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ESTIMATING THE PRICE EFFECTS OF MERGERS AND
CONCENTRATION IN THE PETROLEUM INDUSTRY:
AN EVALUATION OF RECENT LEARNING

Friday, January 14, 2005
Washington, D.C.

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P R O C E E D I N G S

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OPENING REMARKS:

DEBORAH PLATT MAJORAS - FTC Chairman

LUKE FROEB - FTC, Director, Bureau of Economics

CHAIRMAN PLATT MAJORAS: Well, good morning and welcome to the Federal Trade Commission's Conference on Estimating the Price of Effects of Mergers and Concentration in the Petroleum Industry. Boy, we really come up with snazzy titles for our conferences, don't we.

I really want to thank, first, our distinguished panelists for traveling from both coasts and points in-between to be here to offer their expertise and insights. And I thank you in the audience for joining us on this very rainy morning. We really appreciate your interest in this topic.

As aptly stated in an FTC Bureau of Economics report on oil industry mergers, which we released last August, the petroleum industry occupies an unusually prominent position in the American economy. Domestic demand for gasoline and other refined petroleum products generally has increased year after year since the mid-

1 1980s, and changes in the price and availability of
2 gasoline affect consumers directly. Indeed, there may be
3 no other product for which consumers are more acutely
4 aware of price fluctuations, as ubiquitous retail
5 stations loudly announce the current price on large signs
6 visible to all who drive by.

7 In addition, of course, the price and other
8 supply conditions for petroleum products profoundly
9 affect businesses in many sectors of the economy, as
10 illustrated by the fact that announcements about the
11 price of crude oil can move markets quickly in one
12 direction or the other. As the BE oil merger report
13 observed, perhaps no other industry's performance is so
14 visibly and deeply felt.

15 The Federal Trade Commission is, of course, the
16 federal antitrust agency primarily responsible for
17 addressing competition issues in this industry. The
18 Commission has devoted substantial resources to
19 scrutinizing market activity in this industry and, when
20 warranted, to bringing law enforcement actions.

21 I am committed to continuing the Commission's
22 vigilance in this critical market sector. We will
23 continue to apply careful antitrust scrutiny to market
24 behavior, including mergers, and will not hesitate to
25 bring enforcement actions when needed.

1 Since becoming Chairman in August, I have
2 reviewed the FTC's past and current work in this area and
3 focused on implementing a multifaceted agenda that
4 synthesizes new and ongoing projects and uses all of the
5 tools at the FTC's disposal: law enforcement, education,
6 and resource and development. Our work includes vigorous
7 merger review, close consideration of all refinery
8 closings, careful scrutiny of potentially anticompetitive
9 acts and review of gasoline pricing anomalies, which we
10 endeavor to detect through our ongoing monitoring
11 project.

12 We also are focused on disseminating relevant
13 information about market conditions and about FTC actions
14 in this industry for the benefit of consumers and others,
15 and so, for example, we've dedicated a webpage for that
16 purpose.

17 In addition, on December 15th, I appointed FTC
18 staff attorney John Seesel, to fill the newly created
19 position of Associate General Counsel for Energy at the
20 Commission. Highly respected both within and outside the
21 agency, John will play a key role in reviewing and making
22 recommendations on the Commission's energy-related work,
23 including investigations and cases, legislative
24 initiatives, advocacy comments, and studies and reports.

25 I think John's here today. There he is. I

1 know a lot of you already know him from his dedicated
2 service over the years at the Commission, but I hope
3 you'll join me in welcoming him to his new position.

4 The Commission takes seriously the role that
5 Congress assigned in the development of sound antitrust
6 and consumer protection policy. The agency's investments
7 in research inform the development of competition policy,
8 facilitate better case selection, and provide important
9 economic support to aid the agency in enforcement
10 initiatives. That brings me to today's conference.

11 As you know, over the past few decades, the
12 petroleum industry has undergone extensive structural
13 change, including consummation of several large mergers
14 in the late 1990s. The FTC devoted substantial resources
15 to investigating those mergers, and in numerous
16 instances, to challenging and modifying their terms.
17 Last May, the Government Accountability Office (GAO)
18 released a report that sought to analyze how eight
19 petroleum industry mergers or joint ventures consummated
20 in the mid-to late 1980s affected gasoline prices.

21 The GAO reported that six of the eight
22 transactions it examined caused gasoline prices to rise,
23 while the other two caused prices to fall. And that
24 report has led some observers to call for changes in the
25 way the FTC reviews oil mergers.

1 Of course, before any econometric analysis can
2 be used as the basis for making any decisions, its
3 methodology and results must be carefully reviewed. Such
4 analysis must withstand vigorous cross-examination as
5 U.S. consumers, our courts, and the Commission itself
6 demand nothing less. The wrong enforcement decision in
7 either direction can lead to increased prices, decreased
8 output or reduced service.

9 Today's conference provides a public forum for
10 outside economic experts to discuss the conceptual and
11 methodological issues involved in estimating the price
12 effects of petroleum industry mergers and concentration
13 changes. Our panel of experts will explore these issues
14 broadly and within the context of two reports: the GAO
15 report from May of 2004 and a March 2004 BE case study of
16 the effects of the Marathon/Ashland joint venture. I
17 look forward to hearing our expert panels' assessments.

18 Before concluding, I would like to acknowledge
19 the staff of the Federal Trade Commission Bureau of
20 Economics who worked very diligently to put this
21 conference together. Director Luke Froeb, Senior
22 Economic Advisor Liz Callison, Assistant Director Lou
23 Silvia, Deputy Assistant Director Chris Taylor and Deputy
24 Assistant Director Dan Hosken. And from my own staff,
25 I'd like to thank Attorney Advisor Sara Razi.

1 Finally, I'd like to thank Comptroller General
2 David Walker and GAO Chief Economist Scott Farrow for
3 their constructive feedback on our agenda and the GAO
4 staff for participating in very helpful data exchanges
5 with their FTC counterparts.

6 It's now my pleasure to turn the podium over to
7 BE Director Luke Froeb who will provide more detail and
8 context. Thank you very much.

9 MR. FROEB: Thank you very much. It's a
10 delight to be here today. One of the great things about
11 this job is taking credit for what other people do and I
12 really don't deserve any credit at all for this
13 conference.

14 Good morning, I'm grateful to the panelists for
15 being with us today to share their expertise on the
16 issues that are raised in examining the effects of
17 mergers and concentration in the oil industry. I welcome
18 the audience who have joined us here today.

19 Antitrust policy is best thought of as a
20 continuing process of experimentation, evaluation and
21 reform. It has taken the FTC 90 years to get where we
22 are today, and it would be a terrible conceit to think
23 there's nothing more to learn about how best to design
24 and enforce the antitrust laws.

25 At the FTC, we have an ongoing program of what

1 we call "enforcement R&D" to both characterize our
2 enforcement actions, including the decisions not to
3 pursue a matter, to estimate the consequences of those
4 decisions, and to use the learning to improve our
5 enforcement. Today's conference is part of this larger
6 process.

7 The antitrust laws have spread rapidly around
8 the globe. They are one of our most successful exports.
9 Today, the vast majority of the world's economies have
10 antitrust laws and these laws are aimed at cartels,
11 mergers and abuse of dominance, monopolization and
12 vertical restraints, and they're widely varied both in
13 the laws themselves and in the way they are enforced
14 around the world.

15 And this raises some obvious questions like how
16 is the enforcement working and how do we improve, and to
17 answer those questions, as we said, we have a program of
18 enforcement R&D at the FTC where we characterize the
19 cases we are bringing and closing, we follow up on what
20 happened after the decisions, and we try to learn what we
21 can in order to improve policy.

