**Reply to Peer Review for "Local Media Ownership and Media Quality"**
**Adam D. Rennhoff and Kenneth C. Wilbur**
**June 12, 2011**

We are fortunate that our study was assigned a reviewer with such deep methodological expertise. He made many very good points which led to some qualitative changes in the conclusions, and we acknowledge his contributions in the revised version of the paper.

A few points of disagreement are discussed below. We are comforted that these disagreements mainly refer to understanding of the institutional context in which the regressions take place. The reviewer has not worked in this domain extensively andhis comments on variable definitions below indicate a lack of familiarity with commonly available data in this area, so we do not hesitate to rely on our own expertise in these questions. However, we defer to him on methodological questions and have updated our approach, presentation and conclusions to reflect his suggestions.

Unabridged reviewer comments are provided in italics.

*The topic of this paper is important: Does the structure of media ownership affect the quality of media provided? In particular, does local ownership in media – radio, television, and print – have an influence, one way or another, and variety and quality?*

*The paper begins with a valuable overview of the regulatory environment that affects ownership of various media. Because many of these regulations appear to be binding, the authors rightly point out that there may be scope for improving the quality of media by adjusting regulations. The paper does not directly look at the effects of regulatory environment on media quality, and rightly so: because the regulations take place at the national level, there is insufficient variation in regulations to identify their effects.*

*Instead, the authors collect a panel (longitudinal) data set on ownership structure and various measures of media quality, and attempt to find relationships. They are careful to admit that because owner structure is likely endogenous to media quality, at best they hope to describe relationships without necessarily inferring causality.*

We thank the reviewer for his many positive comments.

*Even though the authors are correct about the difficulty in determining whether ownership structure has a causal effect on media quality, I think the authors could have given the data a better chance.*

*Before looking at the data, the authors do summarize the underlying economic theory, and essentially conclude that – according to existing models – the theoretical effects of local ownership on media quality are ambiguous. This is fine, but I would have preferred to see the discussion made in the context of writing down an econometric model. The authors are aware of the idea that certain panel data methods allows heterogeneous market effects to be correlated with ownership structure, but that the most common methods do not allow shocks to media quality to be correlated with market structure. I think these important points could have been discussed most effectively in the context of equation (1):*

$$y_{mt}^{q} = \alpha_m + \alpha_t + x_{mt}\beta_q + \varepsilon_{mt}^{q}$$

*where the $\alpha_m$ are the market effects, the $\alpha_t$ are the time effects, and $x_{mt}$ includes the variables measuring local ownership.*

This point is mainly about writing style. We tried to tailor the paper's writing to its intended audience, policymakers. As such, we have not implemented this suggestion, but we believe it would be appropriate if the paper were intended for econometricians.

*(As a notational point, $\alpha_m$ and $\alpha_t$ should also have $q$ superscripts as they are allowed to vary with the measure of media quality.)*

We thank the reviewer for pointing out this typo.

*Using standard panel data methods – fixed effects (FE) or first differencing (FD) – $\alpha_m$ is allowed to be correlation with $x_{mt}$ . Because time effects are controlled for, the model (1) also accounts for secular changes that affect all markets. The drawback with FE and FD is that they are systematically biased if the shocks $\varepsilon_{mt}^q$ are correlated with the covariates in any of the time periods. (As discussed in Wooldridge (2010, Chapter 10), this is known as the "strict exogeneity" assumption.) One could imagine that shocks to media quality this year could feed back and affect ownership structure in subsequent years.*

*The authors are correct to forge ahead with methods that allow correlation between $\alpha_m$ and $x_{mt}$. But their choice of first differencing is based on faulty reasoning. On page 8 they write, "The market-specific fixed effects in equation (1) are problematic because they are too numerous to estimate with the available data." This comment shows a lack of understanding of fixed effects estimation. The modern view is that the so-called "fixed effects" (or "within") transformation removes $\alpha_m$ by removing time averages. Like FD, the FE transformation results in an equation that is free of the $\alpha_m$. FE simply uses a different transformation to eliminate $\alpha_m$. Trying the estimation both ways is actually a good way to determine strict exogeneity of the covariates: if the assumption is true, the FE and FD estimates should differ only by sampling error. In both the FE and FD cases, the standard errors and statistical inference should be made robust to any serial correlation and heteroskedasticity in $\varepsilon_{mt}^q$; the authors do compute these standard errors for the FD estimators.*

*It also can be useful to obtain the $\hat{\alpha}_m$ from FE even though, with only T=3, these are noisy estimates. One can still estimate the mean and standard deviation of the heterogeneity distribution, and it is of some interest to know whether it is spread out or tightly centered about its mean. Wooldridge (2010, Chapter 10) contains further discussion.*

The reviewer interprets our quote from page 8 in a manner that is inconsistent with its intended meaning, but this is a minor quibble and perhaps can be attributed to our writing. We did not include the fixed-effects approach to removing time averages because, as the reviewer notes, it produces estimates that are less efficient in the presence of serial correlation, so we thought it was dominated by the FD approach. However, we did not consider comparing the FD and FE estimates to check for strict exogeneity. In the new version of the paper, we have included the FE estimates. They mostly strengthened the paper's conclusions.

