. The phrase "QV-investment competition" refers to the process by which supernormal profits are competed away by the introduction of additional QV investments (that create additional or superior product-variants, additional or superior distributive outlets, or additional inventory or capacity [that increases average speed of service when demand fluctuates through time]). Roughly speaking, I measure the intensity of QV-investment competition in a given "market" or ARDEPPS (arbitrarily designated portion of product-space) by the (supernormal) rate of return its most profitable project(s) yield in equilibrium.
have to collect to define the relevant markets in an abstractly correct or practically ideal way. Thus, when criticizing the traditional market-share market-concentration approach to predicting the competitive impact of horizontal mergers, I proceeded on the assumption that markets would be defined in both the ways in question—i.e., both (1) by determining the possibly unique "division of the economy's various products into non-overlapping subsets [that] maximizes the difference between the frequency with which the average member of one subset is second-best-placed to the other members of that subset and the frequency with which the average member of one subset is second-best-placed to a member of another subset"12 and (2) less abstractly and more practically ideally (if somewhat circularly) by determining the groupings of products whose placement in a market would produce market-share and market-concentration figures that would best predict the competitive impact of any mergers that might involve the products in question. On the other hand, when criticizing the Justice Department's Horizontal Merger Guidelines, I proceeded on the (admittedly implicit) assumption that markets would be defined in the second of the above two ways—i.e., I proceeded on the assumption that markets would be defined in the way that would come closest to justifying its HHI-oriented and leading-firm-merger rules.

Third, I analyzed the relationship between (A) the market-share, market-concentration, and HHI figures one could generate from such ideal market definitions and (B) the competitive-impact-predicting-factors first listed. More particularly, I showed that even if markets were ideally defined in the second sense described above, the market-aggregated data one could generate from the market definitions in question would be very inaccurate predictors of the competitive impact of a horizontal merger—that both within a given ideally defined market and a fortiori across all such markets such data would predict poorly even those factors many appear to suppose they predict well (such as the frequency and amount by which each merger partner was the other's closest competitor for the patronage of individual buyers or the pre-merger profitability of contrived oligopolistic pricing).

And, fourth, I showed that the various non-aggregated data one must collect to define the relevant markets are better predictors of the competitive impact of a horizontal merger (correlate more with the competitive-impact-predicting factors first listed) than the market-aggregated data market definitions enable one to calculate. These last two analyses were separately applied to (A) a crude version of the traditional market-

12. Markovits, supra note 1, at 790, text preceding note 70.
share market-concentration approach, (B) the Guidelines’ three crude HHI rules and their crude leading-firm-merger rule, and (C) the Guidelines’ actual, more complicated rules, which (sometimes) take into account various non-market-aggregated structural and behavioral factors that are not captured by market-share, market-concentration, or HHI figures.

Admittedly, my article did delineate and explain a number of empirical propositions that will affect the absolute and relative inaccuracy and hence the extent of the (relative) cost-ineffectiveness of market-oriented approaches to predicting the competitive impact of horizontal mergers. However, the a priori argument I made for my primary conclusion that market-oriented approaches to such questions are inevitably somewhat cost-ineffective does not depend on the accuracy of the empirical propositions in question. Specifically, my argument for my primary conclusion does not depend on the accuracy of my belief that (1) all products cannot “be placed into sets whose individual members are far more competitive with each other (are or are close to being each other’s closest competitor for a particular buyer’s patronage) far more often than they are competitive with anyone else,”13 (2) each product in any given (ideally defined) market will not be “equally competitive with all other products in that market,”14 (3) all products (both within a given ideally defined market and across all ideally defined markets) will not have “the same ratio of best-placed to second-best-placed positions (and of best-placed to close-to-second-best-placed positions),”15 and (4) all pairs of products in a given ideally defined market will not be “symmetrically placed vis-à-vis each other.”16

I also want to clarify the relevance of my critique of the Guidelines’ discussion of those supplementary, non-market-aggregated factors they say sometimes need to be considered in addition to HHI, market-concentration, and market-share figures—i.e., of my demonstration that the Guidelines omit some non-market-aggregated factors that should be considered, deem relevant some such factors that are actually irrelevant, and misstate the relevance of some of the relevant factors they do require to be considered. In particular, I want to emphasize that although my critique of the Guidelines’ more complicated rules partly reflected these de-

13. Id. at 787.
14. Id. at 788.
15. Id.
16. Roughly speaking, my argument that the presence of effective international competitors increases the cost-ineffectiveness of market-oriented approaches to predicting the competitive impact of horizontal mergers is based on the premise that their presence increases the extent to which the four propositions listed in the preceding text reflect reality. See id. at 797-806.
ficiencies in the Guidelines' discussion of such supplementary factors, my basic objection to the Guidelines' rules focused on their use of market-aggregated data and would therefore not be overcome if the Guidelines' supplementary-factor discussions were "second-best" optimal (optimal, given their use of market-aggregated data.)

This summary should enable me to show that Professor Ayres' account of my argument sometimes mischaracterizes it, that his critique of my argument fails to address it, and that his own positive argument for the Guidelines' market-oriented approach suffers from the same deficiencies that led me to reject both the traditional market-oriented approach to predicting the competitive impact of horizontal mergers and the Justice Department's Horizontal Merger Guidelines.

