

**Before the  
Federal Communications Commission  
Washington, D.C. 20554**

In the Matter of:	)	
	)	
Implementation of Section 11 of the Cable Television Consumer Protection and Competition Act of 1992	)	CS Docket No. 98-82
	)	
Implementation of Cable Act Reform Provisions of the Telecommunications Act of 1996	)	CS Docket No. 96-85
	)	
The Commission's Horizontal and Vertical Ownership and Attribution Rules	)	MM Docket No. 92-264
	)	
Review of the Commission's Regulations Governing Attribution of Broadcast and Cable/MDS Interests	)	MM Docket No. 94-150
	)	
Review of the Commission's Regulations and Policies Affecting Investment in the Broadcast Industry	)	MM Docket No. 92-51
	)	
Reexamination of the Commission's Cross-Interest Policy	)	MM Docket No. 87-154
	)	

**DECLARATION OF HOWARD SHELANSKI ON OPP WORKING PAPER**

**Introduction**

1. The purpose of this declaration is to comment on the recent OPP working paper entitled "Horizontal Concentration in the Cable Television Industry: An Experimental Analysis" ("Working Paper"). I will first discuss some important ways in which the economic structure of the study's bargaining experiments differs from the economic characteristics of the real world in which cable operators and cable networks buy and sell programming. I will then discuss why, even taking the study on its own terms, it does not

ultimately address important issues that the horizontal ownership proceeding is most concerned with.

2. My overall conclusion is that the Working Paper does not provide economic evidence relevant to the adoption of horizontal ownership rules in the cable market (nor can it properly be used as the basis for any other policy decisions or analysis of proposed transactions in the cable industry). The paper ignores consequential, economic characteristics of the market in which cable programming is sold, bought, and distributed to viewers. The study, moreover, provides no direct examination of the flow of programming to consumers or explanation for why buyer concentration is responsible for the study's results.
  
3. If an economic experiment is to shed light on the likely results from actual economic activity, the experiment must replicate as closely as possible the incentives, tradeoffs, and environmental forces that real-world agents face. Economic experiments, like economic models, are inevitably abstractions of real life. When properly designed, however, they can yield valuable insights despite their inability fully to mirror the real world. To be sure, even well designed economic experiments raise questions about the correspondence between behavior under laboratory conditions and behavior under similar conditions in the real world.<sup>1</sup> But it is not that general concern, which applies to any economic experiment, which leads me to question the Working Paper's policy relevance. Rather, it is because the Working Paper abstracts too much from the real-world markets it seeks to examine. The laboratory conditions of the study's experiments do not parallel centrally important features of the markets in which the economic activity at issue occurs. When an experiment does not or cannot sufficiently mirror essential aspects of real-world phenomena, then its results cannot be used to predict actual behavior or to make policy judgments about real economic activity. Thus, although the

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<sup>1</sup> See, e.g., Donald W. Katzner, "The Significance, Success, and Failure of Microeconomic Theory," 24 *Journal of Post-Keynesian Economics* 41, 53-54 (2001).

Working Paper may be a somewhat interesting report of the results of a bargaining game, it provides no information useful in evaluating actual bargaining between buyers and sellers of video programming or how that bargaining affects MVPD subscribers.

**I. Important Differences Between the Experimental World and the Real World**

4. Although the Working Paper acknowledges the need for an experimental analysis to begin “with the creation of an experimental market that parallels the market under investigation,” (Working Paper at 9), the differences between the real world and the modeled world of the Working Paper are numerous and important. In particular:
  - The Study’s experiment never tests a market structure for MVPDs that either exists today or is reasonably foreseeable ever to exist (e.g., as a result of pending transactions). If concentration of cable ownership truly matters, the hypothesis that the study purports to test, then it would seem important for the experiments to reflect actual and potential ownership structures from the real world. Moreover, there is no apparent empirical or theoretical justification for the market shares used in the experiment;
  - The experiment addresses a hypothetical world of only 4 sellers and between 3 and 5 buyers, (Working Paper at 3, fn 6, 10, 15), not the real world in which cable operators actually purchase programming, which contains many more buyers (including not only domestic MVPDs, but also foreign buyers and buyers of programming for other media like broadcast and VHS/DVD) and more than 300 national sellers (plus numerous additional regional networks and dozens of emerging networks);

- The Working Paper does not account at all for the fact that there is competition among MVPDs for viewers. Because of this competition, a buyer's market share, as well as its absolute level of subscribers, will change depending on which programs it purchases for transmission (and depending also on the purchasing decisions made by MVPDs with which it competes). Although these considerations are critically important in shaping buyers' incentives in the real world and inevitably affect how buyers actually behave, the study ignores them;
- The "DBS" provider in the experiment is just another player in the game and is undifferentiated, except for slight differences in assumed cost structure, from those players labeled as "cable" operators. (Working Paper at 13, Table 4). The capacity and coverage differences between cable and DBS that make the latter a potent competitor to cable operators in the real world are in no way proxied in the experimental design;
- The study does nothing to account for the fact that cable networks compete with each other. If two sellers offer substitutable products, then they will compete with each other to attract a limited pool of buyers. A cable operator may believe that consumer interest does not warrant allocating more than one channel to a particular kind of content. If there are competing providers of that type of content, the bargaining process will inevitably be affected. But this real-world possibility is not factored into the experiment;
- The experimental design does not account for possible vertical integration of cable operators and program networks—or between program producers and program networks, for that matter—and the bargaining incentives that such relationships might create;

