

July 18, 2002

Ms. Marlene H. Dortch
Secretary
Federal Communications Commission
445 12th Street, S.W.
Washington, DC 20554

Re: OPP Experimental Economics Study Examining Horizontal Concentration in the Cable Industry (CS Docket Nos. 98-82, 96-85, MM Docket Nos. 92-264, 94-150, 92-51, 87-154, MB Docket No. 02-70)

Dear Ms. Dortch:

In response to the Media Bureau's request for comment on OPP Working Paper No. 35,¹ we are submitting the attached analysis prepared for the National Cable & Telecommunications Association by Dr. Carl Shapiro, TransAmerica Professor Business Strategy, Haas School of Business, University of California at Berkeley, and Dr. John Woodbury of Charles River Associates.

While Drs. Shapiro and Woodbury are open to the use of experimental methods to help inform public policy, their analysis concludes that "the limitations of the BKS study, including the gap between their experimental market design and the real-world market for the sale of program services, are sufficiently large that one cannot reach any reliable conclusions about appropriate ownership limits for MVPDs based on [the BKS] study."

The Shapiro-Woodbury analysis makes the following critical points, among others:

- "The economic theory on which the study is based does not in fact predict that the presence of large national MVPDs will impede the flow of programming to consumers."
- "The experiment conducted fails to capture many important aspects of the real world negotiations between programming networks and cable operators."
- The "experiment did not in fact look at the factors that determine the supply of programming, but rather assumed that a fixed set of programs was available. Entry of new programs, exit by existing programs, and more generally, the elasticity of supply of programming were not part of the experimental setup."

¹ "Horizontal Concentration in the Cable Industry: An Experimental Analysis," prepared by Mark M. Bykowsky, Anthony M. Kwasnica and William Sharkey ("BKS study").

- “The experiment ... was improperly implemented, extremely complex, and generated only a small number of observations, making reliable inferences impossible.”
- “Even if one ignores all of these problems, the actual results reported in the BKS paper do not provide clear support for the proposition that the presence of large national MVPDs has an adverse effect on the supply of programming.”

In sum, Drs. Shapiro and Woodbury conclude that “the issues here are sufficiently complex, and the study in question sufficiently flawed, that we strongly urge the FCC not to use this study as the basis for any rulemaking.”

Respectfully submitted,

/s/ **Daniel L. Brenner**

Daniel L. Brenner

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Cable Television Subscriber Limits: A Critique[†]

Carl Shapiro* and John Woodbury**

July 18, 2002

1. Introduction and Summary

How is the supply of programming, the health of cable systems, and ultimately the welfare of cable consumers affected by the national ownership structure of cable operators? Is there a reliable economic basis on which to impose rules limiting the number of subscribers that a single cable company can serve nationally?

We argue here that there is no sound basis in economic theory for imposing such “subscriber caps” on cable operators. We then devote the bulk of our analysis to a critique of a recent working paper released by the Office of Plans and Policy of the Federal Communications Commission (FCC), “Horizontal Concentration in the Cable Television Industry: An Experimental Analysis,” by Mark M. Bykowsky, Anthony M. Kwasnica, and William W. Sharkey (“BKS”).¹ The BKS paper uses experimental methods in an attempt to shed light on how the national structure of cable ownership affects the flow of programming to consumers.²

[†] This paper was prepared on behalf of the National Cable & Telecommunications Association.

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¹ Federal Communications Commission, Office of Plans and Policy, OPP Working Paper #35, June 2002. A revised version of this paper was released by the FCC on July 3, 2002. The changes from the original paper do not affect our critique.

² The use by BKS of the phrase “horizontal concentration” as applied to different cable operators may be misleading. In conventional antitrust terms, “horizontal concentration” usually refers to a measure of market concentration among rivals who compete to either buy inputs or sell outputs. In this case, there is no direct competition among cable operators for programming (since a sale of a program service to one cable operator does not preclude the sale of the same program to another cable operator) or for subscribers (since typically each cable operator serves a franchise area that does not overlap with the franchise areas served by other cable operators). Thus, we shall speak of “subscriber limits” or “the national structure of cable ownership” rather than “horizontal concentration.”

Based on a careful review of the BKS paper, we firmly believe that it does not provide a sound or reliable basis for limiting the national share of subscribers that can be accounted for by a single multi-channel video program distributor (MVPD). While we welcome the use of experimental methods to help inform public policy, the issues here are sufficiently complex, and the study in question sufficiently flawed, that we strongly urge the FCC not to use this study as the basis for any rulemaking. The basic shortcomings of the BKS paper are the following:

- The economic theory on which the study is based does not in fact predict that the presence of large national MVPDs will impede the flow of programming to consumers.
- The experiment conducted fails to capture many important aspects of the real world negotiations between programming networks and cable operators, including the ownership interests that cable operators have in programming networks.
- The BKS experiment did not in fact look at the factors that determine the supply of programming, but rather assumed that a fixed set of programs was available. Entry of new programs, exit by existing programs, and more generally, the elasticity of supply of programming were not part of the experimental setup.
- The experiment conducted by BKS was improperly implemented, extremely complex, and generated only a small number of observations, making reliable inferences impossible.
- Even if one ignores all of these problems, the actual results reported in the BKS paper do not provide clear support for the proposition that the presence of large national MVPDs has an adverse effect on the supply of programming.

In the end, the limitations of the BKS study, including the gap between their experimental market design and the real-world market for the sale of program services, are sufficiently large that one cannot reach any reliable conclusions about appropriate ownership limits for MVPDs based on this study.

The organization of this paper is as follows. Section 2 discusses the economic principles pertinent to the relevant policy question, namely the impact of national subscriber caps on the supply of programming. Section 3 identifies some of the more important gaps between the experimental design and the real-world transactions between MVPDs and program services. In Section 4 we explain why the BKS experiment fails to provide a reliable basis for FCC

rulemaking, even if the experimental design were considered a decent match with reality. Section 5 describes some unusual outcomes that likely reflect the lack of reliability of the results or flaws in the experimental design. Section 6 summarizes our conclusions. Throughout, we assume that readers of this paper are already familiar with the BKS study; a short summary of their experiment is provided in the Appendix to this paper.

2. The Effects of National Subscriber Caps: Economic Principles

BKS conducted a laboratory experiment to evaluate the extent to which different ownership structures in the cable industry could adversely affect the flow of programming to final consumers (i.e., MVPD subscribers). However, the experiment itself deals only with existing program services and thus cannot be used directly to infer the effect of different ownership structures on the flow of programming to MVPDs. Importantly, there is no compelling reason in economic theory to predict an adverse effect of larger MVPDs on the availability of program services to consumers.

Negotiations over carriage between programming networks and cable operators are a classic example of bilateral bargaining: the content supplier has a unique property to offer, and the cable operator has the unique ability to reach many of its subscribers. As a general principle in bilateral bargaining, one party's bargaining power is diminished if the other party can easily turn to close substitutes. Here, the bargaining power of the programming network depends crucially on the quality (ability to attract subscribers) of its programming in comparison with the most attractive programming not carried by the cable operator. Similarly, the bargaining power of a cable operator depends upon the presence or absence of substitute methods (such as DBS) by which a programmer can reach the cable operator's subscribers.

Basic economic theory teaches us that the national size of a cable operator does *not* in and of itself govern its bargaining power with programmers, unless the larger MVPD size generates additional benefits for the program services. While a larger MVPD controls access to more subscribers, large MVPDs need not be in a stronger position than are smaller MVPDs when negotiating *per subscriber* affiliate fees with content providers. Put differently, while large MVPDs certainly have more subscribers to offer programmers, there is no clear basis to expect

them to extract a larger *fraction* of the surplus generated when the programming reaches their customers, and that is what “bargaining power” is all about.

To the extent that large MVPDs in fact negotiate more favorable terms with programming networks, such price differences may reflect cost differences (negotiating and dealing with larger MVPDs is less costly to the content provider than negotiating and dealing with many smaller, more localized, MVPDs) or quality differences (larger MVPDs can more efficiently deliver broad national *reach* to advertisers than can smaller MVPDs).

Furthermore, there are good reasons to question whether a large national MVPD would actually exert *monopsony power*, even if it enjoyed greater bargaining power than smaller MVPDs.

