

(i) The number of competitors. In response to the argument that long distance is characterized by a small number of competitors including three "collusively dominant" firms, we observed that there is a highly aggressive fringe consisting of large, well-funded, growth-oriented firms, that this fringe has a significantly larger market share today (13%) than did MCI and Sprint in 1984 (8%), and that revenue shares and numbers of firms may be misleading indexes of competitive pressures in situations with excess capacity. Elsewhere in our first report (p. 144), we noted that, according to another RBOC witness (McCormick, p.5), AT&T is "weakly positioned to exploit its size." MacAvoy offers no response.

(ii) The stability of market shares. In response to the argument that long distance market shares have stabilized, we pointed out (pp. 154) that the stability of market shares is endogenous, and cannot be regarded as a structural feature that promotes collusion. Stability of shares may reflect collusion if they arise from a structural feature, such as the existence of a natural (e.g. geographic) method of dividing the market, but this is not the case in long distance. Indeed, the alleged stability of shares coincides with high churn, indicating that stability does not arise from collusive forbearance. Finally, we pointed out that shifts in shares have been substantial even by MacAvoy's reckoning. MacAvoy simply ignores these criticisms.

MacAvoy also argued that the stabilization of market shares coincided with the introduction of price caps, and that this phenomenon is indicative of collusion because the price cap regime gave AT&T greater ability to punish its competitors. In response, we pointed out (p. 155) that AT&T's alleged ability to punish was severely constrained under price cap regulation, and that the relative stabilization of market shares subsequent to the adoption of price caps probably resulted from the fact that increased flexibility permitted AT&T better to defend its customer base. These criticisms are unchallenged..

Oddly, of all the RBOC witnesses, only Taylor and Zona revisit the alleged stabilization of market shares. They argue that AT&T allowed its share to stabilize at 60% because this is regarded as a "safe haven" for antitrust purposes (p. 11). This is clearly inconsistent with MacAvoy's theory. More

importantly, it does not explain the fact that shares have stabilized at very different levels for different long distance segments (e.g. wholesale vs. retail). As we pointed out in our first report (p. 163), the commonality here is the date of relative stabilization, and not the market share at which stabilization occurred. This objection is clearly applicable to Taylor and Zona's theory, and they simply ignore it

(iii) The homogeneity of services. In response to the claim that the homogeneity of long distance services promotes collusion (which we discussed in pp. 155-156), it bears repeating here that there is nothing remarkable about the homogeneity of services in this industry as compared to other industries; that the IXCs have not avoided differentiating service offerings when differentiation serves customer needs, even though this would, under MacAvoy's view, undermine tacit collusion; and that other RBOC witnesses (McCormick, p. 22) contradict MacAvoy's characterization of long distance as a homogeneous service. These arguments are un rebutted.

(iv) Similarity of costs. In response to the assertion that IXCs have similar costs due to common fiber technology (and the associated claim that this promotes collusion), we noted (p. 156) that MacAvoy neglects a variety of costs besides access and transmission: that AT&T's costs may differ from those of other IXCs because much of its capacity is inherited from the old Bell system; that AT&T has unique costs associated with the residual effects of incumbency (specifically, it serves more low-volume customers); that regulation is asymmetric; and that market shares are unequal. MacAvoy has offered no rebuttal.

(v) Barriers to entry. We have already discussed this issue in section A, where we noted MacAvoy's failure to respond to the issues raised in our first report.

(vi) Tariffing. In response to the claim that tariffing facilitates collusion by providing competitors with advance notice concerning price changes, we observed that this argument is completely inapplicable to the wholesale market due to the nature of tariff filings (pp. 156-157). MacAvoy concedes this point (p.

25).<sup>84</sup> Consequently, if his reasoning is otherwise correct, he ought to find greater evidence of competition in wholesale services than in retail services. His own evidence contradicts this implication, since he finds essentially the same price-cost margins across wholesale and retail services (see his figures 4 through 10). Thus, MacAvoy's price-cost margins clearly are not measuring the extent of market power, precisely as we argued in our original report (see below for further discussion).

We also pointed out that MacAvoy's analysis concerning tariffing is illogical (pp. 156-57). In some industries, regulatory disclosure requirements might improve a competitor's ability to monitor another competitor's prices. This is not the case in retail long distance, since an IXC cannot notify the public of a price change without simultaneously notifying its competitors. MacAvoy would evidently have us believe that *advance* notification eliminates the first-mover advantage associated with a price reduction (reply affidavit, pp. 23-24), but this is not the case.

To illustrate this point, consider an industry occupied by two firms, A and B. At first, suppose that there are no regulatory requirements to give advance notification of price moves. Suppose also that price changes are immediately observable, but that it takes one week for a firm to formulate a response once a move has been observed. Then if A cuts its price on September 15, B will not respond until September 22.

Now suppose that A and B are both required to file "tariffs," which render their pricing moves observable 14 days prior to implementation. A must now telegraph on September 1 that it will implement a price reduction on September 15. B observes the tariff filing on September 1, and immediately begins to formulate its response. As before, this process takes one week. B files its new tariff on September 8, and its new price goes into effect 14 days later -- on September 22. The result is precisely the same as in the absence of a filing requirement.

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<sup>84</sup>The implications of this concession are profound in light of the fact that Basket 1 services inherit the competitive characteristics of Basket 3 services.

MacAvoy also argued in his first affidavit that AT&T's opposition to detariffing proves his argument. We responded (pp. 157-158) that AT&T was merely attempting to level the playing field. If, in the above example, B is detariffed, then B would be able to respond to A's price moves in 7 days, while A would take 21 days to respond to B's price moves. A's opposition to B's detariffing is therefore perfectly understandable, and has nothing to do with the likelihood of collusion.<sup>85</sup> Indeed, we also noted that AT&T has, contrary to the implications of MacAvoy's argument, opposed advance notification more generally. MacAvoy has offered no response.

