Before the
FEDERAL COMMUNICATIONS COMMISSION
Washington, DC 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 Cable Horizontal and Vertical Ownership Limits 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 ANDREW SCHOTTER
ON BEHALF OF AT&T CORP.

I. QUALIFICATIONS

1. My name is Andrew Schotter. I am Professor of Economics at New York University, which I joined in 1975. At that institution, I am currently the Director of the Center

for Experimental Social Science, and I previously served as Chairman of the Department of Economics and as Co-Director of the C.V. Starr Center for Applied Economics. I have been active in a number of professional organizations, most notably as President of the Economic Science Association. I have also served on the editorial board of the *American Economic Review*, and am an Associate editor of two other journals, *Games and Economic Behavior* and *Experimental Economics.*

2. My main areas of research and teaching are in economic theory, game theory and especially experimental economics. I am the author of a microeconomics textbook that is now in its third edition, and have written or edited four other books. I am the author or co-author of over fifty articles, most of them published in refereed journals. I have testified before Congress and also in court as an expert witness. My consulting work in the field of experimental economics has included such topics as price preference auctions, the free-agent system in baseball, and corporate compensation. Among the areas of experimental economics that I am currently researching is the process by which economic agents go about the task of learning, and the impact of advice on economic decision-making. I am also conducting some experiments on the phenomenon of bank runs.

II. INTRODUCTION AND SUMMARY

3. I have been asked to comment on OPP Working Paper No. 35, "Horizontal Concentration in the Cable Television Industry: An Experimental Analysis," by Mark M. Bykowsky, Anthony M. Kwasnica and William W. Sharkey, which was released by the Federal Communications Commission's Office of Plans and Policy in June 2002 and revised in July 2002. The Working Paper describes experiments designed to investigate the impact of

concentration ratios on performance in the cable industry. Specifically, the paper utilizes experimental economics in an attempt to simulate the behavior of Multichannel Video Programming Distributors ("MVPDs") in negotiating affiliate fee agreements with programming networks under various market structures. All of the data referred to in this declaration were supplied by the authors of the Working Paper, whom I will sometimes refer to as "BKS."

4. In 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. When evaluating the results of an experiment, it is critically important to analyze the market procedures (especially the market mechanism used to make transactions), the information given to the participants, and the incentives they have. This is important not only to determine whether the experiment parallels the pertinent real-world conditions, but also to assess the reliability of the results. What we have learned from years of research is that experiments are very sensitive to these elements; a small change in the procedures, information or incentives can lead to a substantial change in the outcome.

5. My main finding is that we cannot rely on the experiment performed by BKS as the basis for policy because their experiment does not present the subjects with tradeoffs that parallel those existing in the real world market occupied by MVPDs and programmers.

6. The experiment run by BKS involves a trading institution that the authors describe as a "decentralized bargaining market (DBM)."[1] This game, as played in the BKS lab,

[1] Working Paper, p. 18.

represents 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. For any subject, there are subjects on the opposite side of the market who are relatively better matches than others (although the exact value of matches is left ambiguous in the instructions) and a subject's goal is to make a match with those subjects within five or six minutes. The time constraint placed on the subjects is an ever-present factor and if matches are not made, losses can and do occur. To my knowledge, this 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 BKS subjects performed in a typical manner.

7. In my opinion, the conditions in the BKS experiment differ fundamentally from the conditions facing actual MVPDs and programmers. In the experiment, subjects are given almost no information before they begin and virtually no feedback as the sessions progress; hence, they are in no position to assess their bargaining strength. The subjects are not allowed to engage in face-to-face negotiations; in fact, no communications of any kind are allowed, except the transmission of offers and acceptances. Furthermore, subjects are under tight time constraints of only five or six minutes in which to negotiate multiple contracts. Thus, the experiment may show 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 whatsoever on the real-world efficiency of negotiations between MVPDs and programmers.

8. Furthermore, it is my conclusion that the tradeoffs and concerns in this "DBM" game have little to do with the central strategic concerns of the cable industry. In the experiment, buyers cannot gain or lose market share. Yet, as I understand it, the question in the

matter at hand that prompted the Working Paper is whether an increase in concentration in the cable television industry will alter its performance characteristics. To my mind, the strategic matter of interest in answering this question is whether large firms will have strategies at their disposal that small firms do not. If this characterization of the problem were correct, then any experiment run would have to give scope to laboratory cable operators (buyers) to engage in practices that increase their power in the market. In my mind, the experiment would have to be dynamic and one in which market concentration was an endogenous variable where cable operators traded off profits today (by offering better prices or refusing deals) for increased concentration tomorrow. Yet in the BKS experiment, market share is exogenous. 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.

9. The Working Paper describes results in four areas that I will discuss in this declaration: (1) bargaining efficiency (which refers to the success of buyers and sellers in negotiating contracts that produce profits for both), (2) the performance of one particular seller in the experiments, who is supposed to represent a DBS operator rather than a cable company, (3) the effect of most-favored-nations ("MFN") agreements, and (4) the relationship, if any, between the bargaining power of cable operators and concentration in the MVPD market.

10. In my opinion, policy-makers should not rely on the Working Paper's finding that in one particular scenario, there was a "modest reduction in 'economic efficiency'" – that is, a modest increase in the rate of bargaining failures – when the buyers' market was more concentrated. This result was caused by the peculiar manner in which subjects reacted to their time constraints and to the severe lack of information they had both before and during the experiment. In addition, the feedback offered subjects during the experiment was not compatible

with learning if bargaining mistakes occurred. Hence, I am forced to conclude that it would be a mistake to attribute the decrease in efficiency observed in one particular treatment to higher market concentration there. Indeed, what is perhaps most noteworthy is the relatively low efficiencies (*i.e.*, the high rate of bargaining failures) under *all* scenarios – a result that is not observed in real world performance in this industry or in other experimental settings. These low efficiency levels indicate that the DBM institution used in the BKS experiments is simply not a reliable producer of efficient results. Without further investigation of this DBM institution, we have no way of reliably imputing the efficiency levels in this particular experiment. In any event, the one "modest reduction" in efficiency cited in the Working Paper is statistically weak and is accounted for by the unusual behavior of a few students. That is hardly a sound basis for making policy judgments, particularly because the Working Paper does not offer any basis in economic theory to *expect* bargaining inefficiencies to result from increased concentration.

