

Whither Merger Simulation?

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Editor’s Note: *The beginning of Dave Scheffman’s second incarnation as the Economics Bureau Director at the FTC might be regarded by some as a “reign of terror” when the topic was the utility of Bertrand simulations for unilateral effects analysis. Indeed, Scheffman seemed to almost single-handedly bring coordinated effects (warts and all) back into the merger analysis limelight as he “dissed” unilateral effects.*

A recent ABA brown bag on merger simulations included Luke Froeb, the current Bureau of Economics Director, and Greg Werden from the Antitrust Division—both of whom are particularly well-versed in the estimation and use of simulation models—as well as Scheffman. For those who wonder how the DOJ and the FTC view simulations as a methodology for evaluating mergers and for those who might wonder whether time has mellowed Scheffman’s views, cruising through the transcript—an easy read—should be helpful. I, for one, did not see any mellowing.

A few samples of the discussion follow. All three speakers agree that there is no real evidence—or maybe just no evidence—that Bertrand simulations are reasonable predictors of the post-merger effects. Werden and Froeb, however, conclude that if the simulation can predict the present (and its assumptions are, at least in a stylized way, consistent with what we know about the particular industry), it can in fact be used as a tool in merger analysis. While Scheffman seemingly agreed with that conclusion, the inability (in his view) of the model to take into account important competitive variables (such as non-price competition) certainly suggests that the Bertrand simulation is a tool worth leaving to rust in the tool box.

All three presenters seemed to agree that the Bertrand’s focus on price-effects alone is not a sufficient analysis. All agreed that the economist evaluating merger effects must also consider repositioning, entry, and retail-wholesale relationships, and other factors. But (in my read) where Werden and Froeb would use these facts along with the (correctly calibrated) simulation in a complete

evaluation of the merger's effects, Scheffman seemed to believe that the Bertrand model is so far removed from a model of competition that the use of the Bertrand simulation in merger analysis should have a weight close to zero.

Scheffman also opined that one substantial cost of using the simulations in merger analysis is that analysts (lawyers, and maybe even some economists) have supported or opposed a merger based on vague factual (or maybe, metaphysical) views on the closeness of the products of the merging parties, what Scheffman calls "squishy" facts. But Werden and Froeb are clear that for economists, all of the assumptions of the model must be vetted against the facts and the model's sensitivity to those assumptions tested before it can be usefully employed in merger analysis.

My take on this debate is straightforward. The general view that in the context of unilateral effects, the simulation model properly used can be a helpful and insightful tool in merger analysis seems unobjectionable. A simulation model that can capture the essence of price competition in an industry and predicts a non-trivial post-merger price increase tells us something about the burden that must be carried by other arguments—entry, repositioning, advertising, innovation, capacity constraints, etc.—if the merger is to be cleared. Scheffman is surely right that simulations are not a substitute for thinking. But simulations thoughtfully used can be the beginning of a careful and focused merger analysis.

—JOHN R. WOODBURY

MICHAEL BECKER: The topic today is merger simulation. I think today's presentation can provide us with an overview of the pros and cons of merger simulation and help enlighten us on where the agencies think the current state of the art is. We have three speakers today. The first two speakers are Greg Werden and Luke Froeb. Greg is Senior Economic Counsel at the Antitrust Division at DOJ and Luke is the Director of the Bureau of Economics at the FTC. Both Greg and Luke have written on merger simulations. I think it's fair to say they are viewed as proponents of simulation, and they'll be discussing some of the pros of merger simulation and some of the misuse too. The final speaker will be David Scheffman, who is Luke's predecessor at the FTC and is now with Vanderbilt University and LECG. David's role today is to play the critic of merger simulation.

GREG WERDEN: Merger simulation uses standard tools of economics to predict the unilateral competitive effects of proposed mergers. An oligopoly model is selected that reasonably reflects the nature of competition in the industry, and the model is calibrated using prices, market shares, and other observable quantities. The calibrated model is then used to compute the post-merger prices and shares that internalize competition among merging products.

Although merger simulation can be applied to many types of industries, the main application is with branded consumer products. In that application, all of the attributes of the products are held constant, and competition is strictly on the basis of price. A lot of other assumptions are conventionally made, including that marginal costs are constant and, in most applications, that retailers take constant percentage markups over wholesale prices.

The calibration of such a merger simulation is designed to make the model perfectly predict some "but for" world. That usually is the actual world observed before the merger, but it can be something else if the future is likely to differ significantly from the past, absent the merger. Calibration involves prices, shares, and demand elasticities. The prices and shares don't raise many issues, but the elasticities can cause a lot of problems. I'm not going to talk about estimation at all today, but there are a host of issues in the estimation of demand elasticities that can be very challenging.

The advantages of the merger simulation are, first, it provides quantitative predictions of the unilateral price effects of mergers. It's not the only way to do that, but I think it's the main way. It's also easy in a merger simulation to take account of marginal cost reductions from merger synergies and compute net effects on price. Finally, merger simulation can be a great tool for focusing an investigation by identifying critical facts: what things matter, what things don't, how things matter, and how much they matter. By facilitating the identification of vulnerabilities, merger simulation helps indicate how best to allocate the investigative resources.

Of course, there are serious limitations. One important limitation is that a conventional merger simulation requires an assumption about functional form for the demand curve, and the predictions are fairly sensitive to that assumption. My advice on that is to make the assumption that's the least advantageous to whatever side you're working for. Merger simulation does not, as it's generally practiced, allow the investigation of prospects for entry or product repositioning; they are assumed away. And it's perfectly possible that firms in the real world do not play the same strategies that they do in the model, so it's not necessarily the case that merger simulation tells you what's actually going to happen after a merger.

