Referee Report on IJIO-D-22-00489:

Beyond "Horizontal" and "Vertical": The Welfare Effects of Complex Integration

This paper makes an important contribution to the academic literature on merger simulation models, while at the same time developing a set of tools that will be of considerable value to competition authorities around the globe as they engage in merger enforcement. The analysis is carefully done and the presentation is clear. Bravo.

I would be delighted to support acceptance of a revised version that addresses the questions / issues listed (in no particular order) below, which should not be very taxing on the authors:

1. Throughout, EDM is treated as an efficiency. It is not! EDM is a procompetitive merger effect that is *endogenous* to the merger simulation model. An efficiency is a procompetitive merger effect that is *exogenous* to the model. It is critically important to draw this distinction and to make the discussion everywhere consistent with this distinction. For example, it could be misleading to readers to say (as you do) that hybrid mergers (my term) are almost always anticompetitive (on net) in your simulation runs. Hybrid mergers are almost always anticompetitive *absent efficiencies*. Classic analyses of purely horizontal mergers (e.g., homogeneous Cournot, differentiated Bertrand) show that these mergers are *always* anticompetitive absent efficiencies (or synergies). The qualifier "almost" in describing your simulation results should be understood as revealing that EDM effects alone are typically insufficient to render a hybrid merger competitively benign. This is not a good argument for ignoring efficiencies, but might be taken as such by readers who think EDM effects are all there can possibly be to consider on the "pro" side. In this context, it may be worth mentioning the result from an earlier Sheu-Taragin paper that UPP and EDM are typically close to a wash for purely vertical mergers. Failure to draw the EDM/efficiency distinction is probably the single worst shortcoming of the paper.
2. Relatedly, the paper repeatedly refers to upward pricing pressure (UPP) throughout. In my view, Werden's compensating marginal cost reduction (CMCR) is a much clearer analytic concept than UPP. Discussion of the CMCR would also help to emphasize the EDM/efficiency distinction. If EDM is not sufficient to make a vertical or hybrid merger benign, what is the magnitude of the CMCR that would make it so? Making CMCR calculations for every merger simulation seems fairly straightforward and easy to me (but I may be wrong) and showing the distribution of CMCRs for various categories of merger would be highly illuminating. For example, does the distribution of CMCR for purely horizontal mergers "stochastically dominate" that for comparable hybrid mergers?
3. A cancer lies at the heart of vertical merger analyses that feature Nash bargaining upstream. The problem is certainly not unique to this paper; it bedevils all such analyses. Still, I would urge the authors to address the problem, at least in an explanatory footnote. The cancer is that one of Nash's axioms is that bargaining is efficient, whereas vertical merger analyses routinely violate this axiom by assuming that the bargaining is over a linear price (rather

than, say, a two-part tariff). Papers that show how the outcome of strategic, sequential bargaining approaches the Nash outcome in the limit (Binmore-Rubinstein-Wolinsky, Gowrisankaran et al.) likewise assume efficient bargaining. The conclusion is inescapable, I believe, that vertical merger simulation models which feature Nash bargaining over a linear wholesale price upstream *have no sound theoretical basis*. Curing the cancer is outside the scope of the present paper, but I ask the authors to at least note its troubling existence.

4. Relatedly, the authors' simulation results depend on relative bargaining powers. This is common in this literature, so certainly not a particular "problem" for this paper, but it is still a problem. As suggested in point 3 above, if two-part tariffs were feasible, relative bargaining strengths would only affect the lump-sum component of the bargaining outcome, not the linear price—and it is effects on the linear price that is of keen interest to competition authorities. And why aren't two-part tariffs feasible, given that contract terms are set by direct bargaining between sophisticated parties? Would the authors please raise this point, perhaps in a footnote, and offer at least one concrete and credible reason why two-part tariffs (or more sophisticated non-linear pricing schedules) might not be feasible in the bargaining context? The non-public nature of contracting terms and possible opportunism? The need for marginal incentives to induce non-contractible effort? Something—anything?

5. Draganska et al. and other papers that follow in that line assume upstream and downstream prices are chosen simultaneously. This assumption greatly simplifies the analysis, but it is undeniably odd. Upstream demand is usually thought of as a derived demand, derived from downstream market conditions. Simultaneous pricing makes the concept of derived demand meaningless. Put differently, the assumption of simultaneous pricing runs counter to the tradition of treating prices in a vertical supply chain as being chosen sequentially, and using the equilibrium concept of subgame-perfection. I ask the authors to address this, perhaps in a footnote, and to say something *new* about the issue, beyond repeating Draganska et al.'s own rationalization. In particular, consider the following claim: Simultaneous pricing tends to overstate the adverse price effects of a vertical merger, as compared with sequential pricing, because in the former case upstream demand is "less elastic" given that downstream prices are treated as fixed and unresponsive to upstream price changes. True or false? If true, the author should express the caveat, perhaps justifying the simultaneous-pricing assumption as needed for tractability, and maybe noting that sequential pricing is an area for future research. If the claim is false, the authors should explain why.

6. From page 13, on the data generating process underlying the merger simulations: the authors "simulate markets by randomly sampling *shares* from a Dirichlet distribution…" (emphasis added) Please explain why it is legitimate to sample market shares, which are equilibrium outcomes, rather than sampling values for the primitives of the model, which give rise to those equilibrium outcomes? How might your simulation results (their distribution) be affected by this sampling choice? If the rationale for sampling shares is that shares are what competition authorities observe, that shares are the relevant inputs into the simulation model, then please explain why the Dirichlet distribution is relevant. Does the sample of mergers that competition authorities have reviewed follow a Dirichlet distribution?

7. Relatedly, from the next paragraph on p.13: “Our simulations focus on mergers that are more likely to have anti-competitive effects and to therefore come under agency scrutiny.” This selection process has at least a whiff of circularity to it. Given resource constraints, competition agencies typically have policies to screen merger applications to determine which ones merit closer scrutiny. Screening policies are based on an agency’s prior belief about the distribution of harmful mergers in the applicant population. Those beliefs might be updated in light of insights provided by the authors’ models. In any case, I’m not sure what the rationale is for this non-random selection process. Also, more transparency might be in order. What exactly are the selection criteria? (I couldn’t find them in the paper.) Are they something like exceeding the concentration thresholds to qualify the merger for a “safe harbor” according to the 2010 DOJ-FTC Horizontal Merger Guidelines? For vertical and hybrid mergers, do the selection criteria include something about the vertical component of the merger? What?