Monopsony power with respect to an input refers to the ability of a buyer profitably to reduce the price it pays for an input by restricting the quantity of the input purchased (and thus produced). Traditionally, monopsony power has been analyzed in the context of a single buyer setting the single price at which it will buy a homogeneous good. This is clearly not the situation for cable operators negotiating with program suppliers; the economic analysis of programming supply is very different from the classic monopsony situation.

First, classic monopsony power applies in a situation where the supply of a homogeneous input is upward sloping and the monopsonist cannot negotiate a different price for each different input provider. In fact, the supply curve of programming confronting a cable monopsonist is likely to be quite elastic. This is because the underlying inputs used for program services—actors, directors, writers, etc.—are also used for the production of many other services, including television (network, syndicated and local) programs, movies, and theatrical plays. The programming services used by the cable industry likely account for a relatively small fraction of these inputs. As a result, the supply curve as it might appear to a hypothetical national cable “monopsonist” would be quite flat (i.e., elastic), implying that the MVPD has little or no monopsony power to exert.³

³ Indeed, the expansion in the number of program services by 98% percent over the past five years suggests a highly elastic supply of inputs to the program services industry.

Second, even if there were a cable monopsonist who faced an upward sloping supply of program services, that cable monopsonist would have some ability to offer each program service a different price, one that reflects the costs of producing that programming (including a risk-adjusted return to the programming investment). With highly differentiated programming, and bilateral negotiations, the assumption of a single input price that underlies the classic monopsony analysis simply does not apply. Consequently, this monopsonist would have no need (or incentive) to restrict the number of programming services purchased so as to reduce the price of all services.⁴

Even if larger MVPDs were able to negotiate lower affiliate fees in ways that did not reflect lower costs to programmers or greater ancillary (advertising) revenues to programmers, it is far from clear that such discounted affiliate fees would harm consumers or lead to any meaningful reduction in the supply of programming. First, as recognized by BKS but unaccounted for in the experiment, lower affiliate fees represent lower variable costs for cable operators, which tend to lead to lower cable television prices for consumers, a clear consumer benefit. Second, while lower affiliate fees could in principle reduce the supply of programming over time, any such effect would be confined to programming that is already marginal, would depend upon the elasticity of supply of programming, and would tend to be muted by the vertical integration that exists between content providers and MVPDs. At the very least, any such adverse long-term effect on programming should be balanced against the more direct benefits to consumers associated with lower affiliate fees.

Finally, and secondarily, larger cable operators may help overcome free rider and hold-up problems that could adversely affect the supply of programming. Larger cable operators are more likely to internalize any impact of their negotiated affiliate fees on the overall supply of programming, and are more likely to make investments in programming that enhance the supply of programming.

⁴ We would tend to attribute any bargaining inefficiency between cable operators and programming networks more to asymmetric information rather to the bargaining power of one party or the other, much less the overall size of the cable operator.

In summary, in contrast to the underlying premise of the BKS experiment, these arguments all suggest that the presence of larger cable operators may well *foster* rather than impede the flow of programming to MVPDs.⁵

3. The Gap Between the BKS Experiment and the Real World

The “virtual reality” simulated in the BKS experimental environment is far removed from the real-world transactions between MVPDs and program services. While some simplifications are always necessary to build models, and to conduct experiments, we believe that the gap between reality and the BKS experiment is great enough to seriously undermine the reliability of their experimental findings for the purposes of informing public policy. We highlight here some of the more serious differences between the BKS experiment and the real world. We also discuss the failure of BKS to provide the necessary detail regarding the experiment’s “calibration” – the connection between the benefits and costs assigned to participants in the experiment and real world revenue and cost data – a failure that does not permit a complete evaluation of the size of that gap.

A. The Obvious Gaps Between the BKS Experiment and the Real World

First and foremost, the BKS experiment does not account for the ownership interests that some of the larger MVPDs hold in many program services.⁶ Such vertical integration (total or partial) may well promote the supply of programming, e.g., by providing capital and by reducing hold-up problems when programmers negotiate after having already sunk significant costs in the

⁵ Alexander Raskovich, “Pivotal Buyers and Bargaining Position,” U.S. Department of Justice, Economic Analysis Group, EAG Discussion Paper #00-9, December 2000 (revised October 2001).

⁶ According to data in the FCC’s Eighth Annual Report on the Status of Video Competition (FCC 01-389, Appendix D), the largest MVPDs had an ownership interest in roughly 24% of the 287 nationally delivered cable programming services as of July 2001.

development of a new program. Vertical integration also can facilitate efficient bargaining, e.g., by reducing the informational asymmetries between the program supplier and the MVPD regarding the cost or quality of the programming.⁷ To the extent that larger MVPDs are more likely to engage in such pro-competitive vertical integration, the BKS work has omitted a potentially very important factor explaining why larger MVPDs promote rather than retard efficiency in the supply of programming.

A second important omission is the failure of the experiment to account for the subscriber effects of non-carriage. Basic program services—the only services that played a role in the study—are typically not available to MVPD subscribers on an *a la carte* basis. Rather, the MVPD bundles together different program services into one or more basic service program tiers. Suppose that an MVPD failed to carry a particular service that would have been carried but for an attempt by the MVPD to exercise bargaining power. As a result of this failure, the MVPD loses some subscribers. Some subscribers will choose either to forego MVPD subscribership altogether or to subscribe to another alternative, like DBS. What this subscriber loss means is that the MVPD loses not only the revenues it would have earned from carrying the service in question, but also some revenues that it would have earned from the services it continues to carry.

By contrast, in the experiment, the MVPD's willingness to pay for any particular program service is assumed to be independent of any other services carried. Accounting for the interdependence among basic services could well have resulted in the completion of more surplus-maximizing transactions.

This issue of interdependence is particularly important in a more dynamic environment. Actual transactions between MVPDs and program services depend on the history of carriage. A new service whose popularity on an MVPD becomes apparent after carriage will have much more bargaining leverage with the MVPD in the next "trading period." Under these circumstances,

⁷ The investing MVPD may have more information about the costs and revenues of the program service than was available to participants in the experiment. Consequently, "bluffing" by the program service is less likely to be effective and the inefficiency caused by bluffing – the failure of certain surplus-increasing transactions to be consummated – would tend to be smaller in the presence of vertical integration.

programming services are likely to offer discounts to gain carriage initially. By contrast, in the experiment, what happens in one trading period does not carry over to future trading periods. In that important but unrealistic sense, the trading periods are independent of each other. Of course, including the critical real-world interdependence in the experimental design may well be very difficult and may also require considerably more trading periods to allow the participants to learn how different program service combinations affect MVPD revenues.

The assumption that the number of subscribers is fixed leads to another, different kind of gap between experimental reality and real-world transactions. Transactions between MVPDs and program services are largely about the price per subscriber (not a lump sum payment, as assumed in the study), but the contract between the buyer and the seller frequently includes other dimensions such as rebates, volume discounts, promotional services, and carriage duration. Such complications in a real-world setting may in fact increase the likelihood of surplus-maximizing trades by providing other ways for (e.g.) the MVPD to compensate the supplier for program carriage. For example, in exchange for a lower per-subscriber fee, the MVPD may be willing to pay a lump sum to the program service. In this example, even if there were no effect on the profits of the program service, the MVPD would have an incentive to lower the price of its MVPD service to consumers, resulting in an increase in MVPD subscribership and an increase in the MVPD's willingness to pay for the program service. That, in turn, would increase total surplus and make the transaction more likely to be completed.

Another omitted variable that facilitates exchange is the degree and nature of communications between a MVPD and a program service. In the experiment, the only communication between the buyer and seller was in the form of a computer "message" of bid, ask, and accept. In real-world transactions, program services and MVPDs are in extensive communication with each other, and that communication likely facilitates surplus-maximizing transactions. For example, MVPDs can and do conduct surveys of subscribers to evaluate the extent to which they are willing to pay for specific services, while the service provides the MVPD with information on

the projections of how profitable the service is likely to be on that MVPD. Indeed, if a program service is new, it may be willing to pay for carriage on a large MVPD as an endorsement that encourages other MVPDs to carry that same service.

