HU NTER
The City University of New York

Lisa M. George
Assistant Professor of Economics
lisa.george@hunter.cuny.edu

DEPARTMENT OF ECONOMICS
695 PARK AVENUE
NEW YORK, NY 10065
Phone: 212.772.5437
Fax: 212.772.5398

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Michelle Connolly
Chief Economist
Federal Communications Commission
445 12th St., SW
Room 7-C357
Washington, DC 20554

Subj: Peer Review of FCC Media Ownership Study (#3)

Dear Dr. Connolly,

As requested by your memo of August 7, I have reviewed "Television Station Ownership Structure and the Quantity and Quality of TV Programming," prepared by Gregory Crawford to support the ongoing FCC review of media ownership.

The study examines the effect of several television ownership attributes on the fraction of prime-time broadcast television minutes devoted to different programming types, as well as the effect of these ownership attributes on prime-time broadcast advertising minutes and rates. Overall, the study considers an interesting question with appropriate data and methods and should ultimately prove useful for policy purposes. I have three general comments related to the robustness of analytic results, the relationship between empirical estimates and conclusions, and theoretical assumptions about advertising. I outline my general comments below, following the guidelines described in your memo. I also attach a list of specific comments designed to improve the clarity and accuracy of the study.

Robustness of Analytic Results

While the regressions in the analytic portion of the study are consistent with standard econometric methods, the paper does not include specifications that would demonstrate the robustness, or reveal the fragility, of regression results. For example, all of the programming variables are measured as a fraction of prime-time broadcast minutes devoted to a topic. Advertising, however, is measured in raw minutes. No reason is offered for the difference in measurement, yet data transformations of this sort can affect results. The results would be

stronger if all regressions were run as both minutes and percentages. Cases where results substantively differed could then be further studied.

Another useful set of robustness checks would involve programming categories. The author makes reasonable assumptions in associating programs with categories. However, the study would be more informative if it identified whether or not regression coefficients are highly sensitive to those definitions. For example, the "Violent" programming category includes programs rated "TV-PG-V", a category that includes many nature and anthropological programs. It would be useful to run the empirical models using a narrower definition of violent programming and document how the results change. The "Minority" programming category would also benefit from studying alternate definitions.

In addition to checking regressions for robustness, the study should summarize and discuss the distribution of all estimation variables. In particular, the paper should include a table of summary statistics identifying means, medians, standard deviations and extreme percentiles for all variables in the regressions. While the study contains many data tables, it contains no detailed distributional information for estimation variables and no summary information at all for some variables. Distributional information can suggest problems with the robustness of results, for example identifying cases where regression coefficients are driven by a small number of outliers. Summary statistics are also useful in identifying whether the analytical models are appropriate for the data, for example by revealing heteroskedasticity or non-independence in standard errors.

Empirical Results and Conclusions

Although the analytic portion of the study might usefully be expanded along the lines discussed above, at the same time the paper contains extensive information only tangentially related to the effect of ownership structure. This extraneous material clouds the empirical findings of the study and strongly implies a set of results that are not, in fact, supported by the research. For example, the empirical analysis does not include cable television, yet the paper discusses cable television at great length. Similarly, the paper includes text and tables concerning viewership and ratings, yet no ratings data are included in the regressions. The regressions also consider only prime-time hours, yet this caveat is rarely mentioned. As written, the reader might easily assume that the results of the study apply to cable as well as broadcast TV, that the research shows that ownership measures affect program ratings, or that the results apply to daytime as well as prime-time coverage.

A better approach would be to discuss cable television, ratings, and other measures not covered in the regressions only in the context of the analytic results, such as considering which ownership results might hold or not hold for cable television. Including a discussion of this sort *after* the empirical results and taking care to distinguish what is supported by the analytics and what is not would be both more insightful and less likely to generate overly-broad conclusions. The authors should also take care to frame the paper appropriately in the title, introductory material and conclusions. One approach would be simply to state that the paper examines the effect of several television ownership attributes on the fraction of prime-time broadcast television minutes devoted to different programming types, as well as the effect of these ownership attributes on prime-time broadcast advertising minutes and rates.

Theoretical Assumptions about Advertising

While I find the analytic results on advertising minutes to be interesting and expect them to be useful for policy purposes, the assumption that advertising is inversely related to quality cannot be justified in light of existing economic theory. An important idea in the economics literature on two-sided markets is that advertising in media markets functions like a price. In other words, viewers "pay" for broadcast television with advertising minutes. Just as a better steak costs more than a lesser cut and thus commands a higher price, a better television program typically costs more than a weaker program and would be expected to command *more* not less advertising time. While the price analogy is overly simplistic for a number of reasons (e.g. transaction costs might preclude varying advertising minutes by individual program, and prices for advertisements might vary rather than minutes), the strong potential for a positive correlation between advertising minutes and program quality must be taken into account. In terms of the paper, nothing in the regressions is invalid. However, the frequent association of higher advertising minutes with a worse experience for viewers is not accurate. A better approach would be to present the relationship from the regressions as interesting independent of the underlying link to quality.

To supplement my general comments, I attach a list of specific points designed to improve the clarity and accuracy of the study. Thank you for the opportunity to review this research. Please let me know if there are questions.

Sincerely,

Lisa George
Assistant Professor

Attachment

Peer Review of Gregory Crawford's "Television Station Ownership Structure and the Quantity and Quality of TV Programming," comments by Lisa M. George.

1.   Programming is measured as a fraction of prime-time broadcast minutes devoted to a topic, while advertising is measured in raw minutes. Data transformations of this sort can affect results. It would be informative to run all regressions for both minutes and percentages. Anomalous findings should be discussed, although perhaps only one consistent set need be presented.

