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FEDERAL COMMUNICATIONS COMMISSION
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BEFORE THE
FEDERAL COMMUNICATIONS COMMISSION
WASHINGTON, D.C. 20554

In the Matter of	)	
	)	
Implementation of Section 11 of the Cable Television Consumer Protection and Competition Act of 1992	)	CS Docket No. 98-82
	)	
Implementation of Cable Act Reform Provisions of the Telecommunications Act of 1996	)	CS Docket No. 96-85
	)	
The Commission's 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

SUPPLEMENTAL COMMENTS OF TIME WARNER CABLE

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Summary and Introduction

The Commission has invited comment on an "experimental economics" study conducted by the Commission's Office of Plans and Policy.[1] The Commission seeks "comment on the value of the study in providing empirical evidence relevant to the ownership issues raised in the Commission's pending cable ownership rulemaking proceeding."[2] It has asked for comment on, among other things, "the study's conceptualization, underlying assumptions, design, and methodology."[3] In addition, "interested parties are encouraged to examine the released data and draw their own conclusions."[4]

Time Warner Cable believes that there are three reasons why the *Study* can provide no support for a subscriber limit. *First*, the *Study* relies on experiments whose design fails to duplicate key attributes of the real-world video-programming marketplace. *Second*, the experiments' results are statistically suspect. *Finally*, even if one takes the *Study*'s results at face value, they do not support regulatory intervention.

Background

The *Study* attempts to measure the consequences of concentration in the multichannel video programming industry by conducting three play-acting experiments. In each of the experiments, the sell-side was kept constant, with four sellers of programming. The buy-side

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[1]*See* Mark M. Bykowsky, *et al.*, Office of Plans and Policy, FCC, OPP Working Paper Series No. 35 — *Horizontal Concentration in the Cable Television Industry: An Experimental Analysis* (rev. July 2002) ("*Study*").

[2]Public Notice, *Media Bureau Seeks Comment on Experimental Economics Study Examining Horizontal Concentration in the Cable Industry*, 17 FCC Rcd 10544, 10544 (2002).

[3]*Id.* at 10544-45.

[4]*Id.* at 10545.

was varied to become increasingly concentrated. In the least concentrated scenario, there were five buyers serving 26.8%, 24.4%, 17.1%, 17.1%, and 14.6% of the subscriber universe (for a Herfindahl-Hirschman Index ("HHI") score of about 2100). *See Study* 13. In the more concentrated scenario, there were again five buyers, but this time with shares of 51.2%, 17.1%, 13.4%, 11.0%, and 7.3% (for an HHI of about 3300). *See id.* Finally, in the most concentrated scenario, there were only three buyers — with shares of 43.9%, 39.0%, and 17.1% (for an HHI of about 3800). *See id.*[5]

The experiments consisted of mock trading sessions in which college-student volunteers acted as buyers and sellers. Students traded with "pretend" dollars that were later exchanged (at a steep discount) for real dollars; students were allowed to keep their winnings. In each session, the subjects were allowed to trade for eight six-minute periods. Buyers were assigned a "resale value" representing the value of carrying particular programming. For each period, buyers and sellers were assigned fixed costs that they must recover to turn a profit. At the end of each session, the profits and losses were tallied, and the subjects were dismissed. *See id.* at 21. The different scenarios were acted out a number of times with different groups of subjects.

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[5]In addition to these three experiments, the *Study* conducted two additional sets of experiments. *First*, the *Study* conducted experiments intended to explore the role of concentration when channel capacity is unlimited. *See Study* 16. *Second*, the *Study* conducted a series of experiments in which some buyers have "Most Favored Nation" status, meaning that sellers must give them the benefit of any bargains offered to other buyers. *See id.* at 16-17. These two sets of additional experiments were plainly not the focus of the *Study*. For example, the *Study* itself acknowledged that, because these experiments were duplicated not five times but only twice, they yielded too few results to detect statistically significant differences. *See, e.g.*, *id.* at 34. We will not further address them here.

For example, the first — least concentrated — experiment, called "Treatment 1," was conducted five times, each time with a different set of nine students. *See id.* at 14-15.