11. Policy-makers likewise should not rely on the results relating to "DBS." I have deliberately put the term DBS in quotation marks. Although the Working Paper states that one of the buyers in the experiment was intended to represent a DBS operator rather than a cable operator, the paper never demonstrates that the small differences among buyers corresponds to the characteristics that distinguish cable from DBS in the marketplace. Moreover, the DBS results, like the MFN results, are statistically weak and appear to be explainable by the lack of control offered by the experimental design on the *ex ante* bargaining power of the subjects.

12. My view is somewhat different with respect to the bargaining power results. The Working Paper's finding that there is no relationship between bargaining power and a cable operator's market share may be of potential interest from a theoretical point of view. I say this because from my perspective, economic theory offers conflicting predictions about the impact of

market size on bargaining power. (I am referring to economic theory pertaining to markets *generally*, and not specifically to the cable industry, because the DBM institution does not include features in the MVPD market that are said to constrain the exercise of buying power by large cable operators.) Experiments are often designed to offer insight into how individuals behave when there are conflicting predictions by economic theory. Although it may be argued in the abstract that an increase in size might work to the advantage of a buyer, arguments to the contrary can also be made. The results of this experiment help resolve the conflict by suggesting that size is unrelated to bargaining power in this context, even in the absence of important market-specific factors that constrain the exercise of bargaining power. This is a result consistent with the findings of Janusz Ordover, who approaches the question from a different perspective but reaches a similar conclusion.

III. THE EXPERIMENT

A. The Experiment as Presented to the Subjects

13. The BKS experiment was conducted with undergraduates and graduate students as subjects. They were paid $7 to show up, and promised additional payments if they generated a profit during the course of the game. Each student was assigned to act as either a seller or a buyer of "fictitious assets"; they were not told what the product was.[2] In each session of this experiment, four students acted as sellers, and either three or five acted as buyers. The subjects in a session are referred to as Sellers #1 through #4 and Buyers #5 through #9. This type of exercise is known in experimental economics as a "matching" institution because each participant has an opportunity to make multiple bargains with different players.

[2] Working Paper, at 72.

14. In evaluating the experiments, one needs to examine the instructions that were given to the participants. All subjects were assigned a certain amount of fixed costs for each round of bargaining. The fixed costs were set at relatively high levels, so that the subjects needed to realize significant profits from the contracts they negotiated just to break even. In fact, many of the participants lost money in a number of the rounds.

15. The buyers were told that they could purchase the fictitious assets from any of four sellers, and could then immediately resell those assets for a guaranteed amount, depending on the asset. For example, the first buyer was told that Seller #1's asset could be resold for $198, Seller #2's for $204, Seller #3's for $745, and Seller #4's for $1,278. Each buyer was given a different schedule of resale values (which was intended to reflect the fact that some buyers had more customers than others), but the schedules all had the same general pattern: Seller #4's product was by far the most important because it could be sold for more than all of the other products put together. Seller #3's product was also potentially lucrative, with a resale value that was far more than that of #1 or #2. As between #1 and #2, the resale values were always very similar; #1 was sometimes a bit higher, sometimes a bit lower.

16. The buyers were told that if they negotiated a deal with a particular seller, their profit from the transaction was the difference between the guaranteed resale value and whatever purchase price they negotiated.[3] At the end of a round, each buyer's profits were totaled and then the fixed costs were subtracted. The buyers were advised that other buyers did not have the same resale values for the same asset, although they were not told why this was so. Specifically,

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[3] The resale value is referred to in the Working Paper (though not in the instructions given to the subjects) as the buyer's maximum "Willingness To Pay."

the buyers were not informed that their resale values were determined by how big they were relative to the other buyers; in fact, they were not told that the buyers differed in size.

17. The sellers were given more information. They were told how many customers each buyer had. Although the sellers did not know the buyers' actual resale values, they were informed that each buyer had a guaranteed resale value reflecting the number of customers that the buyer had. Thus, for example, in a treatment where sellers were told that Buyer #7 and Buyer #9 each had 14 customers, the sellers could infer that those buyers had approximately the same resale values.

18. In addition, the sellers were told that they would receive "third-party payments" in connection with each sale. The amount depended on the buyer, and was set forth in a schedule given to the seller. No seller, however, was given any information that allowed him to determine how "large" he was relative to the other sellers; for instance, he was not told how his third-party payments compared to those received by other sellers, or how the resale value of his product compared to the resale value of the products offered by others.

19. The sellers were free to enter into contracts with each of the buyers, and were told that their profit from each transaction was equal to whatever sale price they negotiated plus the third-party payment. Like the buyers, the sellers were each assigned a level of fixed costs, and their net earnings from any round were equal to the profits from the contracts they negotiated minus fixed costs.

20. The buyers and sellers had no direct communications with each other. All contact was by computer. Each buyer could submit proposals to any or all sellers and could accept

proposals received from sellers. The sellers could do likewise. Apart from these offers and acceptances, there was no exchange of information.

21. Each set of students participated in a single session consisting of eight rounds (or "periods") in which they attempted to negotiate contracts. The participants were allowed only five or six minutes per round.[4] When a round ended, each subject was told his net earnings. The students were not told how well the others did, or what contracts they negotiated, or what the prices were. This same process was repeated in each round. When the eight rounds were completed, those students who made money received, in addition to the $7 participation fee, one dollar for each $500 of profits.

B. The Intended Interpretation of the Experiment

22. Each element of the experiment was intended to represent some feature of the marketplace. The sellers were meant to represent programmers, and the third-party payments they received were designed to reflect advertising revenues. The buyers were intended to be cable or DBS operators, and their resale values were meant to represent the additional subscriber revenues that an operator would earn if it carried a particular programming channel. The much higher resale values associated with Sellers #3 and #4 indicate that their programming was far more popular than that offered by Sellers #1 and #2.

[4] The instructions appended to the Working Paper indicate that the students in some sessions were told they would be allowed six minutes per round. *See* Working Paper, pp. 75, 82, 89, 97, 112. In other sessions, however, the students were told they would be allowed only five minutes. *Id.*, p. 104. This disparity is not explained.

23. Buyers #5 through #8 were intended to represent cable operators,[5] and #9 was supposed to be a DBS operator, although it is not entirely clear what basis BKS had for drawing this distinction. As discussed below, the only factors in the experimental design that distinguished Buyer #9 from the others were (1) Buyer #9's fixed costs were somewhat lower, and (2) its resale values were somewhat different. For reasons that are not explained, Buyer #9 has a relatively higher resale value for three of the programming networks, but a lower resale value for the fourth (and largest) network.