In summary, the RBOC witnesses have essentially abandoned the argument that conditions in long distance are conducive to tacit cooperation among the IXCs.

## 2. "Binding" price caps

*Conclusion #46: Evidence indicating that AT&T's price caps have been "binding" since 1989 is consistent with the absence of market power in long distance services.*

In his original affidavit, Hausman argued that AT&T's API has been close to its PCI since 1989. From this observation, he has attempted to infer the existence of significant market power. This inference is illogical. As we explained earlier in this report as well as in our first report (p. 149), AT&T has every incentive to raise basic rates as high as permitted under the price cap scheme, for the simple reason that regulation still holds basic rates below costs for the lowest volume customers. No RBOC affiant has challenged the logic of this argument. Hausman only challenges the premise (that basic rates are below costs for low volume customers), and we have already dealt with his arguments on this subject. Thus, evidence on price caps proves nothing other than the existence of a regulatory system that distorts competitive pricing behavior

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<sup>85</sup>Detariffing of B makes it easier to punish defections by A, but more difficult to punish defections by B. The net effect on collusion is ambiguous.

### 3. Price leadership

*Conclusion #47: Evidence of alleged "price leadership" is entirely consistent with the absence of market power in retail long distance services.*

In his first affidavit, Hausman asserted that patterns of "price leadership" in retail long distance are indicative of non-competitive behavior. We responded that, as described in section VI.A.2.b.ii above, the appearance of price leadership in basic rates results from regulatory distortions of competition, due to the asymmetric treatment of AT&T and other IXCs (pp. 150-151). Moreover, we pointed out that there is no evidence of price leadership in discount plans in the higher volume market segment, where competition is unhindered by regulation. With the exception of Hausman's previously rebutted general comments concerning the underlying premise that basic rates are below costs for low volume customers, this position is unchallenged. Thus, there is no reason to regard this alleged phenomenon as inconsistent with competitive behavior, once regulatory distortions are properly considered.

The central "example" of price leadership trumpeted in Hausman's original affidavit occurred in July, 1993. According to Hausman, AT&T's price caps were raised during this month solely as the consequence of an accounting change that had no implications for AT&T's real economic costs, and that had a larger effect on AT&T's accounting costs than on MCI or Sprint's accounting costs. Yet Hausman asserts that AT&T raised its rates, and that MCI and Sprint followed.

We have responded (pp. 151-152) first, that such an event would be entirely consistent with competitive behavior given the kinds of regulatory distortions that exist in retail long distance (again, see section VI.A.2.b.ii above); second, that IXC discount activity increased during this same period, partially or completely offsetting the rate increase; and third, that Hausman has mischaracterized the event. In fact, the rate increase in question was primarily attributable to changes in real costs that were probably common across the IXCs; also, AT&T simultaneously announced increases in quality.

Hausman's response (reply affidavit, pp. 13-14) is remarkable for its failure to come to grips with

any of the substantive economic issues. First, he asserts that none of the AT&T affiliates "ever explain why AT&T is able to raise its prices in responses (sic) to AT&T-specific accounting changes such as the change in SFAS 106" (p. 13). He then repeats his argument. We can only conclude that he failed to read the relevant portions of our first report, which contained a detailed explanation of the manner in which this very phenomenon could arise as the result of regulatory distortions, absent the existence of any market power. Second, he acknowledges our claim that discount plan activity increased during the relevant period, but asserts that we "do no analysis of the other 5 times in the last 3 years when AT&T increased price and MCI and Sprint followed" (p. 14). This, of course, is unresponsive to our arguments (including both the one that Hausman acknowledges, and the two that he ignores) concerning the July 1993 event. And it is this event in particular that Hausman has discussed in detail, holding it out as a supposedly "clean" experiment due to the special factors that precipitated the change in the price cap. Hausman has made no such claims, nor provided analysis containing a comparable level of detail, for any of the other supposed events. In effect, with his response, he has abandoned his "pet" event in favor of other examples that he evidently regards as inferior.

Even with respect to these other supposed examples of price leadership, Hausman has ignored our analysis completely. The unrebutted arguments contained in our first report and summarized above explain exactly the sort of pattern that Hausman alleges, based on known characteristics of the regulatory system, without resorting to the otherwise unsubstantiated assumption of market power. Moreover, MacAvoy's analysis demonstrates our point that discount plan activity has, in general, offset the effects on higher volume customers of the rate increases to which Hausman refers.<sup>86</sup>

Finally, Hausman asserts (p. 14) that we "never examine long distance prices at all, but still claim

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<sup>86</sup>This is evident from an examination of the graphs in MacAvoy's appendix B. The "theoretical minimum" rates displayed in these graphs refer to the best available discounted prices. In most of the graphs, this "theoretical minimum" does not mirror the overall increase in basic rates over the last few years, and typically does not exhibit significant movements that coincide with the movements in "standard rates."

that based on theoretical arguments (but no data) that Basket 1 services are competitive." This is a recurring theme in his report; see also p. 8, footnote 20, where he writes that we "never examine prices for Basket 1 services in spite of [our] vehement (theoretical) claim that prices are competitive." It is time, once and for all, to lay this astonishing misconception to rest. Perhaps Hausman never reads the studies and affidavits of other experts. We do. What we have found is an abundance of data, which we have examined with diligence and care, but precious little coherent economic analysis of that data (particularly in the case of the studies submitted by the RBOC affiants). Unlike Hausman, we believe that more data is a poor substitute for a manifest failure to properly interpret existing data.