II. "MAKING MARKETS LESS ARBITRARY": PROFESSOR AYRES' PART I

The first part of Professor Ayres' Comment (1) implies that my argument against market-oriented approaches is based on the premise that market definitions are inherently arbitrary, (2) claims that my attack on market-oriented approaches is based on the assumption that markets would be defined "on the basis of substitutability in demand and supply," 17 and (3) argues against my conclusion by asserting that my argument does not undermine the Guidelines' market-oriented approach because it does not take account of the fact that the Guidelines adopted a "new approach" to market definition that "represented a significant improvement" in that it "tailor[ed] market definition to an important goal of antitrust law—discouraging collusion." 18 I will now respond to each of these points in turn.

First, at no point did my argument assume that market definitions were inherently arbitrary. To the contrary, my article offered an original, explicit analysis of the conditions under which markets could be defined non-arbitrarily 19 (if one ignored the problem of stipulating the minimum amount of sales a group of products must make to be consid-

17. Ayres, supra note 2, at 863. The quote is from the text preceding note 10.
18. Id. at 864. Ayres' Part I also accuses me of "overlook[ing] the solid consensus about the Guidelines' approach to market definition." Although my article did not refer to the White and Fisher articles he cites (id. at 865 n.19), I have not "overlooked" them. For reasons that my article and this Reply try to make clear, I simply disagree with their conclusion that the Guidelines' approach is an "important conceptual contribution" or "a major step in the direction of sanity." See infra text accompanying footnotes 27-32.
19. See supra text of this Reply preceding note 10 (quoting Markovits, supra note 1, at 790, text preceding note 70). To use the Joan-Robinson expression Professor Ayres cites (Ayres, supra note 2, at 863), my article offers the first operational definition of the concept of "a qualitative gap in the chain of substitutes."
erred to be a market), proceeded on the assumption that those conditions were fulfilled, and self-consciously ignored the minimum-size issue delineated parenthetically above. Indeed, this feature of my article is manifest in one of the sentences Professor Ayres quotes—i.e. in the sentence that explained my preference for the term “ARDEPPS” over “market” by pointing out that “in our monopolistically competitive world, markets (or at least their breadth) cannot be defined non-arbitrarily.”

Second, Professor Ayres is incorrect in asserting that my criticism of market-oriented approaches was based on the assumption that markets would have to be defined by using the “traditional substitutability approach.” I agree that the various “substitutability”-techniques for defining markets should not generally be applied (or in one case should never be applied) in horizontal-merger-analysis contexts (or indeed in any other context). However, as Part I of this Reply should have made clear, my argument against market-oriented approaches did not depend on any assumption that the markets in question would be defined through some unsuitable “substitutability”-approach—i.e., my argument was based on the assumption that markets would be defined in a practically ideal way.

In the present context, nothing more need to be said on this issue. Still, I cannot resist the temptation to point out that Professor Ayres has failed to appreciate the reasons why the two substitutability-approaches he discusses are so unsatisfactory. The first of these approaches proceeds by placing all goods that are “reasonably interchangeable by consumers” into the same market. As I pointed out when analyzing the Guidelines’ requirement that in addition to the relevant HHI or market-share figures decisionmakers consider “the extent to which consumers perceive the products of the merging firms to be relatively better or worse substitutes,” this approach to market definition fails to take account of the fact that the competitiveness of any two products will depend on differences in the marginal costs their suppliers will have to incur to supply them to given buyers (on marginal cost advantages and disadvantages) as well as on the extent to which those buyers find them substitutable (as well as on buyer preference advantages). The second substitutability-approach Professor Ayres discusses uses cross-elasticity of demand to de-

20. See Ayres, supra note 2, at 864, text preceding note 13 (emphasis added) (quoting Markovits, supra note 1, at 760).
21. Id.
22. See id. at 863 n.10 and accompanying text.
23. See Markovits, supra note 1, at 832.
24. This Reply will ignore the cross-elasticity-of-supply issue.
termine whether goods should be placed in the same market. Professor Ayres seems to think that the ground for objecting to this approach is the arbitrariness of picking the particular cross-elasticity at which two goods should be placed in the same market. More specifically, Professor Ayres assumes that cross-elasticity and competitiveness (which he incorrectly equates with "substitutability") are highly correlated but admits that there may be no non-arbitrary way to determine how significant the relevant shift in patronage from one good to another must be for the two products’ competitiveness to warrant their being placed in the same market. In fact, however, the problem with the cross-elasticity technique is not the difficulty of determining how much is enough but the failure of data on the cross-elasticity of demand between two products (say, B and A) to provide the information that advocates of this technique believe it will provide—information about the frequency with which (much less about the amount by which) the producers of A and B are each other's closest competitors. Indeed, the cross-elasticity of demand will not even provide such information for those of A’s customers who were on the margin of switching at least some of their patronage to other suppliers, given the prices they were originally being offered (for those of A’s consumers who originally gained little or no consumer surplus on the units in question). More formally, the cross-elasticity approach must be rejected because it does not even provide predictions about \((\Delta Q_B/\Delta Q_A)\) for some \(\Delta P_A\) (about the percentage of sales that A’s producer would lose if he raised the price of A that would go to B).\(^{25}\)