- The experiment places the buyers and sellers under extremely stringent artificial time pressures that do not exist in the real world, requiring that contracts be negotiated in either five or six minutes rather than over a course of months (Working Paper at 76, 83, 90, 98, 105, 112);
- Actors in the experimental trials have very limited, asymmetric information about each other's payoffs from the transaction at issue, where in the actual programming market buyers and sellers are well-informed about the likely benefits each will receive from a particular bargain (Working Paper at, e.g., 73, 108 (participants not allowed to communicate); 76 (amount of third party payments to sellers unknown to buyers); 108 (sellers privy to each buyer's number of customers)). This is not to say that buyers and sellers in the real world have perfect information about each other or that an experiment need capture every nuance of the real-world information structure. There are many uncertainties in the video programming market that make it extremely difficult for a controlled, limited experiment fully to predict actual outcomes. But what is at issue here is not a nuance. The experiment assumes parties have significantly less information than real-world players are known to have about each other, and that assumption may materially affect the bargaining outcomes in the experiment;
- The players in the experiment do not learn over time about their bargaining opponents and bargains made in one period have no effect on bargaining in the next. (Working Paper at 52 ("[t]he economic experiments may not fully capture the possibility that the bargaining outcomes in successive trading periods in the actual trading market may be correlated")). Yet in the real world, parties learn about each other over time;

- Relatedly, the study does not consider how existing carriage agreements (*i.e.* the results of previous bargaining rounds) may affect current negotiations between MVPDs and program networks. Viewers do not want to lose programming to which they have become accustomed. MVPDs accordingly cannot easily threaten not to carry such programming when contracts are up for renewal. Incumbent networks thus have bargaining advantages over both the buyers of their programming and over new networks trying to get carriage for their programs;
5. The above list contains just some of the ways in which the experiment critically differs from that real-world bargaining environment of the programming market (there are numerous others as well). The Working Paper itself recognizes some of the very issues listed above. It expressly notes that the study does not replicate certain features, like learning over time, vertical integration, or the fact that bargaining in one period may be affected by the bargains struck earlier. Yet those and the other missing factors discussed above are not minor details whose omission can be ascribed to necessary stylization and simplification. It is important to recognize that the listed problems are not simple quibbles or merely inconsequential distinctions. They individually can, and together certainly do, fundamentally alter the bargaining dynamics the Working Paper purports to test. Moreover, those factors affect bargaining in different and unsystematic ways, with some omitted market characteristics favoring sellers and others favoring buyers. It is thus impossible to determine how correcting these omissions would affect the study's results.
  6. So even putting aside the more general problems with the experimental approach—*e.g.* that inexperienced students rather than experienced professionals are playing the game, that the students did not even know what product or service they were bargaining over, and that there was little real consequence to the players from their actions—the Working Paper so deviates

from the actual incentives and tradeoffs of the programming market as to provide no basis for any policy judgments about that market or about transactions among the relevant firms.

7. Professor Andrew Schotter examines the consequences of the FCC experiment's design flaws and analytical gaps in great detail in the declaration he has filed in this proceeding. Professor Schotter's principal findings are (1) that the Working Paper provides incentives and tradeoffs to the experimental subjects that differ fundamentally from those faced by real cable operators and programmers; (2) that the statistical results of the study are not robust and in some cases are driven by a single, atypical action or mistake by a student player (Professor Schotter demonstrates that the efficiency results change radically when these out-lying results are removed); and (3) that the underlying results are not tied to any underlying theory in the study and in fact find no support in current economic theory. Alternative hypotheses turn out to be more likely to explain the experimental outcomes than the market-structure explanation urged by the study. Professor Schotter demonstrates, for example, that the variation in bargaining time over the course of the experiments is a more significant factor in the results than the variation in market structure over the course of the experiments.

## **II. The Working Paper Does Not Address the Flow of Programming to Consumers or Other Central Issues In the Proceeding**

8. Apart from flaws in experimental design and implementation, a second reason that the Working Paper is irrelevant to rulemaking decisions or analysis of proposed transactions is that the study does not say anything directly about how increased concentration would affect the ability of cable operators to affect economic welfare, either for themselves or for consumers. First and foremost, the Working Paper does not address the flow of programming to consumers. Although the Working Paper states in footnote 2 that it is

examining the flow of programming to consumers, consumers in fact appear nowhere in the study. The Working Paper instead examines choices of transactions for some generic product among sellers and distributors of that product, and how those choices affect the total economic surplus to be divided between those parties. It never examines the flow of that product to the distributors' customers. In the MVPD context, then, the experiments could at most (assuming that real-world circumstances were properly replicated) illuminate the effects of buyer concentration on the joint welfare of MVPDs and programming networks, not the effects on subscribers. The division of economic surplus between cable networks and cable operators is at best an indirect and highly conditional measure of consumer welfare.

9. To be sure, consumers will pay most for the programs they desire most. But it is well understood that the link between program profits and the quality, amount and diversity of programming is a complex one, particularly when advertising revenues, and not just subscription revenues, are thrown into the mix.<sup>2</sup> There is, of course, the separate question of whether any of the welfare losses the study finds would have appeared had the experiment been set up to incorporate competition among buyers in the downstream MVPD market, to include larger numbers of buyers and sellers, or to account for how results of previous rounds of bargaining affect a current round of bargaining. But even accepting the study design as it is, the relationship between network/MVPD welfare and the amount and diversity of programming delivered to consumers is never spelled out in the Working Paper and under accepted theory is not a straightforward one by any means.
  
10. Importantly, even if one accepts the Working Paper's link between network/operator welfare and consumer welfare, the study does not make clear how it is *buyer* size that is determining the joint welfare of cable networks and MVPDs. Neither the experimental data, nor the study's analysis,

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<sup>2</sup> See Bruce M. Owen and Steven S. Wildman, *Video Economics* (1992).

explain why the reported efficiency losses are the result of buyer behavior. In fact, as Professor Schotter demonstrates in his analysis of the Working Paper, the worst efficiency result is caused by the behavior of a *seller* of programming, not a buyer. So the study in the end provides no light on the ability of MVPDs to affect either their own, or consumers', welfare through their program purchase decisions.

11. Even if, as in the study, a handful of non-competing cable operators were the only buyers of the cable networks' programming, the experimental results have little bearing on how buyer size affects the programming available to MVPD subscribers in the real world. Putting aside all the other limitations of the Working Paper's experiments, the failure to recognize that consumers in each market usually have three separate choices of MVPDs, each with different mixes of programming to offer, is itself a compelling reason why the experiment is of no utility in fashioning horizontal ownership rules.
  