There are several things I call "non-fatal flaws" in merger simulation. These are problems I don't worry much about. One is that the models available for use in merger simulation cannot possibly capture every nuance of competition in the real world. But that really isn't necessary. Capturing enough makes the predictions good enough. Models are never going to be perfect, and that is not the goal. The price increase predictions are at best rough estimates, but rough estimates are better than no estimates.

Finally, merger simulation cannot predict the long-run evolution of an industry. It cannot say much about entry or product repositioning; it cannot say much about changes in marketing strategy. It indicates only relatively short-term effects: how prices will be adjusted by the merging firms after the merger, and how the non-merging firms will respond to those price changes.

There are also potentially fatal flaws in merger simulation. First, an improperly calibrated merger simulation doesn't predict the merger-induced price changes. While I think it's fairly trivial to calibrate a merger simulation properly, I have seen that not done. I don't mean done improperly, but rather not at all, which produces nonsensical comparisons between the predicted post-merger prices and the pre-merger prices. Secondly, doing a merger simulation requires a well-specified economic model of oligopoly, and the available models don't fit all the industries. Sometimes they don't fit very well at all. They may not explain how manufacturers compete; they may not explain how retailing works; and they may not explain the relations between manufacturers and retailers. If they are far enough off, the predictions may be useless, and the models should not be used.

In a merger simulation, the general practice is to start from prices, shares, and demand elasticities, and work backwards to what the marginal costs would have had to have been if those prices, shares, and demand elasticities were an equilibrium before the merger. This infers the marginal costs, and provides a set of inferred marginal costs that can be compared to whatever evidence there may be on actual marginal costs. This comparison is important. It indicates whether the model jibes with the real world in terms of the general level of intensity of competition. It's not uncommon that some of the inferred marginal costs are very implausible, and something has to be done about that. The something may be abandoning the whole project, but usually there are less drastic and more useful things to do.

The foregoing is all introduction. My basic thesis is that merger simulation should be disciplined, whether in the courtroom or outside, by the Federal Rule of Evidence 702 and the *Daubert* decision (*Daubert v. Merrell Dow Pharmaceuticals, Inc.*, 509 U.S. 579 (1993)) and its progeny,

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—WERDEN

which limit the courtroom admissibility of expert testimony in ways that I think the analysis done outside of court similarly should be limited. Translating Rule 702 into principles for economic testimony, there are basically three conditions for admissibility of expert economic testimony: First, the witness has to be an expert in the relevant field of economics, and I emphasize relevant field. Being an economist, even a Ph.D. economist, is not nearly enough, and the case law is starting to recognize that. Second, the testimony has to employ sound methods from the relevant field of economics. Under the Federal Rules of Evidence, whatever methods economics considers sound may be used in court. Finally, the testimony has to apply those methods reliably to the facts of the case, and that's really the most important thing. Merger simulation should be based on the facts.

My slides go through some case law precedent and offer some arguments based on this precedent. [Ed. Note: Slides are available at <http://www.ftc.gov/speeches/other/040129werden.pdf>] I won't go through them in detail, but there are a couple of interesting points I'll mention. Some antitrust cases have excluded economic testimony because the witness was not considered an expert. The one I find most interesting excluded testimony on the relevant market from a Ph.D. economist who had "no background in antitrust markets." (*Nelson v. Monroe Regional Medical Center*, 925 F.2d 1555, 1572 (7th Cir. 1991)) He was a health care economist, and it was a health care case, so it made some sense to use him as a witness. But in a concurring opinion, one judge in the Seventh Circuit found that he had no business testifying about relevant markets because he didn't know anything about the analysis of relevant markets. I think there will be a lot more decisions in which Ph.D. economists are not allowed to testify about certain antitrust issues because they don't know anything about those issues. I certainly hope we get decisions like that.

In my view, merger simulation testimony should be excluded if the witness doesn't have some experience doing merger simulation. A quote from physicist Werner Heisenberg captures well the issue here: "An expert is someone who knows some of the worst mistakes that can be made in his subject and who manages to avoid them." Luke and I learned through trial and error how to avoid doing merger simulations badly. The only way I know how to learn to do anything is to do it, and experience teaches what not to do.

Economic testimony has been excluded in antitrust cases for failure to meet professional standards in economics. Most such testimony was empirical analysis, but the same rationale would apply to merger simulation. As to whether merger simulation is a sound method, in a sense, the answer is simply "yes." Merger simulation uses sound methods from economics—standard oligopoly models. If estimation is done, it uses standard econometric techniques. All of that is theoretically sound. What we cannot say is that merger simulation is empirically sound; we cannot say that merger simulation accurately predicts the actual effects of mergers. We don't know. On the other hand, there is no evidence that any method accurately predicts the actual effects of mergers, so simulation is on par with other methods of analysis in that regard.

Daubert and progeny have taught us that economists have to go into court with the tools of economics. While the lawyers can get away with invoking presumptions based on market shares and case law precedent, expert witnesses cannot; they must have something else. Merger simulation isn't the only something else, but it's one possibility.

In the slides, I quote dicta from *Daubert* and *General Electric Co. v. Joiner* on the issue of fit. One of my favorite quotes, which I use quite a lot, is that a court should not "admit expert opinion that is connected to the existing data only by the *ipse dixit* of the expert." (*General Electric Co. v. Joiner*, 522 U.S. 136, 146 (1997)) Traditionally, I think a lot of expert testimony was connected to the existing data only by the *ipse dixit* of the expert, and one should be alert for that possibility.