On the basis of the profit tallies of each of the experiments, the *Study* tries to draw conclusions about four measures of market performance: economic efficiency (defined in terms of the amount of aggregate value realized), buyer bargaining power, buyer surplus, and probability that a seller incurs a loss. The *Study* finds the following values:

	Least Concentrated	More Concentrated	Most Concentrated
Efficiency	93.0	83.6	89.0
Buyer Bargaining Power	46.0	41.9	42.6
Buyer Surplus	44.0	40.2	40.0
Probability of Seller Loss	38.8	35.9	32.5

Although the *Study* determined that none of the differences among the number pairs was statistically significant, it found one exception: according to the *Study*, the 93.0/83.6 difference *was* statistically significant. *See Study* 27. Thus, the authors were faced with only one statistically significant difference among 12 number pairs — all of which were supposed to be testing roughly the same thing: the influence of concentration on industry performance. Moreover, the efficiency drop between "least concentrated" to "more concentrated" was contradicted by an increase in efficiency between "more concentrated" to "most concentrated." Thus, one might have expected that the authors would have dismissed the 93.0/83.6 difference as a fluke. But they accepted the result as valid. And, solely on the basis of this one result, they concluded that "higher levels of horizontal concentration . . . led to a modest reduction in 'economic efficiency.'" *Id.* at 49.

The *Study* also sought to measure the performance of one particular buyer: the one labeled "DBS operator." The DBS operator had mostly the same attributes as the other buyers, except that its market share equaled 17.1% — which, the *Study* explained, equaled the current share of DirecTV and Echostar combined. *See id.* at 4 n.9. The *Study* found these values:

	Least Concentrated	More Concentrated	Most Concentrated
Buyer Bargaining Power	53.6	47.4	42.9
Buyer Surplus	50.6	46.5	40.5

Again, the *Study* ignored the differences that were not statistically significant and placed heavy emphasis on the ones that supposedly were. Although the *Study* found that the differences in values between the least concentrated and more concentrated scenarios were not statistically significant, it found that the differences between the least concentrated and most concentrated scenarios were. *See id.* at 34, 36. On that basis, the *Study* concluded that "higher concentration levels would negatively impact the DBS operator's bargaining position." *Id.* at 34; *see id.* at 35-36. According to the *Study*, "[a] reduction in its bargaining power means that the DBS operator can expect to pay higher affiliate fees following the increase in horizontal concentration." *Id.* at 49.

Argument

The *Study*'s conclusions cannot be used to predict cause and effect in real-life circumstances. Any FCC rule based even in part on the *Study*'s conclusions would be set aside as failing applicable scrutiny under the First Amendment and the Administrative Procedure Act.

I. THE EXPERIMENTS' DESIGN IS FLAWED.

The *Study* itself concedes that its experiments failed to duplicate a number of key attributes of the real-world MVPD marketplace.[6] To some extent, this is true for any experimental-economics study, and it is not our goal to debate the merits of experimental economics in the abstract. Rather, we will show that the *Study* abstracts from reality in a number of ways that render its experimental results poor predictors of real-life cause and effect.[7] The main failings are as follows:

First, the *Study* posits a static and one-dimensional programming market that bears only the faintest resemblance to real life. In the *Study*, programming is treated like a commodity whose value to the buyer is expressed in a crude, readily ascertainable resale value. Buyers — caring only about quantity and price — purchase programming the way merchants trade pork bellies and soy beans. The real world is immeasurably more dynamic and complex. The *Study* ignores that, in the real world, MVPDs exercise editorial discretion, selecting not individual video-programming services but composing a mix of programming that together will be most attractive to subscribers. The *Study* ignores that MVPDs must gauge the demographics (and thus the advertising value) of subscribers attracted by particular video-programming services. The *Study* further ignores that carriage negotiations center not on price alone: they concern a variety of factors, including duration, the tier on which programming is carried, and marketing commitments. Finally, the *Study* fails to "take into account that a programming network's

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[6]*See Study* 3, 50 ("[T]he experimental market did not and could not display all the complex characteristics of the actual market.").

[7]The *Study* appears to acknowledge as much. *See Study* 3 ("The absence of some of these characteristics may have affected the results of the study.").

bargaining power in an upcoming affiliate agreement negotiation may be enhanced if it is currently carried by an MVPD." *Study* 3.[8] Because each of these factors may impact buyers' and sellers' bargaining power in ways that the *Study* does not even attempt to approximate, the *Study* can be only of academic interest.