12. The study not only fails meaningfully to address consumers of video programming but also producers of that programming. The experiment confines itself to what purport to be networks and MVPDs (though the students who participated did not even know that), but it ignores the various ways in which producers of programming can ensure the delivery of their programming to consumers regardless of the outcome of *any* future negotiation between any network and any MVPD. In the real world, numerous networks are *already* ensured carriage over cable facilities, either because of de jure "must-carry" rights, the *de facto* "must-have" status of an established network, cable carriage secured through retransmission consent for a broadcast signal, or an existing long-term carriage agreement between a cable network and a cable or DBS operator. A program producer who sells to such a network will inevitably find that its programming "flows" to consumers, no matter what the result of future network-MVPD negotiations.

13. More generally, the study cannot be taken to provide any valid conclusions about the effect of market concentration in the cable market. There is no generally accepted economic theory that predicts that increased concentration will lead to decreased bargaining efficiency. To be sure, economists well understand how concentration among producing firms may lead to inefficient prices and output levels. But that is a very different question from whether concentration in the industries on one side or the other of a bargaining relationship affects the efficiency of that bargaining. Such concentration may well affect the *division* of the pie among bargaining parties, but there is no theoretical basis for assuming that concentration affects the *size* of the pie through bargaining. Yet this assumption is precisely the one the Working Paper makes. The study simply asserts that its volatile and idiosyncratic inefficiency outcomes are the result of concentration.
  
14. I do not mean to suggest that no result can be real without first having a well-understood theory. I do not here question that the Study's experiments in fact showed some modest changes in bargaining efficiency across trial transactions in the study's stylized world. The issue is not the result itself, but the Working Paper's explanation for that result. Concentration is not the only possible explanation for the reduced efficiency yielded by the high-concentration trials. The study, however, never tests alternative hypotheses for the results. As Professor Schotter shows, had the study tested alternative explanations (such as bargaining time), it would have found them much more powerful than the concentration explanation the study advocates. The study never considers, for example, how the information structure of the game that the student subjects play might drive bargaining strategies and outcomes. It is well understood by economists that lack of information in bargaining leads to inefficient results. The very limited and asymmetric information that buyers and sellers were allowed to have in the study's experiments is a more likely explanation for any inefficiencies than concentration. *See* Declaration of Andrew Schotter,

*Comments of AT&T Corp.*, CS Docket No. 98-82, at ¶¶ 10, 34, 45-49 (filed July 18, 2002).

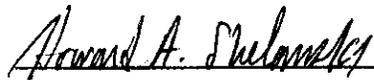
15. There is no economic basis for arguing, as the study does (at pp. 23-24), that oligopoly and monopoly are predictable sources of inefficiency in a market like the one in the experiment. Concentration causes inefficiency when a seller must charge all buyers the same price and cannot price discriminate among buyers. In such cases, prices above marginal cost eliminate transactions with consumers willing to pay marginal costs. In the experiment, however, transactions are matched and independent, and the seller can sell at a low price to one buyer and not lose its ability to sell at a higher price to other buyers. Concentration at the buyer level will therefore not induce inefficiency in such a market. Indeed, the study itself acknowledges in Appendix A that traditional oligopoly and oligopsony models do not apply in the experimental setting (p.54). Yet the study supplies no other theory for why concentration explains the experimental results. The Working Paper's unsupported assertions to the contrary, concentration is most likely not responsible for the study's reported outcomes. As Professor Schotter's analysis clearly and convincingly demonstrates, other features of the experimental design are the much more likely motivators of the results.
  
16. Finally, the Working Paper does not address any of the theoretical arguments that Professor Ordoover and I have made in our separate submissions early in this proceeding, or in the AT&T Comcast merger proceeding, explaining why larger MVPDs would not have buying power in the programming market. Indeed, to the extent the study is relevant to our testimony, it supports our conclusions. The study finds that there is no increase in bargaining power when a firm goes from a 27 percent to a 51 percent share of the MVPD market. Although the actual efficiency levels generated by the Study's experiments are likely incorrect and too low, as Professor Schotter explains, the lack of change in that efficiency level as market share increases is

consistent with my and Professor Ordover's findings. Importantly, when properly analyzed, none of the Working Paper's experimental results challenge the record testimony Professor Ordover and I have filed in both proceedings.

17. In sum, the Study's experiment in no way reflects the structure, incentives, tradeoffs, or economic environment in which cable operators actually bargain with cable networks for programming. For that reason, its results apply to a world quite different from the real one and thus provide no basis upon which any regulatory judgments can be made in this rulemaking or in any other related context. Moreover, the Working Paper does not directly address how its bargaining results affect consumers or why buyer concentration is the true cause of the study's efficiency results. Therefore, even if one accepts the experimental parameters as valid, it is unclear how the study's results bear on the issues central to this proceeding.

**Verification**

I, Howard A. SHELANSKI, declare under penalty of perjury that the foregoing is true and correct. Executed this 16 th day of July, 2002.

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Howard Shelanski

AT&T Corp. & Comcast Corporation  
Ex Parte Notice  
July 18, 2002  
MB Docket No. 02-70

# **Exhibit II**

**Before The  
FEDERAL COMMUNICATIONS COMMISSION  
Washington, D.C. 20554**

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	)	
Reexamination of the Commission's Cross- Interest Policy	)	MM Docket No. 87-154
	)	

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July 18, 2002

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**SUPPLEMENTAL COMMENTS OF AT&T**

Pursuant to the Commission's June 3, 2002 Public Notice ("*Supplemental Notice*") in the above-captioned proceeding, AT&T Corp. ("AT&T") respectfully submits these supplemental comments regarding Office of Plans and Policy Working Paper No. 35, "Horizontal Concentration in the Cable Television Industry: An Experimental Analysis" by Mark Bykowski, Anthony M. Kwasnica and William Sharkey ("*Working Paper*").