22 Here's an example of our characterizing the
23 decisions that we have made. This is the FTC merger data
24 release, about a year old, where we released all the data
25 on our second requests (where we asked for more

1 information about a merger) and this chart represents the
2 markets and the decisions about whether to close the
3 investigation or to ask for some relief -- or such as to
4 accept a consent or to seek an injunction against
5 anticompetitive behavior. And you see that the one that
6 stands out is the oil industry, where we had a number of
7 enforcement actions. A large part of our enforcement
8 resources are devoted to the oil industry. That
9 highlights the importance of figuring out whether or not
10 we're doing the right things in this industry.

11 When we follow up the effect of the merger,
12 it's different than the preliminary investigation. When
13 we investigate a merger, we're trying to predict the
14 future. We can observe the present. We're trying to
15 forecast into the future about what the effect of the
16 merger is going to be.

17 Now, when we do merger follow-up, we have a
18 different inference problem. We observe some price
19 difference, and then we have to try to figure out if the
20 merger caused that difference. It's a very different
21 analytical paradigm which calls for different kinds of
22 methodologies than we use in merger investigations.

23 There are two basic kinds of methodologies that
24 will be illustrated in this conference. One is broadly
25 characterized as natural experiments, where we have an

1 experimental group with the merger, a control group
2 without the merger, and the difference between the groups
3 is an estimate of the merger effect.

4 The other methodology will be price
5 concentration regressions, where you estimate the
6 relationship between price and concentration, and then to
7 draw inference about the effect of the merger, you say
8 the mergers change concentration which change price. And
9 because we've estimated the relationship between
10 concentration and price, we can get an estimate of the
11 effect of the merger.

12 Then, finally, we want to interpret the results
13 from the follow-on studies and say what does this mean
14 for policy, how can we interpret the results? We have
15 one session on general identification issues and one
16 session on price concentration studies. We ask how
17 robust are the results, are the results sensitive to
18 small changes, can we rely on them for policy? And the
19 final session will be what does this mean for policy, to
20 try to wrap and interpret the results specifically for
21 the policy.

22 Now, I'm going to introduce our panelists
23 today. To aid in the preparation for today's conference,
24 we sent all our panelists the GAO merger report and the
25 BE working paper, the Effects of the Marathon/Ashland

1 Joint Venture, and we sent various notes and critiques of
2 those studies by the GAO and the FTC to the panelists.
3 That formed the basis of their studies and their comments
4 here today.

5 While oil mergers and the two studies form the
6 basis for this conference, in a sense, they are but a
7 case example of the enforcement policy R&D. The issues
8 that are likely to be raised and addressed today are
9 similar to those that researchers face in doing any ex-
10 post studies of merger policy decisions. Thus, we
11 believe, and are hopeful, that the experience today and
12 the critiques and guidance provided by our panelists, who
13 are truly the leading econometricians and economists in
14 the field, will be useful to us and to academics and
15 others as we continue to develop expertise in enforcement
16 R&D generally, as well as the specific task of following
17 up on the effects of specific mergers.

18 We are privileged to have panelists that every
19 economist would rank at the very top of the profession.
20 In alphabetical order, we have Dr. Dennis Carlton, a
21 Professor of Economics at the University of Chicago
22 Graduate School of Business. Dennis is a leading
23 academic in IO and econometrics. People are often most
24 familiar with Dennis' work because he's the co-author of
25 one of the best and most popular textbooks in industrial

1 organization. Dennis often provides advice to firms and
2 to government as an economic expert in antitrust matters.
3 He has done work previously for the American Petroleum
4 Institute.

5 Dr. Jerry Hausman is the John and Jennie S.
6 MacDonald Professor of Economics at MIT where he's taught
7 for 30 years. Jerry is a recipient of the John Bates
8 Clark Award granted annually to an economist under 40 who
9 has made the most outstanding contributions to economics.
10 Jerry has published numerous papers in the econometrics
11 and applied micro economics fields in which he is a
12 renowned expert. Indeed, several econometric tests bear
13 his name. Jerry has also appeared as an economic expert
14 in antitrust matters, although none in the oil industry.

15 Dr. Ken Hendricks is a Professor of Economics
16 at the University of Texas-Austin, who's spending this
17 academic year as a visiting professor at Princeton. Ken
18 is also an expert in industrial organization economics,
19 specializing in auction theory and empirical applications
20 of game theory. Like his fellow panelists, Ken is well-
21 published. Ken consults for both businesses and the
22 government as an economic expert for antitrust cases.
23 Ken provided such expertise to the FTC when he was hired
24 by us to analyze competition in the bidding for crude oil
25 exploration rights in the BP/Arco merger in 2000. Ken

1 has not been involved as a consultant in oil mergers
2 recently, but Ken is acknowledged in the GAO report as an
3 expert who reviewed the GAO's econometric models.

4 Dr. Scott Thompson is currently Assistant Chief
5 of the Economic Regulatory Section for our sister agency,
6 the Antitrust Division of the U.S. Department of Justice.
7 Scott attended Stanford and Wisconsin and taught
8 econometrics at the University of Minnesota. He joined
9 the Antitrust Division in 1995 and won the Assistant
10 Attorney General's Distinguished Service Award in 2001.
11 Scott has conducted and overseen numerous merger and
12 price fixing investigations and has conducted follow-up
13 studies of enforcement actions at the Department of
14 Justice.

15 Dr. Hal White is a Professor of Economics at
16 the University of California at San Diego. Hal is an
17 expert in econometrics and statistics. Indeed, Jerry
18 Hausman told us that Hal was among the best
19 econometricians in the world today. Those of you who
20 know Jerry will recognize what a huge statement this is.
21 Jerry did, however, claim some small credit as Hal is one
22 of his former students. Not surprisingly, Hal has a long
23 list of published research in econometrics. Like Jerry,
24 he has an estimator named after him. Hal offers his
25 economic expertise to businesses through consulting. He

1 has not consulted in the oil industry on any merger
2 matters.

3 Finally, I want to introduce Dr. Chris Taylor.
4 Chris is Deputy Assistant Director for Antitrust in the
5 Bureau of Economics here at the FTC. Chris is co-author,
6 along with Dan Hosken, of one of the studies that forms
7 the basis of today's conference, the Economic Effects of
8 the Marathon-Ashland Joint Venture, which was released as
9 a Bureau of Economics working paper in March 2004. Chris
10 has spent a significant portion of his time over the past
11 few years working on matters in the petroleum industry
12 including the development and implementation of the FTC's
13 ongoing gas monitoring project.

14 To start the conference off, Chris will present
15 both the GAO study and the BE working paper study.

16 Chris?

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1 **INTRODUCTION AND PRESENTATION OF TWO RECENT STUDIES**

2 **PRESENTED BY:**

3 **CHRIS TAYLOR, FTC, Deputy Assistant**

4 **Director for Antitrust, Bureau of Economics**

5
6 DR. TAYLOR: Good morning. For the next hour
7 or so, I will take us through the BE working paper on
8 Marathon/Ashland and the GAO report.

9 We're going to start with the Marathon/Ashland
10 working paper. As Luke said, this is joint work with Dan
11 Hosken, who is also a Deputy Assistant Director here at
12 the FTC. Usual disclaimer, the views and opinions
13 expressed in this presentation are those of the author
14 and do not necessarily represent the views of the
15 Commission or any individual Commissioner.

16 So, why do a case study merger retrospective in
17 the petroleum industry? As we've already referred to
18 this morning, the U.S. petroleum industry has undergone
19 some major restructuring during the 1990s; BP/Amoco,
20 Exxon/Mobil, Chevron/Texaco are examples. Concerns have
21 been raised by government officials, consumer advocates
22 and others. So why do a case study?

23 Well, research papers examining petroleum
24 mergers tend to either examine a large number of mergers
25 in a broad cross-section of regions or markets, or have

1 tended to examine wholesale rack prices or retail prices,
2 but seldom both at the same time. So, we decided we
3 wanted to focus on one merger, one region where the
4 market structure and change in market structure would
5 make an anticompetitive effect possible and examine both
6 rack and retail prices.

