

Review of
“Employment and Wage Effects of Radio Consolidation”
by Peter DiCola

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This study asks whether the consolidation of radio changes employment and wages of professionals working in the industry. Using data from the Occupational Employment Survey (OES) and BIA Financial Networks for the 1998-2003 period, the study concludes that consolidation led to job losses and that it reduced wages for the three professions studied here. In the author’s view, the paper demonstrates that “more consolidated markets have fewer radio announcers, news reporters, and broadcast technicians” and that “the employment effects of radio consolidation thus represent a threat to both localism and diversity.” (page 26)

In this review, I will follow the FCC guidelines for peer reviews. The guidelines ask reviewers to assess (i) whether the study’s methodology and assumptions are reasonable and correct; (ii) whether they are consistent with theory and econometric practice; (iii) whether the data are reasonable; (iv) and whether the study’s conclusions flow from the analysis.

To preview my assessment, I find that the paper represents a reasonable attempt to measure the employment effects of consolidation in radio. The study’s conclusions, however, do not follow from the analysis. In the most credible econometric models, the data show that consolidation in radio had no effect on employment and wages.

1. Methodology and Assumptions

The study seeks to measure the employment and wage effects of radio consolidation, suspecting that large station owners need to hire fewer employees. This is a good question to ask. Economies of scale of this sort are one reason why consolidation might have occurred in the radio industry. While an industry model with scale economies seems appropriate, a second critical assumption in the paper is more questionable.

Throughout the study, the author claims that reduced employment implies less localism. This is in fact the main motivation to study the link between consolidation and employment. However, the author does not show that reductions in employment are in fact linked to localism. I find it easy to imagine scenarios in which employment falls and localism increases. To use an example from the paper, an owner might consolidate two competing newsrooms in a market, reducing the number of reporters from 20 (assume that each newsroom had 10 reporters) to 15. In this scenario, localism might increase because the 15 reporters can cover a larger number of local stories. By consolidating the two newsrooms, the owner was able to do away with wasteful duplication (e.g., two reporters attending the same news conference). Similarly, centralizing research might

allow a large owner to better understand local preferences for music and news. There is no reason to believe that a centralized research organization must be oblivious to geographic variation in consumer preferences. As these examples show, the study's maintained assumption that reduced employment must lead to a decline in localism is far from obvious, and there is no empirical evidence in the paper that supports this claim.

2. Econometric Practice

The paper uses variation in consolidation over time to identify the employment effects of industry concentration. There are two concerns with this approach. A first is that it might look as if consolidation reduced employment when the correlation is in fact driven by factors that are unobserved by the econometrician. For example, during the study period, the number of listeners declined considerably. A shrinking audience will create financial pressures and it can lead to layoffs. At the same time, the poor financial performance of stations might also encourage consolidation. In this situation, it might look as if consolidation reduced employment when it did not. The author addresses this difficulty in three ways. First, the models include a time trend that is common to all markets. Second, some models include market fixed effects to control for time-invariant unobserved heterogeneity. These controls are important but they do not completely solve the issue. For example, the models presented in the paper do not account for market-specific changes in listenership. Recognizing this weakness, the author concedes that the paper does not identify a causal effect (page 3). Nevertheless, when discussing his results and in the conclusions to the paper, the author throws caution to the wind, describing the findings as if they were causal.

A second difficulty lies in the measurement of consolidation. The study uses the (mean) number of stations per owner as a proxy for concentration. This measure is market specific. In other words, the study asks if employment in a particular market falls if owners increase the number of stations they control *in this market*. From a localism perspective, this is not the relevant measure of consolidation. The claims about the negative effects of voice tracking, syndication and central programming all have to do with greater incentives to reduce local employment if owners control a large number of stations *elsewhere*. The models in the paper are silent with respect to these effects because they relate local employment to local ownership. To study the effects of interest from a localism perspective, one needed to ask if local employment responds more sharply if an owner who plays a larger role at the national level acquires a station.

3. Data

The paper provides a careful discussion of the limitations of the data that are used in this study (page 16). The list of limitations seems fairly complete. The inability to distinguish jobs in the radio industry from the same jobs in other industries is particularly important.

The paper uses data from 1998 to 2003. This is surprising because both the OES and the Media Access data sets begin in 1996 (page 16). Because consolidation occurred in

the wake of the 1996 Telecommunications Act, it would have been particularly interesting to include these earlier years.

4. Conclusions

Given the careful setup and the author's understanding of the limitations of the data, I was surprised by the conclusions of this study. As the author explains (page 19), it is particularly important to control for unobserved differences across markets. Thus, specification 4 (with market fixed effects) yields the most credible results. These estimates show there is *no significant reduction in employment*. In reaching his conclusions, the author focuses on the results without market fixed effects. The reasons for downplaying the fixed-effect results – “the data may be too incomplete,” “the fixed-effects models may ask too much of the OES data” – are not convincing. There is substantial within-market variation – in the average market, the number of stations per owner increased by 36% between 1996 and 2003 (page 23) – and the resulting estimates are more precise than the estimates in the models with only a time trend.

The finding that radio consolidation has no employment effects appears also consistent with the raw data presented in the paper. As the author explains (page 13), studying the effects of the Telecommunications Act of 1996 is promising because the Act led to significant changes in ownership concentration in a fairly short time. In other academic work, the Act has been successfully used to identify the effect of consolidation on programming variety (e.g., Berry and Waldfogel, *QJE* 2001). Against this backdrop, it is useful to study chart 2 in the paper, which shows radio employment per station from 1982 to 2002. There is no change in employment post 1996 that I can detect, an observation which is consistent with the fixed-effects results. The author's preferred estimates imply a decline in the employment of news reporters by 56% over the study period (-30% for broadcast technicians). Changes of this magnitude would likely be visible in Chart 2 if they were real. My own conclusion from this study is that local concentration had no impact on employment and wages.