**INTRODUCTION AND SUMMARY**

Video programming is *the* critical input to any successful cable television operation. Consumers have countless entertainment alternatives, including the competing offerings of direct broadcast satellite ("DBS") providers that distribute the same video programming and that have

the ability to serve virtually all cable subscribers. In this dynamic environment, no cable operator, regardless of its size, can afford to ignore consumers' video programming preferences. Cable operators devote enormous resources to identifying and obtaining rights to distribute the video programming that consumers demand, and carriage negotiations are among the most sophisticated commercial negotiations, in part because the importance of video programming to cable operators is not lost on the owners of that programming.

The Commission has amassed a wealth of economic and empirical evidence in this remand proceeding that overwhelmingly demonstrates that, given these marketplace realities, even a cable operator much larger than any that exists today would not have the incentive and ability to exercise market power in its dealings with suppliers of video programming. The Commission should base its assessment of speech-restricting limits on cable ownership concentration on this record and these marketplace realities. See *Time Warner Entertainment Co. v. FCC*, 240 F.3d 1126 (D.C. Cir. 2001) ("*Time Warner II*").

The *Notice* asks whether a laboratory game from which the authors of the *Working Paper* report a number of results should inform the Commission's ownership limit determinations. In one respect, the game results are of limited interest in adding to the already vast record evidence that additional cable consolidation would not impede the competitive flow of video programming to consumers. Despite failing to take account of many of the most important marketplace characteristics that constrain cable, the *Working Paper* found no material difference in cable operator "bargaining power" in games that purported to model market structures in which the largest cable operator served from less than 30 percent to more than 50 percent of subscribers.

The *Working Paper* also reports, however, that "efficiency" and DBS "bargaining power" declined with increased concentration in some game scenarios. The *Working Paper* does not

explain how these experimental results are relevant to the Commission's inquiry into the existence of the "real" and "non-conjectural" risk of buyer market power abuse that *Time Warner II* makes clear is a condition precedent to any sustainable limit on cable ownership concentration. As explained below, they are not.

First, as experimental economist Andrew Schotter explains in the declaration that accompanies these supplemental comments, "[i]n designing an experiment to comment on a real-world phenomenon, it is a strict requirement that the experiment present the subjects with the tradeoffs that real-world agents face when they make their decisions, and that the variables of concern to subjects in the lab be the same variables that real-world decision-makers care about." July 18, 2002 Declaration of Andrew Schotter ¶ 4 ("*Schotter Declaration*"). The laboratory experiment described in the *Working Paper* does not meet that requirement.

Real-world carriage negotiators are highly sophisticated and knowledgeable repeat players. In any carriage negotiation, each side knows much about the other side's alternatives, costs and revenue opportunities. Real-world negotiators are highly-trained professionals that meet face to face in drawn out negotiations that routinely take months, sometimes last years, and culminate in complex long-term contracts worth many millions, or even billions, of dollars. In the *Working Paper* experiment, in contrast, the student players had *six minutes* to negotiate multiple "contracts," had almost no information about each other and were playing for only a few dollars. The players were not even told what they were buying and selling, and they were not allowed to communicate except through the computer transmission of numerical offers and acceptances. The *Working Paper* may provide useful information regarding how economic actors negotiate when they have "little information, no opportunity to communicate, and limited

time in which to make multiple deals,” but “it sheds no light on the real-world efficiency of negotiations between MVPDs and programmers.” *Id.* ¶ 7.

Perhaps most importantly for present purposes, real-world buyers and sellers recognize the dynamic consequences to a cable operator – in subscribers lost to DBS and other competitors – of failing to obtain rights to distribute desirable programming. In the laboratory experiment, in contrast, “cable” players that failed to strike bargains suffered no such dynamic market penalties; rather, each player’s size stayed constant from one trading period to the next in each scenario, regardless of its own or others’ “programming” decisions. In this respect, the *Working Paper* suffers from the same fatal defect *Time Warner II* found in the Commission’s “open field” approach to ownership limits – its failure to reflect the reality that “a company’s ability to exercise market power depends not only on its share of the market, but also on the elasticities of supply and demand, which in turn are determined by the *availability* of competition.” *Time Warner II*, 240 F.3d at 1134. Thus, any attempt to set ownership limits on the basis of the *Working Paper*’s efficiency or DBS findings would be doomed to reversal under a straightforward application of *Time Warner II*. *Id.* (“in revisiting the horizontal rules the Commission will have to take account of the impact of DBS on th[e] market power of [cable operators]”).

But even if the experiment had been designed to capture the key real-world variables and had been carried out in a manner that could reasonably be said to replicate real-world negotiations, the efficiency and DBS results could not support ownership limits. The *Working Paper* (at 49) reports a “modest” reduction in “efficiency” between the game’s highest concentration scenario and lower concentration scenarios. But this “efficiency” reduction – more precisely, the fact that some transactions that would have benefitted both the buyer and

seller were not consummated in some of the game sessions – cannot be explained by any theory of buyer market power, because “the students acting as buyers did not even know their relative sizes. If some were more aggressive, it was because of their personalities, not the market structure.” *Schotter Declaration* at ¶ 58.

In any event, the average efficiency level of the highest concentration scenario was seriously distorted by the performance of students in just *one* outlier session. *Id.* ¶ 53. Indeed, a detailed examination of the outlier trading session shows that the anomalous results were primarily due to the failure of *one seller to reach agreement with one buyer in one 6-minute bargaining period*. If this outlier is disregarded, the average efficiency for the highest concentration scenario is exactly the same as that reported for the next highest concentration scenario, which the authors of the *Working Paper* concede is not significantly different from the efficiency observed in the *lowest* concentration scenario. For these and other reasons detailed below, the *Working Paper*’s efficiency results are of no possible value here.

