Comments on "Bridging the Gap Between Horizontal and Vertical Merger Simulation: Modifications and Extensions of PCAIDS"

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Overview

The paper develops a new approach to simulating the effects of mergers. The principal innovation is to endogenize vertical elements as well as the more traditional horizontal ones. In particular, the model is designed to accommodate the possibility that a newly vertically integrated firm will choose to try to foreclose the downstream market by raising its rivals' costs (or worsening their downstream products by denying them an input). The empirical implementation of the approach suggests that the FCC's standard approach to analyzing this type of situation substantially underpredicted the consequences of the Comcast-Time Warner-Adelphia transaction from several years ago.

Overall, I think the paper contributes to an important, topical literature of wide interest to economists in both policy roles and academia. The technical implementation of the model appears correct, and the systematic incorporation of economic decision-making seems a clear advance on the models the author suggests have been used in the past.

However, I believe that the paper could be much improved relative to its current state. In particular, I found it extremely difficult to read and confusing in many instances. Fixing this narrative opacity should be the author's primary focus in future versions, though I also have more substantive questions. Namely, the model's structure appears to depart from intuition in important ways, and there is an insufficient amount of discussion of these departures' implications. In addition, the empirical implementation is not sufficiently detailed for the reader to develop a good appreciation for the approach's overall accuracy/superiority relative to others. Critically, that section appears to omit material allowing the reader to contrast the new method's predictions to those from alternative approaches. In addition, I would think that since a historical event is being analyzed there would be some capacity to gauge the different approaches' accuracy. I do not observe that taking place.

Suggestions

First, the Introduction is far too long and muddled at present. It goes on for nine pages, and it is not exactly obvious what the paper's specific contribution is for most of those. For example, per my reading, it is not until the top of page 8 that the author says what the paper will be doing. The foregoing pages provide a somewhat confusing summary of (a subsection of) the existing literature. I would strongly recommend a dramatic rewrite that clarifies things for the reader. There are certainly any number of different approaches to this task, but I think that the author should definitely give the reader a clear sense of what the paper will be doing inside of 2-3 paragraphs.

Second, while I appreciate that there is a lengthy discussion of vertical foreclosure in the communications-related policy literature, the paper ignores the fact that a number of less narrowly focused IO economists are working in much the same terrain. I think that the papers should include at least some discussion of how its approach differs from the recent work on developing a vertical upward pricing index by Steve Salop and Serge Moresi, which I believe has been published in the ALJ in recent months. Given the very similar subject matter, I feel like it's essential for this paper to discuss its similarities and differences to their suggested approach. In addition, Nate Miller (Georgetown Strategy) has a nice working paper on simulating the consequences of mergers in a bargaining setting. That's obviously a quite different setting than this one, but there is enough of a similar flavor of exploiting a calibrated model that it might be worth referencing.

Third, and more related to the actual technical character of the model, I think much greater discussion should be used to motivate the particular parameterization of the foreclosure question. The fact that it's a binary random variable with some fixed probability is obviously a huge piece of the model, but it's not one that is immediately intuitive. Moreover, it stands in quite strong contrast to the modeling approach taken in ongoing work by Yorukoglu, Crawford, Lee, and Whinston in their paper on vertical foreclosure among cable companies. Obviously, one requires far less data to implement the model in this paper. But that comes at non-negligible cost in terms of model realism. More discussion of why the model's predictions should be considered reliable despite the substantial abstraction from industry behavior would be very helpful.

Fourth, and somewhat related to my first point, the exposition of the technical material is difficult to follow. I would think that the equations must be able to be presented in more parsimonious, digestible fashion. For example, the aforementioned paper by Nate Miller does a nice job of parsimoniously presenting the essentials of the quantitative aspect of the paper. It's very clear what each of the different equations is capturing. In contrast, some of the equations here stretch out over 4 lines with inconsistent formatting and comparatively little discussion about what the essential/novel/relevant portions of the different equations are.

Fifth, and repetitiously, it is quite difficult to judge the convincingness of the empirical application of the modeling approach due to the confusing presentation of the results. An enormous number of tables are presented, but their elements are not well explained. Moreover, there seem to be repeated columns. For example, the Post-Merger columns under "Expect Margin" and "Margin" are always the same. Is this a typo? I really cannot straightforwardly tell what these results are telling me about the different markets and whether those numbers make sense. Furthermore, in the introduction, there is a suggestion that the predictions are quite different than those from more standard FCC approaches. However, I don't see those spelled out in the empirical section. Even more importantly, since these are historical events, is there a way to validate the different models' predictions? If there are significant differences in predictions across models, why should we believe one set over another?