#### **4. Price discrimination**

*Conclusion #48: There is no evidence of price discrimination in retail long distance services that would justify an inference of market power.*

In his original affidavit, Hausman argued that non-cost-based price differentials indicate an absence of competition. However, he proffered only a single alleged example of non-cost-based price differentials: cellular vs. landline long distance. In brief, he claimed that the IXCs charge the same rates for landline and cellular long distance, despite the fact that access charges are cheaper for cellular long distance.

In our first report (p. 147), we responded that "one cannot reach valid conclusions concerning the relative costs of production for two services by examining usage of a single input (access)... [I]t is our understanding that rough parity between the prices of landline and cellular long distance is appropriate because the costs of billing, collections, and fraud are higher for cellular services than for landline services."

The discussion of this issue in Hausman's reply affidavit begins with a complete non sequitur (p. 15): "the IXCs have continuously claimed (including in this proceeding) that access is by far their largest cost, representing approximately 45%-50% of total costs." It must be understood that the referenced claim

refers to long distance generally, which is dominated by landline long distance. The mere fact that access is the largest cost item for landline long distance says nothing about the significance of other costs in cellular long distance.

Hausman then disputes the contention that fraud alone offsets the differences in access costs between cellular and landline long distance. In doing so, he ignores completely the roles of other factors, including billing and collection costs. The core of his argument is that "fraud cost the cellular industry only 3.7% of revenue in 1994 which is far less than the difference in access charges" (p. 15, footnote 36). This too is a non sequitur. The fact that fraud consumes 3.7% of the revenues of cellular companies does not imply that it consumes 3.7% of the cellular long distance revenues of long distance companies. Indeed, Hausman's statistic is easily reconciled with AT&T's contrary representations on this issue. Cellular fraud generally provides a limited time window in which to exploit unauthorized use. One can easily imagine that those engaged in fraud would focus disproportionately on high-value applications, such as long distance.

The implications of this observation are most easily understood through a numerical example. Consider a "typical" \$100 cellular bill, with \$10 in long distance charges.<sup>87</sup> Assume that the total cost of fraud amounts to \$3.33 (3.7% of \$90) for the cellular carrier.<sup>88</sup> Also assume that the burden of fraud falls evenly on cellular carriers and IXCs (due to the considerations mentioned in the previous paragraph). Then cellular fraud would account for 33.3% of cellular long distance revenues (\$3.33 out of \$10).

We also argued in our original report (p. 147) that, given Hausman's inability to count all relevant costs accurately, the absence of any evidence concerning limitations on resale creates a strong presumption against the validity of his conclusion. He ignores this argument completely.

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<sup>87</sup>According to Higgins, p. 13, 10% of total cellular revenues are attributable to long distance.

<sup>88</sup>It is not clear from Hausman's citation whether the 3.7% figure includes the cost of long distance service for long distance calls. To be conservative, we have assumed in this example that it does not.

Hausman does, however, take exception to the fact that we have labeled cellular long distance a "tiny slice" of the market, pointing out that it is large in absolute terms (p. 16). Although the point here is far from central, he has nevertheless missed it. If long distance carriers have market power, then they should price discriminate between all distinguishable groups for which price elasticities differ. Thus, if Hausman is correct, one would expect to find more pervasive and significant examples of price discrimination. No such examples are offered.

##### **5. The relation between prices and demand elasticities**

*Conclusion #49: Taylor and Zona's attempt to estimate the degree of market power econometrically is seriously flawed, and sheds no light on the intensity of competition in long distance services.*

Taylor and Zona's most recent submission contains (pp. 27-32) yet another misguided effort to infer the level of competition in long distance from pricing information. In particular, they examine the statistical relation between prices, access costs, and measures of demand elasticities, with the object of estimating a parameter that is supposed to measure the degree of market power. As we discuss below, this approach is loosely derived from the extant academic literature. Taylor and Zona claim that their results support an inference of non-competitive outcomes, but they are mistaken.

First, the procedure used by Taylor and Zona was not designed to be used in situations where competition is distorted by regulation, and its application in such situations is entirely inappropriate. To understand this point, one must first appreciate the circumstances under which the parameter "theta" can be interpreted as a measure of competitive intensity. The empirical specification is derived from a model of industrial equilibrium governed by "conjectural variations," in a setting with *unregulated* oligopolistic competition. The term "conjectural variations" refers to the competitive price response that each market participant expects to observe after changing its own price. The absence of regulation is important, since only then will the pricing behavior of a firm be governed by the equation (commonly known as a "first order condition") on page 28 of Taylor and Zona's affidavit. If pricing is distorted by regulation -- as we

argue is the case in retail long distance, and as the RBOC affiants agree -- then this equation will not hold. The empirical relation is therefore misspecified, and the estimated value of the parameter "theta" is meaningless. Consequently, it is of no great surprise to us that Taylor and Zona estimate a value for this parameter that is nonsensically high (greater than that corresponding to a monopolist). In short, this exercise amounts to the indiscriminate application of an existing methodology to a situation for which that methodology is manifestly inappropriate.

Second, even if the industry was unregulated, one ought to regard inferences based on this kind of procedure with considerable skepticism. For some time, it has been widely recognized that the statistical identification of such models is dicey at best, in the sense that it depends on fairly arbitrary choices of functional form that determine the manner in which demand elasticities shift with the level of demand (Professor Bresnahan exhibits an acute awareness of this problem in the article cited by Taylor and Zona in their footnote 45, p. 28). Consequently, even under the best of conditions, results of this form -- presented with no information concerning the demand specification, and no sensitivity analysis to other demand specifications -- are unconvincing.