24. The experimental sessions were conducted in different ways to test the effect of three variables. The first variable was whether the buyers could enter into contracts with *all* sellers. (The sellers were always permitted to enter into contracts with all buyers.). This variable was intended to reflect the ability of a cable or DBS operator to carry all programming channels available. Two alternatives were studied. In the unlimited-capacity sessions the buyers could purchase from all sellers; but in the limited-capacity scenario each buyer was allowed to enter into contacts with at most three of the four sellers. The second variable was whether the largest buyer could impose a most-favored-nations requirement. Such provisions were not used in most of the sessions, but in some sessions one or two buyers are "MFN-empowered." The final variable was market structure. Three alternatives were considered:

- The "High/High" structure has five buyers, the largest with a 51% market share; the other shares range from 7 to 17%.
- The "Low/High" structure also has five buyers, but the largest of them has a share of only 27%; the others have shares ranging from 15% to 24%. (The label "Low/High" means low concentration but a high number of buyers.)

[5] In some sessions, there were only two cable operators.

- The "High/Low" structure has just three buyers, with shares of 44%, 39% and 17%. This is the market structure that would result if there were two mergers among the four cable companies in the Low/High case.

Thus, the experiment varied both the number of buyers and the degree of market concentration. In each of the structures, the buyer that was intended to represent a DBS operator had a 17% share.

25. All told, BKS considered eight of the twelve possible permutations on these variables (referred to as "treatments"). For each treatment, BKS conducted between two and five experimental sessions.

C. The Performance Criteria Investigated

26. In any bargaining game there are elements of both cooperation and competition. The parties must cooperate to reach agreements that benefit both, but each also competes with the other to maximize its share of this benefit. The Working Paper evaluates performance in both areas. It examines the "economic efficiency" of the outcomes by considering the extent to which the buyers and sellers succeeded in reaching agreements that maximized their combined profits. It also analyzes the performance of buyers versus sellers, principally by measuring the exercise of "bargaining power." In evaluating performance, BKS generally take into account only the last four rounds of each session. The rationale for disregarding the first four rounds is that the students are still learning how to play.

27. **"Economic Efficiency."** In the Working Paper, the term "economic efficiency" refers to the success of buyers and sellers in achieving the maximum possible amount of profit that is obtainable by trading. The total amount of profit earned by both parties to a contract is referred to in economics as the "surplus." In this experiment, if a deal is consummated, the

surplus is constant for any given buyer and seller, no matter what price they actually negotiate.[6] Furthermore, the surpluses are always fairly large, meaning that within broad ranges any price that the parties agree upon would be profitable to both. If, however, a buyer and seller fail to agree – that is, if there is a *bargaining failure* – then both sides are worse off; the surplus is lost.

28. The Working Paper's specific use of the term "efficiency" must always be kept in mind when evaluating the results. This is the standard definition of economic efficiency used in most experimental market studies, but it should not be confused with the idea of being "cost-efficient" when running a business, or with the "merger efficiencies" that can result when two firms combine. Even if the Working Paper results were accepted at face value, they would not imply that firms with high market shares behave in a less cost-efficient manner or that mergers create inefficiencies. It is sometimes said (perhaps more often by lawyers than economists) that monopolists are not as driven as others to run their businesses efficiently. Whether or not that viewpoint is correct, it is not what this experiment investigated.

29. In discussing efficiency, the Working Paper refers not only to bargaining failures, but also to a second phenomenon: entering into "socially less desirable trades."[7] According to BKS, in the capacity-limited treatment (where each buyer can purchase from at most three sellers), the parties have not acted efficiently if the buyer succeeds in negotiating three contracts, but those are not the *best* three contracts. In this context, "best" does not mean the three

[6] The surplus is constant because the only term the parties negotiate is price. If a negotiation involves several issues and the parties can improve their positions with tradeoffs, then the surplus is usually not a constant.

[7] Working Paper, pp. 4, 49.

contracts that generate the most profit for the *buyer*, but rather the three that yield the highest combined profit for both buyer and seller (*i.e.*, surplus).

30. **"Bargaining Power."** One way to measure how successful a party is in a negotiation is to determine what percentage of the surplus he obtained as a result of the price he negotiated. The buyer's percentage is referred to by BKS as "Buyer Bargaining Power." For example, if the Buyer Bargaining Power is 50 percent, it means that the buyer and seller profited equally from the transaction. To analyze the data, BKS needed a way to aggregate the results of individual trades. They used two approaches, referred to as "Average Buyer Bargaining Power" and "Average Buyer's Surplus."[8]

D. The Experimental Results

31. The Working Paper reports results in four performance areas of the BKS experimental market: (1) whether the subjects bargained efficiently, (2) how the "DBS" operator performed relative to the cable companies, (3) whether most-favored-nations (MFN) agreements influenced the results, and (4) whether bargaining power was affected by market concentration. The key results were as follows:

- **Efficiency.** The Working Paper reports that in one particular scenario, when comparing two of the three market structures (Low/High versus High/High), increased concentration "led to a modest reduction in 'economic efficiency'" that BKS regarded as statistically significant.[9]

[8] The second aggregation approach was used because of the possibility that the first may not take into account the "quality" of contracts (bigger contracts are more important than smaller contracts). *Id.*, p. 25.

[9] Working Paper, p. 49; Result 1, pp. 27-28.

- **DBS.** In one particular scenario, "the DBS operator's bargaining power is higher in the Low/High concentration sessions than in the High/Low concentration treatments."[10]

- **Most-favored-nations agreements.** "The MFN sessions generate similar efficiency levels to the No MFN sessions in the more concentrated treatments, and lower efficiency levels in the low concentrated treatment."[11] However, there was too little data to test this result for statistical significance.

- **Bargaining Power.** "There is no statistically significant difference in the bargaining power of the largest buyer in each of the three concentration treatments."[12] "From the perspective of a programming network, a cable operator that serves 51% of the market is as powerful as one that serves 27% of the market."[13] Also, in the one scenario where BKS could test for statistical significance, they found that the average buyer's bargaining power "is not related to the level of horizontal concentration."[14]

IV. THE EFFICIENCY RESULTS

32. The experiment yielded only one efficiency result that BKS regarded as statistically significant. The "modest reduction in 'economic efficiency'" involved the scenario in which capacity was limited and no MFN agreements were used.[15] The results were as follows:

Market Structure	Average Efficiency Percentage
High/High	83.6%
Low/High	93.0%
High/Low	89.0%

[10] *Id.*, Result 8, p. 34.