7 So, why examine the Marathon/Ashland
8 transaction? Marathon/Ashland was a major transaction
9 with a sizable change in market structure. The change in
10 the state level wholesale HHI was about 1,800 to 2,260 in
11 the State of Kentucky. I'm certainly not judging whether
12 that's a market or not, but certainly that is a large
13 change in concentration.

14 The Marathon/Ashland joint venture included
15 seven refineries. Marathon had refineries in Louisville,
16 Texas, Illinois and Michigan; Ashland had refineries in
17 Kentucky, Minnesota and Ohio. There were 84 terminals,
18 5,400 gas stations and 5,000 miles of pipeline.

19 The parties acknowledged that the FTC was
20 investigating; however, there were no divestitures or
21 other enforcement actions. So, we don't have to, in this
22 case study, try and figure out the effects of a
23 divestiture.

24 To give a little more background, this is a map
25 of part of the Midwest and you can see there the

1 Marathon/Ashland refinery in Eastern Illinois at
2 Robinson. There's a pipeline directly to Chicago and one
3 directly to Louisville. The refinery at Catlettsburg,
4 Kentucky was owned by Ashland and it would barge gasoline
5 down the Ohio River to Louisville. The closest pipeline
6 is the Teppco Pipeline which runs through Southern
7 Indiana. Gasoline is barged up the Mississippi and Ohio
8 Rivers to Louisville from the Gulf.

9 The other Ashland refineries are in Ohio and
10 Minnesota, on the far Eastern and far Western edges, and
11 the other Marathon refineries were in the Gulf and in
12 Michigan. So, Ashland was in Ohio and Kentucky on the
13 east and Minnesota on the west, and Marathon was in
14 Illinois and Michigan as well.

15 The question then is why look at Louisville?
16 Well, I've already talked a little bit about what
17 happened in terms concentration in Kentucky. Louisville
18 uses a somewhat unique formulation of gasoline, which
19 might make arbitrage difficult if there were a price
20 increase. Kentucky was the only state where both
21 Marathon and Ashland were large wholesale suppliers, and
22 the level and change in concentration in retail market
23 was sizable, and the retail market share in Kentucky
24 combined was 32 percent.

25 So, there were possible anticompetitive effects

1 at the bulk supply level refining, also at terminal and
2 wholesaling and possibly at the retail level as well.

3 A little more background, this map shows the
4 Louisville area. The shaded portion is the reformulated
5 zone. This is where reformulated gasoline, both with
6 MTBE and with ethanol was sold. The dots are stations.
7 We purchased a census of all the gasoline stations in
8 these three counties of Kentucky.

9 In the non-shaded areas, conventional gasoline
10 was sold. Also, conventional gasoline was sold on the
11 Indiana side of the Greater Louisville area.

12 We observe a large change in market structure
13 in a relatively isolated area, Louisville. The region
14 uses a somewhat unique formulation of gasoline, RFG both
15 with MTBE and ethanol was available in Louisville. The
16 only city in the Midwest that used reformulated with MTBE
17 was Louisville, the Chicago and Milwaukee areas had
18 already switched to RFG with ethanol.

19 There's conventional gasoline sold in Indiana
20 and surrounding Louisville, and so, our goal is to
21 determine if change in market structure led to a change
22 in gasoline prices. The nearby terminals, outside of the
23 Louisville terminal, did not sell reformulated gasoline.

24 As Luke already alluded to in his opening
25 comments, the difficulty with this type of study is how

1 do you control for the but-for world? What would
2 gasoline prices have been in Louisville if there had not
3 been a merger? And we decided to compare Louisville
4 prices to other cities' prices that arguably are subject
5 to the same or similar demand and supply shocks. We made
6 this choice because we could not find good controls for
7 supply and demand in a localized area.

8 What were some cities that we choose to compare
9 or use as our control cities? The first one was Chicago.
10 It had reformulated gasoline. Ashland was not present;
11 Marathon was, and it receives its marginal supply from
12 the Gulf by pipe.

13 Our second choice was Houston, which is a net
14 exporter of RFG to both the Upper Midwest and the East
15 Coast and also has reformulated gasoline with MTBE.

16 Northern Virginia was another choice. It is
17 supplied from the Gulf by pipe. Marathon and Ashland did
18 sell unbranded gasoline in Northern Virginia, but so did
19 nine other firms.

20 So, we were looking for cities that had
21 arguably similar supply, especially supply but also
22 demand characteristics, and also cities that were
23 relatively large and had a sizable number of competitors.

24 Also, all of the racks in these cities have
25 prices for conventional gasoline posted for use in the

1 surrounding metro areas. So, we could do comparisons
2 both of conventional and reformulated.

3 The gasoline price data we purchased from the
4 Oil Price Information Service, OPIS. These were
5 wholesale prices. We purchased daily branded and
6 unbranded rack prices. We also purchased firm-specific
7 prices, but we used the average prices, and we aggregated
8 to an average weekly price.

9 The retail prices were also purchased from
10 OPIS, but these are from fleet cards used at individual
11 service stations at a sample of service stations, which
12 worked out to be about 50 percent of the gasoline
13 stations in the Louisville area, and from that, we
14 calculated an average weekly price. We had retail prices
15 for the City of Louisville and Chicago, both the
16 reformulated area and the conventional area, the Houston
17 Metro and the Northern Virginia areas.

18 We also purchased a census of service stations,
19 as I mentioned before, from New Image Marketing. So, we
20 know all of the gasoline stations and their locations in
21 those three counties of Kentucky.

22 Our retail prices are net of taxes. We look at
23 prices for both regular and premium gasoline. The data
24 set covers the period from January 1, 1997 through
25 December 31, 1999. Thus, we have data one year prior to

1 the merger, which occurred January 1st, 1998, and two
2 years following the merger. We dropped the data after
3 1999 because of a series of shocks affecting gasoline
4 prices in the Midwest.

5 So, the results of the paper can pretty much be
6 summarized by this graph. The graph shows the relative
7 price in Louisville less the price in Chicago. The rack
8 price difference, which is the undotted line, pretty much
9 moves in a range of zero to negative five cents per
10 gallon until the April-May 1999 time period when it
11 increases. The retail price and margin look relatively
12 constant or -- well, the retail price looks like it has
13 decreased somewhat and then maybe potentially goes up a
14 little bit. The margin seems relatively constant,
15 although it may be dropping. When we get to the
16 statistical results we'll see which of those were
17 actually significant.

18 The next graph shows the relative price, the
19 rack price of conventional gasoline, along with the
20 relative rack price of RFG, so Louisville relative to
21 Chicago for conventional is dotted red line and
22 reformulated is the blue line. There's no change in the
23 relative price of conventional in Louisville after the
24 merger either at rack or at retail. The graph shows the
25 rack prices, but the retail prices are similar, and you

1 can see, as we saw before, the increase in the rack price
2 of RFG in Louisville relative to Chicago starting in
3 April or May of 1999.

4 This next slide shows the basic derivation of
5 the estimating equation. The first equation is the price
6 in Louisville as a function of shifters for 1998 and
7 1999, the first two years after the merger, month
8 dummies, which potentially control for changes in
9 marginal supply during the year, an auto-regressive error
10 term, and a normal error term.

11 The second equation is the equation for the
12 control city, so it's basically the same equation as
13 Louisville without the 1998 and 1999 merger shifters.
14 And the bottom equation is the one that we actually
15 estimated. It is simply the difference between the two
16 equations, so, the relative price between the two cities
17 as a function of the merger shifters and potential
18 monthly changes in marginal supply.

19 This is a rather abbreviated version of the
20 results, but I think it summarizes what we found in the
21 paper fairly clearly. The first three lines show the
22 regression results on the dummy variables for the
23 margins, relative to Chicago, Houston and Virginia.
24 There was potentially some drop, depending on the control
25 city used in 1998, but there's clearly a decrease in the

1 margins in Louisville relative to these three control
2 cities in 1999.