Without a strong connection to the facts, the analysis ought not to be admitted in court or done out of court. Quite a few decisions have excluded expert economic testimony for ignoring important facts. Some of these involved empirical analyses, and some of them did not. My slides give examples that are fairly well known from the *Brand Name Prescription Drugs Antitrust Litigation*, as well as a couple that aren't quite so well known.

My bottom line is that every modeling choice, and there are a lot of them, has to be justified in some way. The justification can be that economic theory dictates that particular choice. For example, I think the only justification needed for the profit maximization assumption is that that's what economists assume.

One particularly interesting case, which I'm sure you've all heard about, is *Concord Boat*, in which the testimony of Robert Hall was excluded and a substantial damage award vacated because his oligopoly model was "not grounded in the economic realities of the industry." (*Concord Boat Corp. v. Brunswick Corp.*, 207 F.3d 1039, 1056 (8th Cir. 2000)) What the Eighth Circuit did in that case is exactly what courts should do when confronted with merger simulation. The court looked at what Hall did and found it just didn't match up with the real world. His model was a perfectly fine model, but just not the right model for that industry. And the way he calibrated the model, to use merger simulation terminology, did not make sense. His model said the defendant's market share wouldn't be more than 50 percent without engaging in the challenged practices, but the defendant's share was 75 percent before it engaged in any of the challenged practices. The court didn't think that made any sense, and neither do I, and a lot of money changed hands because it didn't make any sense.

A less well-known case, but one that has a very nice statement of what was wrong with the expert's analysis (or could have been wrong, since I don't know if it really was wrong) is the *Booksellers* case. Frank Fisher's testimony on how to reckon damages was excluded because it contained "too many assumptions and simplifications which are not supported by real world evidence." (*American Booksellers Ass'n, Inc. v. Barnes & Noble, Inc.*, 135 F. Supp. 2d 1031, 1041 (N.D. Cal. 2001)) Doing any kind of economic modeling requires making assumptions and simplifications. The court was looking for evidence that the assumptions made sense, and it didn't think it had enough such evidence. I have no idea what evidence it had, but I find heartening that it was asking the right question.

Of course, a perfect fit is not required; in fact, it's not even desirable. Trying to make the model fit all the facts makes it too complicated and no longer a useful model. It's not going to predict. In selecting a model, the totality of the evidence should be examined to see what can be learned about how competition works. If there is such a model, a model should be selected that fits the resulting understanding of how competition works. Of course, different people may look at these facts and come away with different views about how competition works. That's okay. *Daubert's* requirement of intellectual rigor doesn't mean that there's only one right analysis.

My bottom line is that every modeling choice, and there are a lot of them, has to be justified in some way. The justification can be that economic theory dictates that particular choice. For example, I think the only justification needed for the profit maximization assumption is that that's what economists assume. A choice can be supported by industry data; for example, observed prices and shares are used to calibrate a model. Assumptions also can be consistent with stylized facts. If the industry sells differentiated products, it shouldn't be hard to muster facts that buyers and sellers perceive the product as differentiated—that brands matter. Or, an assumption can be unimportant. It may be fairly easy to show that nothing much changes from tweaking a particular assumption a little. If that is so, no further justification is required.

—WERDEN

Finally, absent any of the foregoing, some kind of sensitivity analysis is required to show how the predictions depend on the assumptions. Some assumptions are very important: Changing them changes the predictions substantially. With such assumptions, it is best to take a conservative course. Testifying for the government, I would always go with the low range of price increase

predictions, depending on different assumptions like demand functional form, ranges of elasticities, and things like that.

Any sort of economic analysis, and we're talking about merger simulation today, has to be based on something, largely the facts, and Luke and Dave are going to basically continue with that theme.

I think the discipline of modeling, and the link that it exposes between the evidence in a case and the conclusions drawn, are its main virtue. A model forces the economists to "put their cards on the table."

—FROEB

LUKE FROEB: We've been doing merger simulations for at least a decade, and we've learned a lot of things by trying to actually model mergers and fit models to the facts of the industry. There are a lot of surprising things, perhaps the most basic of which is that the merger effects are so small. When we started out doing this, we had some very big mergers, and it just was inconceivable that we were getting such miniscule effects, given the elasticities and all the assumptions that we had made. We've fit models to industries that are capacity constrained, and we've learned that capacity constraints on the merging firms matter a lot more than the capacity constraints on the non-merging firms. Capacity constraints on the merging firms attenuate the merger effect by much more than capacity constraints on the non-merging firms amplify them.

I think the discipline of modeling, and the link that it exposes between the evidence in a case and the conclusions drawn, are its main virtue. A model forces the economists to "put their cards on the table." Attorneys can then challenge a model by challenging the evidence that feeds into it, or they can challenge the link between the evidence and the conclusion. A model should be used to interpret the evidence, not to substitute for evidence in a case. That's my basic outline, and I'm going to illustrate some of the principles that Greg has discussed. [*Ed. Note:* Slides are available at <http://www.ftc.gov/speeches/other/040129froeb.pdf>.]

Let me just start out with some definitions. The "back end" is the use of a structural model, meaning we actually have models about how firms, consumers, and retailers behave, and we have an equilibrium notion about how they all interact and what happens. The "front end" involves the parameters that feed into the models, which come from evidence or estimation. Pre-merger, we observe an equilibrium and post-merger, we're predicting an equilibrium.