Second, the *Study* candidly acknowledges that it makes no allowance for competition between cable operators and DBS operators: "The experiments impose the restriction that the value a particular buyer (*e.g.*, cable operator) places on a particular programming network is independent of the carriage decisions made by another MVPD (*e.g.*, DBS)." *Id.* at 51. That fact by itself makes the *Study* unusable. In real life, cable operators cannot make acquisition decisions in a vacuum: they must consider whether purchasing some programming or failing to purchase other programming will cause subscribers to leave them for a competing MVPD. The *Study* thus ignores that even the largest purchasers of programming are restrained in the exercise of whatever bargaining power they may have. This failing is particularly troubling because it runs directly contrary to the D.C. Circuit's remand instructions.[9]

Third, each of the experiments posits an industry that is far more concentrated than the real-life marketplace: whereas the real-life programming marketplace includes hundreds of

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[8]*See also Study* 51 ("[T]he economic experiments may not fully capture the possibility that the bargaining outcomes in successive trading periods in the actual market may be correlated. Indeed, programming networks may have increased bargaining power in the future if a MVPD presently carries them. This increased bargaining power may be due to the dissatisfaction MVPD subscribers may experience from having a previously carried programming network dropped by the MVPD.").

[9]*See Time Warner Entertainment Co. v. FCC*, 240 F.3d 1126, 1134 (D.C. Cir.) ("in revisiting the horizontal rules the Commission will have to take account of the impact of DBS on that market power"), *cert. denied*, 122 S. Ct. 644 (2001).

domestic MVPDs and countless foreign ones (not to mention myriad emerging outlets, including streaming video on the Internet and broadcasters' multiple digital feeds), the *Study* posits a world of at most five buyers.[10] This is like attempting to determine the harmfulness of salt to people by feeding three groups of lab rats one, two, and three pounds of salt per day. Just as such a study tells us nothing about the impact of salt consumed at moderate doses (which, for all we know, might well be salutary), the *Study* tells us nothing about the effects of concentration at realistic levels. In addition, the experiments' high level of concentration reflects not only the presence of a single large buyer, but also an astonishing level of consolidation in the balance of the industry. Thus, even if the *Study* showed lower efficiency at higher levels of concentration, it would be impossible to determine whether the drop in efficiency was caused by the presence of a particularly large player or by the consolidation of the fringe.[11]

Finally, the *Study* mistakenly assumes that college students conducting six-minute trading sessions are suitable proxies for actual buyers and sellers hammering out agreements in drawn-out negotiations. In real life, programming acquisition involves long and complex interactions between scores of interdependent industry participants who each possess a store of information

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[10]*See Study* 50 ("the experimental market includes far fewer . . . MVPDs than there are in the actual market"). Whereas the real industry — counting only domestic MVPDs — has an HHI of only 905, *see Annual Assessment of the Status of Competition in the Market for the Delivery of Video Programming*, Eighth Annual Report, 17 FCC Rcd 1244, ¶ 153 (2002), even the least concentrated scenario in the *Study* has an HHI of 2112.

[11]To make matters even more complicated, the top cable operator in the *Study*'s most concentrated scenario is actually smaller than the largest operator in the more concentrated scenario (with subscriber shares of 51.2% vs. 43.9%, respectively) — a problem apparently generated by the designers' insistence on featuring a 17.1% "DBS" player in each of the scenarios.

that, although necessarily imperfect and incomplete, is vastly larger than the information-starved subjects in the *Study*'s experiments. In light of high stakes, industry participants rely on highly skilled, highly experienced, and highly paid negotiators who, when results are nevertheless disappointing, are quickly replaced. In contrast, the *Study*'s experiments draw on Penn State students selected on the basis of their willingness to sacrifice a few hours in return for $7 of pocket money.[12] Without additional verification, it is never certain that changes in circumstances affect beginners the same way as professionals.[13] For that reason, one cannot simply assume that changes in concentration would affect Penn State students the same way as skilled programming-acquisition executives.