We certainly appreciate the fact that attempting to account for these and other real-world complexities may well be beyond any reasonable budget for an experiment that intends to capture the highly complex world of MVPDs and programming.⁸ The fact remains, however, that the inability of the BKS experimental design to account for these important factors that play a critical role in the real-world exchange between MVPDs and program services severely undermines the ability of their experiment to predict real-world outcomes.⁹ The shortcomings of the BKS experimental design are highly significant, since their results are driven by the inability of the participants to consummate mutually beneficial trades; with more time to learn and adjust and negotiate in a highly complex environment, and with higher stakes such as exist in the real world, there are strong reasons to believe that market efficiency would be noticeably higher in the presence of large MVPDs.

Moreover, there is at least one important facet of actual MVPD-program service transactions that cannot likely be replicated in an experimental environment with any reasonable degree of accuracy. In contrast to the few dollars that undergraduates might earn from successful participation in the experiment, those individuals who make the program purchase decisions at

⁸ Other omitted complexities include accounting for premium satellite services, such as HBO, and some of the most popular basic services, such as ESPN. It is likely that this category of “very popular” services would, if included in the analysis, increase considerably the maximum surplus that could be generated and the fraction of that surplus realized. In addition, the analysis assumes that all MVPDs have the same carriage capacity, an assumption that is substantially at odds with reality. It is not obvious how accounting for this factor would affect the results, but the fact that the paper does not account for any capacity differences again reduces the ability of policy makers to use the experimental results as a prediction of real-world outcomes.

⁹ Indeed, one observer has noted that “experimentation in economics is likely to be of limited value, save for situations—such as auctions—that exist in conditions of relative isolation and are characterized by low internal complexity.” Nikos Siakanaris, “Experimental economics under the microscope,” Cambridge Journal of Economics May 2000, p. 278.

MVPDs, and those who undertake the selling efforts at the program services, have substantial stakes in the outcomes and can develop extensive experience with these negotiations. In particular, if MVPD employees consistently fail to carry one or more services that would increase the profitability of the MVPD, those individuals would likely suffer salary and bonus losses, career setbacks, and even job losses as a result. There is a corresponding set of consequences for employees of the program services who are so aggressive in their effort to sell at a high effective price that they consistently lose profitable carriage opportunities. These kinds of stakes act to encourage the carriage of program services that are jointly profitable for the MVPD and the program service. The difference between these stakes and the much smaller stakes at risk in the experiment further reduces the ability to transfer the experimental “lessons” to the real world of public policy.¹⁰

B. The Calibration of the Experiment

The experimental behavior of the participants is defined in large measure by the way in which the experiment is “calibrated” to both MVPDs and to the program services—for example, the willingness to pay for each MVPD participant, the costs of each MVPD, and the costs and network advertising revenues of the program services. Thus, understanding the calibration process is also critical to understanding the extent to which the experiment tracks the real-world program service transactions. Yet, BKS barely provide the shadow of the necessary detail required for this understanding. As a result, BKS offers the FCC no reason to believe that even

¹⁰ Because of the homogeneity of the college population relative to the entire population, one recent study concludes that relying on college student responses may not generate results that are transferable to the population at large. The study advises that “[a]t a minimum, research results based on college students need to be replicated with nonstudent subjects prior to the generation of universal principles.” Robert A. Peterson, “On the Use of College Students in Social Science Research: Insights from a Second-Order Meta Analysis,” Journal of Consumer Research, December 2001, p. 458.

within this highly stylized environment of 5 MVPDs and 4 program services, the calibration captured real-world variation among MVPDs and program services.

For example, each buyer (MVPD) is assigned a willingness to pay for each program service that is based on a number of factors and apparently varies by MVPD. This value plays a key role in the behavior of the buyers in their negotiations with program sellers. Yet the reader interested in testing its correspondence to the real world is unable to do so. A solitary note states that “[a] buyer’s willingness to pay for a given programming network is based upon an estimate of the additional subscriber and local advertising revenue it would obtain from carrying the programming network. An estimate of the local advertising revenue was based on estimates of the programming network’s local audience ratings, CPM prices, and the number of local avails.”¹¹ BKS provides no information on (e.g.) exactly how the revenue per subscriber per MVPD was determined, what cable systems served as the model for this calibration, how the individual real-world cable system data were averaged across the underlying cable systems to generate the willingness-to-pay value for each MVPD, how the number of local avails differed across cable systems, if at all, and whether there were any differences in each of these dimensions between cable systems and DBS.

With respect to the assigned cable system cost, BKS states that these estimates were based on 10K reports for Adelphia, Classic, Cox, Comcast, Insight, and Mediacom, and that the estimates included sales and administrative expenses but excluded programming expenses.¹² BKS fail to tell the reader (e.g.) what other costs were excluded, how advertising and promotional costs were treated, and how these costs were averaged over the underlying cable systems. In addition, it is unclear why these cable systems were chosen versus other systems, such as AT&T and AOL Time Warner—the two largest cable operators. Finally, without explanation, each cable system

¹¹ BKS, note 21, p. 10.

¹² BKS, p.11.

was assigned the same costs, despite the large variation in the size of the systems in the experiment.

As one final example, BKS uses selected program services to generate the network advertising revenues and the costs of the four stylized program services used in the experiment. Yet, there were numerous basic services (e.g., USA Network, Animal Planet, Sci-Fi Channel, Country Music Television, TNT, AMC, Fox News Channel) that were apparently not part of the sample. BKS offers no reason why these many other services were excluded from the calibration. And even for some of the services that were included in the sample, there were missing data which were replaced by BKS estimates, without any discussion of what the missing data were or how the estimates were generated.

In short, we lack the information necessary to evaluate the calibration undertaken by BKS, even if one accepts the highly simplified transactional environment they have created. Calibration can be crucial to the results of an experiment or simulation; presumably, third parties and the FCC cannot simply assume that the calibration was defensible. Without knowing the details of the calibration, we cannot completely evaluate the gap between the experimental reality and real world transactions between MVPDs and program services.

4. Reliability Issues

We turn now to discuss the reliability of the BKS results. Our main point here is that these results are not robust or reliable, even if one sets aside the theoretical and conceptual problems noted in Section 2, and even if one ignores the large gap between the experiments and reality just noted. In fact, the BKS results, taken on their own terms, do not provide clear support for subscriber caps: BKS find no difference in the bargaining power of a cable operator serving 27% of national subscribers and one serving 51% of national subscribers. But here we address more generally the lack of reliability of the BKS experiment as a whole, not just that one experimental result.

A. Complexity

The BKS experiment clearly is a very complicated bidding exercise. Within any trading period, each buyer is attempting to conduct trades with some or all of the 4 sellers within a 6 minute period, keeping at least implicit track of how his/her profits are changing with each bid and ask with each seller, including accounting (generally) for the third party payments to each seller if the buyer agrees to carry the seller.¹³ And between trading periods, the buyers had “some time” to calculate their total trading period profits, which could inform them of strategies to be pursued in the next trading period. The task for sellers was even more daunting because they had to reckon the third party payments into their profit calculus.

The authors of the study at least implicitly acknowledge its complexity. Not only were participants tested on the instructions, they all engaged in a “practice” trading period. Moreover, despite the test and the practice trading period, the results reported in the study are based only on trading periods 5-8, “because participants may require a few trading periods to become fully accustomed to the experimental environment.”¹⁴

Given this complexity, it is important to both ensure the clarity and consistency of the instructions to the participants and to test how sensitive the results are to the choices made by the experimenters. If the instructions to the participants are unclear or if experimental outcomes are very sensitive to the design choices, then any reliance on the experimental outcomes for policy-formulation purposes is likely misplaced.

B. Implementation

Unfortunately, some of the instructions to the participants seem confusing at best, which casts doubt on the entire endeavor. In addition, the experimenters did not undertake any analysis of the sensitivity of their results to key design choices. As a result, it is possible—and perhaps even

¹³ Buyers did not know how much the third party payment might be, only that it was larger for the more popular networks. Note that for the limited capacity, no MFN treatments, the instructions indicate that the sellers were told that each trading period lasts 6 minutes (BKS, p. 113) while the sellers were told that each trading period lasts 5 minutes (BKS, p.105). It is not obvious whether this is a typographical error, a design feature, or a mistake that may or may not have been rectified.