Thus, since (1) the cross-elasticity of demand between B and A is defined to equal

\[
(\Delta Q_B/\Delta P_A)/(\Delta Q_A/\Delta P_A) = [(\Delta Q_B/\Delta Q_A)/(\Delta Q_B/\Delta Q_A)] [(\Delta Q_A/\Delta Q_A)/(\Delta P_A/\Delta P_A)] = (\Delta Q_B/\Delta Q_A) (\Delta Q_A/\Delta Q_B) (\text{elasticity of demand for A})
\]

and (2) both pre-merger and pre-price-rise \((\Delta Q_A/\Delta Q_B)\) and the elasticity of the demand for A at A’s producer’s pre-merger profit-maximizing price will vary tremendously from market to market (ARDEPPS to ARDEPPS), a higher-than-average (lower-than-average) cross-elasticity of demand between B and A will not suggest that the merger of their producers is more (less) likely than average to reduce competition by combining firms that were originally each other’s closest competitors—will not suggest that \((\Delta Q_B/\Delta Q_A)\) is higher-than-average (lower-than-average), much less that A and B should be put in the same market (put in different markets). Obviously, a variant of the same argument will suggest that information about the cross-elasticity or cross-elasticities of demand between one or both

\(^{25}\) I have focused on the case of a rise in the price of A for purely expositional reasons. The same point could be made for the case of a decrease in the price of A.
merger partners' products and the product of a remaining rival will be equally useless when the issue to be decided is whether to place a given remaining rival into any market in which both merger partners are said to operate.\textsuperscript{26}

But I should not allow this tangent to draw attention away from my central response to Professor Ayres' claim that my critique of market-oriented approaches assumed that markets would be defined through some sort of traditional substitutability-technique. As this Reply's summary of my argument should have made clear, my critique was based on no such assumption.

The third contention Professor Ayres makes in his Part I is the positive claim that my critique is "less forceful when applied to the Justice Department Guidelines," which "ties the process of market definition to the purpose of the antitrust law" or (more modestly) "to an important goal of antitrust law—discouraging collusion."\textsuperscript{27} Although I share Professor Ayres' skepticism toward approaches that seek "to define markets for all or any abstract purposes,"\textsuperscript{28} I disagree with his somewhat ambiguous claims for the Guidelines' "new approach" to defining markets. Those claims are ambiguous because the word "forceful" is somewhat vague. If Professor Ayres is suggesting that the increase in accuracy he believes the Guidelines' approach to market definition permits creates some possibility that the Guidelines' rules (or some other set of market-oriented rules that are based on the Guidelines' "minimum-collusive-group" market definitions) may be cost-effective (i.e., may be more cost-effective than my or some superior non-market-oriented approach), I of

\textsuperscript{26} The cross-elasticity of demand between B and A also provides little information about whether B should be placed into A's market in cases in which A's market share is being used to predict A's monopoly power. However, in this case, the uselessness of cross-elasticity-of-demand data reflects the limited connection between even ideally calculated market-share data and the relevant firm's monopoly power (limited both because market share indicates the frequency with which the relevant firm is best-placed rather than the amount by which it is best-placed [which is the more relevant determinant of a seller's monopoly power over price] and because market share has little bearing on a seller's monopoly power over QV investment) as much as by its own irrelevance (its inability to provide any information about the amount by which the second product is worse-placed than the product under scrutiny even when it is that product's closest competitor for the patronage of an individual buyer). For an analysis of the market-share approach to predicting the monopoly power of a firm as well as of the relevance of that power in Sherman-Act-Section-2 cases, see Markovits, \textit{Review Article}, supra note 5, at 613-18.

\textsuperscript{27} Ayres, \textit{supra} note 2, at 864.

\textsuperscript{28} \textit{Id.} Thus, after explaining the sense in which markets might be defined non-arbitrarily, I wrote:

[A]lthough there may be some purpose (that I cannot divine) to defining markets in this way, the market definitions this approach will generate will not produce MP-market-share and market-concentration figures [or HHI figures, as I later showed] from which accurate or useful competitive-impact predictions can be derived.

Markovits, \textit{supra} note 1, at 791.
course reject that contention. If, however, he is merely suggesting that the Guidelines' "minimum-collusive-group" approach to market definition reduces the superiority of non-market-oriented approaches by increasing the accuracy of the Guidelines' predictions, I admit that he may be right (though I am doubtful that this is the case and find it difficult to evaluate the claim).

However, I am certain that Professor Ayres has significantly over-valued the Guidelines' "minimum-collusive-group" approach to defining markets. In many cases, that approach will be incoherent, for there will be many situations in which the membership of the "minimum collusive groups" for each merger partner and for each product-rival of each merger partner will be quite different. For example, in individualized-pricing contexts, the minimum-collusive-group approach will be incoherent when the set of firms that contains all firms that are second-best-placed or close-to-second-best-placed a significant number of times to one merger partner is different from its counterpart for the other merger partner or is different from its counterpart for one of the members of the set of product-Rs of either merger partner. But even when the minimum-collusive-group approach can be applied—even when the same "minimum collusive group" will cover enough firms to permit one to utilize this approach to define a market—the market-aggregated data that such a market definition can be used to generate will have much less predictive power than advocates of this approach to defining markets seem to suppose. In part, this judgment reflects that fact that such market-aggregated data must be evaluated in terms of the contribution that they can make to predicting the impact of horizontal mergers on competitive advantages, natural oligopolistic margins, and various determinants of the intensity of QV-investment competition as well as in terms of the contribution they can make to predicting the impact of horizontal mergers on contrived oligopolistic margins (which reflect, inter alia, "collusion" in pricing though no "collusion" will be involved when sellers obtain contrived oligopolistic margins by relying on threats as opposed to agreements). However, primarily, this judgment reflects the numerous detailed arguments I made about the absolute inability of the Guidelines' pre-merger HHI and market-share figures to predict the pre-merger prof-