II. THE EXPERIMENTS' RESULTS ARE STATISTICALLY SUSPECT.

Quite apart from the objection that college students may behave differently from professionals, the *Study* is subject to a similar but even more serious objection. In each concentration environment, the same experiment was acted out by only five different students.[14] Conclusions about the impact of concentration were then drawn by comparing the results of the three five-member teams against each other. The results show, however, that even within

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[12]*Study* 17 ("The subjects were undergraduates and graduate students from Penn State University. All subjects were paid $7 for showing up on time for the session.").

[13]For example, changing baseball rules so that pitchers are allowed only three balls might have different consequences in the major leagues and in college-varsity leagues. In the major leagues, the new rule might cause pitchers capable of great precision to take fewer risks, thereby producing more hits. In college play, where pitchers are not as able, the rule might result in more walks and thus produce *fewer* hits. Thus, if one sought to predict the effect of the rule change in the major leagues on the basis of an experiment in varsity play, one's forecast might point in precisely the wrong direction.

[14]In fact, due to a software glitch, results for one session had to be discarded, leaving the more concentrated team with only four players. *See Study* 14 n.32.

teams, students earned wildly different profits under precisely the same circumstances. The results thus strongly suggest that, whereas some subjects understood the experiment and participated ably and enthusiastically, other subjects simply did not "get it." And no efforts were made either to weed out non-performers or to distribute them evenly over five-member teams.

That some students did not understand the game is obvious from the data. Take the biggest cable operator (Buyer 8) in the least concentrated scenario (Treatment 1). Under precisely the same circumstances, the subjects who participated in the second and third sessions achieved these wildly divergent scores:

	period 1	period 2	period 3	period 4	period 5	period 6	period 7	period 8	average
2d session	1629	1639	1779	1827	1828	1848	1856	1869	**1784**
3d session	-36	-91	-47	-43	42	-135	24	54	**-29**

Clearly, the first subject immediately understood the game and only got better along the way. Meanwhile, the other subject was at sea, drifting around aimlessly.

Or take the "DBS operator" (Buyer 9) in the most concentrated scenario (Treatment 2) and compare the subjects in the first and second sessions:

	period 1	period 2	period 3	period 4	period 5	period 6	period 7	period 8	average
1st session	573	723	854	744	913	900	915	916	**817**
2d session	27	-44	298	-242	55	33	3	134	**33**

Although perhaps getting off to a slow start, the student in the first session quickly mastered a winning strategy and then stuck with it. Meanwhile, the subject in the second session appears to have pressed buttons at random.

That the experiments would show such differing results is perhaps not surprising. The instructions for each of the treatments were long and convoluted. *See Study* 72-115. The game as a whole must have seemed strangely sterile and abstract. *See id.* at 72 ("[Y]ou will have the opportunity to buy and/or sell a set of fictitious assets. The assets are 'fictitious' in that they only exist in the context of the experiment."). The game's user interface was intricate and complex. *See id.* at 19, 21. As the *Study* itself concedes, most subjects needed time to become comfortable with the format.[15] Yet, students were allowed little time to learn, and were not permitted to compare their performance to that of their peers. *See id.* at 13 n.30. Thus, it is hardly surprising that some students simply failed to comprehend the nature of the game, as if joining a card game that they have never played before.

What is surprising, however, is that the authors made no apparent attempt to sift out non-performing subjects. Although the experiments' instructions contain test questions that students apparently had to answer correctly to participate, there is no indication that students answering one or more questions incorrectly were sent home. *See id.* at 17-18 ("Once all subjects had successfully answered these questions, a practice trading period was conducted."). Moreover, the test questions' degree of difficulty is conspicuously out of keeping with that of

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[15]*See Study* 26 ("participants may require a few trading periods to become fully accustomed to the experimental environment"). For this reason, the designers allowed all participants a "freebie" trading period and ignored results from periods 1-4. *See id.* at 18, 26-27.

the body of the instructions. Finally, although students were told in advance that subjects losing money would be asked to leave, underachievers were protected by a generous allotment of working capital, and even the one student (out of 200) who frittered away his working capital was granted amnesty. *See id.* at 18 & n.40.