¹⁴ BKS, p. 27.

likely—that the reported results are very sensitive to the structure of the experiment.¹⁵ Here, we simply highlight some of the difficulties participants may have had in interpreting the instructions and some experimental design dimensions that might have a particularly important effect on the reliability of the results.

1. Instructional Inconsistencies

With respect to the instructions provided to the participants, there are numerous inconsistencies that make comparisons across some of the treatments difficult if not impossible. For example, buyers in the unlimited capacity, no MFN treatments are told that “Buyers earn money by purchasing assets at prices below their assigned resale value that exceed their fixed costs.”¹⁶ It is not clear how the buyers would have interpreted this instruction. In particular, they might have assumed that unless the purchase of each asset satisfied this requirement, they would not earn any money. As a result, some or many buyers may have been willing to forego the purchase of a seller’s service to avoid earning a loss on any one of its assets. Instead, they should have been told that they earn money when the *sum* of their earnings from the asset purchases exceeds the fixed costs. Indeed, perhaps as a recognition of this potential source of confusion, this *is* the instruction that is given to the buyers in the limited capacity, MFN and no MFN treatments.¹⁷

As another example, buyers are provided with illustrations to assist them in calculating their earnings using lump sum payments to sellers.¹⁸ In some treatments, the seller earnings calculation is similarly described in lump-sum terms.¹⁹ However, in the limited capacity, no MFN treatment, sellers are instructed to begin their calculations with a per-subscriber payment

¹⁵ One noted economic theorist has recently made a similar observation: “The small print of any experiment is important. Minor differences in the wording of an experiment may be crucial. The method of selecting data which is reported in a paper may affect the conclusion. Given the relatively small samples we use, even minor mistakes [made by a research assistant or the experimenters themselves] may have a critical effect on the conclusion.” Ariel Rubinstein, “A theorist’s view of experiments,” European Economic Review, May 2001, p. 625.

¹⁶ BKS, p. 74.

¹⁷ BKS, p. 87.

¹⁸ BKS, p. 103.

¹⁹ BKS, pp. 82-83.

from the buyer, rather than a lump sum payment from the buyer.²⁰ As a result, sellers may have attempted to keep track of how their profits change with different bids and asks in a way that was more complicated than necessary, thus increasing the room for error.

These and other kinds of instructional inconsistencies raise the possibility that the results within and across treatments are not comparable, are unreliable and/or misleading.²¹ It seems reasonable to conclude that if the instructions vary by treatments, the outcomes in different treatments will vary as a consequence.

2. Fragility of the BKS Results

Even if the instructions had been consistent (and correct) across treatments, the reliability of the results would still be in question. Perhaps most importantly, there is a general paucity of observations from which any policy implications can reliably be drawn. First, there were only 14 sessions in the limited capacity, no-MFN treatments and only two sessions for each of the other treatments.²² If this were a statistical exercise, one might be concerned that with only 14 observations, the results might be particularly influenced by “outliers.” We have not had the opportunity to explore the individual session results except in a cursory way, and the paper does not discuss this possible source of sensitivity. However, at least one session in this collection seems to be an outlier, generating a substantially smaller fraction of the maximum surplus than the other sessions in the same treatment.²³ As a result, more sessions may well have resulted in a different qualitative or quantitative conclusion.²⁴

²⁰ BKS, pp. 110-111.

²¹ For example, in the unlimited capacity, no MFN treatments, sellers are told that there are 5 buyers. BKS, p. 80. They are then told that there are 4 buyers, labeled 5,6,7,8,9. As another example, in the limited capacity, no MFN treatment, sellers are told that buyer 7 has a MFN. BKS, p. 109.

²² This overstates the number of observations with respect to the effect of varying ownership structures on efficiency and bargaining power in a very important way. There were only three observations, corresponding to three different ownership structures that were evaluated in the limited capacity no-MFN treatments.

²³ This is Treatment 3 (High Concentration/High Numbers), Session 021102a.xls. For example, in the last trading period in this session, the participants realized only 41% of the maximum surplus while in the last trading period in the three other sessions for this treatment, the participants realized in excess of 90% of the maximum.

²⁴ At a minimum, given the paucity of observations, the paper should have considered whether other measures of central tendency, such as the median, would have generated qualitatively or quantitatively different conclusions.

Second, the study offers generalizations from only the last four (out of eight) trading periods in every session. More trading periods within a session may have resulted in a different conclusion per session because it would have afforded the participants an opportunity to better “learn” how to assess and develop profitable strategies.²⁵ Even the last four trades within a session seem to generate a large range of outcomes.²⁶ To the extent that the participants were still learning how to negotiate, the results are likely to overestimate the inefficiencies associated with this bilateral bargaining game. Indeed, one of the most robust results from experimental economics is that markets tend to be highly efficient when participants are given time to learn to bid in classic supply and demand settings (so-called “double oral auctions”).

Third, there are effectively only two treatments that are comparable for purposes of evaluating the effects of MVPD concentration on efficiency as defined in the study. These are the two scenarios where the number of MVPDs (and therefore the number of possible buyer-seller trades) is constant.²⁷ Because only these two comparable scenarios are examined, it is at least difficult to have much confidence in interpolating between them or to extrapolate beyond them.

²⁵ Indeed, one well-known economic experimentalist has suggested that for experiments of this type, at least 15 trading periods should be part of the experimental design to allow for learning. Given the complexity of the experimental design in this study, perhaps even more than 15 would have been appropriate. See Charles A. Holt, “Industrial Organization: A Survey of Laboratory Research,” The Handbook of Experimental Economics (John H. Kagel and Alvin E. Roth (eds.)), Princeton University Press: Princeton, New Jersey, 1995.

²⁶ For example, in Treatment 1, Session 022602a, the range for the last four periods is between 60% and 99%. As another example, in Treatment 2, Session 021202.xls, the range for the last four trading periods is between 50% and 100%. As noted above, the authors might have considered whether using some other measure of central tendency, such as the median of the last four trading periods to characterize the results of a session, would have qualitatively or quantitatively altered their conclusions.

²⁷ One characterization of these two scenarios is in terms of the MVPD with the largest share. In one treatment, that share is 26.8%; in another, it is 51.2%. Another characterization is in terms of the Herfindahl-Hirschman Index (HHI) of market concentration typically used in merger analysis, although not directly applicable here because cable operators do not compete with each other. (The HHI is calculated as the sum of the squared market shares, with a HHI of 10,000 indicating a monopoly (i.e., with a share of 100).) In one scenario, the HHI is about 2100; in the other, it is about 3300. These are higher, in some cases substantially higher, than current concentration levels. According to data from Kagan World Media, Nielsen Media Research and SkyReport, the largest MVPD now is AT&T with a 14.5% subscriber share. Including Comcast, that share will be about 24%. The current HHI is only about 850. Combining the AT&T and Comcast shares increases the HHI to 1120. (These calculations do not include any systems for which these MVPDs have an attributable interest. From the perspective of antitrust analysis, it would certainly be incorrect to assume that a 5% interest in a cable system is equivalent to complete ownership of the cable system, an assumption inherent in the FCC’s attribution rules. If the experiment had been designed to account for and measure the effects of partial ownership interests, then a calculation with analogous attribution might have been more appropriate.)

In addition, while each buyer and seller were assigned unavoidable costs, those costs (because they are unavoidable) should have had no influence in the experiment on the set of strategies pursued by the participants. For example, a seller could lose money in trading period 1 and still continue to play in trading period 2, notwithstanding the losses in trading period 1. Thus, these unavoidable costs were sunk in the experiment's "short run," i.e., a trading period. But as anyone who has taught undergraduate economics knows, students do not always immediately understand that if costs are unavoidable, then those costs should not figure into the possible short-run strategies considered or adopted. If somewhat higher or somewhat lower unavoidable costs were to generate results that differed significantly and importantly from those described in the study, then that would have exposed either a design flaw or apparently "irrational" behavior (the classic "sunk cost" fallacy) by the participants.

These and other important sensitivity tests should have been an integral part of the experimental design.²⁸ That the study did not undertake these tests—whether for budgetary or other reasons—is a substantial failing. For this reason alone, the results of the study should not be relied on for policy-making purposes.

