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Review for the FCC

Broadcast Ownership Rules and Innovation
Andrew S. Wise.

Overview

This paper provides a statistical examination of the deployment and use of multicasting. It focuses on using statistical methods to understand the role of market structure and regulatory rules in encouraging or discouraging deployment. The paper offers a theoretical review of the effect of market structure on innovation, using that review to develop hypotheses and guide the statistical exercise. It then offers an extensive statistical examination of the causes behind deployment and use of multicasting. The paper finds that a few aspects of market structure explain variance in the deployment and use of multicasting, such as more competition and greater market size. Little else, such as FCC ownership rules, seems to matter in the statistics.

Overall, the paper makes a clear contribution on a novel question, framing that question in terms of an established economics literature. The topic is sound, and the key question is reasonable. The writing is clear for the most part. The statistics are well done. The explanations are thorough and meet the generally-accepted standards for research in this area.

One general remark needs to be highlighted. From the viewpoint of a statistician, the paper takes advantage of a propitious combination of circumstances. The author deserves considerable credit for finding such a combination, developing it, and executing the research question. The author has overcome some empirical challenges that have stifled others.

Specifically, while plenty of theories about market structure and regulatory rules suggest they ought to shape innovative outcomes, and plenty of theories suggest that other factors matter more for the outcome, rarely do researchers get to test those theories. This is because it is nearly impossible to observe any relationship in practice. Real life is not a laboratory with test tubes using different treatments that a researcher can rearrange and compare. Rarely does real life provide a clear natural experiment for inferring causal relationships between innovation and its determinants. For similar reasons real life does not often allow for clear measurement of the economic importance of market structure or regulatory rules in sufficient variance to support estimating a statistical model. But this paper has found a situation that overcomes those challenges. There are multiple media markets across the country, and the structures of these markets vary considerably from one another. The setting is not entirely ideal, because markets are not entirely independent from one another, and the data provide indirect evidence in some important respects, but the paper notices this and is honest about the limitations.

Were the author to seek to publish this manuscript in a major industrial economics journal I would expect the editors to require a number of changes to the manuscript. My comments will focus on a number of areas in which the author could improve the manuscript for that audience and outlet.

Suggestions.

Introduction:

While the paper actually does issue a number of warnings, the author should be very cautious about claiming to examine the relationship between market structure and innovation. While the paper does express some appropriate caution, I think a little more precision would be useful, and for one particular reason. Innovation means many things to many people, and the word “innovation” gets thrown around rather carelessly in most conversation. There is no reason for this paper to contribute to that carelessness. Instead, it should set an example.

In particular, to some readers *innovation* means *invention*, namely, stretching the frontier of prototypes in directions that lead to developing new products or services, sometimes far in the future. To other readers the meaning of *innovation* is quite different. It means *commercializing* invention, namely, translating frontier science or engineering into a mass market product for which a profit-making firm can charge revenue to cover the costs of operation and production. To another group of readers *innovation* means something altogether different. It means *adoption*, namely, watching users make use of a product or service because they find value in it, and that product or service once was a frontier piece of science or engineering. This paper uses something closest to the middle definition. This paper looks at the commercial version of a service that once was a frontier piece of engineering, and it examines variance in supplier propensity to deploy that version. This paper has little to say about invention, and it makes only a number of indirect observations about user adoption.

Innovation theory:

By the norms of published research this section of the paper wanders far afield, and into areas that largely do not matter for the research goals of the paper. I think the one cause is the lack of precision about which type of innovation this paper examines, similar to the comment above. If the paper recognized that it is addressing a specific question, then it could focus on reviewing the literature for that topic. It would not need to review all the writing about innovation. Said another way, because the existing writing about innovation sprawls into many topics unrelated to this paper’s research goals, the paper’s literature review follows that sprawl (in a somewhat meandering way) into areas that really do not matter for the paper’s goals. For example, modern growth theory (e.g., Grossman/Helpman, etc) might have motivated the author at one time, but is hardly relevant for understanding what happened with multicasting and why broadcasters made use of multicasting.

This section also does not go deep in precisely the right way. In that sense it also feels incomplete, especially about the policy literature on market structure and innovation. I would suggest the author examine two recent literature reviews about the relationship between market structure and innovation and antitrust policy, one by Rich Gilbert (<http://www.nber.org/books/jaff06-1>) and the follow-up by Jon

Baker (http://papers.ssrn.com/sol3/papers.cfm?abstract_id=962261). Though focused more on antitrust policy than regulatory policy, these two papers cover most of the general economic issues about the relationship between market structure and innovation and policy. Those papers also help the author sharpen the statements of the Schumpeterian hypotheses and the alternatives.