29. In fact, as I noted in my contribution to this Symposium, one might even argue that since "collusion" (contrived oligopolistic pricing) and acting so as to produce retaliation barriers to entry or expansion are independently illegal, the tendency of a horizontal merger to facilitate such behavior should not count against its legality—i.e., one might argue that such behavior should be attacked separately post-merger when and if it occurs. Admittedly, I did conclude that, given the difficulty of discovering and proving such illegal behavior, this argument should be rejected. See Markovits, supra note 1, at 770 n.42.
itability of contrived oligopolistic pricing in the "market" in question as well as about the absolute inability of the Guidelines' increase-in-HHI and increase-in-market-share figures to predict the extent to which the merger would increase the profitability of contrived oligopolistic pricing even when the relevant markets are defined in a practically ideal way. \textsuperscript{30} Readers who want the full story should review pages 760-62 and 810-19 of my contribution to this Symposium and perhaps the more complete discussion contained in my \textit{Texas Law Review} article on horizontal mergers. \textsuperscript{31} That discussion will make clear \textit{inter alia} that inter-ARDEPPS differences in the HHI of the "minimum collusive group" will account for only a small percentage of inter-ARDEPPS differences in the pre-merger profitability of contrived oligopolistic pricing and that both within and across ARDEPPSes differences in the merger-induced increase-in-HHI will account for only a small percentage of inter-merger differences in the amount by which particular mergers increase the profitability of various sellers' raising the contrived oligopolistic margins they obtain.

Of course, I agree with Professor Ayres that "it will be harder to collude if more people have to agree." \textsuperscript{32} The problem is that that undoubted truth provides very little support for the Guidelines' technique for defining markets, even less support for the contention that the Guidelines' rules are reasonably accurate, and no support whatsoever for the conclusion that the Guidelines' rules are more cost-effective than the non-market-oriented approach I have proposed.

Frankly, I find it difficult to understand how Professor Ayres could have criticized me for not considering the significance of the Guidelines' approach to market definition in as much as my critique of the Guidelines was based on the assumption that their employers would define markets in the way that would come closest to justifying their various HHI-oriented and leading-firm-merger rules across all cases in which they might be applied. \textsuperscript{33}

\textsuperscript{30} \textit{Id.} at 809-19.


\textsuperscript{32} Ayres, \textit{supra} note 2, at 864.

\textsuperscript{33} Professor Ayres concludes his Part I with the salvo that "Markovits borders on being disingenuous by concluding" that market-oriented analysts "have never been explicit about their assumptions about markets." Ayres, \textit{supra} note 2, at 865 (quoting Markovits, \textit{supra} note 1, at 787). The factual issues to which the assumption-reference in the quotation referred are those stated in the text of this Reply preceding footnotes 13 through 16 respectively. I repeat my non-disingenuous claim that advocates and practitioners of market-oriented approaches have never explicitly addressed the factual issues in question. Had Professor Ayres and other experts who have expressed approval of the Guidelines done so, they would have been far less sanguine about the ability of the
In short, Professor Ayres' Part I (1) incorrectly suggests that my critique of market-oriented approaches to predicting the competitive impact of horizontal mergers assumes (A) that market definitions are inherently arbitrary and (B) that market definitions will have to be made by using some "abstract" substitutability-approach, (2) fails to recognize the specific deficiencies of the two major substitutability-approaches that have been employed, and (3) unjustifiably implies that the Guidelines' instruction that markets be defined by including within them all firms the merging firms would have to include in a profitable cartel-agreement may make their use of HHI figures cost-effective.

III. "CONCENTRATION AND COLLUSION": PROFESSOR AYRES' PART II

Professor Ayres' second part implies that my critique of the Guidelines' rules is significantly based on the arbitrariness of the specific numbers they employ,\(^\text{34}\) argues that the plausibility of the Guidelines' approach is suggested by the relationship between any actual distribution of firms with unequal market shares and the number of equal-sized firms that would produce the same HHI, and outlines a superior ("much richer") structural approach to "assessing the likelihood of collusion" that employs non-market-aggregated data. Once more, I will respond to each of these points or efforts in turn.

First, Professor Ayres is simply wrong to the extent that he is implying that my critique of the Guidelines rests on the arbitrariness of the particular numbers the Guidelines' crude rules cite. To the contrary, I assessed the Guidelines on the realistic assumption that in practice the post-merger HHI numbers 1000 to 1800 and the merger-generated-increase-in-HHI numbers 50 and 100 will not play critical roles in the application of the Guidelines—that the Department will assume that the relevant relationships are continuous and not discontinuous.\(^\text{35}\)

Second, I do not understand what Professor Ayres thinks he is accomplishing by showing that one can determine the number of equal-sized firms that would produce any given HHI. Does Professor Ayres Guidelines' HHI and market-share figures to predict the impact of a horizontal merger on "collusion"—i.e., on contrived oligopolistic margins—much less on the intensity of competition in all of its manifestations.

\(^{34}\) At least, this seems to me to be the implication of his beginning his response to my argument by admitting that "Markovits is correct that the specific numbers [the Guidelines rules appear to make critical] are arbitrarily selected and not derived from an explicit model of oligopoly." Ayres, supra note 2, at 866.