5. The Confluence of Design and Reliability Failures: Unexplained Outcomes

In the light of the problems just noted, it should not be surprising that some of the experimental results themselves are inexplicable, even to the authors of the study, while others have implications that are not consistent with actual carriage behavior. These results serve to illustrate why the study would serve as a poor policy guide in considering whether or to what extent the subscriber share of MVPDs should be limited.

The most obvious example of such an outcome is the paper's calculation of the maximum surplus attainable under the different treatments. One would have thought that when buyers are

²⁸ In terms of increasing one's confidence in the reliability of these results, it would have been helpful to gauge the sensitivity of the results to the payments received by the students for participating in the experiment, to the willingness-to-pay maximums, to the use of graduate students vs. undergraduate students, to the availability of "free" working capital (which may have encouraged risk-taking on the part of the program service participants), and to the failure of the instructions to make clear to the buyers that in the 14 limited capacity, no MFN sessions, no buyer competed with any other buyer for the right to carry a program service.

allowed to buy 4 services (rather than 3 services) in the unlimited capacity environment, the maximum surplus would increase—more services are carried so the joint profits of the MVPDs and the services should rise. Instead it falls by about 5% in the Low Concentration/High Numbers scenario and by nearly 25% in the High Concentration/High Numbers scenario, both relative to the limited capacity treatments.²⁹

According to a note, these differences are due to “small, inconsequential variations in the parameters used and the lack of a DBS buyer in [High Concentration/High Numbers] treatment. In addition, the first two of the Limited Capacity-No MFN experiments had slightly different parameter values than the remaining 12 experiments.”³⁰ Needless to say, given all of the differences between this set of treatments and the others, drawing any useful inferences from these results seems like an exercise in futility.

Another example is related to the treatments that include a MFN. In conventional wisdom, the use of MFNs in circumstances in which sellers individually or collectively possess market power has the prospect of elevating selling prices.³¹ In the absence of a MFN, the seller views the price to each buyer as independent of the price to other buyers. With a MFN, each seller knows that if it offers a low price to one buyer, it must offer the same low price to all MFN “protected” buyers. Thus, the increased profit from selling to an additional buyer by lowering price must now be offset by the reduction in profits that will result from offering that same lower price to the buyers with MFNs. Consequently, each seller has a reduced incentive to lower the price to any buyer than it would in the absence of a MFN.

By contrast, the experimental results suggest that fees paid to sellers are lower and the MVPD’s bargaining power is greater with rather than without a MFN. As the authors note, “[t]here is no obvious explanation why the inclusion of an MFN augmented the average buyer’s bargaining power. Most striking is the fact that the MFN not only augmented the bargaining power of the

²⁹ BKS, p. 23.

³⁰ BKS, note 49, p.23. There is no discussion of why the DBS provider was excluded from these treatments.

³¹ See, for example, Dennis Carlton and Jeffrey Perloff, Modern Industrial Organization, Harper Collins: New York, New York, 1994, p. 416; Jean Tirole, The Theory of Industrial Organization, MIT Press: Cambridge, MA, 1988, pp. 330-332.

MFN-endowed buyer, but it also augmented the bargaining power of the non-MFN endowed buyers.”³²

One outcome that is distinctly at odds with reality is the profit performance of some of the stylized program services. The two least popular program services experience losses in every treatment and even the “moderately popular” service loses profits in some treatments. Yet, some of the “less popular” programming services which apparently serve as a basis for the calibration (costs, third-party payments, and buyer willingness to pay) of the services used in the experiment have survived for some time.³³ Similarly, services that were used to calibrate the “moderately popular” experimental service have survived for an even longer period of time.³⁴ More generally, the experimental results would suggest that the number of services available would quickly dwindle to a core of relatively popular services. In fact, in the presence of large MVPDs, the array of program services available has for years included a large and increasing number of “less popular” services.

As one final example of an inexplicable result, in almost every treatment, the DBS operator—accounting for 17% of all MVPD subscribers throughout—has more bargaining power than any cable operator, including one that accounts for more than 50% of MVPD subscribers. In addition, in some treatments, the magnitude of the DBS bargaining power does not decline monotonically with the increases in cable operator concentration.³⁵

These examples suggest that because of a failure to test the sensitivity of the results or because of design failures or because of instructional inconsistencies or all three, the correspondence between the experimental results and reality seems quite low.

³² BKS, p.31. For reasons that are unclear, this statement does not appear in the revised version of the study, although the results described in the revised version are virtually identical to those in the earlier version.

³³ For example, according to NCTA, Ovation was introduced in 1996; Great America Country was introduced in 1995 and the Health Network was introduced in 1999.

³⁴ For example, according to NCTA, the Cartoon Network was introduced in 1992 and Court TV was introduced in 1991.

³⁵ See the treatments with limited channel capacity and MFNs, Table 10, p. 34. The DBS operator’s bargaining power in the least concentrated environment (Low Concentration/High Numbers) is higher than in the most concentrated environment (High Concentration/Low Numbers) but smaller than in the “moderately” concentrated environment (High Concentration/High Numbers).

6. Conclusions

We do not believe that the results of the experiment reported in the BKS study should serve as the basis for any FCC policy that would limit the share of national subscribers that can be served by a single cable operator.

First, economic theory offers the FCC little reason to believe that in and of itself, the larger size of a cable MVPD would impede the supply of programming or harm cable consumers. While the surplus associated with serving the customers of a larger cable operator will be greater than that associated with a smaller cable operator, simply because the larger operator serves more consumers, there is no obvious reason why the *fraction* of the surplus obtained by the large cable operator should differ from that obtained by the small operator. Moreover, even if large cable operators were able to obtain a larger fraction of the surplus, at least some of the savings in per-subscriber payments to program services would benefit cable subscribers in the form of lower cable subscription prices. Any policy analysis by the FCC should account for those consumer benefits. In short, we believe the FCC should conclude that there is no strong basis in economic theory to believe that larger MVPDs on a national basis will have an adverse effect on the supply of programming or the welfare of cable consumers. The fact that the experimental design itself does not allow for program supply effects underscores this conclusion.

Second, the correspondence between the experimental environment in the BKS paper and the real world in which transactions occur between MVPDs and program services is quite weak. The assumption that cable operators have no financial interests in programming alone is a significant departure from reality. Furthermore, the stakes in the experiment—the payments made to the experiment's participants—are so small in comparison to those in real-world transactions as to call into question whether the experimental results are predictive of real-world behavior. Nor does the inability of the experimental participants to reach mutually beneficial trades indicate that similar inefficiencies are at all likely to arise in the real world when commercial entities have much more time to learn, negotiate, and conclude such deals. Thus, we believe the FCC should conclude that the experimental results are not a realistic reflection of MVPD and program service interaction. In addition, BKS fail to provide the necessary information to determine how closely the calibration of their model — the willingness-to-pay

and the costs of the MVPDs and the advertising revenues and costs of the program services — captures the variation of actual MVPDs and program services. Without such detail, it is impossible completely to evaluate the gulf between the experimental reality and those real-world transactions.

Third, the results are not reliable. Given the complexity of the experiment's structure, BKS should have ensured that their results would not be tainted by instructions to the participants that are either inconsistent or incorrect and are not sensitive to changes in the design parameters. However, the written instructions are very inconsistent and sometimes incorrect and no sensitivity analysis is supplied. Moreover, some of the results are in stark contrast to real-world outcomes. As a result, we believe the FCC should conclude that the results from the experiment are not a reliable basis on which to base its ownership policy.

Appendix: The Structure of the BKS Experiments

Using undergraduate and graduate students as subjects, BKS paid each subject \$7 to participate in the study, and provided them with a pre-specified portion of any realized “experimental” profits. Seven to 9 subjects participated in each experimental session and we understand that each participant only participated in one session. Each session, in turn, comprised 8 different trading periods. In each trading period, 4 program sellers were instructed to negotiate with a number of MVPD buyers ranging from 3 to 5, the number depending on the particular “treatment” or assumed set of market conditions in the experiment. After the end of one trading period, the negotiated price is recorded and the participants are given “some time” to calculate their period profits. Then, the next trading period begins with the same assumed conditions as in the previous trading period. Both sellers and buyers retain their identity across trading periods within a session. Each trading period lasts 6 minutes.