[11] *Id.*, Result 3, p. 28.

[12] *Id.*, Result 7, p. 33.

[13] *Id.*, p. 49.

[14] *Id.*, Result 6, p. 32.

[15] *Id.*, p. 49.

33. Only the difference in efficiency levels between the High/High and Low/High market structures – 83.6% vs. 93% – was said to be statistically significant. The difference between Low/High and High/Low (89% vs. 93%) was not statistically significant.[16] In other words, BKS did not find a statistically significant change in economic efficiency when the four cable companies merged into two cable companies with shares of 44% and 39%. Likewise, there was no statistically significant difference in efficiency between the High/Low and High/High market structures.

34. In my opinion, the efficiency results reported in the Working Paper should not be credited by policy-makers. As explained more fully below, I reach this conclusion for several reasons. First, the actual DBM market institution used in these experiments appears to be an inefficient mechanism for making trades. Hence we cannot distinguish the impact of buyer size on market efficiency from the unreliable performance of this mechanism in general. Second, from the standpoint of analyzing bargaining failures, the conditions in the experiment are radically different from the conditions in the cable industry, and so the results in the experiment shed no light on how the participants in the cable industry are likely to behave. Furthermore, subject behavior in this experiment was dominated by concerns of loss avoidance and time pressure that led to a perverse set of actions responsible for the observed inefficiencies. These concerns and time pressures do not characterize the real markets of relevance. In any event, the efficiency results are not at all robust, and appear to be due to the atypical behavior of one or two students in one experimental session. No policy conclusions should be drawn from these results, which do not appear to be predicted by any economic theory.

[16] Working Paper, p. 27.

A. The High Rate of Bargaining Failures in All Treatments: The Unreliability of the DBM Institution

35. At the outset, what is most striking about the experimental results is that the subjects were inefficient bargainers *regardless* of market concentration. In only four of the treatments did the participants achieve an average efficiency above 85 percent, while only twice were efficiencies above 90 percent. The subjects in this experiment regularly failed to reach agreements that would be mutually profitable, and these failures were not limited to the small programming networks; the buyers also failed to negotiate contracts for the most popular programming. Yet in the real world, as far as I know, MVPDs and programmers do not behave in this fashion. They do not regularly fail to come to terms when it is possible for both sides to profit. This pronounced disparity between the outcome in the experiment and conduct in the real world is, by itself, a sufficient reason why policy-makers should not rely on the Working Paper's efficiency results. It tells us that there is some underlying cause of the bargaining failures in the experiment that is not found in the actual marketplace and that is not due to concentration. Even if we could not identify the underlying cause of the problem, the experimental results would be unreliable as a guide to policy.

36. The efficiency levels observed in the BKS tests, with an average of only 87.21 percent, are also quite low compared to the levels observed in other experiments designed to replicate matching markets.[17] The levels observed here are certainly low when compared to the double oral auction institution that has frequently been studied in experimental economics. This is a further reason to view the results with skepticism, and it points to a need to investigate why

[17] *See, e.g.*, Nalbantian, Haig and Schotter, Andrew, *Matching and Efficiency in the Baseball Free-Agent System: An Experimental Examination*, 13 Jnl. Law & Econ. 1-31 (1995).

the institution examined in this experiment produced efficiency results that differ so markedly from the institutions in other matching experiments. As explained below, the reasons are not hard to find.

B. The Artificial Limits on Information and Communication

37. The conditions in the BKS experiment differ so fundamentally from the conditions facing actual MVPDs and programmers that a policy-maker should not give any weight to the efficiency results. The BKS subjects are information starved. They are given virtually no data at the outset that would allow them to assess their bargaining strength. They are not allowed to engage in face-to-face negotiations, even though that has been noted to be an efficient form of bargaining.[18] Furthermore, when subjects negotiate, they are not allowed to share any information, even though that ability can undoubtedly facilitate the reaching of agreement.

38. The BKS experiment also denies the subjects virtually any feedback of information after each round of trading. Experimental economics teaches that feedback is important to the efficient operation of markets. In fact, one of the reasons why trading institutions like the double oral auction produce such efficient outcomes is that, despite the paucity of information given to traders at the outset, the feedback they receive is sufficient for them to learn the parameters of the market. The same is true of experimental asset markets where markets prove to be very efficient in aggregating the disparate information held by subjects into one market price. This ability is possible only if sufficient feedback is provided.

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[18] Radner, Roy and Schotter, Andrew, *The Sealed Bid Mechanism: An Experimental Study*, 48 Jnl. of Econ. Theory 179-220 (June 1989).

39. The feedback provided in the BKS experiment is not of that type. At the end of each round subjects learn nothing about the market except their own experience. They have no way of knowing whether the deals they made were good or bad. They have no way of knowing whether others have traded or not, or what prices were negotiated by other buyers and sellers. 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. Believing that they cannot do better, the subjects make offers that are accepted and never explore other possibilities. In fact, as discussed below, there are indications that a number of sessions in the BKS experiment arrived at such a self-fulfilling equilibrium, in which inefficient market outcomes persisted despite what appears to be irrational behavior. This type of inefficiency would have a much harder time persisting if the experiment had provided better feedback, in fact, feedback that is more consistent with the informational attributes of the cable industry.

40. These severe limitations on information and communication are not present in the actual marketplace that the BKS experiment is supposed to mimic. MVPDs and programmers actually have a great deal of information bearing on the terms that the other side is likely to accept. A programming network will have dealt with dozens of cable companies over many years, and is quite knowledgeable about the going rates for programming. A cable operator is likely to be well informed about the advertising revenues that programmers earn if their shows are carried by the cable company. This is obviously vital information in assessing the terms that the programmer is likely to accept. Yet in the experiment, buyers were given no information whatsoever about the size of the "third-party payments" that sellers received from some mysterious source. They had no idea, for example, that Seller #4 could cover its fixed costs and

make a tidy profit even if it gave its programming away for free. Further, in the real world, the parties have opportunities to communicate information. One can imagine a programming network saying to a cable operator, "I just negotiated deals with three other cable companies at 15 cents per subscriber per month," or "My network is being carried by DirectTV; do you want to risk losing customers by not carrying it?" The ability to communicate such information can facilitate agreement. In addition, cable operators and programmers receive feedback after each round of negotiations. By taking note of what programs are carried by which MVPD operators, they know who was successful in negotiating contracts. They may learn of the prices that other parties negotiated. And when the ratings are released, they presumably get an idea of the advertising revenues generated by the programming.