Antitrust, or certainly the model side of antitrust, hasn't been in the prediction business for very long, and we can learn from our sister field of macroeconomics. Macroeconomists had written down all these models to describe what they could observe, and somebody got the bright idea of using the models to forecast what we can't observe. When they started forecasting out of sample, they found out that they really couldn't predict as well as they thought they could. The Clayton Act put us into the prediction business, so what we'd like is evidence on the accuracy of out-of-sample forecasts, but for a lot of reasons, we're not going to get much. It's difficult to estimate the effects of mergers after the fact, and even if you do estimate the effects of a merger, it's very difficult to go back and actually run a simulation using evidence that would have been available only at the time of the merger. So, for a variety of reasons, we're not likely to get good evidence on out-of-sample forecasting accuracy. What are we then left with? How do we evaluate the reliability of these models?

What Greg is proposing is that if a model does not accurately predict what we can observe, we ought to be very suspect about using the model to forecast what we can't observe. If you can't predict the present, you shouldn't use the model to forecast the future. Having said that, the three of us don't completely agree on how well the model should be able to predict what is observed, e.g., what facts are relevant, to determine whether a model is reliable or not.

Much of the criticism of structural modeling comes from attorneys who worry both about the costs of merger simulation—estimating the parameters of the models can be a long and costly

undertaking—and about the way they can focus an investigation on evidence that is unlikely to inform the fundamental questions raised by the case. For example, attorneys who have used modeling have run up large bills only to see the estimates as being wacky or not consistent with their prior beliefs. So merger simulation has gotten a bit of a black eye. The pro estimation side of the debate would be to say: Data are the only source of knowledge; the way to interpret data is econometrics; and any problems we run into we have satisfactory solutions. The con side is: The data just doesn't speak to the policy question that we're trying to answer; a lot of problems don't have satisfactory solutions; and it's a very costly proposition.

Where do I come away on the estimation debate? This is going to annoy you as I'm sure it annoys my students at Vanderbilt when I tell them, the answer is, "it depends." I always wait until they've paid their tuition before I tell them that, but it does depend on what you're trying to do, and merger simulation should not be used as a black box that applies in every case. It's a tool, and you can only judge the utility of a tool by the use to which you put it. It's appropriate for some jobs, but not for others.

Is simulation likely to convince the key decision makers? Again, it depends. It depends on how well the model describes the evidence. Is it likely to reduce uncertainty? Before you engage in a long estimation exercise, ask the economists what would we get if we didn't have to estimate the demand, what if we knew exactly what the model was going to predict, what would we be able to say? Would that convince anybody? If you estimate an outcome that favors your client, would that convince anybody that you weren't just fishing for that outcome? So put your economists to the test, ask them some questions. We're going to give you some questions to ask them.

Is simulation necessary for defensive reasons? Critiquing methodology is really hard without first replicating it. Does some number beat no number? I think that's certainly true in a damage estimation, and these models are used in damage estimation. In a case I worked on shortly before coming to the FTC, we successfully defended a damages case without putting out our own number, but it was difficult. Without a model, you have to argue that their model is not any good at all; whereas if you put up your own model, your burden is lower as all you have to do is argue that your model is better than their model.

The crucial question is, are we simplifying too much? Do the models abstract away from crucial pieces of behavior, of reality?

—FROEB

One of the reasons we started doing simulations is that, in the late eighties, the Department of Justice got away from coordinated effects stories for a lot of reasons. We weren't bringing merger cases in the kinds of industries where we saw collusion. We were bringing cases in the kinds of industries where we didn't see collusion, and the stories evolved into unilateral effects stories that fit the facts of those cases much better. The basic unilateral effects story is that, pre-merger, the merging firms price independently, and post-merger they take account of cannibalization, which changes their profit calculus. That changes the equilibrium, and the merged firms raise their prices, and the non-merging firms raise their price sympathetically. This makes it seem like the only issue is how much, but behind the unilateral effects story is a very complex structural game theoretic model built on assumptions about how firms, retailers, and consumers behave.

How do we know when these models give reliable forecasts? Well, we don't know. The ultimate proof is prediction. There's been very little out-of-sample prediction with these models. We're seeing more of this. We're trying to do some of that at the FTC, but it's very difficult to do. In the absence of evidence of good out-of-sample predictions, I think we go back to the *Daubert* discipline and ask how well these can models describe what we can observe.

I'm going to illustrate how to bring the *Daubert* discipline to the modeling choices. The crucial question is, are we simplifying too much? Do the models abstract away from crucial pieces of behavior, of reality? What we three have come up with is a rule saying: If an assumption matters

to the model and matters to how you interpret the results of the model, then the assumption either ought to be supported by evidence or, if you don't have good evidence, you ought to do sensitivity analysis to give the decision makers some idea of how sensitive the model is to the assumptions. Let me give you some examples.

One of the things that we commonly do when we use merger simulation is to ignore the retail sector, or assume it just marks up the wholesale price and passes on whatever upstream merger effect there is. That story could be right, but if it isn't right, merger effects can disappear. If a retailer and a manufacturer jointly maximize the profit pie between them, there's one price that maximizes the profit pie, and that price is not going to change post-merger. That's a second story. The split of the profit pie might change, but the retail price does not. So if that's how the retailers are behaving, there is no merger effect. Then a third story is that there's double marginalization, and that can either amplify or attenuate merger effects. This assumption really matters. You have to gather evidence on it. One thing you might do is try to look at retail margins and wholesale margins and see if you can distinguish among these three stories.