\(^{35}\) Markovits, supra note 1, at 809.
really think that (his example) a market with a dominant 60% firm and four firms each having 10% shares would behave in the same way as a market with two and a half equal-sized firms or that a merger that increased the HHI of the former market by a given amount would have the same effect as a partial buy-out that would increase the HHI of the latter market by the same amount? I see no reason to think that either of these alleged equivalencies is realistic. Of course, personally, I see no way to predict the intensity of competition in either market from the market-share of HHI figures in question. Nor do I think that one can predict the likely decrease in that intensity that a given merger or buy-out will cause from data on the amount by which it will increase the relevant market-share or HHI figures. Thus, I think that the intensity of both price and QV-investment competition will vary substantially among different markets that consist of one 60% dominant firm and four 10% firms (as it will among markets that consist of three equal-sized firms). Admittedly, I am far more skeptical than most on these issues. But observers who are less skeptical than I will suspect that the competitiveness of 60-10-10-10-10 markets will be different from that of (heuristically admissible) markets that consist of two and a half equal-sizes firms (or, more generally, that the intensity of competition may vary substantially in markets with the same HHI). And again, observers who are less skeptical than I will suspect that mergers that raise a given HHI by the same amount may have very different effects on the intensity of competition.

Professor Ayres' reminder that there is a number (not necessarily whole) of equal-sized firms that will generate all relevant possible HHIs supports my point (that the Guidelines' use of HHI figures is cost-ineffective) rather than his point (that the Guidelines' use of such data is defensible). It reminds us that very different market-share distributions (different not just in the abstract but in what they would imply to non-skeptics about the intensity of competition) will be assigned the same HHI figure. And, I would add, the same is true for the additional (related) reminder Professor Ayres failed to give us—that mergers or buy-outs between firms with very different combinations of market shares will raise the HHI by a given amount.36

Professor Ayres' third point seems inconsistent with his second. Even if, like Professor Ayres, one (unaccountably) limits the usefulness of non-market-aggregated data to assessing the likelihood of collusion,

36. In fact, my article used the fact that very different market-share distributions would produce the same HHI and that very different mergers would produce the same increase-in-HHI to justify its conclusion that HHI data have very little predictive value. See, e.g., Markovits, supra note 1, at 816 n.100.
how can one acknowledge their usefulness (even for this limited purpose) and still argue the cost-effectiveness of using the kind of market-aggregated data on which the Guidelines' crude rules focus? Of course, the absolute inaccuracy and relative cost-ineffectiveness of the Guidelines' market-oriented rules is more obvious once one acknowledges that non-market-oriented data are essential not only for predicting the effect of a horizontal merger on contrived oligopolistic margins but also for predicting its impact on competitive advantages, natural oligopolistic margins, and QV-investment competition.

In short, Professor Ayres' second part (1) incorrectly implies that my critique of the Guidelines is partially based on the inevitable arbitrariness of the HHI and merger-generated-increase-in-HHI figures their crude rules employ, (2) attempts to support the Guidelines' rules in a way that undermines them by revealing that they assume the equivalence of market situations or changes in market situations that do not seem likely to be equivalent, and (3) acknowledges the usefulness of non-market-aggregated data despite the fact that that acknowledgement undermines his attempt to support the Guidelines' market-oriented approach to predicting the competitive impact of horizontal mergers.

IV. "MARKOVITS' COMPETING STRUCTURAL THEORY":
PROFESSOR AYRES' PART III

After kindly congratulating me for the "theoretical imagination" I showed in developing my competing structural theory, Professor Ayres' third part criticizes me for writing an article that is "awash with empirical pronouncements without cited empirical support," accuses me of "ignor[ing] legions of empirical studies assessing the effects of concentration on markups," chastises me for making an unjustified "claim that the market approach is 'mindless,'" and suggests that the relative cost-effectiveness of using non-market-aggregated and market-aggregated data to predict the competitive impact of horizontal mergers "deserves some empirical attention." Once again, I will address each of these

37. Ayres, supra note 1, at 869. I would like to note that my list of non-market-aggregated factors is more theory-based than, more comprehensive than, more specific than, and in many respects, opposed to the alternative lists Professor Ayres has in mind. I would also like to note that although one part of my approach relies heavily on Stigler's excellent piece A Theory of Oligopoly, 72 J. POL. ECON. 44 (1964), the rest was developed and published well before the publication of the various alternative "structural theories" Professor Ayres has cited (including his own).
38. Ayres, supra note 2, at 869, text preceding note 33.
39. Id. at text preceding note 35.
40. Id. at 870.
41. Id.
points in turn.

I would like to make three comments in response to Professor Ayres’ criticism that my article contains many unsupported empirical "pronouncements." First, none of my basic conclusions (certainly not the primary conclusion to which Professor Ayres objects) depends on these "armchair-empirical judgments" (if I may use my own predictably gentler expression). Second, the reason that I have not offered empirical support for those judgments is that I am a theorist, not an empirical researcher, and no one else has done the necessary research (which is not surprising, given that most of the empirical issues in question relate to concepts that are unique to my work and given that I teach in a law school and hence am not well-placed to induce economics graduate students to test my theories). Third, I would like to suggest that although (like me) Professor Ayres is a lawyer as well as an economist his opposition to my use of armchair-empirical judgments is very much the reaction of a pure economist, or at least a pure economist who is wearing his academic hat. Since academic lawyers are more likely to be interested in policy questions than academic economists when they are doing their "professional or academic" work, they are more likely to be willing to proceed (in the absence of good data) on armchair-empirical judgments. I should add that academic lawyers are more likely than professional economists to be concerned with such issues as the best way for legislators, administrators, and judges to react to uncertainties about the relevant facts, more likely to do (admittedly informal) "sensitivity" analyses of the dependence of particular conclusions on particular factual assumptions, and more likely to try to write articles that analyze at one time all the economic, ethical, and process-related issues that are relevant to the resolution of some policy questions.