41. In short, the experiment deprived the subjects of the elements needed to bargain efficiently – information, communications and the opportunity for direct negotiations. It is no surprise that the subjects in fact bargained inefficiently.

C. The Impact of Artificial Time Limits and Fixed Costs

42. The BKS experiment had two other elements that contributed to the inefficient results: fixed costs and time limits. These factors, combined with the severe limitations on information and communication, led to behavior that is quite different from what one might expect in the cable industry.

43. BKS imposed tight time limits in their experiment, which is yet another feature distinguishing this institution from the operation of the actual marketplace. In the experiment, the subjects had only five or six minutes per round; they had multiple contracts to negotiate, all with the same deadline; and each buyer (or seller) had only one person who could conduct these

simultaneous negotiations. In the real world, MVPD operators and programming networks do not face similar time constraints. There are deadlines, of course, but negotiations begin months – not minutes – before the deadline. Further, the contracts do not all have the same deadline, and the parties have the ability to use as many negotiators as they need to do all the deals.

44. I conducted several tests to see how the time limits affected the behavior of the students in the experiment. One test was to examine when the trades actually occurred. Appendix 1 shows the percentage of agreements reached during each 30-second interval. In Treatments 6, 7 and 8, a high percentage of the agreements were reached during the first 30 seconds, and not many agreements were reached thereafter. Although these subjects acted quickly, they were among the worst at reaching agreements. It appears that students who make use of the time allowed are more successful at reaching agreements.[19] In other words, more time to negotiate results in more agreements.

45. I also ran a simple regression to examine the significance of time as a factor. I regressed the surpluses generated by all buyers and the two big sellers – which is a measure of efficiency[20] – on time, with dummy variables for the treatment and the period. The regression shows that time is a significant factor in predicting efficiency. Taking a longer time to reach deals generates higher efficiencies, yet in many sessions of the experiment subjects rushed to make unsatisfactory deals and then repeated the same mistake in later trading periods.

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[19] An alternative explanation – which casts even greater doubt on the results – is that the students in Treatments 6, 7 and 8 did not take the experiment very seriously.

[20] The surplus – which is the sum of the profits generated by both parties to a contract – is equal to the buyer's willingness to pay plus the seller's third-party payments. For simplicity, I did not include the two small sellers in the regression. The results would not differ if I included them because the two large sellers dominate the efficiency measures. The results of the regression are summarized in Appendix 2.

46. Whatever the relationship of time limits to economic performance in the experiment turns out to be, the point I am making is that time is a significant variable in explaining these results and we would not expect it to be so central to the ability to reach agreements in the real world. Hence, there is no reason to expect the bargaining inefficiencies observed in the experiment to occur in the real world cable industry.

47. I also considered the relationship between the time limits and a feature of this experiment that is unique to experimental economics: Both buyers and sellers have fixed costs that must be met. Fixed costs are not present in most auction and market experiments. In some experiments, one side (*e.g.*, producers) has fixed costs and the other side (consumers) does not. To my knowledge, however, there have never been experiments in which fixed costs are present on both sides of the market. For a subject trying to avoid losses, these fixed costs are of paramount importance. They present the subjects with a hierarchical set of incentives. First, find a set of matches and prices that cover fixed costs, and then try to make money.

48. The presence of these two factors – the need to cover fixed costs and the time limits for negotiations – leads to a conjecture that the subjects' behavior is more consistent with *loss avoidance* than with *profit maximization*. For example, profit maximization is consistent with bargaining hard and long for every dollar of surplus. Loss avoidance, on the other hand, is consistent with making sure that no losses occur. This might be achieved by finding a set of partners to make matches with that result in no loss, selecting a no-loss price, and then making deals with them as fast as possible before they change their mind.

49. This pattern of loss avoidance is observed in the data. For example, Appendix 3 shows how often a particular buyer or seller lost money in the first four rounds of negotiations

and in the last four rounds. Losses were far more common in the early rounds. It would appear that these losses were eliminated by subjects simply finding matching partners, arriving at a price that covered fixed costs, and then repeatedly making that match at about the same price in period after period. Appendix 4 analyzes the prices that buyers negotiated with the two big sellers (#3 and #4). Note that the standard deviation of prices is dramatically smaller in the last four periods than in the first four. Again, this suggests that buyers and sellers simply stopped negotiating and made the same matches repeatedly at approximately the same prices. This is precisely behavior I would consider to be loss avoiding rather than profit maximizing, given the low efficiencies realized in almost all sessions.[21] If subjects truly were motivated to maximize their profits, we would expect them to search out better deals.

50. When evaluating the efficiency results in the Working Paper, a key question is whether the subjects in the experiment viewed their task in the same (isomorphic) manner as MVPD operators and programmers. The answer has to be a resounding no. In the experiment, the subjects had a time constraint; this factor, combined with the fixed cost element, led to behavior consistent with loss avoidance. In the real world, MVPD operators and programmers face no comparable time limit, and they undoubtedly behave in a manner consistent with profit maximization. In the experiment, no buyer can gain or lose market share regardless of what contracts are negotiated. So the only concern of a subject is whether he makes the correct matches or enough of them. These tradeoffs and concerns have nothing to do whatsoever with the cable television market. The experiment presents an interesting first step in testing a new

[21] The fact that efficiency levels were low is important to this conclusion. I would also expect low price variances once a competitive equilibrium was reached, but that should only be the case once all unexploited gains from trade had been extracted.

market institution that may or may not turn out to be a useful matching mechanism, but it is not related in any manner to bargaining efficiencies in the cable industry.

51. For all of these reasons, I conclude that the experiment differs in so many ways from the actual marketplace it is supposed to mimic that no conclusions should be drawn by policy-makers from the efficiency results.

D. Robustness of the Efficiency Results

52. Even if the market institution in the experiment did resemble the actual MVPD marketplace, the efficiency results are not sufficiently robust to even consider drawing any conclusions for policy-making purposes. There was very little data. The "modest reduction in 'economic efficiency'" observed in the High/High scenario was based on the performance in just four experimental sessions.[22] Furthermore, the average efficiency level was seriously distorted by the poor performance of the students in just one session. The actual data points are listed below:

Average Economic Efficiency Observed in Each Session

Session #	Low/High	High/High
#1	95.0%	94.1%
#2	92.7%	90.0%
#3	91.3%	67.6%
#4	85.8%	83.3%
#5	99.9%	[Results thrown out]

[22] Working Paper, p. 15. There were actually five "sessions involving the "High/High" scenario, but the results of one session had to be thrown out because of a programming error. *Id.* at 14 n.32.

