Report on "Local Media Ownership and Media Quality" by Adam D. Rennhoff and Kenneth C. Wilbur

A Study Commissioned by the Federal Communications Commission

Comments by:
Jeffrey M. Wooldridge
Department of Economics
Michigan State University
East Lansing, MI 48824-1038
wooldri1@msu.edu

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.

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):

$$y_{mt}^{q} = \alpha_m + \alpha_t + x_{mt}\beta_q + \varepsilon_{mt}^{q} \tag{1}$$

where the α_m are the market effects, the α_t are the time effects, and x_{mt} includes the variables measuring local ownership. (As a notational point, α_m and α_t should also have q superscripts as they are allowed to vary with the measure of media quality.) Using standard panel data methods – fixed effects (FE) or first differencing (FD) – α_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 ε_{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 α_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 α_m by removing time averages. Like FD, the FE transformation results in an equation that is free of the α_m. FE simply uses a different transformation to eliminate α_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 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:

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

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

$$Cov(x_{ms}, \varepsilon_{mt}^q),\ s \leq t; \tag{3}$$

this is a version of sequential exogeneity. 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. The authors can run first-stage regressions,

$$x_{mt} - x_{m,t-1} \text{ on } x_{m,t-1} \tag{4}$$

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_{mt} and ε_{mt}^{q} 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.

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.

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.

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.)

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. 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.

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. Also, the authors should think about different functional forms. For example, should some variables be in logarithmic form? If a FE analysis is use, or the Arellano and Bond approach, the authors might try interactions among the explanatory variables, too.

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. 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.

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.

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.

Additional References

Thompson, S.B. (2011), "Simple Formulas for Standard Errors that Cluster by Both Firm and Time," *Journal of Financial Economics* 99, 1-10.

Wooldridge, J.M. (2010), *Econometric Analysis of Cross Section and Panel Data*. Second edition. MIT Press: Cambridge, MA.