Another thing that matters is demand curvature. I've plotted four demand curves between the common competitive equilibrium and the four different monopoly equilibria. These four demand curves all share the same competitive point, which is a quantity of ten and a price of four. When we extrapolate to the post-merger equilibrium, we get dramatically different results. If you had only data at the pre-merger equilibrium, how could you distinguish between those four demand curves? There's no way to do it. So here's an assumption that matters a lot. You get three times the merger effect if you use a constant elasticity demand curve than if you use a linear demand curve. If you don't have any evidence, and you won't, you have to do a sensitivity analysis and make a conservative assumption. If you're the plaintiff, assume linear or logit demand. If you're the defendant, assume a constant elasticity or an AIDS demand.

Demand elasticities matter a lot in these models. They're difficult to estimate precisely. If you use a strict classical confidence interval, assuming you could compute one—and that's not a trivial exercise—often the uncertainty is so large that both the pro- and anticompetitive scenarios are in that confidence interval. One of the things you can do is a sensitivity analysis, and here I've taken data from the WorldCom-Sprint merger that was challenged by the DOJ and then abandoned. I've used a logit demand model calibrated to the WorldCom demand elasticity. This demand curve depends on only two parameters, which reflects an assumption about what the cross elasticities look like. Given this assumption, the merger effects don't depend that much on the elasticities. You get from a half a percent to about a 1.25 percent industry price rise following this merger, and these are relatively easily offset by small efficiencies.

Be careful about hiring academics, and I say this as an academic. Don't trust us. We are interested in methodological innovation, while practitioners want to know how well does a methodology fit the facts of the case, and I think that's the crucial concern. The conclusion of every academic article is, "it depends," and you show the tradeoffs in some kind of elegant way, and you get published in a journal that nobody reads. A practitioner wants an answer, and it's very difficult to get an academic to come down on one side or the other. As academics, you get peer review. I've got five of these sitting on my desk, and I'm probably going to devote a little bit of time to each one of them. But if you use one of these models in a damage case, you'll get a \$100,000 referee report. In many ways, I think that the legal standard is higher. The analysis you come up with has got to be practicable and it's got to fit the facts of the case.

My final flag is probably going to go over the heads of anybody who hasn't read *Das Kapital*, but I'm trying to put this in a larger framework. What are we trying to do here? We're trying to push

economics forward, make it more relevant and more useful to practitioners. Greg and I are older, wiser, and much more cautious than we were ten years ago. Dave has been a very useful critic of ours, forcing us to reexamine everything we thought was true, and hopefully we'll come up with a synthesis. Modelers must pay attention to the details of the industry—you can't just assume a can opener—you've got to make it fit the facts of the case. A lot of times we're dealing with complex situations that are simply beyond our capabilities or we have an alternate, simpler, empirical reduced-form methodology—natural experiments—that we can exploit to draw inferences about the effects of the merger.

One of the things that the Chairman asked me to do when I came here is follow up on the enforcement R&D started by my predecessor by actually trying to do some of the merger retro-spectives and provide evidence on out-of-sample forecasting that's necessary to validate these things, and finally to come up with more realistic or better models to do merger analysis. I expect that this effort will help us better identify those situations where simulations work.

DAVID SCHEFFMAN: This will be part love him and—given that Greg and I are here—well, knowing Greg, I don't know what the other part will be. I've been very critical of simulations, publicly, and I'm going to say some things that are somewhat critical today. Let me say I have the highest regard for Luke and Greg. I'll say, and I don't give compliments easily, I don't think anyone's contributed more to antitrust economics over the last twenty plus years than Greg. He's very careful, very thorough, and his contributions have all been very important, both to economics and to policy and particularly how we analyze mergers. Luke is a first rate applied econometrician and industrial organization economist and the two of them together have made a lot of contributions, one of which is merger simulations.

I won't talk about how old I am and where my perspective comes from. I am an economist, which some of you don't believe. I do believe in models and modeling, that models and modeling can be useful and, yes, models simplify things but they necessarily have to. My colleagues who I argued with often when I was back at the FTC when I made all these pronouncements about merger simulations, it's not so much that they were wedded to merger simulations but that they developed the view that I was criticizing economics, and they always made the point, as I have in that last paragraph there on that slide, which is absolutely right. [*Ed. Note:* Slides are available at <http://www.ftc.gov/speeches/other/040129scheffman.pdf>] They say, but wait a minute, look what's on the other side. Look what the lawyers are doing. They have some sort of model, who knows what it is, that leads to their recommendations on a merger case. At least we have some economic methodology, we're up front about our assumptions, and that's better than the alternative, and as an economist, I'm inclined to be sympathetic to that view.

I think one thing that happened with the rise of unilateral effects analysis was that it was a dangerous tool for lawyers. Simulations weren't the problem because simulations actually didn't have much effect one way or the other, at least in the U.S. And simulations may turn out to be beneficial as we learn more. The problem is that, from the point of view of how the agency lawyers would look at unilateral effects—as I've often said, we went back to the 1960s style of merger analysis—lawyers concluding that two companies compete with one another and on the basis of often very squishy arguments, concluding that they compete particularly closely with one another. So, *voilà*, we have a unilateral effects theory. That's just not right as a matter of law and it's certainly not right as a matter of economics, and so we have a problem. I'm not criticizing the lawyers at the FTC—both agencies are absolutely superb at what they do—and they're very good at basic economics. But the problem we have here is a gap between very formal models that the economists use

and what we more often do: We apply economics in a very squishy situation but hopefully with some economic discipline. That requires you to be very sophisticated as an economist or that you be a lawyer who's pretty good in basic economics but doesn't have the background or sophistication to do simulations.