2.   Table 15 & 16 show substantial differences in ownership and programming between "Big 4" and "other" stations. It is not clear how these differences affect regression results. It would be especially helpful to know how much of the variation comes from "other stations". Summary statistics might be useful, but running the regression results separately for the two categories would be most informative.

3.   While the paper contains many data tables, it does not include distributional information for key variables in the regressions. A table of sample statistics should identify the mean, median, standard deviation and key percentiles for all the variables included in the regressions. Without such data, it is difficult to fully evaluate the results. Are there outliers in the data? How are they handled? Are the distributional assumptions for ordinary least squares regression valid, or should robust standard errors be used? The sample statistics should include the affiliate dummies, which are not summarized in any of the tables. It would also be useful to include minutes as well as percentages.

4.   The role of geography in the paper is not well explained, yet its handling may affect results. The regressions include DMA dummies, but are run at the station level. Some markets thus have more stations than others. How much are results influenced by the largest markets? Should standard errors be clustered by DMA? Also, while DMA fixed effects account for time-invariant market attributes, there remains a potential for time-varying market attributes to affect programming. For example, markets with a rapidly growing Hispanic population may see more Hispanic programming. The relationship between stations and markets and the potential for unobserved market attributes to influence results warrants more detailed discussion.

5.   The programming data in table 10 is particularly important because these variables appear in the regressions. This table (and other like it less central to the empirical results) are difficult to follow. The percentage data do not appear to add up to 100%, which may frustrate the reader who does not immediately recognize that the categories may not be mutually exclusive. The paper would be much improved by limiting the tables to categories included in the regressions and identifying areas where percentages do not add to 100%.

6.   Very little attention is devoted to interpreting regression coefficients other than noting their statistical significance. Some perspective on larger and smaller effects, robust and fragile estimates, and percentages versus minutes would help the reader understand the findings.

7.   Tables 23 & 24 seem identical – one is likely an error.

8.   The interpretation of the "Spanish Language Affiliate" dummy on the right hand side of the equations requires some consideration and explanation, especially with Spanish language programming on the left hand side.

9.   The number of significant digits in the tables should be increased to show differences in coefficient estimates. As it stands, it is not clear how the numerous "0.00" entries should be interpreted.

10. The program categories developed by the author seem reasonable. Nonetheless, the study would be improved by demonstrating that results are robust to alternative definitions. For example, the threshold for violent programming is set rather low, at TV-PG-V, and it would be helpful to know if the results are similar for a higher threshold.   Some examples of television programs in different categories would also be very useful.

11.   The ownership variables which are central to the analysis are not well described. This is especially surprising given the extensive discussion of other variables which are not included in the regressions. What are some examples of "local" stations? Of non-local ones? Some stations are owned by content providers – how do these stations affect results? The controls for parent corporations are especially confusing. What exactly are parent-corporations, and how do they fit into figure 1? How many stations share a parent? Should the regressions include parent fixed effects? It is difficult to interpret the parent coefficient estimates without some context on these firms. In particular, if programming among stations owned by a single parent are correlated, then assumption for ordinary least squares regressions might be violated and modifications to the model might be needed.

12. Despite considerable space devoted to definitions of networks, stations and content providers, the concept of "station" used in the regressions is not entirely clear. Are channels the same as stations? If not, what do the channel fixed effects represent?

13. The aggregation of stations into networks is quite difficult to follow. Given that the regressions are done at the station level, it does not seem necessary to discuss aggregation in this study.

14. Tables 3 & 4 state that they link program types in the raw data to those used in the estimation. However, the estimation data do not include these program types, but rather more general categories such as family programming and violent programming. It would be more useful to see tables directly related to the estimation. For example, it would be useful to know more about the "violent" category, such as the fraction of violent programs that are educational, that concern public affairs, or are dramas or movies.

15. The fact that the results apply only to prime-time programming should be emphasized earlier and more visibly throughout the paper.

16.   The analytic results on advertising minutes are interesting and likely to be useful for policy purposes,. However, the assumption that advertising is inversely related to quality cannot be justified in light of existing economic theory. An important idea in the economics literature on two-sided markets is that advertising in media markets functions

like a price. In other words, viewers "pay" for broadcast television with advertising minutes. Just as a better steak costs more than a lesser cut and thus commands a higher price, a better television program typically costs more than a weaker program and would be expected to command *more* not less advertising time. While the price analogy is overly simplistic for a number of reasons (e.g. transaction costs might preclude varying advertising minutes by individual program, prices for advertisements might vary rather than minutes), the strong potential for a positive correlation between advertising minutes and program quality must be taken into account. In terms of the paper, nothing in the regressions is invalid. However, the frequent association of higher advertising minutes with a worse experience for viewers is not accurate. A better approach would be to present the relationship from the regressions as interesting independent of the underlying link to quality.

17. To some extent, the argument above applies not only to advertising but ratings or also to viewership. (Higher viewership for a particular program would only indicate quality if advertising minutes were equal.) However viewership is not included in the regressions, so biased coefficients are not a concern.

18. The first and most important paper on two-sided markets and their application to media is Anderson and Coate (2005). This paper should be cited in addition to the handbook chapter. The author may wish to relate his findings and methods to other empirical studies examining the effect of ownership measures on media content such as Waldfogel (2001) and George (2007). See bibliography for complete references.

References

Anderson, Simon P. and Stephen Coate (2005). "Market Provision of Broadcasting: A Welfare Analysis." *Review of Economic Studies*, Vol. 72, No. 4, pp. 947-972.

Berry, Steven T. and Joel Waldfogel (2001). "Do Mergers Increase Product Variety? Evidence from Radio Broadcasting." *The Quarterly Journal of Economics*, Vol. 116, No. 3, pp. 1009-1025.

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