The *Working Paper*’s “DBS” conclusions are no more robust or relevant. Indeed, the conclusion that in one scenario, “the DBS operator’s bargaining power is higher in the Low/High concentration sessions than in the High/Low concentration sessions,” *Working Paper* at 34, reflects the observation of just *five* students in each of those two “treatments.” Moreover, the two “DBS” players who were the *most* successful were, in fact, in the High/Low concentration sessions; it was only because the two least successful “DBS” players were also in that group that the *average* bargaining power was lower in the High/Low sessions. *Schotter Declaration* at ¶ 62. “Given these disparate results, it seems untenable to argue that the increased concentration in the High/Low structure *causes* DBS operators to have less bargaining power.” *Id.*

More fundamentally, the *Working Paper* provides no basis for characterizing one player as a DBS operator and the others as cable operators. The game's "buyers" were told only their uncovered "fixed costs" and the "resale value" of each of the unidentified "fictitious" assets they could purchase. The costs and resale values assigned to the "DBS" player differed from those assigned to the "cable" players, but the *Working Paper* does not explain the derivation of these figures, much less demonstrate that they are representative of real-world differences between cable and DBS. In any event, the "DBS" player actually earned *more* profits, on average, than the "cable" player with an equal number of customers. "It is hard to see why any public policy issue is raised if DBS operators have less bargaining power but nevertheless earn higher profits than cable companies." *Id.* at ¶ 68.

In short, the *Working Paper* may be a useful contribution to the emerging field of laboratory study of bargaining under incomplete information, but it has no buyer market power predictive value and provides no non-conjectural basis for any cable ownership limit.

## ARGUMENT

### I. THE LABORATORY EXPERIMENT REPORTED IN THE *WORKING PAPER* IGNORED KEY REAL-WORLD CONSTRAINTS.

Experimental economics examines economic interactions "in controlled laboratory settings." *Notice* at 1. By observing students playing bargaining and other games, experimental economists seek to test and refine economic theories. That approach has proven useful in studying certain real-world institutions, such as auctions, where it is possible to create laboratory environments that are close facsimiles of real-world environments. However, "[e]xperiments are usually *not* suited to address empirical issues about the underlying structure of industrial markets." John H. Hagel & Alvin E. Roth, *ed.*, *Handbook of Experimental Economics* at 355 (1995) (emphasis added).

And although bargaining behavior is a frequent subject of laboratory experiments, leading experimental economists urge “healthy skepticism” with respect to claims that “the phenomena observed in the laboratory are likely to generalize to the wider world.” *Id.* at 329. That is because the environments explored in the laboratory are necessarily “quite simple and artificial” while “bargaining outside of the laboratory virtually always takes place in more complex environments.” *Id.*<sup>1</sup> “Consequently, some of the phenomena that appear important in the laboratory may have much diminished importance in naturally occurring negotiations, and phenomena that have no opportunity to emerge in the laboratory may assume much more importance.” *Id.*

The predictive value of experimental economics is at its nadir where, as here, bargaining under incomplete information is modeled. The “emerging experimental study of bargaining under incomplete information” is “especially difficult” and “especially susceptible to controversy,” because so much depends upon the players’ subjective beliefs, which are “essentially unobservable parameters.” *Id.* at 322.

Even where experiments can play a useful predictive role, they can only do so if the laboratory environment mirrors the real-world environment. As Dr. Schotter explains, “it is a strict requirement that the experiment present the subjects with the tradeoffs that real-world agents face when they make their decisions, and that the variables of concern to subjects in the lab be the same variables that real-world decision-makers care about.” *Schotter Declaration* at ¶ 4. Attention to the experimental procedures – e.g., the instructions given to players and the incentives they have as well as time and other constraints – is also important. “[E]xperiments are

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<sup>1</sup> See also Working Paper at 3 (“the experimental market did not and could not display all the complex characteristics of the actual market”).

very sensitive to these elements; a small change in procedures, information or incentives can lead to a substantial change in the outcome.” *Id.*

Measured against these criteria, it is clear that the laboratory results described in the *Working Paper* could not serve as the basis for structural regulation in this proceeding. According to the *Working Paper* (at 18), the laboratory experiments involved a trading institution described as a “decentralized bargaining market (DBM).” “[T]his particular institution has never before been studied in experimental economics, and so we have no knowledge about how other subjects have responded to it and no basis for evaluating whether the [*Working Paper*] subjects performed in a typical manner.” *Schotter Declaration* at ¶ 6. *See also Handbook of Experimental Economics* at 425 (“It is especially risky to claim that a single experiment confirms a general theory or establishes a stylized fact”).

The subjects of the *Working Paper* experiment are “given almost no information before they begin and virtually no feedback as the session progresses; hence they are in no position to assess their bargaining strength.” *Schotter Declaration* at ¶ 7. The experiment thus involves precisely the type of bargaining under incomplete information from which experienced experimental economists are especially reluctant to draw generalized conclusions. And, as detailed below, the experiment procedures (particularly the very short time limits) and the erratic behavior of a few players appear to have seriously distorted the experimental results.

Most fundamentally, the laboratory environment bears no resemblance to the real-world. The experiment was “a time-constrained matching market played under conditions of incomplete information about the market parameters, in which all subjects have fixed costs that must be covered by profitable trades in order to avoid losses.” *Id.* at ¶ 6. Players were told only that they were buying and selling unidentified “fictitious” assets. *See Working Paper* at 72. The players

assigned buyer roles had no information about their relative sizes, *Schotter Declaration* at ¶ 16, and no information (other than an assigned “resale value”) about the sellers’ products, costs, revenue opportunities or bargaining histories. No face-to-face negotiations were allowed; indeed, no communications of any kind were allowed, except for the computer transmission of offers and acceptances. *Id.* ¶ 7.<sup>2</sup> Buyers in the game could not gain or lose market share, and, as noted, no buyer even knew how large it was compared to other buyers. Similarly, the sellers were given no information that would allow them to gauge the relative values of their products. *Id.* ¶ 18. “A player’s only concern is with making correct matches or enough of them – not the player’s current or future share of the market.” *Id.* ¶ 8.