To illustrate the identification problem, imagine for the moment that Taylor and Zona had estimated a demand function exhibiting constant elasticity over the relevant range. Then their data would rule out the existence of significant market power. A central tenet of the theory underlying their analysis is that firms with market power charge markups over marginal cost. Thus, with constant elasticity of demand, a one dollar reduction in marginal cost should lead to more than a one dollar reduction in price (assuming that the firm in question does have market power)<sup>89</sup> But their central finding alleges that prices have declined *less* than dollar-for-dollar with reductions in access charges (p. 17).

Third, even if the issue of statistical identification was straightforward, it must also be remembered that Taylor and Zona's empirical specification is founded on a model with *fixed exogenous*

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<sup>89</sup>This is a direct implication of the equation on page 28 of their affidavit.

conjectural variations. This framework has been universally rejected by theorists, and all but discarded by the profession more than twenty years ago as a tool for the theoretical analysis of industrial behavior, in favor of more logically coherent game-theoretic approaches with endogenous conjectural variations -- to the extent that concept usefully describes behavior. The "conjectural variations" implied by the most widely used theoretical models of industrial behavior are inherently *not* fixed, and this observation has fundamental implications for the validity of the approach adopted by Taylor and Zona. Indeed, it has recently been proven that the parameter "theta," estimated as in Taylor and Zona's affidavit, in general bears no relation whatsoever to the true level of market power in an industry.<sup>90</sup> Thus, Taylor and Zona are absolutely incorrect in asserting (p. 32) that they use "settled economic theory" to assess the level of competition. It is far more accurate to say that they have misused settled economic theory.

#### **6. The magnitudes of price-cost margins**

*Conclusion #50: The alleged existence of significant differences between long distance prices and marginal costs sheds no light on the existence or absence of market power.*

MacAvoy asserts repeatedly (both in his first report and in his reply affidavit) that significant market power can be inferred from the existence of substantial differentials between price and marginal cost (see e.g. p. 2 of his reply affidavit). His empirical analysis of price-cost margins is, of course, susceptible to criticisms of the manner in which he has measured prices and marginal costs. We will discuss these issues below. To address them in any detail here would obscure the much more fundamental point that data of this kind would be completely uninformative even if price and marginal cost were measured perfectly. As we wrote in our first report (p. 161),

"Even if his calculations were correct, MacAvoy's analysis would still be fundamentally uninformative, since one would not expect to observe marginal cost pricing in this industry. Long distance companies incur significant fixed costs of operation. These fixed costs include items such

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<sup>90</sup>See Kenneth Corts, "Conduct Parameters and the Measurement of Market Power," mimeo, Harvard Business School, August 1994. For empirical confirmation of the theoretical problems raised in Corts' paper, see David Genesove and Wallace Mullin, "Validating the Conjectural Variation Method: The Sugar Industry, 1890-1914," mimeo, Massachusetts Institute of Technology, October 1994.

as general overhead and administration, network maintenance, non-volume sensitive marketing and sales expenses, R&D, and possibly some fraction of the company's debt service. In the presence of significant fixed costs, the relevant theoretical paradigm is *contestability*, rather than perfect competition. In a contestable market, price is driven down to *average* cost, which exceeds marginal cost. Prices cannot fall below average costs, or the firm will lose money and shut down... A number of the RBOCs' witnesses -- other than MacAvoy -- certainly recognize this principle [citing McCormick, p. 21, and Barro, p. 7]."

In his reply affidavit, MacAvoy never once acknowledges, let alone responds to, this issue (nor, for that matter, does any other RBOC affiant). There is, quite simply, no coherent defense of his most central claims.

### **7. Trends in prices and price-cost margins**

*Conclusion #51: None of the evidence presented in this proceeding concerning movements and co-movements of prices and costs sheds any light on the existence or absence of market power in long distance services.*

MacAvoy, Hausman, and Taylor/Zona all argue that market power can be inferred from evidence of rising price-cost margins during the post-Decree period. In its simplest form, this argument is made by the allegation that API has not fallen by as much as access costs.

Once one recognizes that the correct benchmark for this market is contestability, rather than perfect competition, it is immediately obvious that one cannot learn very much by examining trends in price-(marginal) cost margins. The presumption (of Hausman, MacAvoy, and Taylor/Zona) that this exercise is informative is inherently linked to the assumption that the benchmark of perfect competition is appropriate -- only then would one expect increasing competition to generate convergence to marginal cost pricing. No such implication follows if the correct benchmark is contestability -- at most (in the single product case), one would expect convergence to average cost pricing.

If an industry is contestable, price-(marginal) cost margins may rise over time without any decline in competitiveness. This would occur if, for example, average costs rose relative to marginal costs. For long distance telecommunications, it is entirely possible that certain marginal costs have fallen due to the introduction of fiber technology and reductions in access charges, while fixed costs (which include more

standard expenses such as billing, administration, and so forth) have risen with the prices of other inputs (such as labor costs). Thus, one should hardly be surprised to observe an increase in price-(marginal) cost margins. No inference about the intensity of competition (properly defined) would be warranted, even if the RBOCs' characterization of the evidence proves to be accurate.

This argument was set forth with abundant clarity in our original affidavit (see e.g. p. 161). Despite its fundamental importance, none of the RBOC witnesses have even acknowledged it, let alone responded to it. Once again, there is no coherent defense for their most central claims.

Instead, the RBOC witnesses attempt to obscure the vacuousness of their analysis by focusing in their reply affidavits on criticisms concerning the proper measurement of prices and costs. We believe that their analysis of price-(marginal) cost margins is still highly problematic, and we will therefore take up at least some of the measurement issues. However, in doing so, we wish to emphasize that these issues are entirely secondary. Since the RBOC witnesses have misunderstood the implications of competition in the presence of significant fixed costs, their interpretations of the data will be hopelessly flawed even if the data are perfect.