The four sellers represent program services and were “calibrated” to represent more or less popular basic satellite programming services.³⁶ Two services are constructed to be “less” popular services, apparently on the order of the popularity of Ovation and the Great America Channel. One of the services is calibrated to represent a “moderately” popular service, such as the Cartoon Network and Court TV. Finally, one service is calibrated as a very popular service, such as CNN and MTV.³⁷

Each seller is assigned an unavoidable cost, which differs among program services. If the seller wins carriage on any particular MVPD, it also receives a pre-determined specific amount of national advertising revenues (that varies by MVPD) in addition to the price paid by the MVPD

³⁶ By “calibrated,” we mean that each of the four services were assigned costs and national advertising revenues based upon the kinds of services they represented.

³⁷ To be clear, Ovation and the Great America Channel were among the program services used to calibrate the two “least popular” experimental services. The Cartoon Network and Court TV were among the program services used to calibrate the “moderately popular” experimental service. CNN and MTV were among the services used to calibrate the “popular” experimental service.

for the right to carry the service.³⁸ Moreover, each seller is provided with a certain amount of “working capital” at zero cost, with the sellers of the two least popular services receiving the most working capital.

The number of buyers (MVPDs) varied across the experimental market environments from 3 to 5. In each case, there was a DBS buyer who accounted for about 17% of all MVPD subscribers. In one environment, there were 4 cable buyers, each accounting for between 14.6% and 26.8% of all MVPD subscribers. (This environment is referred to in the study as the Low Concentration/High (Buyer) Numbers environment.)

In a second environment, there were again 5 buyers, but one cable buyer accounted for about 51% of all MVPD subscribers, with the other 3 cable buyers having subscriber shares ranging from about 7% to about 13%. (This environment is referred to in the study as High Concentration/High Numbers environment.)

In a third environment, the number of cable operators was reduced to 2, with one cable operator serving 39% of MVPD subscribers and the other serving 43.9% of subscribers. (This environment is referred to as the High Concentration/Low Numbers environment.)

In each environment, the MVPD buyer is assigned an unavoidable cost and a maximum willingness to pay for each program service. On a per-subscriber basis, the maximum that the MVPD is willing to pay for each of the four services depends on the popularity of the service. This is a value that is assigned to each buyer by the experimenters and it represents the per-subscriber revenues that the MVPD would receive by providing the service to its subscribers.

³⁸ These advertising revenues were determined in advance by the authors and depend on the subscriber base of the MVPD, the number of network spots, the price per thousand viewers, and the ratings of the program service. While the study reports the estimates for each of these variables, it is not apparent precisely how the authors made these estimates. Similarly, it is not apparent how the authors estimated the unavoidable costs for each of the four services.

The maximum amount that the buyer would be willing to pay for each service would then be the per-subscriber revenues earned for the carriage of that service times the number of subscribers served by the MVPD, which is fixed throughout the experiment. These are the total revenues that would be earned by the MVPD if the service were provided to the MVPD's subscribers. Apparently, the per-subscriber amount includes an estimate of the local advertising revenues that could be earned by the MVPD as well as revenues from the direct payment by subscribers for the services. The paper does not provide enough details to determine how these estimates were derived.

These market environments are married to other market characteristics to generate a "treatment." These other characteristics include unlimited and limited channel capacity for MVPDs and the presence or absence of a Most Favored Nation clause in their contract with program services. (In treatments that included the limited capacity characteristic, buyers can purchase at most 3 of the 4 program services.)

There are a total of 24 experimental sessions. The paper evaluates the outcome of each trading period in each session by a number of metrics, which are then averaged across the trading periods to calculate a treatment average for all trading periods. From a policy evaluation perspective, the most important metric is the fraction of the maximum surplus that is realized by the players, what the paper refers to as the measure of "economic efficiency." The maximum surplus is the maximum total profits that could be earned by all players (buyers and sellers) in the treatment.³⁹ The realized surplus is that actually attained by the experiment's participants. To the extent that the realized surplus falls short of the maximum, there were missed opportunities

³⁹ In the limited capacity treatments, the maximum surplus attained occurs when each MVPD carries the three services with the largest total surplus (the sum of the MVPDs' maximum willingness to pay and the network advertising revenues received by the seller). In the unlimited capacity treatments, the maximum surplus is attained when each MVPD carries all four services.

for program service carriage that would have made the services collectively, the MVPDs collectively, or both, more profitable.⁴⁰

The treatments for which there are the most experimental sessions (i.e., observations) are those in which there is limited capacity and no MFN. Over the different buyer/seller environments with limited capacity and no MFN, there were a total of 14 sessions for these treatments. The study concludes that for this set of sessions, “when the number of programming networks exceeds the cable operator’s channel capacity, higher levels of concentration led to a modest reduction in ‘economic efficiency.’” To be precise, for the no-MFN sessions, an average of 93% of the maximum surplus was obtained for the Low Concentration/High Numbers environment and an average of 83.6% was obtained in the High Concentration/High Numbers environment. But in one of the unexplained outcomes in this study, High Concentration/Low Numbers results on average in obtaining 89% of the maximum surplus, nearly as much as that in the Low Concentration/High Numbers sessions.⁴¹

⁴⁰ It is not obvious that use of the maximum surplus as the benchmark is correct. In particular, it is possible that the level of mistakes in conducting (or failing to conduct) trades is such that on average, only about 85% of the surplus tends to be attained. Thus, in a world with frictions, the “practical” maximum surplus—on average—might be 85% or 90% of the theoretical maximum.

⁴¹ In an odd turn of phrase, the study (p. 4) also concludes that “the bargaining power of a cable operator that serves 27% of the MVPD market does not differ substantially from...[one] that serves 51% of the MVPD market. From the perspective of a programming network, a cable operator that serves 27% of the MVPD market is as powerful as one that serves 51% of the market.” An alternative way of restating this last sentence is that “a cable operator that serves 51% of the market has no more bargaining power than one that serves 27% of the market.”

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Dr. Woodbury's principal fields of expertise are industrial organization, regulation, antitrust, law, and economics. He is an expert in and has published on the economics of antitrust and regulation in broadcasting, cable, telecommunications, and other industries.

PRIOR PROFESSIONAL EXPERIENCE

Microeconomic Consulting and Research Associates, Inc. (formerly Competitive Analysis Group, ICF Consulting Associates)

Principal, 1989–1992. Responsible for providing antitrust and regulatory advice to clients.

Analysis Group

Research Associate, 1989. Responsible for providing antitrust and regulatory advice to clients.

Federal Trade Commission (1985–1989)

Associate Director for Special Projects, Office of the Bureau Director, Bureau of Economics. Responsible for: initiating, conducting, and reviewing economic studies on Commission and other regulatory policies (including telecommunications); drafting speeches for the Chairman; and reviewing Bureau participation in FTC cases.

Assistant Director for Rulemaking, Division of Policy and Evaluation, Bureau of Consumer Protection. Responsible for managing the Commission's Rulemaking Agenda, and drafting recommendations to the Commission from the Bureau Director. Rules reviewed include Holder-in-Due-Course, Vocational Schools, Cooling-Off, and Funeral Rules.

Deputy Assistant Director, Regulatory Analysis, Bureau of Economics. Responsible for conducting or supervising studies or filings before regulatory agencies, including the Federal Communications Commission, the International Trade Commission, and the National Highway Traffic Safety Administration.

National Cable Television Association

Vice President, Department of Research and Policy Analysis, 1983–1985. Responsible for conduct or supervision of studies related to cable television, including consumer costs of the



franchising process, deregulation of cable prices, effects of copyright fees on consumers, and the extent of competition with cable TV.

Federal Trade Commission

Senior Economist, Regulatory Analysis Division, Bureau of Economics, 1982–1983.
Responsible for broadcasting and telecommunications.

Federal Communications Commission (1979–1982)

Chief, Economics Division, Common Carrier Bureau. Senior economic advisor to Bureau and Commission on common carrier policy. Directed 25 subordinates in policy analysis.