53. Session #3 of the High/High treatment was clearly an outlier; in the other sessions, the results match up closely. For whatever reason, the students in Session #3 behaved very differently from everyone else. Perhaps they misunderstood the instructions, or did not take the experiment seriously, or were simply inept. (It only takes one or two poorly performing students to ruin a negotiating session.) If this data point is disregarded, then the average economic efficiency for the High/High Scenario would be 89.0%, not 83.6%. This is exactly the same average economic efficiency that the Working Paper reported for the High/Low scenario – a fact that is noteworthy because "[t]he difference in the efficiency levels observed in the High/Low [89%] and Low/High [93%] treatments is not statistically significant at standard levels of acceptance."[23]

54. 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 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 – no longer an outlier), and there would no longer have been a statistically significant difference in efficiency levels related to buyer concentration. Obviously, it would be foolish to reach any policy conclusions because of the results of one negotiation between Seller #4 and Buyer #7 in Period #8.

55. Although the Working Paper found the "modest reduction" in efficiency to be "statistically significant at standard levels of acceptance," that conclusion is questionable. The authors relied on the p-value calculated with the Wilcoxon-Mann-Whitney test. Some analysts

[23] Working Paper, Result 1, p. 27.

believe that the p-value must be less than 5 percent to be significant; others apply a less stringent 10 percent threshold. Even under the less demanding test, the p-value here was a borderline 9.52 percent. This is, at best, a weak result based on an extremely small sample.

56. Indeed, if one drills more deeply into the data, there is further reason to question the significance of the results. The second lowest average involving the High/High scenario was 83.3% in Session #4. This result, in turn, appears to have been distorted by one particular 6-minute trading round that produced an efficiency of only 37.7%. For some reason, the students (who performed quite well during the other rounds) did extremely poorly this time. If that one 6-minute trading period were disregarded, the average economic efficiency for this session would have been 98.9%, not 83.3%.[24] And if that result were used, then the High/High structure would actually have produced the *most* efficient average outcome, not the least efficient.

E. Absence of an Underlying Theory

57. It would be especially unwise to use these results as a guide to policy because they are not accompanied by any economic rationale. What is the link between (1) an increase in buyer concentration and (2) the failure to enter into contracts that would be profitable for both parties? The Working Paper does not identify any economic theory that would predict this result, and I am not aware of one. In the real world, where the parties have good information about the market in which they do business and a strong economic motive to bargain efficiently, I would expect MVPDs and programmers to reach agreements when it is possible to do so on terms that yield profits for both. Although bargaining failures can occur, I do not know of any basis in economic theory to expect bargaining efficiency to decline as a market becomes more

concentrated. Even in the market institution designed by BKS, with the severe limitations on information, I do not know of any basis in economic theory to expect such a relationship. When parties lack information, they are more likely to misjudge their adversary's position and thus fail to reach efficient agreements. But in this experiment, the information was identical under all market structures.

58. It might be suggested that a very large buyer would tend to be especially aggressive in negotiations, and this aggressiveness might result in more bargaining failures. However plausible this hypothesis might be about bargaining generally, it cannot explain these particular results. On the contrary, the Working Paper found that the larger buyers did *not* have greater bargaining power or negotiate better terms. Indeed, 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.

59. The mystery is even greater when one analyzes the biggest bargaining failures in the experiment. Consider High/High Session #3 (which, as explained above, is an outlier that drives the results claimed to be statistically significant). Three of the buyers were much less successful in negotiating contracts than their counterparts in other sessions. But these buyers were the three *smallest* cable companies – not the operator with a 51% share. If the hypothesis is that increased buyer concentration leads to reductions in efficiency, why is it that the smallest buyers are the ones acting inefficiently?

[24] During this session, the results of the last five rounds were efficiency percentages of 99.8%, 37.7%, 99.8%, 99.8% and 96.0%.

60. Therefore, even if the market institution in the experiment resembled the real world marketplace (and it does not), and even if the efficiency results were robust (and they are not), a policy-maker should not rely on these results because they are not tied to any underlying theory. When experimental results agree with theory, it gives the policy-maker greater confidence that the theory is correct. On the other hand, where (as here) an experiment produces results that are not predicted by theory, further study may be warranted – but policy conclusions are not.

V. DBS CONCLUSIONS

61. The BKS experiment yields an assortment of results regarding the buyer who is supposed to represent a DBS operator rather than a cable company. In the limited capacity scenario *without* MFN agreements, "the DBS operator's bargaining power is higher in the Low/High concentration sessions than in the High/Low concentration treatments."[25] According to BKS, "[t]his result suggests that higher concentration levels would negatively impact the DBS operator's bargaining position."[26] Yet the opposite pattern was observed in the limited capacity scenario *with* MFN agreements. Here, the DBS operator exhibited the most bargaining power when the market was most concentrated.[27]

62. None of these results are robust. They are based on the performance of just *five people* in each treatment. In other words, the BKS conclusion is based on the fact that, *on average*, the five subjects representing the DBS operator in the High/Low sessions exercised less

[25] Result 8, p. 34.

[26] *Id.*

[27] In this scenario, the DBS operator's bargaining power was 64.6% with the High/High structure, 50.1% with Low/High, and 44.2% with High/Low. *Id.* BKS could not test those results for statistical significance. *Id.*

bargaining power than their five counterparts in the Low/High sessions. But, in fact, the two DBS subjects who were the *most* successful bargainers in this scenario were in the High/Low sessions; however, the average was lower because the two least successful bargainers were also in the High/Low sessions. 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. It is far more likely that the results reflect the bargaining skills of the particular individuals participating in the experiment.