Moreover, the players received almost no feedback from one round to the next. “They have no way of knowing whether the deals they made were good or bad” or “whether others have traded or not.” *Id.* ¶ 39. It is well recognized in experimental economics that the “absence of information and feedback can lead to what is known as a ‘self-fulfilling equilibria’ in which subjects quickly develop a set of erroneous views about what trades are possible.” *Id.* “Believing that they cannot do better, the subjects make offers that are accepted and never explore other possibilities.” *Id.*

The real-world is, of course, very different. Professional, real-world negotiators have a great deal of highly relevant information, communication and feedback. Sellers of video programming know each buyer’s market share and channel line-up, and, presumably, will also know the popularity of their own programming. Sellers will generally also know what each

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<sup>2</sup> See also *Handbook of Experimental Economics* at 295 (“a careful comparison of face-to-face and anonymous bargaining . . . found that face-to-face bargaining captured over 99 percent of the gains from trade in an environment in which anonymous bargaining captured only 92 percent”).

buyer has been willing to pay in the past for comparable programming, what programming each buyer has recently added and dropped, and the general rate of inflation in buyers' programming costs. Sellers most definitely know that all buyers face retail competition and that this competition provides powerful incentives for buyers to purchase desirable, competitively-priced video programming, because the failure to do so will result in lost share.

In the real-world, buyers and sellers can and do communicate, often face-to-face. A seller, for example, can say to a potential cable buyer "my network is being carried by Echostar; do you want to risk losing customers by not carrying it?" And in the real-world there is near constant feedback – buyers and sellers can see which deals are made and which networks are carried on which systems. *Id.* ¶ 40.

In addition, virtually none of the variables that are most important to real-world programming buyers were reflected in the bargaining game. One particularly "important element of the marketplace that was not reflected in the experiment is the competition between DBS and cable operators for the same subscribers – a rivalry that would tend to diminish the bargaining power of both when negotiating with programmers (because one would be at a competitive disadvantage if it failed to carry programming offered by the other)." *Id.* ¶ 85. The experiment likewise failed to reflect the real-world fact that carriage contracts are multi-year contracts and that expected profits to the seller turn not on the buyer's existing share of subscribers, but on the buyer's expected number of subscribers over the life of the multi-year contract (which turns, in part, on other buyers' programming decisions). The experiment ignores both the existence of "must-see" networks that suppliers of video programming can and do bundle with less desirable channels and the marketplace reality that owners of video programming can obtain cable carriage even without the cable operator's consent by contracting

with these “must see” networks or “must carry” broadcast networks. The experiment did not account for the real-world constraint that any buyer that is large enough to be “pivotal” to the seller’s decision whether or not to produce desirable programming cannot credibly threaten to refuse to pay its “share” of the production costs. Although the experiment provided for additional payments to sellers meant to represent advertising revenues, it did not reflect the many other revenue opportunities available to owners of video programming, including revenues from foreign distribution. And the experiment modelled only very highly concentrated market structures with only three to five buyers and “HHI” concentration levels more than twice existing levels. Thus, although experimental economists warn that it is always dangerous to generalize from the results of laboratory bargaining under imperfect information, it would plainly be irrational to do so here.

It would also be unlawful. If nothing else, *Time Warner II* makes clear that any “assessment of a real risk of anticompetitive behavior” in this context must take account of the “availability” of cable alternatives and the extent to which that competition constrains cable market power. *Time Warner II*, 240 F.3d at 1134 (“in revisiting the horizontal rules the Commission will have to take account of the impact of DBS on th[e] market power of [cable operators]”). As the original public notice in this remand proceeding explained, it can no longer be doubted that “the availability of an alternative MVPD outlet affords programmers access and consumers choice, and erodes cable’s or an MSO’s market power irrespective of current market shares.”<sup>3</sup> See also *Schotter Declaration* at ¶ 8 (“the experiment would have to be dynamic and one in which market concentration was an endogenous variable”). Because the experiments

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<sup>3</sup> *Implementation of Section 11 of the Cable Television Consumer Protection and Competition Act of 1992, et al.*, Further Notice of Proposed Rulemaking, 16 FCC Rcd. 17312 ¶ 50 (2001).

described in the *Working Paper* did not “take account” of this key factor (or many of the other market-specific variables that constrain real-world video programming carriage negotiations), the Commission could not, consistent with *Time Warner II*, impose horizontal ownership limits on the basis of the experimental results.

## **II. THE WORKING PAPER’S “EFFICIENCY” AND “DBS” RESULTS ARE OF NO VALUE IN THIS PROCEEDING.**

Even if it was appropriate to generalize from the results of laboratory bargaining under imperfect information (it is not) and even if these particular experiments had been designed and implemented to mirror real-world carriage negotiations (they were not), the *Working Paper*’s “efficiency” and “DBS” results could not be credited. As explained below, neither of those experimental results is reliable or says anything at all about the risk of buyer market power in real-world video programming carriage decisions.

### **A. The Working Paper’s “Efficiency” Results.**

The experiment yielded only one efficiency result that the *Working Paper* regards as statistically significant. This “modest reduction in ‘economic efficiency’” involved the scenario in which cable capacity was limited and carriage contracts did not contain “most favored nations” provisions (as they often do in the real-world). Three scenarios were modelled, a “High/High” structure in which the largest of five buyers had a 51% “market share,” a “Low/High” structure in which the largest of five buyers had a 27% share, and a very highly concentrated “High/Low” structure in which there are just *three* buyers with shares of 44%, 39% and 17%. In each scenario, one of the buyers (the one with the 17% share) was deemed the DBS operator; all other buyers were deemed cable operators. Notably, the *Working Paper* found *no* statistically significant difference in efficiency when the four cable buyers in the Low/High

structure “merged” into just two cable buyers with shares of 44% and 39% in the highly concentrated High/Low structure.

Rather, the only difference deemed statistically significant was between the Low/High structure in which the largest cable operator had a 27% share and the High/High structure in which the largest cable operator had more than a 50% share – and even that difference could be deemed significant only under a very undemanding test of statistical significance, *see Schotter Declaration* ¶ 55. For a number of reasons, “[n]o policy conclusions should be drawn from th[is] result[.]” *Id.* ¶ 34.