Industry Economist, Network Inquiry Special Staff. Responsible for the analysis of the program supply industry and the competitive impact of new broadcast technology.

Civil Aeronautics Board

Brookings Economic Policy Fellow assigned to Office of Economic Analysis, 1978–1979.
Responsible for the development of merger policy, international aviation policy, and service to small communities. Position: Assistant Chief, Policy Analysis Division.

State University of New York at Albany

Assistant Professor of Economics, 1977–1978.

Federal Reserve Bank of New York

Economist, International Research Department, 1975–1977. Responsible for assessing bank-reported capital flows and exchange-rate movements.

Southern Illinois University—Carbondale

Lecturer, 1974–1975.

EXPERT WITNESS ACTIVITIES

Expert Witness before the Copyright Arbitration Royalty Panel, Direct and Rebuttal Testimony, regarding the determination of reasonable license fees for digital performance right in sound recordings and ephemeral recordings of music performed on public radio websites. Prepared on behalf of National Public Radio/Corporation for Public Broadcasting. April and October 2001.

Expert Witness before the Illinois Commerce Commission, regarding the Proposed SBC/Ameritech merger. Prepared on behalf of Sprint Communications Company, L.P. July 1999.



Expert Witness before the Commonwealth of Virginia State Corporation Commission, regarding the Proposed Bell Atlantic/GTE merger. Prepared on behalf of Sprint Communications Company, L.P. March 1999.

Expert Witness before the Ohio Public Utilities Commission, regarding the Proposed SBC/Ameritech merger. Prepared on behalf of Sprint Communications Company, L.P. December 1998.

Expert Witness before the Illinois Commerce Commission, regarding the Proposed SBC/Ameritech merger. Prepared on behalf of Sprint Communications Company, L.P. October and December 1998.

Expert Witness to Copyright Arbitration Royalty Panel, Direct and Rebuttal Testimony, regarding the determination of reasonable rates for the digital performance of sound recordings. Prepared on behalf of Music Choice and DMX. June and July 1997.

Expert Witness to Copyright Arbitration Royalty Panel, Rebuttal Testimony, regarding the shares of Royalties due Copyright Claimants. Prepared on behalf of the Motion Picture Association of America. March 1996.

Expert Witness before the Copyright Royalty Tribunal, Rebuttal Testimony on the Value of Distant Signal Sports Programming. Prepared on behalf of the Motion Picture Association of America, December 1991.

Expert Witness preparation in five antitrust investigations, 1988–1992, on behalf of the FTC.

Expert Witness, FTC v. Elders Grain, Preliminary Injunction Proceeding, Sixth District Court. Testimony prepared on behalf of the FTC, June 1988.

Expert Witness before the International Trade Commission and Department of Commerce, Imports of Japanese Semiconductors. Testimony prepared on behalf of the FTC, 1986.

Expert Witness, Texas International/National/Pan American Acquisition Case and Continental/Western Acquisition Case. Testimony prepared on behalf of the Civil Aeronautics Board, 1978–1979.

OTHER SELECTED CONSULTING ACTIVITIES

Submitted a report, “Economic Analysis of Unilateral Effects in the P&G/Clairol Merger.” With Steven R. Brenner, Serge X. Moresi, and Professor Steven S. Salop. To the U.S. Department of Justice on behalf of Procter & Gamble, 2001.

Submitted a report, “The Impact on Competition of LVMH’s Partial Ownership in Gucci.” With Serge X. Moresi, Shihua Lu, and Professor Steven S. Salop. To the European Commission on behalf of Gucci, 2001.



Submitted a report, "The Incentives of Cable Operators to Carry Multiple ISPs." With Stanley M. Besen and Patrick J. DeGraba. To the Federal Communications Commission on behalf of The National Cable Television Association, 2000.

Submitted a report, "Economic Analysis of the Statement of Objections in the AOL-Time Warner Merger." With Steven C. Salop. To the Merger Task Force of the European Commission, Brussels, Belgium on behalf of America OnLine, L.P., 2000.

Submitted a report, "The Staff's Flawed Economic Analysis of Harm from Control Over 'Inactive' Programs" With Steven C. Salop. To the Federal Communications Commission on behalf of CBS Corporation and Viacom, Inc., 2000.

Submitted a report, "An Economic Analysis of the Effects of the AT&T-MediaOne Merger on Competition in the Supply and Distribution of Video Program Services: Response to the Critics." With Stanley M. Besen and Serge X. Moresi. To the Federal Communications Commission on behalf of Sprint Communications Company, L.P., 1999.

Submitted a report, "An Economic Analysis of the Proposed Bell Atlantic/GTE Merger." With Stanley M. Besen and Padmanabhan Srinagesh. To the Federal Communications Commission on behalf of Sprint Communications Company, L.P., 1998.

Submitted a report, "An Economic Analysis of the Proposed SBC/Ameritech Merger." With Stanley M. Besen and Padmanabhan Srinagesh. To the Federal Communications Commission on behalf of Sprint Communications Company, L.P., 1998.

Submitted a report, "An Economic Analysis of the FCC's Cable Ownership Restrictions." With Stanley M. Besen. To the Federal Communications Commission on behalf of Tele-Communications, Inc., 1998.

Submitted a report, "Comments on Dertouzos and Wildman, 'Programming Access and Effective Competition in Cable Television.'" With Stanley M. Besen. To the Federal Communications Commission on behalf of Tele-Communications, Inc., 1998.

Submitted a report, "An Economic Analysis of the Effects of Partial Ownership Interests in Cable Systems." With Stanley M. Besen, Daniel P. O'Brien, and Serge X. Moresi. To the Federal Communications Commission on behalf of Tele-Communications, Inc., 1998.

Submitted a report, "A Response to Ameritech's New Media's 'Allegations of a Price Squeeze' by Vertically Integrated Cable Operators." With Stanley M. Besen. To the Federal Communications Commission on behalf of Tele-Communications, Inc., 1998.



Submitted a report, "A Further Analysis of the Effects of Cable Diversion, Premium Service Buy Rates, and Volume Discounts on Primestar's Competitive Incentives: A Response to Dr. Rosston." With Steven C. Salop, Stanley M. Besen, and E. Jane Murdoch. To the Federal Communications Commission on behalf of PRIMESTAR Partners, L.P., 1998.

Submitted a report, "An Economic Analysis of the Impact of the WorldCom-MCI Merger on the Provision of Internet Backbone Services." With Stanley M. Besen and Padmanabhan Srinagesh. To the Federal Communications Commission and the European Commission on behalf of Sprint Corporation, 1998.

Submitted a report, "A Comparison of Primestar's Costs with Those of a Standalone Entrant." With Steven C. Salop, Stanley M. Besen, and E. Jane Murdoch. To the Federal Communications Commission on behalf of PRIMESTAR Partners, L.P., 1998.

Submitted a report, "An Economic Analysis of Primestar's Competitive Behavior and Incentives: Reply to the Oppositions." With Steven C. Salop, Stanley M. Besen, and E. Jane Murdoch. To the Federal Communications Commission on behalf of PRIMESTAR Partners, L.P., 1998.

Submitted a report, "An Economic Analysis of Primestar's Competitive Behavior and Incentives." With Steven C. Salop, Stanley M. Besen, and E. Jane Murdoch. To the Federal Communications Commission on behalf of PRIMESTAR Partners, L.P., 1998.

Conducted statistical and other analyses of anticompetitive allegations surrounding Time Warner's acquisition of Turner Broadcasting. Prepared on behalf of Tele-Communications, Inc. for presentation to the Federal Trade Commission, 1996.

Submitted a report, "Competitive Market Considerations in the Licensing of the 37-40 GHz Band." With Steven R. Brenner. To the Federal Communications Commission on behalf of WinStar Wireless, Inc., 1996.

Conducted statistical and other analyses of anticompetitive allegations surrounding the acquisition of CapCities/ABC by the Walt Disney Company. Prepared on behalf of the Walt Disney Company for presentation to the Department of Justice, 1995.

Assisted in the preparation of testimony for the DC District Court regarding the competitive effects of the "must-carry" rules imposed on cable systems, 1996.

Submitted a report, "A Competitive Markup Approach to Establishing Rates When Adding Cable Program Services." With Stanley M. Besen. To the Federal Communications Commission on behalf of Tele-Communications, Inc., 1994.