3 At the rack, once again depending on the
4 control city, there may have been a change in 1998, but
5 it is not robust to the choice. Two of the three control
6 cities did not show a statistically significant change in
7 the rack prices. However, in 1999, all of the cities
8 show that the relative rack price in Louisville for
9 reformulated gasoline increased. However, when we look
10 at to retail, there is no retail effect in either year
11 relative to either control city.

12 I didn't put a slide up here for conventional
13 gasoline simply because there was no effect on margins,
14 rack or retail prices of conventional gasoline. So,
15 that's a summary of the results to this point. We see no
16 systematic change in retail prices following the
17 Marathon/Ashland joint venture. We see no change in the
18 rack price of conventional gasoline. Reformulated rack
19 prices increased in 1999, roughly 15 months after the
20 Marathon/Ashland joint venture. The implied retail
21 margin of Louisville gas stations with RFG decreased in
22 1999. These results were fairly robust to measures of
23 price margin.

24 In the paper, we looked at branded and
25 unbranded rack prices. We looked at premium and regular

1 grades of gasoline, and obviously, we looked at the three
2 control areas of Chicago, Houston and Northern Virginia.

3 Two interesting questions that come out of
4 those results. First, why did wholesale RFG prices
5 increase in Louisville in 1999? In the paper, we argue
6 this may have been the result of a supply shock. St.
7 Louis entered the RFG program at the same time of the
8 wholesale price increase. The first posting of
9 reformulated gasoline at the St. Louis rack happened on
10 May 1st, 1999, almost perfectly coincident with the price
11 increase, especially when you figure in the shipments of
12 gasoline to St. Louis had to occur before May 1st of
13 1999.

14 St. Louis uses RFG MTBE and RFG with ethanol,
15 like Louisville, at least in 1999 and they received their
16 shipment from the Gulf. St. Louis reformulated demand,
17 in 1999, was basically equal to the RFG demand in
18 Kentucky; so, a significant change in the demand for
19 reformulated gasoline in the Midwest.

20 Secondly, there is an interesting question in
21 terms of vertical relationships -- why was the rack price
22 increase not passed through retail prices? We saw an
23 increase in the rack price, but we did not see an
24 increase in retail pricing.

25 Going back to the data that we purchased on a

1 census of the gasoline stations in those counties in
2 Louisville, part of that census told us how those
3 stations were supplied, whether they were supplied from
4 the rack or they were directly supplied by the oil
5 companies. Those rack prices are the wholesale prices
6 for approximately 70 percent of the stations in the
7 reformulated area of Louisville. The remainder of those
8 stations may not have experienced a wholesale price
9 increase. In fact, in a moment, I will show you how the
10 relative prices of rack and DTW changed during that
11 period.

12 The rack-supplied stations also compete with
13 stations across the border in Indiana, which sold
14 conventional gasoline, which, as we've already pointed
15 out, did not experience a price increase. Also, they
16 competed with the stations on the fringe of Louisville
17 that sold conventional gasoline.

18 So, to test if our hypothesis about
19 differential effects on rack-supplied and DTW stations we
20 split the station level data into those two groups and
21 compared the relative prices. That analysis suggests
22 that the retail price of rack-supplied stations in
23 Louisville increased by about .6 cents per gallon in 1999
24 relative to the DTW-supplied stations. Thus, you did see
25 some shift between those two types of gasoline stations,

1 in how they're being supplied.

2 This is a graph of the difference between
3 Kentucky DTW prices and rack prices. We've got our DTW
4 prices from the Department of Energy. These are monthly
5 prices that are only available at the state level. So,
6 not as fine a disaggregation as our rack prices. But you
7 can see that previous to the spring of '99, the price
8 difference, on average, was five to six cents per gallon
9 between rack and DTW, DTW being more expensive.

10 This price difference decreases and actually
11 inverts during 1999. This is similar to what we've seen
12 in other supply shocks in California and in the Midwest
13 such as refinery outages and the formulation change in
14 the spring of 2000. This inversion is likely due to the
15 contractual supply assurances that are given a lessee
16 dealer.

17 So, to summarize the paper and the results we
18 found, there was no systematic change in retail pricing,
19 either for reformulated or conventional. This would lead
20 us to conclude that there was no anticompetitive effect
21 from this merger or joint venture. There was a
22 relatively large increase in wholesale price of RFG,
23 roughly 15 months after the merger. We spent a little
24 time trying to think about how we could test relative St.
25 Louis prices to Louisville. This was difficult to think

1 about, given that there was no data prior to May 1st of
2 1999 for St. Louis.

3 We see no change in the wholesale price of
4 conventional. We see a large drop in the implied margin,
5 since wholesale prices went up and retail prices were
6 unchanged. We discussed, in the paper, reasons why this
7 whole price increase may have been caused by the St.
8 Louis supply shock. And I think one of the interesting
9 and important results to come out of the paper, it is
10 possible to have a sizable change in rack prices with no
11 change in retail prices.

12 Okay, I'm going to take a minute and load up
13 the slides for the presentation of the GAO merger report
14 and price concentration report.

15 Okay, this is a summary of the empirical
16 analyses in the GAO's report, the Effects of Mergers and
17 Market Concentration in the U.S. Petroleum Industry. I
18 will do this primarily by showing materials or quotes
19 directly from the GAO report, and as usual, the same
20 disclaimer applies, that the opinions I'm about to
21 express are mine.

22 What was the motivation for the GAO report?
23 Very similar to why the FTC was doing merger
24 retrospectives since the 1990s, the U.S. Petroleum
25 industry has experienced a wave of mergers, acquisitions

1 and joint ventures. The ranking minority member of the
2 permanent subcommittee of investigations of the Senate
3 Committee on Governmental Affairs, Senator Levin of
4 Michigan, requested that the GAO examine petroleum
5 industry mergers since the 1990s.

6 While there are a number of issues about the
7 petroleum industry discussed in the GAO report, I will
8 present the information in Chapter 5 and Appendix 4 which
9 comprise the empirical work in the report.

10 In this report, the effect of eight mergers --
11 these include joint ventures and sales of assets which
12 occurred between 1997 and 2000 -- are examined on
13 wholesale, rack gasoline prices. In addition, the
14 relationship between price and concentration,
15 concentration being measured at the PADD level and
16 gasoline prices is estimated. They examined the
17 wholesale price of three types of gasoline, conventional,
18 reformulated with MTBE, and carb gasoline formulated with
19 MTBE. And they examined both branded and unbranded rack
20 prices for each formulation.

21 As an introduction to the empirical analysis
22 used in both the merger event studies in the price
23 concentration analysis, I wanted to talk first a little
24 bit about the data set to give you some background on the
25 structure. The basic data set used in both the merger

1 event and the price concentration analysis is a panel of
2 terminal rack prices weekly over a number of years. So,
3 for example, when they're looking at branded conventional
4 gasoline, there are 282 terminals, so 282 prices weekly
5 for seven years.

6 And the basic model used in both the price
7 concentration and the merger analysis is very similar.
8 This is directly from the report, but let me give you the
9 basic understanding of it. This is the basic equation,
10 basic model for both the price concentration analysis and
11 the merger analysis. The dependent variable is the rack
12 price minus crude oil price, and that is Y in the
13 equation. There is a constant term, a vector of
14 explanatory variables, X. These could include regulatory
15 factors, demand and supply factors, other things that
16 shift the relative price of gasoline.

17 Then there is a rack city specific error
18 component and then the normal error term. And in later
19 specifications, they allow for correlation across these
20 racks so prices across racks can be correlated.