The measurement of prices is necessarily controversial because there is no single price for long distance services. As we have emphasized repeatedly, the market is segmented according to volume, and different prices are (quite appropriately) quoted in different segments.

Despite this observation, all of the RBOC witnesses appear determined to use a single measure of long distance price. As Taylor and Zona note (quite correctly), any aggregate price measure is necessarily flawed (see their reply affidavit, p. 20). Consequently, if one insists on proceeding down this rather unpromising path, one must inevitably use a price index that is flawed in one way or another.

As emphasized in our first report (and by Hubbard and Lehr), API is clearly an inappropriate price index. Since it does not properly reflect the effects of discounts through OPCs, trends in API inadequately reflect the effect of discounts provided to the high-volume segment. For the low-volume

segment, rising price-(marginal) cost margins are certainly to be expected, since (as discussed earlier) capped prices were originally set at levels that were insufficient to allow IXCs to recover the fixed costs of serving these customers. Thus, trends in API, or in the difference between API and marginal costs, are fundamentally uninformative.

Both Hubbard/Lehr and Hall have proposed the use of data on Average Rate Per Minute (ARPM), a practice that is criticized in the RBOCs' rebuttal affidavits. Several criticisms are offered.

First, according to MacAvoy (p. 41), there is a "presumption... that AT&T's actual charge for any specific call and the tariff designation of charges for a call of that type have to be the same. That presumption does not exist for the relationship between AT&T's charge for any call and its average revenue received per minute on all calls." Yet it is equally true that no such presumption exists for the relationship between AT&T's charge for any call and any of MacAvoy's price indexes. Thus, MacAvoy's criticism of ARPM entirely misses the point. If one is going to use a single composite measure of price, then one must aggregate information across calls. ARPM is one particular way of performing this aggregation. In contrast to MacAvoy's indexes, ARPM has the enormous advantage of being a volume-weighted average of prices actually paid, rather than a customer-weighted average of hypothetical prices.

Second, both MacAvoy (p. 43) and Taylor/Zona (pp. 20-22) criticize ARPM on the grounds that it doesn't hold the composition of demand constant. They provide hypothetical examples to illustrate the point that ARPM might change due to a shift in demand between times of day, or toward longer calls. In all of these hypothetical examples, the RBOCs' affiants simply assume the existence of a large *exogenous* shift in the composition of demand. They provide no evidence to support the view that significant exogenous changes in the composition of demand have occurred. Of far greater importance, they have ignored entirely the fact that changes in the composition of demand may be *endogenous*, rather than

exogenous; that is, they may be *caused* by changes in prices. To the extent shifts in demand have been induced by discounts, the practice of holding initial composition constant necessarily understates the gains to long distance consumers during the post-Decree period.

It is easy to illustrate this point with the RBOC affiants' own examples. Imagine that rates are initially uniform over time of day. Customer A regularly calls his mother each day between 8 PM and 10 PM, spending \$100 for a total of 100 minutes. He is roughly indifferent about the time of his call within this time slot, and ends up calling roughly as often before 9 PM as after 9 PM. Now suppose that AT&T offers a 20% discount for calls placed after 9 PM. Being relatively indifferent as to time, customer A now places all of his calls to his mother in the 9 PM to 10 PM time slot, spending \$80 for a total of 100 minutes.<sup>91</sup> In this example, the true welfare gain to customer A is approximately equal to \$20, or 20% of his initial bill. Moreover, ARPM would, in this instance, decline by the same 20% (from \$1.00 to \$0.80). If, however, one uses fixed weights based on initial composition (as, for example, in the case of the Taylor/Zona Lespeyres index), one would compute a price reduction of only 10% (from \$1.00 to \$0.90). Based on this figure, one would impute a gain to the consumer of only \$10, despite the fact that his true gain is twice this amount.

A similar example can be given for length of calls. If the introduction of greater discounts for longer calls causes customers to make longer calls, then one underestimates the gains to consumers by measuring prices based on the lengths of calls before the discounts are enacted. Perhaps the most dramatic example of this endogeneity principle concerns total calling volume. Consider the effects of AT&T's True Savings program. Suppose customer B initially spends \$7.50 per month on long distance. However, once the TrueUSA program is established, she increases her calling volume by one-third to qualify for the 25% discount. Thus, customer B's total monthly bill is \$7.50 with the discount program, and \$7.50 without the discount program; however, with the discount program, she uses roughly 33% more minutes. In this

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<sup>91</sup>To simplify the illustration, we assume that the customer's overall demand is price-inelastic.

example, ARPM would correctly measure a decline of 25% in price. However, if one uses fixed weights based on initial composition, one would conclude falsely that price has not changed at all, and that customer A has not received any benefit from this program.

MacAvoy and Taylor/Zona each propose the use of alternative long distance price indexes. We have already mentioned several problems with these indexes. MacAvoy's indexes are weighted by users, rather than by volume, and therefore severely understate the true economic significance of discounts.<sup>92</sup> All of these indexes fail to reflect appropriately the gains to consumers associated with changes in the composition of demand that are induced by the discounts themselves. The analysis in the text of MacAvoy's affidavit (pp. 31-40) is also seriously deficient, in that it calculates price indexes separately for each discount program. This completely misses the point that the IXC's escalate competition and lower prices by introducing new plans. MacAvoy himself evidently recognizes this deficiency, and therefore presents (tucked away in appendix B) an index which he refers to as the "theoretical minimum price." This disparaging label was presumably chosen to distract attention from the fact that, of all his series, these come closest to measuring actual current prices.<sup>93</sup> This is important, since these series generally fail to demonstrate the recent upward movements in nominal long distance prices noted in the text of MacAvoy's affidavit. As discussed previously, they also typically fail to exhibit co-movements with "standard rates" in recent years. MacAvoy's central claim -- that discount prices have increased because "tariff rates have increased in recent years and percentage discounts have been constant" (p. 23) -- is therefore undermined by his own data.

