Referee report by Stefano DellaVigna on
"Diversity in Local Television News"
by Lisa George and Felix Oberholzer-Gee
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This paper considers an important issue for media markets, the extent to which regulation of competition between media in a market is likely to affect the diversity of the news provided, and consumed, in a market.

The policy-maker is presumed to have an interest in ensuring diversity in a media market, so that citizens with interest in different news items, interest in different politicians, or interest in news regarding different locations are informed.

The topic is a hard one to tackle on both empirical and theoretical grounds. On empirical grounds, the difficulty is that we do not have access to the ideal data sources to provide evidence on the effect of the regulation of competition on the provision of diversity, and the consumption of diverse news. Ideally, an empirical researcher would like to observe random or quasi-random regulation of entry and exit into different media markets, and use this variation to estimate the effects on diversity.

To clarify, consider a hypothetical example in which the government limits media market consolidation in markets 1 through 20, but not in markets 21 through 40. Further, suppose that markets 1 through 20 (the "treatment markets") are randomly drawn out of a set of 40 markets, with the remaining markets forming the "control markets". Since the selection of treatment markets was random, the researcher needs not worry about the fact that the two sets of markets may be different due to other factors. In this hypothetical situation, the econometrician has a very easy evaluation task, which is to compare the average diversity in treatment markets, in which consolidation was limited, to the average diversity in control markets, in which consolidation instead was not limited.

In practice, an empirical evaluation is considerably more complicated. Of course, government agencies do not randomly vary regulation (nor we are advocating that they should, to be clear). In addition, in this case it is also impractical to find natural experiments, cases in which due perhaps to local institutions or details of a rule, different areas differ in the local norms applied.

The economist, therefore, in studying the impact of local competition on diversity is left with only one imperfect option, which is to compare local television markets which differ in the observed concentration of local media (by one measure or another) and evaluate how they compare in the observed diversity of news provided and consumed. Of course, this comparison runs in the obvious difficulty that media markets which differ in media concentration may differ in other ways, for example in their demographic composition, which means that the analysis of the impact on diversity of media provision can be biased. Hence, this is a tricky empirical study.

To deal with this difficulty, the authors of this paper resort to what is likely the best alternative strategy, which is to consider only local media markets which experience a change in the local number of stations and/or their concentration, and then examine whether these markets which change over time experience different changes in diversity compared to markets which do not change, or which change in the opposite direction. This strategy, which goes by the name of fixed effect estimation, is widely used for cases such as this. While it is not perfect – for example, it is possible that the districts which change in media concentration do so because of, say, changes in demographic composition --, it does tend to provide evidence which is more convincing than a pure cross-sectional comparison. I return to the empirical implementation below.

The second difficulty is theoretical. As the authors explain well in the paper, economic theory does not provide obvious guidance as to whether more or less concentration should lead to more or less diversity. On the one hand, a monopolist corporation which owns multiple stations (assuming that it keeps all the stations open) will tend to diversify the content across the stations more than in the case in which the stations are independently owned. (This is because if the stations were independently owned they would be more likely to produce similar content to steal business from the competing stations) On the other hand, a monopolist may tend to produce similar content in different stations to save on the production costs of the material. Fundamentally, therefore, economic theory does not provide the kind of clear guidance which can help to tighten an empirical test.

Against this background, the authors overall do a very careful job in the analysis of the topic, as I would have expected of two experts on media markets as both Lisa George and Felix Oberholzer-Gee are. The paper is thorough and clear in implementing the above-discussed fixed-effect strategy, looking at markets which change in media concentration.

The most impressive part of the paper is the careful collection of a novel data set (as far as I know) of diversity in local media markets. The authors apply a methodology used by others (Groseclose and Milyo *QJE* among the first and Gentzkow and Shapiro *Econometrica* among the others) to code automatically content in the media, and apply it to the analysis of diversity. In particular, the authors use the NewsBank database which contains transcripts of the news coverage by local news stations which are affiliates of major networks between 2006 and 2010. They then develop an automated measure of diversity based on these transcripts. While the authors do this to illustrate the effect of diversity in a number of dimensions, let me illustrate the effect using their first example, which is topic of coverage. Suppose that in market A all stations spend most of the coverage on crime topics, while in market B one station discusses mainly crime topics, while another station discusses education mostly. We would like to say that Market B provides more diversity than market A. The measure of diversity employed in the paper does so essentially as follows: it computes the standard deviation across news outlets in a media market of the share of news items devoted to crime. In market A, this standard deviation is very low because all stations spend time on crime, while it is high in market B. Notice that the same conclusion would follow if instead of measuring the standard deviation of coverage of crime we did so for coverage of education. In the paper, the authors develop measures along these lines for a range of topics, do so very thoroughly, and examine effects along a variety of diversity measures (content, politician coverage, and local coverage, among them).