*The authors are away of the Arellano and Bond (1991) (AB) approach but opt not to use it. I agree that applying AB with only three time periods is pushing it, but it can – and should – be done. Under a "sequential exogeneity" assumption – see Wooldridge (2010, Chapters 10 and 11) – one can use (1) differenced between periods two and three and one and two:*

$$y_{mt}^{q} - y_{m,t-1}^{q} = \gamma_t + (x_{mt} - x_{m,t-1})\beta_q + (\varepsilon_{mt}^{q} - \varepsilon_{m,t-1}^{q}),\ t = 2,3$$

*and then use* $x_{m,t-1}$ *as instruments for* $(x_{m,t} - x_{m,t-1})$*. Provided there is sufficient correlation between the changes and* $x_{m,t-1}$*, the IV estimator is consistent under the assumption*

$$Cov(x_{ms}, \varepsilon_{mt}^{q}),\ s \leq t;$$

*this is a version of sequential exogeneity.*

This is a point of disagreement. Before we address it, we acknowledge a weakness in the exposition of our paper, which may help to explain why the reviewer raised the point. We presented two brief arguments against the application of Arellano/Bond in this study: (1) potential weakness of the instruments and (2) potential violation of the instrumental variables exogeneity assumption. The reviewer basically says that argument (1) may be flawed and may be easily checked in the data. His point is correct and, reasonably, one might expect the instruments to be sufficiently strong for application of Arellano/Bond (AB). In retrospect, we should not have presented this first argument in the paper.

The second argument, however, remains. Application of AB requires assuming that ownership variables in period t-1 are uncorrelated with shocks to media quality in period t. (We believe the reviewer meant to write $\mathrm{cov}(x_{ms}, \varepsilon_{mt}^{q}) = 0, s \leq t$ above.) This requires assuming that owners of media stations are not able to forecast changes in supply or demand for media within their markets. Given that media stations are valued using forecasts of discounted future earnings, as are most businesses, and that these forecasts underlie their valuations and therefore sale prices, this assumption simply is not credible.

The exogeneity assumption is necessary for the validity of the AB approach. It is not testable and our understanding of the market leads us to believe strenuously that it is likely to be violated. Therefore, we have not implemented it.

Further, we are puzzled by the advice to implement something that is "pushing it"—our preferred approach to policy-oriented research is to take care and minimize the likelihood of drawing erroneous conclusions. This advice might be more appropriate for purely academic work.

We have improved upon our discussion of why AB was not used, and thank the reviewer for helping to point out the need to explain this fully and without extraneous arguments.

*The authors are incorrect is saying that condition (3) restricts the serial correlation in the errors. It does not: it is an assumption about shocks and current and lagged local ownership variables.*

Under the assumption that the $x$ variables are jointly determined with the $y$ variables, a discussion presented in the paper without objection from the reviewer, $\mathrm{cov}(x_{ms}, \varepsilon^q_{mt}) = 0, s \le t$ implies assuming $\mathrm{cov}(y^q_{ms}, \varepsilon^q_{mt}) \ne 0, s \le t$. This does indeed restrict the degree of serial correlation allowed for within the errors.

*The authors can run first-stage regressions,* $(x_{m,t} - x_{m,t-1})$ *on* $x_{m,t-1}$ *to determine if the instruments are sufficiently strong. They might not be, but this is essentially an empirical question. It is even possible to allow* $x_{m,t}$ *and* $\varepsilon^q_{mt}$ *to be correlated in (1). Just use the differenced equaton from periods two three and instrument using the first period local ownership variables. Again, the first-stage regression can be used to see if the strategy is worth pursuing.*

These comments are only worth implementing under the IV exogeneity assumption discussed previously, which we strenuously believe does not hold.

*Some other aspects of the authors' econometric work are troubling. For example, the authors claim to employ the two-way clustering proposed in Cameron, Gelbarch, and Miller (2011). Yet the theory for these standard errors relies on large cross section and time series dimensions. With only two time periods in the FD equation, there is no way to justify two-way clustering. Thompson (2011) recently sketched the theory underlying two-way clustering.*

This is a valid point and we have removed the Cameron, et al. clustering approach from the paper. We may perhaps be forgiven for not being aware of Thompson (2011). We appreciate the correction but "troubling" seems a harsh adjective for this point, given that the paper was fully transparent about how the Cameron, et al. approach influenced the significance of the parameter estimates. Two sets of benchmark results were provided alongside the Cameron, et al. results.