63. I have a more fundamental problem with the DBS results. The Working Paper never explains why one particular buyer was characterized as a "DBS operator." The students participating in the study did not know that Buyer #9 was supposed to represent a DBS operator. In fact, they were never told what product they were buying and selling, or what kinds of buyers and sellers they were; all they knew is that the experiment involved "fictitious assets." Moreover, the information that the subjects received does not appear to capture the characteristics that distinguish DBS from cable operators. Consider, for example, the difference between Buyer #7 and Buyer #9 in the Low/High scenario. (This is the scenario that gave rise to the results claimed to be statistically significant.) These two buyers – one of whom is supposed to represent a DBS operator and the other a cable company – had exactly the same number of customers. How did they differ? The buyers were given two pieces of information. First, they were told their fixed costs: For Buyer #7, the fixed costs were $434; for Buyer #9, they were $339. Do these figures accurately depict the relative levels of fixed costs of cable and

DBS operators with equal numbers of customers? The Working Paper does not explain the derivation of these cost assumptions.[28]

64. Even more troubling is the second set of input values given to the buyers. Each of them is told the resale value of the "fictitious asset" offered by each seller. Buyers #7 and #9 were given the following resale values:

Resale Value of the Asset Sold by Each Seller

Seller #	Resale Value to Buyer #7 (cable)	Resale Value to Buyer #9 (DBS)
#1	121	143
#2	126	134
#3	600	627
#4	956	717

65. These numbers make no sense. They indicate that the DBS operator can get a slightly higher price than the cable company for the programming offered by Sellers #1, #2 and #3, but a much lower price for the programming of Seller #4. Why would that be? Once again, the authors do not justify these figures by showing that they correspond to the relative revenue streams available to DBS and cable operators.

[28] Actually, although the students were told that these amounts were fixed costs, that is not what they really depicted. "The analysis assumed that a vast majority of the buyer's costs were already covered by an existing flow of revenue." The so-called fixed costs therefore "represent the costs the buyer must cover through its trades included in the experiments." Working Paper, p. 11. From a theoretical standpoint, it may be useful to inform subjects of the amount of (what I will call) their "uncovered costs." But it seems unlikely that BKS has any information about the relative level of uncovered costs for cable and DBS operators; they certainly have not cited any. Yet the Working Paper relies on these relative amounts to distinguish "cable" from "DBS" operators.

66. Finally, as we shall see in our discussion of bargaining power, although we may very well be able to supply a theory of bargaining power in these experiments that may shed some light on the DBS results, it is clear from the data itself that these factors were swamped by the influence of the personalities of the few subjects in the experiment who played these roles. For example, consider the actual profits earned by the subjects playing the role of Buyers #7 and #9 in BKS's Low/High sessions:

Average Profits Earned by Buyers #7 and #9

Session #	Profits Earned by Buyer #7 (Cable)	Profits Earned by Buyer #9 (DBS)
#1	641	823
#2	999	1022
#3	531	939
#4	499	316
#5	1,209	1,305
Average	776	847

67. What is immediately striking about these results is the enormous variation in the profits earned by the five students acting as the "DBS operator" and by the five students acting as "cable operators." The five subjects who are supposed to represent a cable company earned profits ranging from $499 to $1,209. The five subjects who are supposed to represent a DBS operator earned profits ranging from $316 to $1,305. 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." 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.

68. Finally, it is noteworthy that the "DBS operator" actually earned more profits, on average, that the "cable operator" 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. In short, the Working Paper does not include any results about "DBS" operators that should guide policy-makers.

VI. THE MOST-FAVORED-NATION RESULTS

69. My discussion of the MFN conclusions will be brief because the Working Paper itself recognizes that little stock should be placed in those results. The Working Paper states: "The MFN sessions generate similar efficiency levels to the No MFN sessions in the more concentrated treatments, and lower efficiency levels in the low concentrated treatment." However, the Working Paper acknowledges that there was too little data to test the results for statistical significance.[29] In fact, there were only *two sessions* for each MFN treatment. BKS also conducted regression analyses based on the results of those two sessions. The regression results are reported in the Working Paper, but because the data did not support the normality assumption, "[t]his outcome weakens the reliability of the statistical tests."[30]

70. Not only are the MFN results lacking in statistical significance, but the performance of the subjects was distinctly peculiar in ways that have no apparent relationship to most-favored-nations agreements. For example, as Appendix 1 demonstrates graphically, in the MFN sessions (Treatments 6, 7 and 8), the subjects negotiated their agreements with extraordinary speed. In Treatment 6, approximately 90% of all trades were made in the first 30

[29] *Id.*, Result 3, p. 28.

[30] *Id.*, p. 41 & n.63.

seconds; in fact, practically all were made in the first 10 seconds.[31] Yet these were not good trades. Sellers #1 and #2 lost money in all rounds, and the other sellers also incurred losses in most cases. Hence, the sellers in this experiment behaved in an extremely odd manner. They reached agreement quickly and suffered losses throughout the experiment yet never took steps to avoid these losses.

71. The price data are even more unsettling. For instance, Buyers #8 and #9, who have guaranteed resale values in the hundreds or thousands of dollars, were consistently able to buy the products for less than ten dollars. Further, the variance in these prices over the last four round is extremely small. It appears as if the sellers simply settled for low prices and either thought that was all they could get or simply gave up. Finally, there is no significant difference in the prices received by sellers in their trades with the various buyers despite their drastically different willingnesses to pay.

72. In sum, the BKS results do not offer any policy guidance whatsoever about the effect of MFN agreements contracts between MVPDs and programmers.

VII. THE BARGAINING POWER RESULTS

73. The bargaining power results are the most interesting aspect of the Working Paper. In this area, unlike the others, the experiment seems designed to test specific predictions about the operation of the actual marketplace, and it produced results that are remarkably consistent. In this section, I will first describe what I will call the "industrial organization" theories that the experiment was designed to test; then I will summarize the results of the

[31] The data reported here are from the last four periods of each sessions. As noted previously, these are the only sessions that the Working Paper analyzed.

experiment; next I will explain the pertinent principles of game theory bearing on these bargaining power issues; and finally I will evaluate the experiment both from the standpoint of the industrial organization theories being tested.

A. The Industrial Organization Theories

74. I understand that one issue raised in these proceedings is whether an increase in the market concentration or market share of cable operators will give them more bargaining power when negotiating affiliate fees with programming networks. One view, which has been explained most fully by Janusz Ordover, is that bargaining power should not be affected because (among other reasons) programming is not a "rivalrous" asset. A programmer can provide its content to an unlimited number of buyers, and its costs do not increase appreciably when it makes additional sales.[32] Professor Ordover has also identified various aspects of the market that would constrain the ability of a large cable operator to exercise bargaining power (if, indeed, it had such power in the first place).

75. A second, alternative view is that the cable industry is like other markets in which larger buyers are thought to have more bargaining power. Under this theory, at some point a buyer's share might be so large that it could exercise monopsony power.

B. The Experimental Results

76. The BKS experiment yielded a number of results consistent with the hypothesis that increases in market concentration or market share would not result in increases in the cable operators' buying power. The authors found:

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[32] Declaration of Janusz A. Ordover on Behalf of AT&T Corporation ¶¶ 67-72, filed in CS Docket Nos. 98-82 & 96-85 and MM Docket Nos. 92-264, 94-150, 92-151 & 87-154.