Let me turn now to Professor Ayres' accusation that I have "ignore[d] legions of empirical studies assessing the effect of concentration on markups." In the lawyers's phrase: I demur—i.e., "I did it; so what?" I will argue in a moment that the existence of a universal tendency for increases in concentration to cause increases in markups would not be at all relevant to the assessment of my conclusion that market-oriented approaches to predicting the competitive impact of horizontal mergers are cost-ineffective and would be far less relevant than Professor Ayres supposes to the assessment of my claim that such approaches are extremely inaccurate. But even if I were wrong in these respects, the empirical literature to which Professor Ayres refers would not be rele-

42. Id. at 869.
vant to either of the issues in question. As I have demonstrated in several previous articles, the "literally scores of papers" to which Professor Ayres refers are irrelevant because they do not in fact test the price theories they purport to test. Virtually all of the empirical studies in question purport to test (oligopolistic) price theory by running inter-market regressions of concentration (or concentration and barriers to entry) on rates of return. Even if these tests would otherwise be satisfactory (even if they distinguished between the effects of increases in concentration on competitive advantages and oligopolistic margins and even if their market definitions and rate-of-return estimates were reliable), they would be invalidated by their failure to take account of the fact that price-fixing or anything else that raises markups at the relevant market's original QV-investment level will tend to increase equilibrium QV investment in the market in question (in my terminology, by raising the highest-supernormal-profit-rate curve). I will attempt to outline the relevant argument here, though readers who are not familiar with my work may find the discussion that follows hard-going.

Two basic points need to be made in this context. First, the fact that price-fixing will lead to an increase in equilibrium QV investment implies that it may not even raise the equilibrium average supernormal profit-rate in the market in which it is practiced. Indeed, price-fixing may not even raise the equilibrium highest supernormal profit-rate (the rate generated by the most profitable project) in the relevant market. In particular, in the case in which both the no-price-fixing and the price-fixing entry-deterring QV-investment levels are higher than their entry-barred expansion-deterring QV-investment-level counterparts and QV investment will equal the original entry-deterring level in equilibrium, price-fixing will not raise the equilibrium highest supernormal profit-rate unless the potential entrant who would just be deterred from entering at the price-fixing equilibrium QV-investment level would face higher barriers than

43. See, e.g., Markovits, Proving (Illegal) Oligopolistic Pricing: A Description of the Necessary Evidence and a Critique of the Received Wisdom About Its Character and Cost, 27 STAN. L. REV. 307, 322-23 (1975). See also Markovits, supra note 1, at 839-40.

44. Ayres, supra note 2, at 869.

45. For a detailed discussion of this issue, see R. Markovits, Oligopolistic and Predatory Conduct, supra note 5.

46. In my terminology, potential entrants or expanders are said to face barriers to entry or expansion "to the extent that [ignoring QV-investment disincentives or incentives] the most profitable QV-investment project available to them at the relevant point in time should be expected to generate a lower certainty equivalent supernormal rate of return over its lifetime than the lifetime certainty equivalent supernormal rate of return the established firms should be expected to realize on their most profitable project(s) in the relevant area of product space." Markovits, supra note 1 at 763-64. For the definition of the four different types of barriers I have found productive to distinguish, see id. at 764.
the potential entrant who would have just been deterred from entering at the lower, no-price-fixing equilibrium QV-investment level. Similarly, in the case in which both the no-price-fixing and the price-fixing expansion-deterring QV-investment levels are higher than their entry-deterring counterparts, price-fixing will not raise the equilibrium highest supernormal profit-rate unless the sum of the barriers and QV-investment disincentives\textsuperscript{47} facing the potential expander who would just be deterred from expanding at the price-fixing equilibrium QV-investment level is higher than its counterpart for the counterpart best-placed expander at the lower, no-price-fixing equilibrium QV-investment level. Admittedly, these conditions will often be fulfilled since the potential entrants and expanders who are successively best-placed as QV investment rises will tend to face progressively higher barriers since each successive new project will tend to be inferior to its predecessor. But, even when the relevant above condition is fulfilled and price-fixing therefore does raise the relevant equilibrium highest supernormal profit-rate in the ARDEPPS in which it is practiced, price-fixing may not raise the equilibrium average supernormal profit-rate, for at the same time that it raises the highest rate(s) in a question, it will tend to increase the percentage of QV investment in the ARDEPPS that consists of projects that generate rates of return that are below not only that highest rate but even the no-price-fixing average rate (by inducing the execution of additional, worse-than-ARDEPPS-average QV investments).