It should be obvious that the *Study* would have been worthless if five-member teams had been drawn from a pool in which, say, 20% of the subjects were not students but chimpanzees. Chimpanzees would not understand the instructions, would push buttons at random (if at all), and would likely score very few points. Thus, a team's score would overwhelmingly depend on the number of chimpanzee members assigned. Here the pool of course did not contain chimpanzees, but it did include students who — for whatever reason — simply did not understand the game. Assigning even one such student to a team could dramatically reduce the team's score.

That might not have been troubling if all five-member teams had included equal numbers of non-performers. But no effort was made to spread non-performers equally over teams. And, assuming random drawing, the chance that each of the three five-member teams would have featured precisely the same number of non-performers is quite small.[16] Putting the share of random button pushers at only 20% (which, judging from the data, seems a low estimate), the odds that each of the three teams would have fielded precisely the same number of random button pushers are only 1 in 9.[17] Put differently, 8 out of 9 experiments will report

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[16]It equals $a^{15} + (5a^4(1 - a))^3 + (10a^3(1 - a)^2)^3 + (10a^2(1 - a)^3)^3 + (5a(1 - a)^4)^3 + (1 - a)^{15}$, with "a" being the share of non-random actors. In the example in the text, $a = 0.8$.

[17]Even with a pool containing only 10% random actors, the odds of equal distribution are still only 1 in 4.

results influenced by a random-actor factor that, in all likelihood, has a far more powerful impact than the differing concentration levels.

The *Study*'s DBS results provide a graphic illustration of how random actors can skew results in ways against which the authors' statistical-significance tests apparently do not protect.[18] The five-member DBS team in the least concentrated scenario contained only one random actor (player 4):

least conc.	period 1	period 2	period 3	period 4	period 5	period 6	period 7	period 8	average pd. 1-8	average pd. 5-8
player 1	522	688	358	347	877	436	996	982	651	823
player 2	443	658	911	1007	1023	1035	1050	979	888	1022
player 3	444	835	534	500	501	1069	1086	1098	758	939
player 4	**209**	**85**	**274**	**268**	**289**	**315**	**326**	**334**	**263**	**316**
player 5	675	438	1092	548	1128	1134	1138	1139	912	1135
team average									694	847

Meanwhile, the team in the most concentrated scenario contained two underachievers (players 2 and 4):

[18]One possible explanation for the authors' finding of statistical significance may be their reliance on period data — *i.e.*, multiple observations for each subject. Repeated observations from the same subject will be highly correlated. Thus, they do not represent independently drawn data points, as required by most statistical tests (including the Wilcoxen-Mann-Whitney test). Using such data has the effect of expanding the data set without adding a commensurate amount of new information. This results in a greater likelihood of finding a statistically significant difference between two groups when, in fact, none exists. Whether that is what happened here cannot be determined from the *Study* — which is a problem in itself. A proper scientific analysis should provide reviewers the opportunity to replicate results, which is impossible where, as here, a study fails to provide a description of the procedures used.

most conc.	period 1	period 2	period 3	period 4	period 5	period 6	period 7	period 8	average pd. 1-8	average pd. 5-8
player 1	573	723	854	744	913	900	915	916	817	911
player 2	**27**	**-44**	**298**	**-242**	**55**	**33**	**3**	**134**	**33**	**56**
player 3	238	479	673	678	891	543	960	454	615	712
player 4	**273**	**60**	**248**	**263**	**89**	**279**	**287**	**348**	**231**	**251**
player 5	326	417	836	980	1081	1129	1139	1141	881	1123
team average									515	611

With twice as many sleepwalkers holding it back, it is no wonder that the DBS team in the most concentrated scenario performed more poorly.

III. EVEN IF TAKEN AT FACE VALUE, THE EXPERIMENTS' RESULTS DO NOT SUPPORT ANY REGULATORY INTERVENTION.

Although virtually all of the *Study*'s results show that increased concentration has no statistically significant effect at all, the *Study* nevertheless attaches importance to the two results that it does find statistically significant. Based on these two results, the *Study* concludes (1) that "higher levels of horizontal concentration . . . led to a modest reduction in 'economic efficiency,'" *Study* 3, 49; and (2) that "higher concentration levels would negatively impact the DBS operator's bargaining position," *id.* at 34.

Each of these conclusions is strongly undermined by the *Study*'s many metrics that yielded no statistically significant results: because each of these values was designed to measure market performance, the two statistically significant differences among the mass of non-statistically significant differences are best dismissed as flukes. Even if they are not, neither difference can support regulatory intervention.