First, a reduction in “efficiency” as defined in the *Working Paper* principally reflects a failure to reach agreements that would be mutually profitable to buyer and seller. The *Working Paper* offers no economic theory why there would be a link between an increase in buyer concentration and the failure to enter into contracts that would be profitable for *both* parties. Certainly, these results cannot be explained by any theory of buyer market power, the relevant consideration in this proceeding. The *Working Paper* “found that the larger buyers did *not* have greater bargaining power or negotiate better terms.” *Schotter Declaration* ¶ 58. “Indeed, the students acting as buyers did not even know their relative sizes.” *Id.* Thus, “[i]f some were more aggressive” and that aggressiveness resulted in more bargaining failures, “it was because of their personalities, not the market structure.” *Id.* Moreover, the bargaining failures that drove the results claimed to be statistically significant were not the product of decisions by the players acting as the largest cable company, but failures of the players acting as the three *smallest* cable companies. *Id.* ¶ 59. “[W]here (as here) an experiment produces results that are not predicted by theory, further study may be warranted – but policy conclusions are not.” *Id.* ¶ 60.

Second, the students playing the experimental games *regularly* failed to reach agreements that would have been mutually profitable. As Dr. Schotter explains, the efficiency levels observed in the *Working Paper* games, “with an average of only 87.21 percent,” are “quite low compared to the levels observed in other experiments designed to replicate matching markets.” *Schotter Declaration* at ¶ 35; *see also id.* (“what is most striking about the experimental results is that the subjects were inefficient bargainers *regardless* of market concentration”). The experimental results are also flatly inconsistent with real-world observations. In the real-world the most popular programming networks *always* get carriage, but in the experiment, the student acting as the most popular programming network frequently failed to reach agreements with the students acting as cable buyers. *Id.* Indeed, as explained below, the failure in one of the trading sessions of the most popular network to reach a mutually profitable deal largely accounts for the efficiency difference the *Working Paper* deems statistically significant.

This pronounced disparity between both conduct in the real-world and prior experimental economics experience is further reason to review the experimental results with great skepticism. “[T]here is some underlying cause of the bargaining failures in the experiment that is not found in the actual marketplace,” and, as Dr. Schotter explains, that cause is most likely the design flaws identified above. *Id.* In the actual marketplace, commercial actors have information and experience, can communicate, and constantly receive feedback. In the experiment, subjects had little information and no experience, could not communicate, and received no relevant feedback – in short, “the experiment deprived the subjects of the elements needed to bargain efficiently.” *Id.* ¶ 41. Moreover, Dr. Schotter’s examination of the trading data suggests that the need to cover fixed costs and the very short time limits for negotiation produced behavior that “is more consistent with *loss avoidance* than with *profit maximization*.” *Id.* ¶ 48. “In the real-world,

MVPD operators and programmers face no comparable time limit, and they undoubtedly behave in a manner consistent with profit maximization.” *Id.* ¶ 50.

Finally, even if the market institution in the experiment did resemble the actual marketplace, “the efficiency results are not sufficiently robust to even consider drawing any conclusions for policy-making purposes.” *Id.* ¶ 52. Very little data – only four experimental sessions, each involving only five student “buyers” – were used to measure “economic efficiency” in the “High/High” scenario. *Id.* And the average efficiency level observed in that scenario – the figure that served as the basis for the *Working Paper*’s efficiency finding – “was seriously distorted by the poor performance of the students in just *one* session.” *Id.*

The results of sessions 1, 2 and 4 of the High/High scenario match up relatively closely to each other and to the efficiency results measured in the other scenarios. Indeed, the average efficiency from those three sessions was the *same* as the average efficiency measured for the High/Low scenario. As the *Working Paper* found, the High/Low scenario average efficiency was not significantly different from the Low/High average efficiency. In other words, if the *Working Paper* had properly disregarded the outlier session 3 in the High/High scenario, it would have found *no* significant difference in efficiency results between *any* of the scenarios.

And High/High session 3, with an average efficiency of only 67.6%, clearly was an outlier. “[A] detailed examination of the outlier trading session shows that the anomalous results were primarily due to the failure of *one seller to reach agreement with one buyer in one 6-minute bargaining period.*” *Schotter Declaration* ¶ 54. That seller, # 4, was the one designated as the most popular/valuable programming network, and because he was by far the “biggest” seller, that failure had a very large impact on the efficiency level for the session as a whole. “If Seller #4 had reached a deal with Buyer #7 in Period #8 (a result they had achieved in previous

rounds), then the efficiency level for this session would have been 16% higher (*i.e.*, an 84% average), and there would no longer have been a statistically significant difference in efficiency levels related to buyer concentration.” *Id.* “Obviously, it would be foolish to reach any policy conclusions because of the results of the negotiation between Seller #4 and Buyer #7 in Period #8.” *Id.*<sup>4</sup>

Moreover, the second lowest average involving the High/High scenario (83.3% in session # 4) was also distorted by one particular 6-minute trading round that produced an efficiency of only 37.7% (from students that performed quite well in the other 6-minute trading rounds in that session). *Id.* ¶ 56. If that one 6-minute trading period were disregarded, the average economic efficiency for session # 4 would have been 98.9%, not 83.3%. *Id.* “And if that result were used, then the High/High structure would actually have produced the *most* efficient average outcome, not the least efficient.” *Id.* For these and other reasons explained in the *Schotter Declaration*, the *Working Paper*’s efficiency results are entitled to no weight in this proceeding.

**B. The *Working Paper*’s “DBS” Results.**

The *Working Paper*’s “DBS” results are equally irrelevant here. The experiments in the *Working Paper* yielded a variety of seemingly conflicting results regarding the buyer that was supposed to represent a DBS operator. In the “MFN” scenario (in which the largest cable operator could impose a most-favored-nations requirement), the “DBS” player’s bargaining power was higher in the more concentrated High/Low scenario than in the less concentrated Low/High scenario, but when the largest cable operator could not impose an MFN, the DBS

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<sup>4</sup> It is widely recognized that experimental economics is ill suited to explain why mutually beneficial transactions do not take place. See *Handbook of Experimental Economics* at 321 (“there remains considerable room for improvement in our understanding of the causes of disagreement and delay”).

player's bargaining power was *lower* in the more concentrated High/Low. *Schotter Declaration* ¶ 61. The *Working Paper* has no rational explanation for this disparity and none is apparent.