21 A little more straightforward representation of
22 that equation, this is the equation used in the merger
23 analysis. Price -- and price here, once again, is
24 measured as the difference between the rack price and the
25 price of crude oil -- is a function of the merger

1 variables, the inventory ratio -- and I'll discuss that
2 in a little more detail later -- utilization rate -- this
3 is a refinery utilization rate -- and the Midwest and the
4 West Coast crisis dummies. These are measures of supply
5 shocks in the Midwest and the West Coast. And there are
6 separate equations.

7 This equation is estimated separately for the
8 three formulations, so conventional, reformulated and
9 carb, and for branded and unbranded gasoline for each of
10 those specifications. You can think about this being
11 estimated six times, once for each formulation and once
12 for branded and unbranded for each formulation.

13 This is the estimation equation for the price
14 concentration analyses and, quite simply, if you took
15 away the merger variables and inserted a measure of
16 concentration, this would be the basic equation. So,
17 once again, price relative to crude oil, a measure of
18 concentration -- I'll talk a little more in detail about
19 it, but it basically is a PADD level refinery
20 concentration -- inventory ratio, utilization rates, once
21 again, the Midwest crisis and the West Coast crisis
22 variables.

23 I'll talk about the data in a little more
24 detail. Price is the difference between the rack price
25 and the spot price of crude oil. This was deflated by a

1 measure of inflation, which was the annual PPI for
2 finished energy. GAO purchased rack prices from the Oil
3 Price Information Service. These projects have generated
4 a fair amount of business for OPIS.

5 These are weekly observations of branded and
6 unbranded rack price. But while I'm discussing rack
7 prices, let me digress for a minute. A rack price from
8 OPIS is for a particular terminal or aggregation of
9 terminals as determined by OPIS. So, for example, there
10 are multiple terminal locations in the Greater Fairfax,
11 Virginia area, but OPIS reports an aggregate Fairfax
12 price. You can also get the data by firm.

13 In other cities, such as Dallas, there are
14 multiple prices reported for terminals in the Greater
15 Dallas area. So, for example, you can get a Dallas price
16 for the South Fork terminal.

17 For conventional gasoline, the data set runs
18 from February 1994 through December 2000. There are 282
19 branded and 256 unbranded racks. For reformulated
20 gasoline -- and this is reformulated gasoline with MTBE
21 -- the data runs from March of '95 through December of
22 2000 and there are 22 branded and 19 unbranded racks.
23 For carb gasoline, the data runs from May 1996 through
24 December of 2000 and there are six branded and seven
25 unbranded racks.

1 On the next couple slides, I'm going to talk
2 about how some of these variables are defined. In terms
3 of the competition variables, you have to define which
4 racks were potentially affected by which mergers, so
5 we'll talk about them as overlap racks. A rack was
6 defined as an overlap if both firms posted any gasoline
7 price at the rack in the year prior to the merger. So,
8 for example, if two firms posted branded but not
9 unbranded prices at a rack, those firms would be defined
10 as competing at both the branded and unbranded rack.
11 Thus, once you define an overlap, it is the same for the
12 branded and unbranded price analyses.

13 This merger variable is a standard dummy
14 variable. It is zero before the consummation of the
15 merger or, in some cases, the date was adjusted for the
16 completion of the divestiture, and one thereafter for
17 each rack, which was defined as an overlap. A rack may
18 be affected by multiple mergers. So, a single rack may
19 have a sequence of mergers.

20 Concentration is measured by operable crude oil
21 distillation capacity, so basically the capacity of the
22 crude oil distillation units in the refinery. It is
23 measured at the PADD level. It is annual data and the
24 data was not available for two years of the analysis,
25 1996 and 1998. The survey was not completed or was not

1 available from the Department of Energy. So, the data
2 was linearly interpolated from the missing years.

3 The measure of crude oil was the West Texas
4 intermediate spot price. It was deflated into year 2000
5 dollars, as I mentioned earlier, using the annual
6 producer price index for finished energy. So, this is a
7 weekly national variable.

8 The utilization rate was the refinery capacity
9 utilization rate. This was a weekly national variable.
10 The supply disruption variables -- the Midwest gasoline
11 variable was for June of 2000 and affects PADD II, the
12 Midwest. And the California refinery outages were in
13 1999 and 2000 and these variables were weekly at the PADD
14 level.

15 The inventory ratio was a measure of lagged
16 gasoline inventories to expected demand, it's weekly and
17 at the PADD level. I'm going to spend, actually, the
18 next couple slides, hopefully, walking you through what
19 that variable is because this is an important variable in
20 the analysis because it is one of the main ways to
21 measure supply and demand shifters in this analysis.

22 How do you calculate the inventory ratio? The
23 numerator is the weekly normalized gasoline inventory for
24 a PADD. So, gasoline inventories of all types of
25 gasoline -- say, for example, conventional and

1 reformulated -- were normalized using the PADD mean over
2 the sample period. Thus, you're scaling each weekly
3 observation by the average inventory level over that
4 period of years.

5 The denominator is the monthly expected demand
6 for the PADD, and expected demand is estimated using a
7 simplified demand equation for each state. So, you have
8 the quantity of gasoline that has been consumed monthly
9 in every state, and you estimate the equation at the
10 bottom of the slide, which is the normalized volume in a
11 period as a function of the normalized volume in the
12 previous period, month dummies, a trend and a trend
13 squared. You take the predicted values from that
14 equation, so the estimated monthly state level demands,
15 and then average them together to obtain a predicted or
16 expected PADD level demand for each month. The inventory
17 ratio is then the one period lagged normalized weekly
18 PADD level inventory variable divided by this estimated
19 monthly PADD level demand. In this way, you end up with
20 a weekly PADD level variable, and the way you do that is
21 you use the same monthly PADD level expected demand for
22 each week in that month.

23 Just a few notes on how these equations were
24 estimated. The model estimation included rack level
25 fixed effects which were implemented by demeaning the

1 data by the rack location. The XTGLS command in STATA
2 was used to estimate feasible generalized leased squares
3 for panel data. The estimator used accounted for a
4 common or single auto correlation across all the racks
5 and there was a separate error variance for each rack and
6 also covariance was allowed between each set of racks.

7 One word -- one slide -- about endogeneity of
8 some of these relationships, were they independent
9 variables, and I'm sure this topic will come up later
10 today, and this is a quote from the GAO report. Since
11 two of the explanatory regressors in the price equations
12 might be endogenous, inventory ratio and utilization
13 rates, we test for their endogeneity using the Hausman
14 1978 specification test.

15 If exogeneity of the variables was rejected,
16 GAO used a two-step estimation procedure with
17 instrumental variables. Instrumental variables were used
18 in some of the analyses and not others. They were used
19 in the unbranded conventional merger analysis, the
20 unbranded carb merger analysis, the unbranded
21 conventional price concentration analysis for the entire
22 country, and the branded conventional price analyses for
23 PADDs I through III and PADDs IV and V, and the unbranded
24 price concentration analysis for carb.

25 Now we're going to get to which mergers and

1 what the results were. There were eight transactions
2 examined in the GAO analysis in order of their timing,
3 Tosco/Unocal in 1997, which GAO specified is affecting
4 PADD V, so the West Coast of the United States; the
5 UDS/Total merger in 1997, which was supposed to affect
6 PADDs II through IV, so the middle of the country; the
7 Marathon/Ashland joint venture, which I've already spoken
8 about in 1998, which was PADDs I through III;
9 Shell/Texaco I which was the Equilon joint venture in
10 1998, which was PADDs II through V, so the middle of the
11 country west; Shell/Texaco II which was Motiva, also in
12 1998, and that was for the eastern half of the U.S.,
13 PADDs I through III; BP/Amoco merger in 1998, which was
14 also PADDs I through III; the Marathon/Ashland purchase
15 of the UDS assets in Michigan, which was PADD II; and the
16 Exxon/Mobil merger of the year 2000, which GAO specified
17 as affecting PADDs I through III.

