

**Reply to Peer Review for
“Local Media Ownership and Viewpoint Diversity in Local Television News”
Adam D. Rennhoff and Kenneth C. Wilbur
June 20, 2011**

The peer review for our study made many positive comments, but also offered several comments that we were unable to understand; several that were demonstrably false; and several that did not provide enough detail as to how they could be implemented or if they were feasible. Our understanding is that, procedurally, it is our decision as to whether we revise our study in light of our peer reviewer’s comments. We have revised our paper, however the primary source of our revisions is not from the present peer review.

The other paper we wrote for the media ownership review, “Local Media Ownership and Local Media Quality,” was reviewed by Jeffrey Wooldridge, who literally wrote the book on panel econometrics. His methodological comments were excellent and helped improve the analysis of that paper significantly. That study’s econometrics shared many features with this study, so we decided to revise “Local Media Ownership and Viewpoint Diversity in Local Television News,” where applicable, in accordance with Wooldridge’s methodological comments. A detailed reply to that peer review was written and explains how we responded to Wooldridge’s comments and why; many of those responses explain the changes we made to this paper also.

Our adoption of Wooldridge’s suggestions does not preclude us from also using suggestions in the current peer review to improve the paper, but we found few opportunities. All of the following direct quotes were taken from the review, in italics.

“The authors try to separate out the demand side factors so that they can construct a supply side measure of media diversity. They do this with a structural model based essentially on comparing relative viewership on local versus national news.”

We are explicitly estimating a reduced-form model, not a structural model. The theory only provides a functional form to construct a viewpoint diversity metric from the viewing data. This is very different from using a structural model.

“The measure of diversity, then, is an absolute measure of media diversity (i.e. based solely on differences in perspective across locations independent of differences in preferences across locations).”

We presume the clause in parentheses was a typo, but we are unable to discern what it was intended to mean.

“the identification also relies upon...assumptions that viewership is solely decided upon using political perspective as opposed to quality”

Nowhere in our paper did we assume that viewership relies on political perspectives as opposed to quality; the reviewer imposed his own interpretation on our assumption of a single dimension of news program differentiation. As the paper stated (including the footnote), “Assume that the

programs are differentiated by a single dimension of viewpoint diversity, as in Hotelling (1929). The range of possible viewpoints can then be represented by a single horizontal line, and the viewpoint expressed by each program j in market m may be represented by a point x_j^m on that line.¹ It is true that the theory does not admit the component of quality that is typically interpreted as vertical product differentiation, but it explicitly admits the component of quality that is typically interpreted as horizontal product differentiation.

"Assume that differences in metric of ideology should be measured as differences in inverses of readership from an associated normal distribution."

We are unable to discern the meaning of this sentence.

"Differences in viewership shares lead to larger differences in measured ideology when the viewership shares are very unequal (i.e. 10% and 60% as opposed to 35% and 35%); however, this is not true, for example, if a uniform distribution is used. Moreover, it is not clear that the preferences over the ideological component of demand for news follows a normal distribution."

We agree; we noted that "The Normal distribution is less tractable than the typical assumption of uniform preferences but is more realistic." The reviewer may have had other distributions in mind as better candidates than the Normal, but he did not suggest them. Surely he would not(?) mean to imply that the uniform distribution is better suited to empirical work than the Normal. We now include the distributional assumption in an expanded discussion of the measure's limitations in section 2.5.

"The assumption of normally distributed political preferences with a common metric is probably incorrect (and verifiably so - i.e. if one media market has high viewership for national news 1 and 3 and another media market for 2 and 3, they would be incompatible)."

We do not understand this comment. Our model explicitly allows for market-specific means and variances in preferences for national news.

"Instead, I would argue for a more 'non-parametric' approach which partials out national media by controlling for national media viewership rates."

We would agree with this comment if we thought it were feasible. We searched hard for tractable models under less restrictive assumptions in the course of writing the paper, as we explained in depth in section 2.5. We would have been happy to implement the reviewer's suggestion, but he did not provide enough detail to adequately explain if he had thought about it enough to know whether it was feasible or not.

Given that the national media data contain two degrees of freedom and our approach contains two parameters, it seems very difficult to argue that we could make fuller use of the data by relaxing parametric assumptions.

¹ Higher-order viewpoint spaces are not considered because the available data do not allow for the nonparametric identification of additional dimensions of program differentiation. The single line in the model could be thought of as the first principal component of a higher-dimensional viewpoint diversity space.

"It wasn't clear to me how the decision of what is considered media outlets 1, 2, and 3 are."

The paper discussed this at length. It contained all of the following direct quotes:

“Consider a market m that is served by three local news programs, indexed in order of ascending market share by $j = 1, 2, 3$.”

“Assume for simplicity that the programs are ordered such that program 1 is closest to the left side of the line and program 3 is closest to the right side of the line.”

“It is assumed that all three national news programs are available in many local markets and are indexed with $k \in (A, B, C)$ in ascending order of national audience share.”

“For simplicity, assume national news program A is closest to the left side of the line and program C is closest to the right side of the line. Note that national news program A does not necessarily correspond to local news program 1, and that the two positions of the national and local news programs on a particular station need not be correlated. “

"What about benchmarking diversity measures with Fox, CNN and MSNBC viewing? These should be available in all but a few localities."

Indeed, this was our original intent. Unfortunately, the data provider did not provide local market viewing of national cable networks, despite our repeated and persistent inquiries. We now note this in the data section.

We could go on, but this list should suffice to show why we made few changes in response to this reviewer's comments. For a description of Jeffrey Wooldridge's comments on our original econometric technique, and our revised approach, please see the materials pertaining to peer review for FCC Media Ownership Study #1.