So what do I like about simulation? I like Greg's and Luke's paper in the Swedish volume, I think—the general argument about why simulations are a good idea. That is, you should try in some disciplined way to put all the factual information together—cost, demand, what you know about competition and other sorts of things—to try and reach a sound conclusion. Very often in the agencies and on the outside, things get pretty squishy and you say, well it's anticompetitive based on a lot of relatively squishy stuff, which might be right. I think what economists can do is try and put all that evidence together and come up with a conclusion, nothing anywhere near as precise as a Bertrand simulation model, but that at least provides something economists can argue about as to what assumptions, what facts are important as to whether we really thought the price increase was going to be 10 percent or 1 percent and why, etc.

My concern is that, as

we all know, we don't

usually use models in

most merger analyses.

Most matters are too

complicated.

—SCHEFFMAN

So I like that paper a lot, I like the work that both Luke and Greg have done. It's been recognized academically, helped Luke get tenure at a first-rate university, and rightfully so, but I think it's just the beginning. It's a very promising beginning. But we have much more work to do.

Enough lovefest. Let me talk about my concerns. My concern is that, as we all know, we don't usually use models in most merger analyses. Most matters are too complicated. Fortunately, in industrial mergers, we can rely a lot on customer opinions and other sorts of things. In bidding situations, we can use models and, in my view, those are probably the situations where models are most useful. But on average, in the agencies we economists don't use models. We often do not even have the data really to do much modeling.

But, when we say "differentiated products" and we have some data at the consumer level, *voilà!*—all of a sudden we can model because we have a 150-year-old model—Bertrand—and we have some data two or more levels down. I'm not saying that's not a potentially useful exercise but it requires some thought.

Now here's where the "non-economist," or as I would rather put it, the "augmented economist" side of me comes in. I have been a strategy and marketing professor now for fifteen years at a pretty good business school, and clearly Bertrand does not at all replicate the way competition really works. It does not have anything very important to say about how competition occurs in branded products mergers. That's because, as I'll discuss in a minute, Bertrand is not really a model of competition.

So my concern is that we have something which is actually very complicated to explain, really much more complicated than our typical industrial products merger because we have branding, we have retailers, we have consumers, we have all sort of complexities, complex relationships between retailers and manufacturers and we say, *voilà!*—because it's differentiated products we can explain it with a simple model. I'm very skeptical of that.

Nonetheless, I would not say that that can't be true, and that's because even though it clearly does not at all replicate real behavior in consumer products markets, it may be a useful metaphor for explaining equilibrium. That's what I understand to be the "Willig justification"—what I ascribed to Bobby, but probably came from Greg. The Willig justification for unilateral effects says if you think about Bertrand, all that's really going on in Bertrand is internalizing the cannibalization that occurs between the competing brands, and that might provide a post-merger incentive to raise price. What you could say, perhaps, is that in any sort of model where cannibalization considerations are going to be important, Willig is probably right.

The question is, how important? And does that internalization of cannibalization dominate how the competition really works in the model, in reality? That's the important question. It's possible that Bertrand is right, but it's certainly not right as a depiction of how competition actually works. Nonetheless, it could conceivably be the explanation of equilibrium in the long run. That is, the internalization of cannibalization could dominate the pricing decision, but I would need a lot of convincing that that was true, and I would require much more testing of Bertrand than we normally do, and I'll talk about that.

Now one of the reasons why I think Bertrand is such a strange model for us to use in mergers is that we're only looking at mergers where there are a very small number of competitors, usually four, or at most five, these days. So we're evaluating the classic oligopoly situation that economists have been looking at for decades and decades, which is the problem of the small number of competitors. With a small number of competitors, each competitor realizes that not only does it affect the marketplace by what it does, but it knows what it does will stimulate a reaction by its competitors and therefore a competitor is going to take that into account in what it does. That is, Bertrand has none of that oligopolistic interaction which absolutely has to be true as a matter of behavior in a market where there's only four or five (or fewer) competitors.

Now I agree that it's possible that, despite all that competitive interaction, what happens in the equilibrium might actually be explained by a Bertrand model, but that's an empirical question. While it's possible, there really is no basis in actual market behavior to think that the Bertrand model is right—there's no basis to believe that the Bertrand model in any way replicates what really goes on in competition. This is certainly true in consumer products where the real competitive interaction is taking place between the retailers and the manufacturers, and down to the consumer level and the manufacturers. But it's still an empirical issue. It could be right in some circumstances as a metaphor for equilibrium. We have to figure out when it's right in what circumstances.

I think Greg and Luke pointed out very well the problem with simulation, and they stress it's far more complicated than you think it is. Lawyers agree with that, they think it's black magic, but economists who have done this a lot—and no one has probably done more of these than Greg—know that it's really quite subtle and complicated even though it seems a simple model. You have to really think through the assumptions and the interrelationship among the assumptions, so you have to do a lot of testing. Greg's paper on bread is very good. I know Greg would never do a case unless he immersed himself in all the evidence in the case and convinced himself that the simulation tool was valid and useful in a given situation. Now I may or may not agree with Greg in a specific case, but I know that Greg would have covered all the bases and would have certainly absorbed all the facts in the situation and factored those into his decision.