18 Now, a direct quote on the results, "GAO's
19 econometric modeling shows that the mergers GAO examined
20 mostly led to higher wholesale gasoline prices in the
21 second half of the 1990s. GAO's analysis shows that the
22 majority of the eight specific mergers examined resulted
23 in higher prices of wholesale gasoline in the cities
24 where the merging companies supplied gasoline before they
25 merged." As we'll see in a moment, the effects differed

1 across merger depending on gasoline formulation.

2 This is a reproduction of the summary table out
3 of the report for conventional gasoline and the merger
4 study. UDS/Total, which had a premerger period of '94 to
5 September of '97, and a post-merger period of October '97
6 through January of '98, was estimated to have decreased
7 wholesale gasoline prices in those cities where they were
8 judged to compete by approximately one cent a gallon, and
9 this is in conventional gasoline. The Marathon/Ashland
10 joint venture, which has a pre-period of 1994 through
11 early 1998 and a post-period of the first half of 1998,
12 was estimated to have increased conventional gasoline
13 prices by about half a cent to three-quarters of a cent
14 per gallon. This is a different result than the one I
15 presented this morning from the working paper that I
16 helped author.

17 I won't go through them all one by one, but
18 Shell/Texaco II, which affected the eastern half of the
19 United States, was estimated to have decreased prices by
20 approximately a cent to two cents a gallon. The
21 superscripts you see next to the numbers denote
22 statistical significance. So, most of the other mergers,
23 Shell/Texaco I, BP/Amoco, Marathon/UDS and Exxon/Mobil
24 were estimated to have increased wholesale gasoline
25 prices anywhere from half a cent a gallon to five cents a

1 gallon.

2 These are the merger results for reformulated
3 gasoline. Marathon/Ashland for reformulated gasoline was
4 estimated to have increased prices by slightly less than
5 a penny a gallon. Because the Marathon/Ashland overlaps
6 in the GAO study included more than Louisville, I think
7 they had a slightly lower rack effect than we did.

8 In Shell/Texaco II, it was estimated that
9 branded prices decreased by a little less than half a
10 cent a gallon. There was no statistically significant
11 effect on unbranded. BP/Amoco had no statistically
12 significant effect on reformulated prices, and
13 Exxon/Mobil was estimated to have increased prices by a
14 cent to a cent-and-a-half per gallon for reformulated
15 gasoline in those cities in which Exxon and Mobil were
16 present prior to the merger.

17 The results of the carb study, Tosco/Unocal,
18 with a pre-period of May of 1996 to April of 1997 and a
19 post-period of April 1997 through early -- the first
20 month of 1998, was estimated to have increased branded
21 prices by almost seven cents a gallon for carb gasoline.
22 However, it had no estimated statistically significant
23 effect on unbranded gasoline. Shell/Texaco was shown to
24 -- or was estimated to have about three-quarters of a
25 cent a gallon decrease in the price of branded but have

1 no effect on the price of unbranded gasoline.

2 Now, I want to shift and summarize the price
3 concentration analyses and results. Conventional
4 gasoline was examined in PADDs I through V, so the entire
5 country with data from February of 1994 through December
6 of 2000. Reformulated gasoline was looked at in PADDs I
7 through III, so the eastern half of the U.S., and this
8 was, once again, for reformulated with MTBE. The Upper
9 Midwest that used ethanol was not included in the
10 reformulated estimation. The data was February 1995
11 through December of 2000. And, finally for carb
12 gasoline, the price concentration relationship and merger
13 effects were estimated with data from May of 1996 through
14 December of 2000 for California and the West Coast.

15 Verbally, the results: "For market
16 concentration, GAO's econometric analysis shows that
17 increased market concentration resulted in higher
18 wholesale gasoline prices. Prices for conventional
19 gasoline increased by less than one-half cent per gallon
20 for branded and unbranded gasoline." "The wholesale
21 price increased by about one cent per gallon for boutique
22 fuel sold on the East Coast, so this is reformulated
23 gasoline, between 1995 and 2000, and by over seven cents
24 per gallon in California between 1996 and 2000."

25 Now, a summary of the price concentration

1 results, the price concentration effects for conventional
2 show a positive and significant relationship for all but
3 one of the specifications on this table. For the entire
4 country, the 300-point increase in HHI from 803 to 1,101,
5 shown on the table, when multiplied by the estimated
6 coefficient on the price concentration relationship,
7 shows a 0.15 cent per gallon total increase in the price
8 of gasoline. The total effect of the change in
9 concentration of the price of unbranded is larger at
10 about .33 cents per gallon for the 300-point increase.

11 I want to make it clear this 300-point increase
12 was calculated as the total change in PADD level
13 concentration of crude distillation capacity for each
14 PADD, weighted by the number of price observations in
15 that PADD. Thus, to aggregate PADD level HHIs to
16 national, the number of rack price observations in each
17 PADD was used as a weight. So, when estimating the price
18 HHI relationship for just the Eastern U.S., there is an
19 increase of .25 cents per gallon for branded and no
20 statistically significant change for unbranded. In the
21 Western U.S., there is a price increase of .56 cents per
22 gallon in branded and 1.29 cents per gallon in unbranded.

23 And these are the price concentration results
24 for reformulated and carb gasoline. For reformulated
25 gasoline, estimated effect of approximately a cent per

1 gallon for both branded and unbranded gasoline. However,
2 in carb, the 300-point increase in that area was
3 estimated to have increased prices between seven and
4 eight cents per gallon.

5 Finally, just a quick summary of the report
6 from the executive summary, "GAO's econometric analyses
7 indicate that mergers and increased market concentration
8 generally led to higher wholesale gasoline prices in the
9 United States from the mid-1990s through 2000. Six of
10 the eight mergers GAO modeled led to price increases
11 averaging one cent to two cents per gallon. GAO found
12 that increased market concentration, which reflects the
13 cumulative effects of mergers and other competitive
14 factors, also led to increased prices."

15 That is my summary of both the Marathon/Ashland
16 working papers and the GAO's report on mergers and price
17 concentration. I guess we're going to take about a 10 or
18 15-minute break before we get on to the first panel this
19 morning.

20 **(Whereupon, a brief recess was taken.)**

21 MR. FROEB: The presentation for this hearing
22 will be on the website in about a week.

23 As I said, there will be a series of four
24 panels today. The first panel is General Identification
25 Issues in Merger Event Studies. Jerry Hausman is going

1 to be the moderator of this panel. All of our panelists
2 will have an opportunity to comment on each of these
3 issues. We've tried to break them down into some kind of
4 rational taxonomy here, but there will be lots of overlap
5 and leakage between the topics on each of the panels.

6 Jerry?

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1 **PANEL ONE: GENERAL IDENTIFICATION ISSUES IN MERGER EVENT**
2 **STUDIES**

3 **MODERATOR: JERRY HAUSMAN, Ph.D.**

4 **PANEL MEMBERS: DENNIS CARLTON, Ph.D.**

5 **HAL WHITE, Ph.D.**

6 **KEN HENDRICKS, Ph.D.**

7 **SCOTT THOMPSON, Ph.D.**

8

9 PROFESSOR HAUSMAN: Well, I'm pleased to be
10 here today. As Luke said, what we did, with Liz
11 Callison's help, was to split things up into four
12 sessions, and we decided, the panelists, that for each
13 panel we'd start off with about a 15-minute presentation.
14 So, that's what I expect to do.

15 So, the way that I thought we would set this up
16 is I would give a 15-minute presentation, then each of
17 the panelists would speak, and then if we have time,
18 we'll take questions at the end.