Literature review:

As another example of a study of the relationship between market structure and the commercialization of innovation, the paper might want to cite the National Broadband Plan, particularly chapters 3 and 4, which discuss the importance of the number of competitors for the quality of broadband. Or, for simplicity, see Wallsten and Mallahan (http://papers.ssrn.com/sol3/papers.cfm?abstract_id=1684236).

Theoretical framework:

The model seeks to frame discussion of the specification of the exogenous variables. For that purpose it needs to make two improvements if it were to be published in a major journal. First, the discussion should be much more precise about the importance of (and difference between) fixed costs and sunk costs for supporting innovation (or for supporting multi-casting, more specifically). Many innovations require firms to expend costs once on technology-specific activities, which make such costs sunk thereafter. Other innovations require firms to expend resources at regular intervals in order to maintain a capital investment necessary to make the innovation useful, which makes those costs fixed. In this paper's static model the difference between fixed and sunk is not readily apparent. Yet, in practice it will matter for the deployment of multi-casting, and for the organization of budgets to support that deployment, so it might matter for the empirical results. For example, I think a careful discussion about fixed cost (instead of sunk costs) will generate a precise prediction about adoption by firms with larger sizes, and firms with large sizes will tend to arise in markets with a large number of viewers, generating a correlation between market size, firm size, and adoption (as found in this data).

Second, the paper argues that quality is endogenous to the quality chosen by other rivals, and it is endogenous to the extent of competition that each firm faces. While the discussion recognizes the correct point, and states it correctly in principle, the paper does not go on to discuss how that observation complicates empirical studies of firm adoption behavior. It is especially complicated in a setting with a small number of decision makers. For a general review of the entry literature, which uses models similar to those in this paper, see the review by Berry and Reiss, 2006, <http://www.stanford.edu/~preiss/hand-entry.pdf>. For a specific application, e.g., Greenstein and Rysman (<http://onlinelibrary.wiley.com/doi/10.1111/j.1756-2171.2006.tb00062.x/abstract>). This type of complication is one of the big reasons why standard statistical approaches today tend not to examine adoption behavior at the firm level. The statistical approach for estimating models of firm behavior is still approaching the frontier of research. Consequently, it is standard to examine the market level, as done by this paper. The paper can use that observation to explain why it does not examine firm level choices in detail. The paper also should identify the development of firm level models as an important and interesting open topic for future work.

Empirical models:

The author deserves praise for compiling an extensive list of exogenous variables. While there is nothing obviously wrong with the equations estimated by the author, the author could help readers understand them better if the author presented more information. At present the author does not present any tables of correlations between the exogenous variables, for example, so it is not possible to tell if this list of exogenous variables induce multi-collinearity in the estimation, which would reduce standard errors. One might worry that the present list of variables produces multicollinearity because it includes several variables that might grow in size together because all demographic numbers will tend to grow or shrink together as the sample varies between large and small cities.

Equations (7) and (8) also could be re-expressed in a different way, which would help readers understand what is identifying the main effects in the paper. The author could include total population as a control, modifying the other demographic variables to be expressed in per capita terms (such as African American, Hispanic, College plus, Over 25, etc). While the latter probably will not be identified in a fixed-effect regression, the advantage of the latter is that it then has one clear measure of market scale, which is population. Everything measured on per-capita terms then becomes a measure of market composition. Since the fixed-effect results are not particularly impressive (as it presently stands), there is little lost in sacrificing the fixed-effect specification in order to experiment with other specifications that take advantage of the cross sectional variance in a single year of the sample. Such a re-specification will not shape the key qualitative results unless variance in market size was essential for identifying the key coefficients (which is possible, but unlikely in practice). To reiterate the point: this is the type of robustness that a major journal would require for publication, so the author might consider doing it sooner rather than later.

Results:

The discussion of result shows a great deal of care, though I think the author goes a bit too far in giving almost equal billing to the results from the fixed-effect regression. It is not surprising that the fixed effect regression does not add much insight to the analysis. There simply is not enough variance over a short period of time to identify much.

Conclusion:

It is worthwhile to reiterate that this type of statistical analysis can identify the effects of variance in ownership, not rules that apply equally everywhere. In that sense, the paper must be cautious in its conclusion. The author does say this, but it is worthwhile to repeat these limitations in the conclusion.