In addition to this analysis, the authors also use micro data from Nielsen on news consumption to document not just offering of diverse news, but also consumption of diverse news. This latter part is important because the ultimate outcome of interest is presumably not the offering of diversity per se, but whether people take advantage of it.

One main result is that the available measures of concentration in local media markets are not generally associated with much variation in the measures of diversity. This conclusion suggests that changes in diversity may not be the primary impact of changes of market presence. This finding is qualified by the fact that the authors do find some evidence of impact on some aspects, for example for minority-related content for Hispanics.

This result is subject to several caveats. The most important, and my biggest objection to the paper, is that the estimates are actually quite noisy presumably because the variation in media concentration over time is limited. We can consider to illustrate this one of the conclusions of the paper on diversity by topics, on page 14. Citing from the paper, "*an additional station increases the standard deviation in media-related word shares by 0.0137. This effect is statistically significant and of economic importance. A one-standard-deviation increase in the number of stations (4.5) almost triples the mean level of diversity. By contrast, a one-standard-deviation increase in locally owned stations (1.7), while statistically significant, lifts the level of diversity only by about 30% of the mean.*" The second effect is not statistically significant despite the fact that it is associated with an increase of 30% in diversity, an increase of a size which I think regulators would care about. To say this otherwise, the empirical methodology to look at changes in a panel of media markets provide estimates which are informative, but quite imprecise. This is not a problem if the methodology estimates significant effects (which are bound to be large), but does imply that even effects that are not statistically significant can be economically important.

Why does the methodology lead to estimates which are not so precise? The reason is almost surely the fact that the change in the market concentration over time is quite limited, and some of the apparent changes may be due to measurement error rather than actual changes (this would bias the estimates towards zero). This is a classical problem in fixed effect estimates, which improve the cross-sectional estimates with respect to bias, but lead to less precision and likely more measurement error.

I have four constructive suggestions in regard.

First, while the authors recognize this problem in the very last paragraph in the paper, it would be useful to have a prominent discussion also in the Introduction and throughout the paper.

Second, when discussing the economic magnitudes of the findings in the paper, it would be very useful to have a discussion for the statistically insignificant results of what size effects can be rejected (that is, of the confidence intervals). Relatedly, the Tables, which are overall very well-structured, should add at the bottom a row with the mean of the dependent variable, so that one can more easily evaluate the size of the effects

Third, it would be very informative to have more information on the media markets which do change over time, and on the nature of the changes taking place. Are most of the changing markets the large ones? Or are they located in particular areas? Are the changes all of one type? Relatedly, I would much like to see more graphs on the changes occurring, such as time trends overall, and maybe an event-study type analysis which plots changes in diversity before and after a change in market concentration. I realize that the paper is long and that it already delivers much, but this is where I would have liked to see more evidence.

Fourth, it would have been interesting to see how the panel estimates differ from cross-section estimates (that is, without market fixed effects). While I agree that the cross-sectional estimates are likely biased, it would be useful to see which direction the bias goes.

A small comment is that I believe that is a typo on page 2 where the authors say "*By contrast, if the two stations are owned by different companies, the firms have only weak incentives to consider revenue cannibalization, suggesting that dispersed ownership might lead to greater variety.*" I believe the sentence should end in "*less variety*". The idea is clear in the context, but if this is indeed a typo it should be fixed.

Overall, I found much to like in this paper. The authors clearly put in a lot of effort, did a very comprehensive study covering several different measures of diversity, included as many media markets as possible, and attempted to cover also viewing diversity and not just supply of diversity. This study can, and should be used, to inform regulatory decisions.

I also pointed out limits to the study, most importantly that the estimates of the effects are quite imprecise. Many of the limits of the conclusions that one can draw from this study are limits of the data, rather than of this study. Still, I would have liked to see more emphasis as I detailed above on the nature of the variation over time of the different measure.