- The Average Buyer's Bargaining power is not related to the level of horizontal concentration among cable and DBS operators.[33]
- There is no statistically significant difference in the bargaining power of a cable operator with a 51% share and an operator with a 27% share.[34]
- There is no statistically significant difference in the Average Buyer's Surplus (a different measure of bargaining power) across concentration levels.[35]

C. Bargaining Power in Game Theory and Experimental Economics

77. Bargaining power is a topic that has consumed the attention of game theorists for many years. According to the Nash Bargaining Theory, the bargaining power of an economic actor in a bilateral negotiation is affected by two things: (1) his *status quo* payoff (*i.e.*, the payoff he would receive if he failed to reach an agreement with his bargaining partner), and (2) his share of the joint payoff received if a bargain is struck. In other words, we should expect a bargainer to be weaker than his opponent if he would suffer relatively more from a failure to agree. Given that *status quo* payoff, however, we would expect that same bargainer to receive relatively more of the joint gains from trade if his incremental contribution was greater. This theory is predicated on perfect information, however. It assumes that all of the *status quo* and joint payoffs are known to all of the players. Still, the principle is clear. You are relatively stronger in a bargaining situation if you are relatively protected when trade does not occur, and vulnerable otherwise. You also gain strength if you are able to make a substantial contribution to the mutual gains from trade available.

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[33] Working Paper, Result 6, p. 32.

[34] *Id.*, Result 7, p. 33. This comparison was between the biggest buyer in the High/High Scenario and the biggest buyer in the Low/High scenario.

[35] *Id.*, Result 9, p. 35. Again, this result refers to the limited-capacity, No MFN scenario. The figures are very close in the other scenarios as well.

78. When there are many agents with whom one can make deals, these same principles apply, albeit in a more complicated manner. For example, since different bargaining partners may serve as substitutes for each other in making deals, the market modifies these simple rules slightly.

D. The Design of the Experiment: Industrial Organization Theories

79. I examined the experimental design to determine whether it actually tested the two industrial organization theories. In other words, (1) was the experiment structured so that the product was non-rivalrous, and (2) were the participants given information and incentives so that the largest buyer was in a stronger bargaining position when it had a higher market share?

80. The first question is easy to answer. The experiment was structured so that programming was a non-depleting, non-rivalrous product. Sellers were permitted to sell their assets to all buyers, and their costs did not increase when they made additional sales. Indeed, the sellers were specifically instructed that "[t]he asset is 'non-depletable' in that its sale to one buyer does not diminish the amount of the assets available for sale to another buyer."[36]

81. To answer the second question, we must examine the information that the subjects were given and the incentives created thereby. The sellers were told how many customers each buyer had; thus, they had complete information about the market share of the buyers. In order to test the effects of market concentration on bargaining power, the experiment considers three alternative market structures in which the largest buyer had shares ranging from 27% to 51%. The experiment was designed so that sellers in the more concentrated market have a greater need to negotiate a contract with the biggest buyer. A way to examine this point is to consider the

[36] Working Paper, pp. 75, 80, 88, 93, 104, 108.

seller's situation if he succeeds in negotiating a contract with all the buyers except for the biggest. The seller is assigned a certain level of fixed costs, which he must recover before he can begin to earn a profit. The seller knows what third-party payments he will receive from a contract with each buyer. The following table shows the third-party payments that the seller will receive if he has a contract with all the buyers other than the biggest, expressed as a percentage of the fixed costs that must be recovered before the seller can break even:

Third-Party Payments as a Percentage of Fixed Costs Without a Sale to the Biggest Cable Operator

Seller	Low/High	High/Low	High/High
#1	50%	38%	32%
#2	48%	38%	32%
#3	72%	54%	47%
#4	116%	87%	75%
Biggest seller's share	24%	44%	51%

82. It is important to recognize that any seller can calculate his particular percentage from the information he receives. He can readily determine the extent to which the third-party payments will cover his fixed costs. This table shows that, as the share of the biggest buyer increases, sellers are less likely to make a profit if they fail to enter into a contract with the largest buyer. In this sense, the experiment puts the largest buyer in a stronger position when its market share is larger, and therefore tests whether that stronger position actually results in the exercise of more bargaining power.

83. To sum up, 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. I therefore conclude that the experiment gives the subjects incentives that tested the second hypothesis as well as the first. If the "non-rivalrous" hypothesis is correct, then the experiment should show no relationship between concentration and bargaining power. If the alternative hypothesis is correct, then the experiment should result in sellers having more bargaining power (other things being equal) when the largest buyer has a smaller share.

84. As explained earlier, a weakness in the experiment is that the subjects were given very little information that would enable them to evaluate how well they had negotiated; they had no way to judge how much money they were "leaving on the table." The absence of information can lead to inefficient behavior, such as the failure to reach agreement on a mutually-profitable price, and for this reason and others I have discounted results cited in the Working Paper about bargaining failures. In this section, however, I am discussing only the results pertaining to bargaining successes (*i.e.*, contracts actually negotiated), and I am only examining the differences in the results among various assumed market structures. For this particular purpose, the experiment can provide insight that may be useful to policy-makers.

85. In sum, the Working Paper provides experimental support for the conclusions of Professor Ordover and others, which were based on principles of industrial organization, that the bargaining power of cable operators is not affected by increases in cable ownership concentration or market share. In this regard, it should be noted that because of the many simplifying assumptions in the experiment, it did *not* reflect a number of characteristics in the real market cited by Professor Ordover that would tend to constrain the ability of a large cable operator to exercise bargaining power (if, indeed, it had such power in the first place). An 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). Likewise, the experiment did not reflect the fact that a pivotal buyer cannot demand too low a price or else the network may not be able to afford to produce the programming; that a large buyer is less able than a small buyer to extract lower prices with hold-up or free-rider tactics; and that buyers cannot afford to be without "must-see" programming, thereby giving sellers the ability to bundle those channels with less desirable channels. In citing these factors, it is not my intention to comment on how important they are in the actual marketplace. My point is that, if the idealized experiments in the Working Paper failed to reveal any increase in bargaining power due to increased concentration, *without* factoring in the characteristics that would constrain the exercise of bargaining power, then it is even less likely that an increase in concentration in the real market would lead to an increase in the bargaining power of cable operators.

/s/ Andrew Schotter