Submitted a report, “Exclusivity and Differential Pricing for Cable Program Services.” With Stanley M. Besen and Steven R. Brenner. To the Federal Communications Commission on behalf of Tele-Communications, Inc., 1993.

Submitted a report, “An Analysis of Cable Television Rate Regulation.” With Stanley M. Besen and Steven R. Brenner. To the Federal Communications Commission on behalf of Tele-Communications, Inc., 1993.

Evaluated the prospects for Direct Broadcast Satellites on behalf of a potential investor, 1992.

Assisted in the preparation of testimony on the value of distant signal programming to earth station owners on behalf of the Motion Picture Association of America, 1992.

Prepared estimates of the supply elasticity of crude oil production and a paper, with F. Warren-Boulton and K. Baseman, on the alternatives to traditional pipeline regulation for a pipeline client, 1991–1992.

Prepared analyses of liability and damage estimates, with F. Warren-Boulton, on behalf of NEC in a bid-rigging allegation and presented those analyses to Justice Department officials, 1991.

Prepared a report, “Economic Analysis and Policy Implications of the Financial Interest and Syndication Rule.” With F. Warren-Boulton. On behalf of the Motion Picture Association of America, 1990.

Submitted a report, “Assessing The Effect of Rate Deregulation on Cable Subscribers.” With Sherman and Baseman. To the Federal Communications Commission on behalf of the National Cable Television Association, 1990.

Submitted an affidavit, “Economic Implications of the Pac Tel/Chicago Waiver Request.” To the Department of Justice on behalf of the National Cable Television Association, January 1990.

Submitted an analysis of sham litigation allegations to the Justice Department on behalf of a software client, 1989.

PUBLICATIONS

“Analyzing Vertical and Horizontal Cross Ownership in Cable Television: the Time Warner-Turner Merger (1996),” in J.E. Kwoka and L.J. White, *The Antitrust Revolution: Economics, Competition, and Policy*, Scott, Foresman. With S. Besen, E. Murdoch, D. O’Brien, and S. Salop. Third Edition, Oxford University Press, 1999.

“Telecommunications in the US: Evolution to Pluralism.” With S. Besen and S. Brenner. In B. Lange (ed.), *ISDN in the USA, Japan, Singapore and Europe*, 1996.

“Market Structure, Program Diversity, and Radio Audience Size.” With R. Rogers. *Contemporary Economic Policy* 1996.



“Rate Regulation, Effective Competition, and the Cable Act of 1992.” With S. Besen. *Hastings Communications and Entertainment Law Journal*, 1994.

“Assessing Competition and Deregulation in Telecommunications: Some Observations on Methodology.” In B. Cole (ed.), *After the Breakup: Assessing the New Post-AT&T Divestiture Era*. New York: Columbia University Press, 1991.

“Deterrence and Justice.” With J. Bilmes. *Research in Law and Economics*, 1991.

“The First Amendment, Cable MTV, and the Must-Carry Rule: Towards a Cost-Benefit Analysis.” *Proceedings of the Airlie House Conference on Telecommunications*, 1987.

“Video Competition and Consumer Welfare.” In E. Noam (ed.), *Proceedings of the Arden House Conference on Video Competition*. New York: Columbia University Press, 1986.

Misregulating Television. With S. Besen, R. Metzger, and T. Krattenmaker. Chicago: University of Chicago Press, 1984.

“Regulation, Deregulation, and Antitrust in Telecommunications.” With S. Besen. *Antitrust Bulletin*, Spring 1983.

“Determinants of Network Television Program Prices: Implicit Contracts, Regulation, and Bargaining Power.” With S. Besen and G. Fournier. *Bell Journal of Economics*, Autumn 1983.

“Advertising, Price Competition, and Market Structure.” With A. Arterburn. *Southern Economic Journal*, January 1981.

“Exchange Rate Stability and Monetary Policy.” With B. Putnam. Albany Discussion Paper #95 in *Review of Economics and Business Research*, Winter 1980.

“Capital Market Integration Under Fixed and Floating Exchange Rates: An Empirical Analysis.” *Journal of Money, Credit, and Banking*, May 1980.

OTHER COMPLETED RESEARCH

“Empirical Evidence on Efficiencies in the Common Ownership of Broadcast Stations.” With K. Anderson. Comments on FCC Proceeding, 1991.

“Do Government-Imposed Ownership Restrictions Inhibit Efficiency?” *Working Paper of the Bureau of Economics*, No. 169, 1988.

“Over-the-Air Television and Cable Prices: An Econometric Inquiry.” With M. Bykowsky. Served as basis of FCC decision deregulating cable prices, 1985.

“The Effect of Rate Regulation and Franchise Delay on Program Availability.” With D. Koran. Comments on FCC Proceeding, 1985.



“Pricing Flexibility and Consumer Welfare: The Deregulation of Basic Cable Rates.” NCTA White Paper, 1984.

“Economic Assessment of the Financial Interest and Syndication Rules.” With K. Anderson. Comments on FCC Proceeding, 1983.

“Domestic Fixed Satellite Transponders Sales.” Comments on FCC Proceeding, 1982.

An Analysis of Television Program Production, Acquisition, and Distribution. With R. Metzger. Network Inquiry Special Staff, Preliminary Report, Federal Communications Commission, June 1990.

“Production Abroad: Theoretical Considerations and Empirical Analysis.” Mimeo, 1978.

“Scale Economies in the Airline Industry: A Survey.” Mimeo, 1978.

PRESENTED PAPERS

“Market Structure, Program Diversity, and Radio Audience Size.” With R. Rogers. Meetings of the Western Economics Association, July 1993.

“The Effects of Rate Deregulation on Cable Subscribers.” With K. Baseman. Policy Approaches to the Deregulation of Network Industries: An American Enterprise Institute Conference, October 1990.

“Economic Analysis and Policy Implications of the Financial Interest and Syndication Rule.” Telecommunications Policy Research Conference, Airlie House, October 1990.

“The Design and Evaluation of Competitive Rules Joint Ventures for Mergers and Natural Monopolies.” With F. Warren-Boulton. American Economic Association Meetings, December, October, 1990.

“Do Media Ownership Restrictions Reduce Economic Efficiency?” Telecommunications Policy Research Conference, Airlie House, November 1989.

“The Conflict Between Spectrum Efficiency and Economic Efficiency.” With R. Rogers. Telecommunications Policy Research Conference, Airlie House, November 1989.

“Regulation versus Antitrust.” Annenberg Conference: The Divestiture Five Years Later, March 1989.

“Regulating Cable Television.” Telecommunications Policy Research Conference, Airlie House, September 1987.

“An Empirical Analysis of Television Program Prices.” With S. Besen and G. Fournier. Meetings of the Southern Economic Association, November 1981.

“Flexible Exchange Rates and Market Integration.” With B. White. Federal Reserve System Conference on Financial Market Research, June 1979.



“Advertising, Price Competition, Market Structure.” With A. Arterburn. Meetings of the Southern Economic Association, November 1978.

“The Effects of Exchange Rate Systems on International Capital Market Integration.” With B. White. Federal Reserve System Conference on International Research, November 1977.

OTHER PROFESSIONAL ACTIVITIES

Chair, “Competition between Cable Television and Telephone Companies.” Telecommunications Policy Research Conference, September 1991.

Discussant, “Competition and Ownership in the Media.” Telecommunications Policy Research Conference, September 1991.

Chair, “Spectrum Management Session.” Telecommunications Policy Research Conference, Airlie House, September 1988.

Book Review, *Productivity in the United States* by John Kendrick and Elliot Grossman, *Southern Economic Journal*, April 1981.

Discussant, “Deregulation of Telecommunications.” Meetings of the Western Economic Association, July 1981.

Referee, *Southern Economic Journal*, *RAND Journal of Economics*, Harvard University Press.

AWARDS

Award for Excellence in Economics (FTC), 1988.

Competition Advocacy Award (FTC), 1987.

Brookings Economic Policy Fellow, 1978–1979.

SUNY Faculty Research Grant, 1978.

NSF Traineeship, 1973–1974.

Finalist, Woodrow Wilson Fellowship Competition, 1971.