*The description of the results in Table 2 are puzzling. On page 13, the authors write, "The first thing to notice is that the point estimates are virtually unchanged under all estimation techniques." In fact, there is only one estimation technique: pooled OLS estimation of the FD equation (2). The only statistics that should differ across the columns is the standard errors. So the single set of point estimates should be reported but three different standard errors. Actually, only two: the usual OLS standard errors and the clustered standard errors.*

This is a valid point and we have implemented the expositional suggestion.

*The authors comment on the 28% R-squared, but I am not convinced this is the correct measure. Two time intercepts are reported in Table 2, which suggests an intercept was not included (otherwise one time period dummy wouild drop out). If an constant was included, the reported R-squared is "uncentered." This is almost never the best measure because it assumes the mean of the dependent – in this case the change in the media quality variables – is zero. Similar comments can be made about the discussion on page 14 where the authors note the R-squared has increased. (Incidentally, there is no particular relationship between sample size and R-squared. If anything, it is easier to get a better fit with less data.)*

A linear combination of the two included year dummies produces a specification that is identical to the one the reviewer argues for, so it is unclear why changing the dummy structure would alter the results or fit statistics. To adhere to the reviewer's suggestions on FD and FE approaches to estimation, we used the dummy structure implied by the application of those approaches to equation (1).

*There are some other puzzling aspects of Table 2. My understanding is that there were 205 local markets and two differenced time periods. Yet Table 2 reports 2,050 "observations." It looks like this was gotten by pooling across the five media quality variables. This is incorrect. There are 205 cross section observations with two time periods. These are the data used to estimate equation (1) for each of the five responses.*

This is a valid point and we have implemented the expositional suggestion.

*The paper by Zellner on seemingly unrelated regressions appears in the references but is never mentioned in the paper. Did the authors do a SUR estimation but include equations in counting the observations? In this case, SUR is identical to OLS on each equation, and so each equation should have its own R-squared. In Table 2, the total sample size should be 410 for each of the five equations.*

This is a valid point and we have implemented the expositional suggestions.

*In looking at the figures in Table 2, I agree that it seems hopeless to tease out any patterns. Nevertheless, the authors should at least discuss what the magnitudes mean. Just knowing a variable has a positive or negative effect is not enough.*

This is a valid point. We now provide and discuss confidence intervals on the elasticities implied by the estimates.

*Also, the authors should think about different functional forms. For example, should some variables be in logarithmic form?*

Many different functional forms were considered at early stages of the analysis. They were found to not matter. We now discuss this in the paper.

*If a FE analysis is use, or the Arellano and Bond approach, the authors might try interactions among the explanatory variables, too.*

Given the imprecise estimates on the main effects of ownership variables, we disagree that interactions will illuminate the relationships of interest. If there were a theoretical basis for this suggestion, there would be a strong basis to consider it.

*As the authors recognize, there are likely important omitted factors in the analysis. I wonder if some measure of population could be found and included. It seems that population would be correlated with both the media ownership and quality variables. It seems that the variables need to be scaled in some way, or population controlled for directly.*

The reviewer seems not to have understood that population was used to scale three media quality variables. *NewspaperCirculation* was explicitly defined on a per capita basis, as noted in the paper. *LocalEveningRating* and *LocalNewsRating* are both explicitly calculated as fractions of television viewing populations. Two definitions of *RadioNewsStations* were considered to ensure that differences in market populations were not driving the results.

We considered scaling the ownership variables by the number of stations in the market, which should be roughly proportional to market size. However, we found this approach to produce misleading results. As we discussed in the paper, “All ownership variables are defined as count data. Percentage definitions were found to be misleading, as they are influenced by changes in the base number of television stations in the market. Small independent TV stations sometimes start or stop broadcasting, which then changes all cross-ownership and co-ownership percentage variables in the market. However, because these changes typically occur on the fringe of the TV market, they seldom indicate meaningful changes in station ownership concentration.”

*Median income might also be a useful control. I realize that matching up geographic reasons and years can be difficult, if not impossible, but it may be worth a try.*

As we discussed in the paper, we considered doing this, but a primary concern is using data consistent with other FCC ownership studies, and this variable was not reliably available for all markets in all time periods. Further, market-level demographics typically don’t vary over time so its effect would be nearly impossible to separate from the market-specific unobservables.

*In summary, the paper is interesting and the ambiguous results may very well be the best one can do – given the data limitations. But I think the authors can use better econometric methods and supplement the data they have.*

To reiterate, we thank the reviewer for his suggestions and believe they have greatly improved the paper. We were fortunate to be assigned a reviewer with such deep methodological expertise.

*Minor Point*
*The year intercepts for 2009 in the NewspaperCirculation equation are wrong in the last two columns. They should be identical to the first column, with different standard errors. It appears there was a transcribing mistake from the RadioNewsStations equation.*

This is a valid point, thank you for pointing this out.