19 This is a very difficult problem that we're
20 talking about today, which is to evaluate what happens
21 after mergers. But in a sense, there's an even more
22 difficult problem that arises before this, which, of
23 course, the agencies, by which I mean Justice and the
24 FTC, confront, and that is that they actually have to do
25 a prediction before a merger takes place and decide

1 whether to challenge it. And for those of you who might
2 want to take a look at that, there was a lot of
3 econometrics done in the recent Oracle trial, which I
4 should disclose that I was Oracle's witness, and that is
5 all up on the website from the court in San Francisco.
6 There was quite a bit of econometrics done there and you
7 can see what Preston McAfee, who was the government's
8 witness -- he's a professor at Cal Tech -- and I did in
9 terms of trying to predict before a merger takes place
10 what would likely happen.

11 Here, today, we're looking at what will happen
12 after a merger, and I'm going to make some general
13 econometric points. I decided I'm only going to take a
14 look at the -- of the types of models that were used.
15 For the two GAO models, I'm only going to look at the one
16 in which you have the indicator variable or dummy
17 variable approach of the merger. The HHI approach we'll
18 leave to a later session.

19 There's been a lot of work in the last 20
20 years, perhaps started in large part by my colleague Josh
21 Angrist, in labor economics about using natural
22 experiments. The typical natural experiment is the state
23 passes a regulation. Part way through a sample period,
24 you have a panel of states, and what you do is you
25 compare what happens in the states that change

1 regulations to what happens in the states that didn't
2 change regulations. That's called a natural experiment.

3 I think that's had a lot of influence recently
4 in applied econometrics. But I need to say that I think
5 the problem here today is a good deal more difficult than
6 this state regulation example, and this is something
7 which was realized by both studies that were introduced
8 before, but perhaps was not sufficiently appreciated.
9 That is, in a merger, we typically expect that the
10 structure of the markets will change after the merger.
11 It's not to say this can't happen when you changed
12 regulations in the state, but in a merger, anything
13 that's close to problematic -- I mean, I'm not talking
14 about, you know, two dentists merging their practices in
15 Phoenix, but more generally, we expect the market
16 structure to change and, of course, that's what the
17 agencies get worried about, that that may lead to
18 anticompetitive effects.

19 So, no matter what approach you use -- and I'm
20 going to talk about the two approaches today -- I think
21 you need to think very hard about what are we going to
22 do, or what am I going to do, about this structural
23 change which is very likely to take place. This is my
24 overarching comment today. I'm not going to get into too
25 many particular critiques of the models. When this

1 conference was first set up, I thought we were going to
2 get the data and I actually volunteered to do my own
3 analysis, but I guess for the usual confidentiality
4 reasons, we were never given the data. So, I don't have
5 a strong view on exactly what the right answer is.

6 But when you take into account that the
7 structure of the market is going to change, at least the
8 markets in which the merger has an effect, I think you
9 need to test for this structural change and this is what
10 makes it different from the premerger case. In the
11 Oracle trial, we could look at various situations in
12 which there were different numbers of bidders and
13 different numbers of products being bought. But you have
14 to make the assumption that after the merger, things are
15 going to work very much like they worked before the
16 merger in certain observational situations.

17 After a merger takes place, you can actually
18 test for whether or not the structure of the market
19 changed and whether you're capturing it. Since you have
20 observations before and after the merger in both merger-
21 affected markets and non-merger-affected markets,
22 typically, the easiest way to think about it -- I'm not
23 saying that you wouldn't want to use a more sophisticated
24 method -- but the easiest way to think about it is to
25 allow interactions of the right-hand side variables or,

1 in other words, allow the coefficients to change pre and
2 post-merger. And since you have merger-affected markets
3 and merger non-affected markets, you can do that.

4 It's as if when you're doing a regression, you
5 know, and you want to put in dummy variables, you not
6 only have a dummy variable that allows the indicated
7 variable -- that allows the left-hand side variable to
8 shift, but you also allow the coefficients on the right-
9 hand side variable to shift, and really in neither
10 approach that I'm going to discuss, you know, the FTC and
11 GAO approaches before, was that done to the extent that I
12 think it probably should be?

13 With that being said, now I'm going to give an
14 overview of identification approaches and spend a little
15 bit of time talking about both studies.

16 So, what is identification? Well,
17 identification means you want to be able to estimate the
18 price effect of a merger. However, the effect takes
19 place over time and other events occur, and these go by
20 very many different names and statistics in econometrics,
21 but a common name is confounding factors.

22 And so, in some sense, you have to control for
23 these factors. So, if you just looked at a market in
24 which there is a merger. Let me take Boston. The New
25 York Times bought the Boston Globe 10 years ago, probably

1 in the early '90s, if you looked at ad rates in Boston,
2 and compared the early '90s to late '90s, you're going to
3 find that ad rates went up a lot. You would incorrectly
4 conclude that that merger had anticompetitive effects;
5 while, in fact, what happened in the '90s is the tech
6 boom hit Boston and all my former students were getting
7 rich and so there were a lot of help wanted
8 advertisements and so on.

9 So, then the idea is, well, you pick a city
10 that that probably didn't happen in, New Haven, you know,
11 which was -- Macy's was closing and the last hotel was
12 closing and so it was going downhill, which has been
13 happening for the last 20 years, and that provides your
14 control. So, you must control for these other factors.

15 Now, of course, we at MIT would never think
16 that Yale could provide a control, but that's sort of the
17 idea.

18 Okay, so, there are two factors involved. A
19 priori, can you specify a model that allows you to tell
20 merger effects from other factors? This identification
21 is a problem Tjalling Koopmans posed in the late 1940s,
22 he started the identification analysis in econometrics.
23 And it's important to note that identification is always
24 based on non-testable prior knowledge. So, in the end,
25 reasonable people, or perhaps unreasonable people, can

1 differ about this. You should always remember that. I
2 always tell my students when I introduce this at MIT that
3 in different cultures, everybody has the same assumption,
4 this is sort of Claude Levi-Strauss, but, you know, the
5 Indians think that the world is on top of a turtle and
6 then on top of the turtle is another turtle and so on.
7 But at some point, at the bottom of the pile, there has
8 to be a turtle that you believe in that allows you to
9 identify things. And this is typically non-testable,
10 which is very important.

11 Now, if you have more than enough knowledge,
12 than you can test the over-identifying restrictions using
13 approaches started by Denis Sargan or myself.

14 Then the second point, which I'm not going to
15 talk a lot about today is, given identification, can you
16 estimate effects precisely enough to be useful? That
17 then turns out to be a question of efficiency of the
18 estimator and the amount of data you have and there's
19 always this trade-off. You'd like to add more and more
20 data. For instance, in the GAO study, they look at a lot
21 of mergers together, which will allow them to get more
22 efficient estimates. But then, of course, you run the
23 risk when you do that that the various mergers may not
24 really belong to the same underlying model.

25 This depends on the correct size and power of

1 the test. And another worry is that pretty much all of
2 the GAO stuff is based on general least squares
3 estimation, and as I'll point out later, it's pretty
4 clear that the standard errors that they calculate are
5 all biased downwards. However, by and large, I'm not
6 going to make a big deal of this because the effects the
7 GAO found were strong enough that if you correct for
8 that, it might not make much of a difference. But since
9 I don't have the data, I really -- you know, I can't
10 determine that for sure one way or the other.

11 Okay, now, let's think about identification a
12 little bit more. Regression gives the conditional
13 distribution of price or price change given the right-
14 hand side variable. So, you should always remember that
15 you can always go out and estimate a regression and the
16 regression always gives you the conditional distribution
17 and you can always do this by using least squares. You
18 can use a fancier method to get more efficient or more
19 precise estimates, but least squares gives you the
20 conditional distribution.

21 The identification problem then is, given the
22 conditional distribution, can you uniquely determine the
23 structural model that leads to the conditional
24 distribution? Now, you might say, well, look, I learned
25 this in my simultaneous equations class, but that's not

1 really what we're talking about here. But, it is what
2 we're talking about here because this whole argument is
3 about whether there are left-out variables, do
4 coefficients change and all that. Well, you have
5 estimated a conditional distribution, but the question
6 is, can you rule out the presence of left-out variables
7 and other things, other potential problems, in order to
8 allow you to get at the structural economic model here,
9 you know, the effect of the merger?