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<sup>92</sup>Insufficient detail on the Taylor/Zona index is available in their report to determine whether a similar problem arises.

<sup>93</sup>For the other reasons discussed in the text, the "theoretical minimum price" series are still severely flawed, and understate significantly the gains to consumers from reductions in long distance prices during the post-Decree period.

The RBOCs' affiants also assert that the use of ARPM would not overturn their conclusions. Hausman, for example, believes that it is not sufficient for ARPM to have declined in real terms during the post-Decree period, and he suggests that a comparison with cellular prices (which declined more rapidly) is appropriate (pp. 10-11). We think it distinctly odd for an economist to focus on nominal price changes, rather than real price changes. The analogy to cellular also strikes us as far-fetched. Cellular is a relatively young technology. It is not at all unusual to observe rapid declines in prices for young technologies, since producers remain for some time on the relatively steep portion of the learning curve.<sup>94</sup> There is no technological or economic reason to expect similar price trends across cellular and long distance services.

In his original affidavit, Taylor also tried to make much of his finding that long distance prices allegedly fell more rapidly before the Decree than after the Decree. We explained in our first report (p. 170) that a comparison of this sort sheds no light on the existence or absence of market power. Indeed, Taylor's view has the absurd implication that, once competition is established, prices should decline at an accelerated rate forever. In their reply affidavit, Taylor and Zona merely claim that the same pattern holds true for ARPM (pp. 22-23), but they offer no response to our objection.

Thus far, we have focused primarily on the measurement of prices. The measurement of costs is also problematic. Ultimately, none of the RBOC witnesses conducts a sufficiently comprehensive examination of costs to justify the assertion that post-Decree trends in prices are not cost-justified under the appropriate competitive standard (indeed, MacAvoy refuses to consider a variety of cost components proposed by Hubbard and Lehr, on the grounds that they are not, in his view, *marginal* costs). To test the contestability hypothesis through a direct comparison of prices and costs, one would have to compute average costs, including costs associated with the amortization of fixed capital. This is precisely the object

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<sup>94</sup>Hausman also notes that "the DOJ recently claimed [cellular prices] were not competitive," but this is a non sequitur. Even firms with market power should reduce prices when costs decline.

of calculating an accounting return, but the RBOC witnesses have characterized this as a hopeless undertaking (Hausman, pp. 7- 8, and MacAvoy p. 47). Thus, they have conceded the proposition that one cannot test long distance against the relevant competitive benchmark through direct comparisons between prices and costs.

#### **8. The relation between price-cost margins and concentration**

*Conclusion #52: MacAvoy's analysis of historical correlations between concentration and price-cost margins is fatally flawed both theoretically and as a matter of empirical methodology. The results of this analysis are spurious, and shed no light on the existence or absence of market power in long distance services.*

In his original affidavit, MacAvoy argued that, as a matter of theory, equalization of market shares facilitates tacit collusion. In his view, AT&T has ceded customers to MCI and Sprint in order to reduce their incentives to act as mavericks, and to induct them into a collusive partnership. MacAvoy then claimed to find empirical support for this theory, in the form of a negative historical correlation between concentration and price-cost margins.

We have challenged this analysis both on theoretical and empirical grounds. The importance of the theoretical challenge cannot be understated since, in the absence of a coherent theory, one is obliged to conclude that MacAvoy's results must reflect spurious factors, such as those described below.

In our first report (p. 162), we noted that MacAvoy's theoretical argument attempts to stand generally recognized antitrust principles on their heads. If MacAvoy is right, then antitrust authorities should regard high concentration as pro-competitive, and lower concentration as potentially problematic. From time to time, startling insights do overturn and even reverse generally accepted principles. However, this is most certainly not one of those instances. Our first report identified three critical flaws in MacAvoy's theoretical reasoning (pp. 162-163).

First, we pointed out that MacAvoy's argument presupposes the absence of capacity constraints. There is no need for a "dominant" firm to bribe smaller rivals with larger market shares unless these rivals

are capable of absorbing a significant fraction of demand. At the time of the Decree, each of AT&T's rivals had only a limited ability to absorb AT&T's clients. The capacity of these firms has, however, expanded enormously since the Decree. Now, both MCI and Sprint are capable of serving all of AT&T's customers, and even WilTel could, by itself, handle a substantial share of the AT&T's traffic. As the capacity of AT&T's rivals has grown, their incentives to deviate from any hypothesized agreement have also grown, and the likelihood of collusion has therefore diminished.

Second, we emphasized that MacAvoy's analysis neglects the presence of smaller rivals. According to MacAvoy, the existence of small rivals twelve years ago (MCI and Sprint) was more effective at maintaining market discipline than the existence of large rivals (again MCI and Sprint) today. If this is true, smaller rivals (like LDDS) should have the same competitive impact today that MCI and Sprint did in the first few years after the Decree. Thus, the growth of MCI and Sprint cannot have reduced competition, since other smaller firms have taken their places.

Third, we pointed out that MacAvoy's argument has the implication that collusion should have become feasible once the degree of inequality between AT&T, MCI, and Sprint was reduced past some threshold. This view cannot account for the observation that AT&T "ceded" significantly more market share in wholesale markets than in retail markets. As we have already noted in another context, market shares for different long distance service segments began to stabilize at the same point in time, and not at the same level of concentration, indicating the importance of some other precipitating event (e.g. the implementation of price caps).