Second, even if contrary to my expectations price-fixing in a given ARDEPPS would tend to increase the equilibrium average supernormal profit-rate in that ARDEPPS, the percentage of the inter-ARDEPPS variance in equilibrium average supernormal profit-rates that is attributable to inter-ARDEPPS differences in contrived oligopolistic margins (or [P-MC] gaps) is far too low to permit one to test a hypothesis about the effect of concentration (or concentration and barriers to entry) on contrived oligopolistic margins (or on the sum of such margins and competitive advantages) by regressing cross-market concentration (or concentration and barriers to entry) on equilibrium average supernormal rates of return. This second proposition in turn reflects four relationships, "facts," or plausible assumptions: (1) the fact that the equilibrium highest supernormal profit-rate in a given ARDEPPS often equals the lower of the sum of the barriers and disincentives facing the best-placed potential expander at the relevant equilibrium QV-investment level and

\textsuperscript{47} In my terminology, a potential QV investor is said to face a QV-investment disincentive "to the extent that the QV investment he is contemplating would reduce the profits his pre-existing QV-investment projects would generate even if it would not induce any rival to retaliate." \textit{Id.} at 764-65.
the sum of the barriers and disincentives facing the best-placed potential entrant at the relevant equilibrium QV-investment level;\(^48\) (2) the "fact" that (or, if you would rather, the plausibility of the assumption that) variations in the size of different ARDEPPses’ average contrived oligopolistic margins (or [P-MC] gaps) account for a relatively small percentage of inter-ARDEPPS differences in the relevant, lower barrier-plus-disincentive sums; (3) the fact that the relationship between an ARDEPPS’ highest and average equilibrium supernormal profit rate will depend on the ratio of “most profitable” QV investments to equilibrium QV investment and the rate at which the profitability of those projects that do not belong to the set of most profitable projects declines; and (4) the "fact" that (the plausibility of the assumption that) inter-ARDEPPS differences in contrived oligopolistic margins (or [P-MC] gaps) account for only a small percentage of the inter-ARDEPPS variance in the ratio of highest-to-average equilibrium supernormal profit rates.

Unfortunately, limitations of space have precluded me from reproducing the diagrams and analyses that would have made this critique of the purported empirical tests of (oligopolistic) price theory more comprehensible. However, I hope that this outline of the relevant argument has provided some basis for assessing my decision not to discuss the empirical literature Professor Ayres cites: since changes in the gap between P and MC at a given QV-investment level will lead to changes in equilibrium QV investment and since the average and highest equilibrium supernormal profit rate in a given ARDEPPS will be largely determined by factors other than the competitiveness of its prices, one cannot test hypotheses about the tendency of changes in concentration to cause changes in contrived oligopolistic margins or (P-MC) gaps by running cross-market regressions of concentration on rates or return.

Professor Ayres’ third part also argues that the empirical studies in question force me to “abandon [my] claim that the market approach is ‘mindless.’”\(^49\) In one sense, I have already responded to this third con-

48. At least, this is the case in two typical situations: (1) when (a) the established firms can take full advantage of the opportunity to restrict their own QV investments that is created by the barriers to entry successive best-placed potential entrants face and (b) the barriers to entry facing successive best-placed potential entrants did not rise sufficiently quickly to induce the established firms to allow some entry to occur in order to reduce equilibrium QV investment below the original entry-preventing level and (2) when the established firms are unable to take any advantage of the opportunity to restrict their own QV investments created by the barriers to entry successive best-placed potential entrants would face. When neither of these sets of conditions is fulfilled—e.g., when condition 1(a) but not condition 1(b) is fulfilled, the determinants of the equilibrium highest supernormal profit-rate will be far more complicated to analyze. See R. Markovits, Oligopolistic and Predatory Conduct, supra note 5, chapter 1.

49. Ayres, supra note 2, at 870.
tention by explaining why I find the empirical studies to which Professor Ayres refers unpersuasive. Nevertheless, I want to make two additional points. The first of these is substantive. Professor Ayres seems to think that "if, as [he believes] the studies indicate, market concentration is positively correlated with collusion," I must "abandon [my] claim that the market approach is 'mindless.'" In this, he is mistaken. My claim did not depend on the proposition that there is no inter-market correlation whatsoever between concentration and contrived oligopolistic margins or (P-MC). Nor, indeed, did it depend on the proposition that there is no tendency for increases in concentration to cause increases in contrived oligopolistic margins or (P-MC) gaps. One does not have to deny the existence of some such correlation or causal relationship to deny the existence of any defensible theoretical or empirical support for basing competitive-impact predictions even *inter alia* on market-aggregated data such as market-share, market-concentration, and HHI figures. The gravamen of my accusation of "mindless[ness]" is that no one has ever given any reason to believe that the use of such market-aggregated data is as cost-effective as the use of the non-aggregated data one should employ to generate the market definitions the calculation of the market-aggregated data presupposes. Nothing Professor Ayres has said or cited refutes this contention. Certainly, the existence of a cross-market correlation between rates of return and concentration would not do so.

My second point is stylistic. I have always tried to conduct debates in what my undergraduate university's Student Conduct Code called "a decent and respectable manner." The adjective "mindless" is a strong word, and I hesitated to use it. It is therefore important to me to repeat the full paragraph in which it was employed. I actually wrote:

50. *Id.* It is worth pointing out that once more Professor Ayres has focused solely on contrived oligopolistic margins—*i.e.*, has ignored the effects of concentration on competitive advantages—and has assumed that all contrived oligopolistic margins reflect "collusion." The exclusivity of the former focus is regrettable, and the latter assumption is false. Unfortunately, the focus and assumption in question is not unique to Professor Ayres. As my contribution to this Symposium indicated, the Guidelines made the same "mistakes." Markovits, *supra* note 1, at 832. I have also encountered the same pattern of "errors" in other authors. See, for example, Markovits, *Oligopolistic Pricing Suits, supra* note 5, in which I point out that then-Professor and now-Judge Posner's price-discrimination test for oligopolistic pricing presumes that there are no competitive advantages or at least no competitive advantages that vary from buyer to buyer (*id.* at 940-42), that his pronouncements about the relationship between most sellers' highest non-oligopolistic prices and marginal costs vastly underestimate the size of the average competitive advantage of best-placed firms (*id.* at 936-38), and that his legal analysis ignores the legal significance of the fact that oligopolistic prices can be contrived without collusion, which is relevant because the Sherman Act fails to prohibit unsuccessful attempts to contrive oligopolistic margins by using threats—given that Section 1, which does include an attempt provision, covers only agreements (collusion) and Section 2 does not include an attempt provision (*id.* at 933-35). (Professor Posner also ignored the legal significance of the fact that oligopolistic prices can be obtained naturally.)
I have tried to be careful, exhaustive, and objective even at the cost of being pedantic, exhausting, and boring, but in the end I have to let my frustration break through. The real reason I find it so difficult to determine whether the HHI-oriented approach is more intellectually respectable (involves less significant intellectual errors) than the traditional market-share concentration-ratio approach is that both approaches have been surprisingly mindless: "surprisingly," given the length of the period during which the traditional approach has been employed and the obvious intelligence of many of the people associated with the production of the Guidelines, but "mindless" nonetheless, given the failure of either approach's authors and advocates to provide them with any significant amount of theoretical or empirical support.51

Professor Ayres concludes his third part by arguing that "surely" the relative cost-effectiveness of using non-market-aggregated and market-aggregated data to predict the competitive impact of horizontal mergers "deserves some empirical attention."52 I am not so sure. As Part I of this Reply indicated, I believe that my article demonstrates the (greater) cost-effectiveness of using non-aggregated data on a priori grounds. The only reason one might want to investigate the relative cost-effectiveness of the two types of approaches would be to measure the extent of the superiority of the non-market-oriented approach. Admittedly, such information would be relevant if one wanted to determine whether it was desireable to incur certain costs to bring about the replacement of the market-oriented approach to predicting the competitive impact of horizontal margins by some more or less refined version of the kind of non-market-oriented approach I have proposed.

V. PROFESSOR AYRES' "CONCLUSION" AND MY OWN

Professor Ayres concludes by criticizing me for failing to cite various theoretical articles that he believes are "related" to my own—most particularly, Landes and Posner's,53 by describing my approach as "a revolutionary theory that deserves more analytic and empirical attention,"54 and by expressing some concern that my "glorious revolution will remain private."55 I would like to thank Professor Ayres for this positive assessment and then conclude by commenting on the Landes-Posner approach and the risk that my own conceptual schemes and theories will be ignored.

52. Ayres, supra note 2, at 870.
53. Id. (citing Landes & Posner, Market Power in Antitrust Cases, 94 HARV. L. REV. 937 (1981)).
54. Ayres, supra note 2, at 871.
55. Id.
First, I did not cite the Landes and Posner article because—unlike Professor Ayres—I do not find it “sympathetic to [my] enterprise.” In fact, the Landes-Posner piece was one of the articles I had in mind when I wrote in the Conclusion of my original article:

In the late seventies and early eighties, dissatisfaction with the traditional market-share, market-concentration-ratio approach to horizontal-merger analysis became more general, but this dissatisfaction did not extend to the general category of market-oriented approaches. The perception was that one need only tinker with or revise and supplement the traditional approach to produce an analytic method that was truly cost-effective. Actually, when the Landes-Posner article appeared, I considered writing a response but decided that it would be more productive for me to get on with my own work instead—that I could communicate my own ideas more effectively by expositing them on their own rather than by using someone else’s approach as a foil. As I hope that my contributions to this Symposium have made clear to anyone who is familiar with the Landes-Posner piece, my objection to their approach is far more fundamental than an objection to “focus[ing] on the markup as the exclusive indicator of monopoly power.”

Professor Ayres’ final point is obviously of great concern to me. No academic wants his work (be it revolutionary or not) to remain private. One does not have to be an out-and-out Kuhnian to realize the difficulty of persuading scholarly communities to shift their paradigms (even if the persuader is not an outsider, teaching in a different faculty). I realize that my conceptual systems (my “idiosyncratic terminology” in Professor Ayres’ words) “raise the reader’s barriers to entering [my] intellectual scheme,” but I can do nothing about that fact: I use these new concepts because they are essential to my work—because I cannot raise the new questions I have posed or answer the various questions I have analyzed without using these terms (or some equally novel and troublesome equivalents). Although many economists are far more skeptical than

56. Id.
57. Markovits, supra note 1, at 858.
58. As Professor Ayres suggested, supra note 2, at 871. Professor Ayres’ observation that I object to identifying monopoly power with markup is correct. I do so not only because—as he says—this approach ignores monopoly power over QV investment but also because it ignores the distinction between monopoly power over pricing (competitive advantages) and oligopoly power over pricing as well as the volume of sales on which the relevant monopoly power is enjoyed. The connection between a firm’s markup and the absolute elasticity of the demand it faces at its actual price (on which Posner and Landes focused)—$\epsilon_0$—can be established by differentiating total revenue with respect to quantity, substituting marginal costs for marginal revenue (on the ground that the two will be equal if the seller is a sovereign profit-maximizer), and engaging in various algebraic manipulations. In particular, this procedure can be used to prove that $P/MC = \epsilon_0 / (|\epsilon_0| - 1)$.
59. Ayres, supra note 2, at 871.
Professor Ayres of the value of my work, I am encouraged by their inability to undermine my claims as well as by the increasing number of originally skeptical experts who have become convinced that my approaches are well worth the initial costs they impose on readers. I can do no more than set forth my ideas as carefully as I am able and hope that readers will take the time and make the effort to comprehend and assess them.