None of the DBS results are robust. Indeed, the conclusion that "the DBS operator's bargaining power is higher in the Low/High concentration sessions than in the High/Low concentration sessions," *Working Paper* at 34, reflects the observation of just *five* students in each of those two "treatments." Moreover, the two "DBS" players who were the *most* successful bargainers were, in fact, in the High/Low concentration sessions. The *average* bargaining power was nonetheless lower in the High/Low scenario, because the two least successful "DBS" players were also in that group. *Schotter Declaration* at ¶ 62. "Given these disparate results, it seems untenable to argue that the increased concentration in the High/Low structure *causes* DBS operators to have less bargaining power." *Id.* "It is far more likely that the results reflect the bargaining skills of the particular individuals participating in the experiment." *Id.*

In any event, the *Working Paper* provides no basis for characterizing one player as a DBS operator and the others as cable operators. The game's "buyers" were told only their "fixed costs" and the "resale value" of each of the unidentified "fictitious assets" they could purchase. The costs and resale values assigned to the "DBS" player differed from those assigned to the "cable" players, but the *Working Paper* does not explain the derivation of these figures, much less demonstrate that they are representative of real-world differences between cable and DBS. For example, buyers 7 and 9 in the Low/High scenario had the same number of customers. Buyer 7 was told that he needed to cover \$434 in uncovered fixed costs; Buyer 9 was told that he needed to cover \$339. *Id.* ¶ 63. It seems highly unlikely that the authors of the *Working Paper* had any reliable information "about the relative level of uncovered costs for cable and DBS operators; they certainly have not cited any." *Id.* n. 28. The game also assigned different resale

values to the buyers 7 and 9, notwithstanding that they were buying the same programming and offering it to the same customer base. Again, the Working Paper does not even attempt to justify these figures “by showing that they correspond to the relative revenue streams available to DBS and cable operators.” *Id.* ¶ 65.

But even if these seemingly arbitrary uncovered cost and resale values did reflect real-world differences between cable and DBS operators, examination of the trading data confirms that “these factors were swamped by the influence of the personalities of the few subjects in the experiment who played these roles.” *Id.* ¶ 66. The five students who played buyer 7 (“cable”) earned profits ranging from \$499 to \$1209. *Id.* ¶ 67. The five students who played buyer 9 (“DBS”) earned profits ranging from \$316 to \$1305. *Id.* “These variations should give one pause before attempting to draw any conclusions from a few data points about the bargaining power of ‘DBS operators’ versus ‘cable operators.’” *Id.* “The outcomes would certainly seem to be influenced much more heavily by the bargaining capabilities of the individual students than by the parameters that distinguish ‘DBS’ from ‘cable’ operators.” *Id.*

In any event, the “DBS” player actually earned *more* profits, on average, than the “cable” player with an equal number of customers. “It is hard to see why any public policy issue is raised if DBS operators have less bargaining power but nevertheless earn higher profits than cable companies.” *Id.* at ¶ 68.

**III. NOTWITHSTANDING THE FAILURE TO REFLECT KEY MARKET-SPECIFIC FACTORS THAT CONSTRAIN BARGAINING POWER, THE WORKING PAPER FOUND NO CORRELATION BETWEEN “BARGAINING POWER” AND CABLE OPERATOR SIZE ACROSS THE MARKET STRUCTURES THAT WERE MODELLED.**

The most interesting aspect of the *Working Paper* is the cable bargaining power results. Unlike the efficiency findings, which, as discussed above, were based primarily on bargaining *failures* likely caused by problems with the experimental design (e.g., severe limits on

information, communications, feedback and time), these bargaining power results relate only to bargaining *successes* (e.g., contracts actually negotiated by the game players). The problems of generalizing from laboratory bargaining under imperfect information are therefore less pronounced with respect to the bargaining power results (although still quite substantial). Moreover, the sellers in the game did have information about the relative size of the the buyers and thus this aspect of the experiment “was designed so that (1) sellers can make more money from dealing with buyers having larger shares, (2) sellers know how important it is to enter into a contract with the largest buyer, and (3) sellers are better able to say ‘no’ to the biggest buyer (and still make a profit) in less-concentrated markets than in more-concentrated markets.” *Schotter Declaration* ¶ 83. The *Working Paper* found that sellers fared equally well against the largest buyer, regardless whether the largest “cable” buyer had a 27% share or a 51% share. That was true notwithstanding that the experimental world, as discussed above, failed to take account of many of the market-specific real-world factors (e.g., the ubiquitous availability of DBS) that contrain buyer power in this particular context.

This is not to suggest, of course, that the *Working Paper’s* bargaining power results should play an important (or even any) role in this proceeding. As noted above, there are many reasons why it would be arbitrary to base industrial policy on the first laboratory experiment of its kind, particularly one that strays so far from real-world conditions. Moreover, the record in this proceeding is replete with evidence that *does* take account of the important market-specific factors and that deals directly with the relevant questions of buyer market power and is therefore considerably more compelling. However, the *Working Paper’s* bargaining power results do give the lie to the arguments of proponents of low cable ownership concentration limits, who argue that it simply cannot be that a cable buyer that serves more than 30 percent of subscribers lacks

market power over suppliers of video programming and that, based upon this “intuition,” the Commission must disregard the record evidence and established economic theory.

### CONCLUSION

For the foregoing reasons and the reasons set out in AT&T’s initial and reply comments in this proceeding, the Commission should conduct this proceeding in accordance with the dynamic market power analysis mandated by *Time Warner II* and the Commission’s longstanding policies on the basis of the market-specific evidence already in the record.

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July 18, 2002

**CERTIFICATE OF SERVICE**

I hereby certify that on this 18<sup>th</sup> day of July, 2002, I caused true and correct copies of the forgoing Supplemental Comments of AT&T Corp. to be served on all parties by mailing, postage prepaid to their addresses listed on the attached service list.

Dated: July 18, 2002  
Washington, D.C.

/s/ Peter M. Andros

Peter M. Andros

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