MacAvoy's reply affidavit is notable for its failure to address any of these three criticisms. MacAvoy has therefore implicitly conceded that his contrived theoretical construct is inapplicable to long distance telecommunications. In view of the fact that our theoretical criticisms are unrebutted, MacAvoy has no coherent basis for predicting a negative correlation between concentration and price-cost margins. Nevertheless, in his reply affidavit, he continues to cite evidence which, he claims, provides support for

his theoretically incoherent hypothesis (see e.g. p. 44). To the extent he continues to find the correlation "predicted" by his debunked theory, one can only infer that the pattern is attributable to spurious factors, such as those discussed below.

Our previous report also identified five separate flaws in MacAvoy's empirical analysis, any one of which could account for his spurious result (p. 164). First, we pointed out that MacAvoy's measures of prices and costs are problematic. He responds (p. 40) that "[m]argins are estimated in a straightforward procedure based on FCC tariff data. If that is of poor quality, it would have to be because AT&T, MCI, and Sprint have not abided by the tariffs in the offerings of standard and discount plan services." This is incorrect. As we have explained, the price data is flawed not because the underlying tariff data are incorrect, but rather because MacAvoy improperly aggregates the prices charged to different customers, and for different services. MacAvoy's cost data is flawed primarily because he omits relevant costs. However, it is worth remarking that, according to Hausman, cost data are particularly unreliable when they are gathered in a regulatory setting (see Hausman's reply affidavit, p. 7).

Second, we argued that, since the appropriate benchmark is contestability and not perfect competition, any analysis based on the difference between price and marginal cost, rather than the difference between price and average cost, is meaningless. This point is un rebutted.

Third, we noted that there are a variety of econometric problems associated with regressing price-cost margins on measures of concentration (including, but not limited to, endogeneity), and that most economists have come to regard such exercises as unreliable. This point is un rebutted.

Fourth, we observed that, under MacAvoy's theory, there is no reason to expect a continuing negative relation between concentration and margins once sufficient equality of market shares has been achieved to permit cooperation. Yet MacAvoy finds that this correlation persists, even after 1991. This strengthens the presumption in favor of alternative explanations.

Finally, we emphasized that MacAvoy's econometric results simply reflect the spurious effects of a common trend. MacAvoy has, in effect, taken two variables, both of which exhibit a clear trend, and regressed one on the other. Since trends can arise for an enormous variety of reasons, economists and statisticians generally regard this kind of exercise as uninformative. One could just as well "demonstrate" that the rise in MacAvoy's price-cost margins was caused by the increasing prevalence of handguns, or by the progressive thinning of the ozone layer. At an absolute minimum, the robustness of the results ought to be checked by adding a time trend and other trending variables, and by estimating the relation in differenced form, rather than in levels. However, since MacAvoy has refused to provide us with his data, we have been unable to conduct any of these robustness checks.

MacAvoy responds to this final criticism in only the vaguest possible way, saying that it amounts to the "insight that the 'phenomenon' of rising profit margins in the presence of falling market concentration 'may have occurred for a variety of reasons'" (p. 40). MacAvoy appears to be insinuating that we did not name a specific alternative, and that in the absence of such an alternative one must accept simplistic results based on simple correlations between co-trending variables. The insinuation is incorrect. In addition to the possibility that the correlation is an artifact of poor data construction, we have named and defended a specific alternative (see our first report, pp. 161-162). Falling concentration reflected increased competition as MCI, Sprint, and others made significant inroads against AT&T. Over the same time period, marginal costs fell relative to average costs, thereby producing larger price-(marginal) cost margins (as measured by MacAvoy), even though market outcomes were approximately (or even increasingly) contestable. Under this view, the phenomenon of falling concentration and rising margins is entirely artificial, and tells us absolutely nothing about competitive performance. Yet MacAvoy has done nothing in his reply affidavit to rule out this or any other alternative explanation for his result.

## 9. Earnings

*Conclusion #53: Taylor and Zona's evidence on earnings per share sheds no light on the existence or absence of market power in long distance.*

In their most recent submission (pp. 42-43), Taylor and Zona report that AT&T's earnings per share (EPS) have increased during the post-Decree period. We are not clear why this is held out as proof of anything. All of the other RBOC affiants who have weighed in on the subject of interLATA competition (Hausman and MacAvoy) roundly criticize the use of accounting profits (as noted above), and their objections are certainly applicable here. Moreover, the rise in AT&T's earnings may be attributable (at least in part) to activities other than long distance. Earnings per share may also rise because of changes in scale, or changes in the number of outstanding shares.<sup>95</sup>

Taylor and Zona also describe the results of a regression analysis, which estimates the historical correlation between EPS and access charges (p. 43, footnote 66). With EPS rising and access charges falling, it is hardly surprising that they find a positive coefficient. Evidently, they make no effort to control for any other factors, let alone detrend or difference the variables in an attempt to mitigate spurious correlation. The exercise is therefore of no conceivable significance.

## 10. Asset prices

*Conclusion #54: The appropriate interpretation of asset pricing data lends some support to the view that long distance is highly competitive, and does not support the view that AT&T (or any other IXC) has significant market power.*

In his original affidavit, MacAvoy examined stock market reactions to five rate increases by AT&T, and argued that the share prices of AT&T, MCI, and Sprint tend to rise in response to such events. He asserted that this pattern is inconsistent with competitive behavior.