So merger simulation is a tool, it's not an answer. No one, none of these economists who created this would say you could make a decision on a merger based on a simulation. That's not the way they do work—that's not the way anyone does. One of the problems is in the past several years is that lawyers have been hiring economists to come to the agencies knowing only what their simulation model results are, which is worthless at the agency and should be. Merger simulation is not a substitute for thinking or assessing the totality of the institutional setting, I think we all agree on that.

Let me get to the part that will get Greg's juices going. For lots of reasons, I'm absolutely convinced Bertrand is not the right model. But as I discussed earlier, it might be, it could be right as a model to explain equilibrium. I would just need a lot of convincing of that. This view is quite consistent with the discussion in the Fudenberg and Tirole game theory text, which makes clear what Bertrand's limitations are.

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What are my specific gripes? Well, I've been griping a long time about retail versus manufacturer. I simply don't believe that you can look at elasticities estimated at the consumer level and jump to any inference about what that means about mergers at the manufacturer's level. You really have to do a lot of thinking about that and I think Greg and Luke and I agree on that. I think we probably may differ as to how important that is but I think we all agree now that that's an important issue. As they pointed out, Bertrand doesn't include what I, the marketing professor, think is really the most important sort of activity in terms of competition, which is about products and advertising and promotions with retailers and shelf space and placement—i.e., nonprice competition—all those things, that's what the real competition is about and you need to take that into account. I certainly agree that it's possible that all those are important, but with respect to pricing and effects of a merger on pricing maybe that would be separable, in some sense. It's possible. I would need a lot of convincing on a given case that that was true, but it's possible.

Bertrand is really a very simple model. What Bertrand is, I'll say this very strongly, Bertrand is a reduced form monopoly model. Think about what each Bertrand competitor does. It maximizes profits subject to a *ceteris paribus* demand curve. That is what the monopolist does. Now what does Bertrand do? It forces the equilibrium of these reduced form monopoly maximization problems to be consistent among all those reduced form monopolies.

In a simplistic dynamic sense, there is "some" competition in Bertrand. Bertrand is a posted price model. Somehow, there's some demand curve, you post your price, some people come, they buy, you figure out whether you sold what you expected to sell. If not, you change your price up or down. That's all there is to the competition. You post your price. You change it if sales were not consistent with your expectations about demand. There's nothing about trying to get business away from your competitors. There's nothing about positioning your product differently, doing any of the sort of things which real world marketing is all about. I think those things are likely to be very important, but it's the empirical issue. The issue is really, does the internalization of cannibalization—which is *all* that drives Bertrand simulation models—dominate all that other stuff which is really very important? It might, but I think we need more work to figure out whether that's true.

Okay, let's come back to the lawyers. I criticize the lawyers but the lawyers are asking the right questions. The most important question in analyzing the potential for unilateral effects arising from a merger is, does the competition between the parties to the merger make the prices lower than it otherwise would be? That undoubtedly is the answer to the question of whether the merger is likely to be anticompetitive. Now where the lawyers go wrong is that we don't usually have evidence on whether competition makes a difference in the price, unless the economists would be able to do some analysis from natural experiments. But the lawyers are asking the right question. Where the lawyers go wrong is taking a lot of squishy evidence and arguing that two products really compete with one another and maybe to some extent stretching the evidence to argue that they compete "particularly closely."

That will not get to the right answer—but the question is the right question. That is, I think we would agree as economists this is the real question, which is a *Staples/Office Depot* sort of question. Does the competition between the parties make the prices of their products lower than it would be if they didn't compete? Bertrand doesn't tell us the answer to that question. Bertrand does tell us if we get the elasticities right, there's an incentive to internalize the cannibalization. Now, whether you actually can internalize the cannibalization by changing your price depends on what? On the nature of competition, interaction with retailers, all that sort of stuff. So if Bertrand is important, it's an input into the analysis of the "real" question, it does not address directly THE

question—THE question is the *Staples/Office Depot* sort of question. We need more economic tools to try and get better at the question.

I don't know that I disagree with Greg and Luke—we'll find out quickly in a minute—but they've always been careful in their papers in talking about calibration and in part looking at demand elasticity versus the Lerner equation and bringing other cost information to see whether the implied margin (or marginal costs) are consistent with estimates of the actual margin (or marginal costs). If they're not, obviously there's something wrong with the model.

I certainly agree with that, but in my view that's much too weak a test; that's like using one degree of freedom, one observation to test the model—it's more than that, but it's a very weak test of whether the model is "right," particularly when you're talking about whether it predicts merger effects accurately. So I've proposed a number of other tests. The tests really should be (and I don't think Greg and Luke and I disagree on this), whether the Bertrand model really explains the historical data—not just in terms of satisfying the Lerner equation. For example, if you have enough history and you know there's been significant changes in costs (or other shocks), the Bertrand model makes very specific predictions about what should happen to prices. Did it happen or not? You're looking for certain natural experiments in the past to see whether the Bertrand model correctly predicted explained the price path.

Another issue—is there volatility in shares? As both a competitive analysis and a marketing professor, I think the most important thing in analyzing branded products mergers is, what do the different competitors think about their shares? Do they have inconsistent views about shares? Is it like the beer industry in which for the last twenty years, Anheuser Busch has had increasingly aggressive share targets and has achieved those targets because it diverted sales away from its competition? That's a situation of a very competitive market where I wouldn't be very worried (other things equal) about mergers of producers not involving Anheuser Busch, or I'd be less worried than I would be if the shares are relatively stable, as in, say, the breakfast cereal industry in the '70s, where, to overcharacterize, everyone seemed to be pretty happy with what they were doing. So Bertrand with respect to price might fit (but I would need to really test it). But if there's volatility in shares, is that explainable by Bertrand? In my experience, the volatility is not usually going to be explained by shifts in competitor-specific costs. And in my experience, shifts in demand are generally stimulated by nonprice competition. So in that case, what can Bertrand tell us?