A. The Efficiency Conclusion Does Not Support Regulatory Intervention.

There are two main problems with the *Study*'s efficiency conclusion. *First*, the conclusion is simply not a fair interpretation of the data. The efficiency values for each of the three concentration scenarios were 93.0, 83.6, and 89.0, for the least, more, and most concentrated scenarios, respectively. Thus, although efficiency was lower in the more concentrated scenario than in the least concentrated scenario, efficiency was actually *greater* in the most concentrated scenario than in the more concentrated scenario. That fact is directly at odds with the conclusion that "*higher levels* of horizontal concentration . . . led to a modest reduction in 'economic efficiency.'" It shows that an increase in concentration diminishes efficiency in some stretches of the concentration spectrum, but enhances efficiency in others.

The *Study* ignores this on the ground that the difference between the least concentrated and most concentrated scenarios is not statistically significant — a conclusion that the *Study* also draws with respect to the difference between the more concentrated and most concentrated scenarios. But that is not convincing: to say that a difference is not statistically significant does not mean that a value does not exist — it means that the value cannot be viewed as different from the value to which it is compared. Plainly, by pointing in an opposite direction, the 89.0 value strongly undermines the certainty with which one can draw *any* conclusions. At a minimum, the 89.0 value negates the notion that an increase in concentration *always* diminishes efficiency. At best, then, the *Study*'s authors have shown that some concentration diminishes efficiency while, mysteriously, more concentration does not.

Second, and more fundamentally, even if the *Study* had succeeded in showing a small difference in efficiency, that showing still would not justify regulatory intervention. For one

thing, Congress has instructed the Commission to concern itself only with market failures that "impede . . . the flow of video programming from the video programmer to the consumer." 47 U.S.C. § 533(f)(2)(A). It did not authorize the Commission to impose a subscriber limit to fine-tune some ill-defined measure of efficiency. Plainly, "a modest reduction in 'economic efficiency'" is a far cry from an impediment to programming flow. Moreover, Congress specifically instructed the Commission to "account for any efficiencies and other benefits that might be gained through increased ownership or control." *Id.* § 533(f)(2)(D). Plainly, the Commission could not base a subscriber limit on perceived decreases in efficiency in programming acquisition without ascertaining that the decrease is not offset by an efficiency enhancement in some other aspect of the industry (*e.g.*, efficiency enhancements resulting from cable operators' ability to build clusters).

B. The DBS Results Do Not Support Regulatory Intervention.

Even if one accepts the experiments' results, the conclusion that "higher concentration levels would negatively impact the DBS operator's bargaining position" (*Study* 34) again is not a fair interpretation of the data. The *Study* found that there is a statistically significant difference between the least concentrated and most concentrated scenarios — but not between the least concentrated and more concentrated or the more concentrated and most concentrated scenarios. In other words, the conclusion that "higher concentration levels would negatively impact the DBS operator's bargaining position" is again literally unsupported: it holds true for only one of two elevated concentration levels.

Equally remarkably, the places here are reversed. Whereas the supposedly significant difference in efficiency (discussed *supra*, in Part III-A) was between the least and more

concentrated scenarios, the supposedly significant difference in DBS power is between the least and most concentrated scenarios:

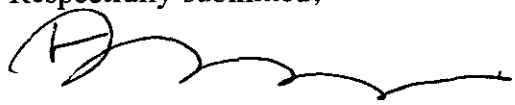
	Least Concentrated	More Concentrated	Most Concentrated
Efficiency	93.0	83.6	89.0
DBS Buyer Bargaining Power	53.6	47.4	42.[illegible]
DBS Buyer Surplus	50.6	46.5	40.[illegible]

The *Study* thus in effect concludes that, whenever there is a statistically significant difference between the least concentrated scenario and *either* the more concentrated *or* the most concentrated scenario, one can credibly conclude that increased concentration has bad effects — even if the measurement for the third scenario shows no statistically significant difference (or, for that matter, a statistically significant difference the other way). That kind of "heads I win, tails you lose" reasoning is not persuasive.

Conclusion

For the reasons set forth above, the Commission should attach no weight to the *Study*'s conclusions.

Respectfully submitted,



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