In response to this study, we advanced four conceptual criticisms and two methodological

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<sup>95</sup>It is also evident from an examination of Taylor and Zona's figure 9 that they have exaggerated the increase in earnings per share by exploiting the fact that their period of observation begins and ends on an uptick. A trend line would show a more moderate increase.

criticisms. The conceptual criticisms (p. 158) established that the entire exercise is fundamentally misguided and, even if flawlessly conducted, has no potential for shedding light on any of the issues of interest in this proceeding.

First, we pointed out that an event study of this kind simply measures the instantaneous impressions of Wall Street analysts, and not the retrospective analysis of competent economists. In the absence of some demonstration that the analysts' expectations were confirmed (something that MacAvoy has made no attempt to provide), the analysis is therefore inconclusive.

Second, we noted that the interpretation of MacAvoy's "finding" is dicey at best. Implicitly, MacAvoy has assumed that investors believed AT&T to have market power that it unaccountably failed to use, until AT&T's managers inexplicably woke up and surprised the market. It strikes us as highly unlikely that investors believe they understand AT&T's market power, or lack thereof, better than AT&T. More generally, the interpretation of stock price reactions is at best obscure, since it depends on unstated assumptions about what the market learned.

Third, we pointed out that MacAvoy's alleged factual pattern is entirely consistent with our analysis of competition subject to regulatory distortions. Since basic rates determine prices to low volume customers, and since the IXCs lose money on these customers, any indication that AT&T (and thus the other IXCs) can raise basic rates toward competitive levels should be taken as good news by the market.

Finally, even if the conceptual base for MacAvoy analysis was sound (which it is not), his findings would only establish that the market is not *perfectly* competitive (see our first report, p. 142-143). Neither of MacAvoy's affidavits even hint at a method of estimating the degree of market power from share price reactions.

Although MacAvoy does respond to our methodological criticisms (see below), he ignores these conceptual issues completely. But once the conceptual foundation for his analysis is undermined, the methodological issues are irrelevant.

By emphasizing the conceptual issues, we by no means intend to concede that the methodological issues have been resolved by MacAvoy's rebuttal. On the contrary, MacAvoy's responses do little more than to underscore the futility of the exercise.

The most obvious problem with MacAvoy's empirical analysis is that his "events" are contaminated by other concurrent developments. For each of MacAvoy's rate increases, we have identified other important contemporaneous events that influenced the market's valuation of AT&T, MCI, and Sprint (see our first report, pp. 150-160). MacAvoy has defended his work by arguing that some of the confounding events fall outside his event windows. He identifies these events with asterisks in table 7 (p. 53) of his reply affidavit. Only two of these events were discussed in our affidavit: the FCC's decision to allocate spectrum for PCS licenses (9/17/93), and the announcement of MCI's fiber optic local network venture (1/2/94).<sup>96</sup> MacAvoy has completely ignored our reason for including these events. Both of these developments were the subject of study by *other* RBOC affiants: Lehn studied the PCS event, and Spiller studied the MCI event. The point is not that these events fall inside of MacAvoy's event windows, but rather that MacAvoy's events fall inside of Lehn and Spiller's event windows. Thus, according to RBOC affiant Lehn, the PCS event may well account for the IXCs' share price reactions on and around the rate increase on 9/17/93. And according to RBOC affiant Spiller, the MCI event may well account for the IXCs' share price reactions on and around the rate increase on 12/29/93. Thus, MacAvoy cannot defend his study without implicitly indicting the work of other RBOC affiants.<sup>97</sup>

Second, MacAvoy challenges the notion that the confounding events were actually problematic. Actually, he discusses only a small subset of these events -- a fact which he attempts to disguise by introducing his criticisms with the phrase "for example" (p. 54). He ignores completely many of the

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<sup>96</sup>The other four events that are marked with an asterisk in Table 7 were discussed in the affidavit by Hubbard and Lehr.

<sup>97</sup>Indeed, his assertion (p. 54) that the MCI event was "*neither unanticipated nor significant for stock appreciation on a one-time, three-day basis*" (emphasis added) is truly remarkable, given Spiller's analysis.

confounding events described in our first report, including a highly favorable earnings announcement by Sprint (which the business press actually cited as an explanation for Sprint's share price appreciation), an important court decision, the introduction of a high-profile discount plan by Sprint, and the establishment of larger discounts by AT&T. The last of these is potentially the most illuminating. As we noted in our first report (pp. 159-160), AT&T announced the increases in rates and discounts simultaneously, noting that the one would offset the other. Thus, "[u]nder MacAvoy's hypothesis, stocks should have reacted very differently to this event than to the other four; yet he finds that the reactions were essentially identical" (p. 160). On this point, MacAvoy is conspicuously silent.

MacAvoy does, however, challenge the relevance of the California PUC's announcement of plans to open up the intraLATA toll market (7/20/93). He does not deny the obvious fact that this was a positive and potentially important development for the IXCs. Instead, he responds that the event was anticipated, and cites an article written on 7/16/93. MacAvoy would evidently make much of the fact that there are four days between the news story that he cites, and the event that he studies. Yet his choice of a three day event window is completely arbitrary. It is true that, in many branches of the financial literature (e.g. those that examine share price reactions to earnings announcements), the use of three day windows is common. But this is only because the literature contains exhaustive examinations of the appropriate window widths for such events. There is no reason at all, however, to believe that the temporal pattern of information release is the same for a regulatory event as for an earnings announcement -- a fact which other RBOC affiants (such as Lehn) certainly recognize. Earnings announcements are made at regular, predictable intervals, and analysts prepare themselves to evaluate the new information quickly, whereas news of impending regulatory action may filter its way through the market more slowly, as analysts investigate rumors and attempt to evaluate the event's significance. Thus, the close proximity of these news stories to MacAvoy's event create a strong potential for contamination.