However, I agree with Greg and Luke on the following, and I said this to my colleagues in the Bureau of Economics: It's good for the economists, and a necessity for the economists, to take all the stuff you're putting together and try to put it into some consistent story and say, "Well, as a result of this, I think prices are going to rise to such and such, and this is why—here are my assumptions, here's the sort of model I'm using." Maybe it's a formal model, maybe it's not. Or the economist might say, "Here's why I think prices aren't going to go up and here are my assumptions, etc." But we need to do things in a disciplined way. I think good economists do that. I think we need to do it more often and write it down and show it to the attorneys so they can see the disciplined analysis—where you lay out your assumptions and you test those assumptions, and how you reached a conclusion, as opposed to the way the lawyers do it, which again—they do a good job but there is not a rigorous discipline in what they do. That uses up my time. Thank you very much.

GREG WERDEN: Dave said I was going to respond, and I wouldn't want to disappoint him. I need to say a few things about the Bertrand model. It took economists 50 to 75 years to understand how they ought to be thinking about such oligopoly models, but now we've got it sorted out. All that these models are telling us is, "What is the result of competitor interaction?" They do not say quite

how that process works, just where it ends up. So the test is: Does the model really tell you where the industry ends up?

The Bertrand equilibrium is a set of prices such that competitors are happy with their prices, given the prices they observe for their rivals; they don't want to move. Bertrand equilibrium is a specific application of the concept of Nash non-cooperative equilibrium, which is basically the only equilibrium concept industrial organization economists have used for the last quarter century and what earned John Nash his Nobel Prize. Nash equilibrium applied to price competition—that competitors don't want to change their prices—is Bertrand equilibrium, and it's only fairly recently that it was understood that this is not a naïve equilibrium concept. It does not assume away all strategic interaction. The Nash equilibrium is the result of the infinite regress in which everyone anticipates what everyone else is doing, and everyone anticipates being anticipated, and so on. Nash equilibrium is where things end up when all that is figured out, so it's a sensible equilibrium concept.

Bertrand, in particular, is a short-run equilibrium concept. The short-run in economics is a period sufficiently short that there is no entry, exit, or changes in capital investment. If all those things are held constant, what is there to compete about? Well, the big thing, of course, is price, and that is what the Bertrand model focuses on. By focusing just on price, the Bertrand model leaves things out, which may be important, but Dave tends to think they are more important than I do. I have studied branded products industries in which I thought the Bertrand model reflected the competitive process quite well.

In terms of the tests Dave suggested, let me just say what it is that we are trying to test, and that is whether the model can reasonably accurately predict the average level of prices over a period of a year or two. It is ridiculous to try to predict price on a particular day or how price will change from week to week. Why would anyone care? What matters is whether the merger is going to cause prices, on average, to go up for a sustained period of time. If it does, the fine details are immaterial. Asking the Bertrand model to explain certain fine details is asking for it to do too much.

I think a very important test, I would say the most important test, is whether the margins implied by Bertrand equilibrium are consistent with the actual margins in the industry. If they are consistent, that says to me that the Bertrand model accurately predicts the intensity of competition, as measured by the difference between prices and marginal costs. That's awfully important. Industries could be more intensely competitive than the Bertrand model says they are; they can be less intensely competitive than the Bertrand model says they are. In either case, the Bertrand model is not a very good model to use. But it might be a very good model to use if the intensity of competition is what the Bertrand model says it is.

I'm not very worried about share volatility. It's an issue; it may mean that there is more variance in the predictions than one would like, but that is not a big thing. We three agree that many considerations should go into deciding whether Bertrand is an appropriate model, but I could not write down a definitive list of what to look at. One has to come away from looking at all those things with a conviction that Bertrand is the right model and a conviction that one could persuade a trier of fact that it's the right model. I was always prepared to persuade a trier of fact when I was preparing to bring this model into court.

DAVID SCHEFFMAN: I'd look at anything Greg did and, if he came in from the outside with a simulation model, I'd look seriously at it because I'd know he'd have a thoughtful basis for it. As I said, a problem on average, and I've come in from the outside, is that we don't see that sort of thing. In part, being on the outside—you don't have all the information you have on the inside, but I urge

you—because I see a lot of consultants here—get your clients to go back more to what they were doing in the 1980s, which is pay you to read the documents because you'll get flushed in a second if you do not know the basic facts at issue in the matter and how those affect your conclusions. I've seen it often enough where economists come in and they don't know the documents—our economists won't pay attention to you—you've got to master the industry and you've got to be able to explain what you did and why the data are relevant, and why you should pay attention to the results, because that's the way economists inside do it.

LUKE FROEB: I just want to put a plug in for looking at the ex-ante competition to get onto the shelf. I worked on a couple of cases where I thought that was an important dimension of competition that was being overlooked. And this is an example where using the wrong model can lead an investigation astray. If there is ex-ante competition to get onto the shelf, or to “win” an agreement with a retailer so that you can go on special three out of every four weeks, it is important to model and understand this competition. This is a case where an acquisition could increase the ex-ante competition for shelf space. But if you looked only at the ex-post competition, by estimating and simulating a merger using estimated demand, you would be missing the ex-ante competition. ●