10 And the main problem is, since other factors
11 are changing over time, you need either to control for
12 them or to determine, based on a priori knowledge, that
13 they are uncorrelated with the merger effects.

14 Well, what are the approaches to this? The
15 first is, which we always talk about as econometricians
16 and then skip over, if life were good -- and then we'd be
17 out of business -- you'd have randomized experiments.
18 However, it turns out even randomized experiments don't
19 do it for you in economics because you have attrition.
20 So, I wrote some papers on this a long time ago.
21 Economists got very psyched up about experiments. We had
22 all the negative income tax experiments, but it turns out
23 that people drop out of experiments. When R. A. Fisher
24 did this in England, the pea pods didn't drop out of the
25 experiment, they had no choice. But, unfortunately,

1 here, you don't have that luxury.

2 However, you may have what's called a natural
3 experiment, and this is where Josh Angrist came in, that
4 approximates a randomized experiment. So, his natural
5 experiment turns out to be the draft lottery during the
6 Vietnam War, and that was the natural experiment.

7 More often, and more recently, what people have
8 done is look at a change in regulation. I've already
9 given you that example. Certain states change a nursing
10 home regulation; other states don't, and then you take a
11 look and see the effect on nursing home utilization, for
12 instance.

13 Well, here, what we typically have are
14 geographic markets affected by a merger compared to
15 markets not affected by a merger. The one thing I don't
16 have time to talk about today, but really is quite
17 important, is it's often very difficult to define
18 geographic markets. I mean, we can get into long debates
19 about those during mergers, and if you don't get
20 geographic markets right, it can lead to problems.
21 However, in thinking about the studies today, it seemed
22 to me that one of the problems would be, if anything, it
23 would attenuate the estimated effects, and that doesn't
24 seem to be the problem. But, nevertheless, this
25 geographic market definition can be quite difficult.

1 However, if you have geographic markets, this
2 is Boston versus New Haven in my example, then you have
3 the time period before the merger, before the New York
4 Times bought The Globe and the time period after the
5 merger, and you do this double comparison of Boston to
6 New Haven, before and after, and this is called the
7 differences-in-differences approach, DID.

8 Now, it turns out that DID, as done by labor
9 economists, is not efficient and they also get the wrong
10 standard errors, and, not surprisingly, I have a paper on
11 that, but that's not what we're here to talk about today.

12 So, the second method you can use is
13 instrumental variables(IV), In instrumental variables,
14 you use a priori knowledge again that the instrument is
15 correlated with the right-hand side variables that are
16 orthogonal, in other words, uncorrelated to the
17 stochastic disturbance which is often called the
18 residual. And in a survey paper that I wrote back in
19 '82, I demonstrated that in some sense all IV estimation
20 comes down to an IV approach. So, this may be a useful
21 way to think about things, and you can put differences-
22 in-differences into an IV approach as well, and then it
23 becomes quite straightforward to see what the assumptions
24 you need are.

25 And here that would be an exogenous change. We

1 might well think that the world price of oil is exogenous
2 to what goes on mostly in the United States, although the
3 U.S. is a big consumer, and this would allow you to get
4 identification.

5 Okay, let's go back to ordinary least squares.
6 So, the orthogonality of the un-correlation assumption is
7 typically based on economic analysis. So, this is why
8 econometrics is different from statistics and we're able
9 to think about the problem and decide what may be there
10 or what may not be there.

11 The biggest thing that you have to worry about
12 is possible omitted variable bias -- if there's some
13 variable that you've left out of your model that changes
14 over time and it's going to get confounded with the
15 merger effect that you want to estimate. So, you want to
16 do a sensitivity analysis, and the real question you want
17 to ask is how sensitive are results to a small failure of
18 the orthogonality assumption? It would be very unlikely
19 that we'd be in a situation in which you believe that the
20 orthogonality holds exactly when you want to look at a
21 small failure.

22 So, it turns out, for instance, in the IV
23 situation with weak instruments, a very small failure of
24 orthogonality can be absolutely disastrous. Not
25 surprisingly, I have a paper about that as well, which is

1 for another -- yet another seminar.

2 But, ordinary least squares may be less
3 sensitive to a small failure of orthogonality. So, this
4 would be the kind of thing that you would want to think
5 about.

6 In my view, and this is sort of the one thing
7 I'm going to say about how the agencies work. I guess
8 Deb Majoras has long since left. But I think general
9 complaints or questions do not invalidate results. So,
10 I've come in many times to discuss my analysis of a
11 merger. Staff, of course, wants to get its way; I want
12 to get my way. You know, let's face it, that's how human
13 nature works. But when the staff doesn't like the
14 results, the staff will say, well, you know, there may be
15 this variable you left out and the staff always "wants
16 the benefit of the doubt," and I think maybe they get it
17 when they have the star chamber meetings with the
18 Commission here, in which, of course, we're not present.
19 You have to be a student of history to know what star
20 chamber means, but you can look it up on the web.

21 But anyway, in my view, the key approach should
22 be does it matter, not that you can think of something
23 that might hypothetically be wrong.

24 So, testing approaches with the prior
25 information, you can do the Hausman Specification Test,

1 GAO did that type of thing. You can do the Sargan Test
2 of Over ID, which the GAO also did, although they nicely
3 named it after me, but it's actually Denis Sargan who
4 used to teach it.

5 But I think another thing that you can do which
6 is quite useful in these type of tests -- and
7 interestingly enough, the FTC staff technical report
8 mentioned this, but didn't actually talk about how to do
9 the tests, which I'm going to call the Hausman-White-
10 Newey-Berans test. So, again quoting Luke, Hausman,
11 White and Newey -- Hal is here and Whitney Newey, of
12 course, a world famous econometrician, who's also a
13 student of mine, but Herman Berans' is not and I take no
14 credit for it, now at Penn State.

15 Okay, so, now what I want to do is turn to the
16 particular studies and talk about them and what they must
17 assume and what may or may not hold true. So, the first
18 is the paper of Taylor and Hosken, which was described a
19 few minutes ago, and which I found very interesting. So,
20 the question is whether the Marathon-Ashland joint
21 venture (MAP) led to higher, more volatile gas prices in
22 the Midwest. In the end, they basically just look at
23 higher, not more volatile. They look at Louisville,
24 which I can never pronounce quite correctly, and they
25 look at the wholesale and retail prices of gasoline.

1 Their basic approach is to compare Louisville to non-
2 merger geographic markets, and then they use the basic
3 DID approach that I mentioned before.

4 So, they have data on price changes, but they
5 must control for exogenous changes in supply and demand
6 and market structure that may have affected price.

7 So, they look at the price in Louisville
8 relative to other markets "unaffected by the merger"
9 facing similar supply and demand conditions. So, they
10 use Chicago, Houston and Northern Virginia markets that
11 use RFG and their claim is that Marathon was small in
12 Chicago and Ashland wasn't present, and then they claim
13 similar demand positions in Louisville and Chicago and
14 similar cost conditions, and retail margins are
15 significantly higher in Chicago, which the FTC might want
16 to investigate all by itself -- I thought that was an
17 interesting fact -- by about 50 percent. But,
18 nevertheless, they thought this was stable over time.

19 Now, I think the crucial non-testable
20 assumption is that Chicago, Houston and NVA stands for
21 Northern Virginia, are unaffected by the merger. That's
22 the bedrock assumption of this study. So, if you go back
23 and you think about things in my Boston/New Haven
24 example, New Haven wasn't affected by the merger. I
25 don't think anybody in New Haven ever looked at the

1 Boston Globe. But again, you're saying whatever changed
2 after the merger, we can look at New Haven and take out
3 the time effect in New Haven and subtract that from
4 Boston and then look at the effect of the merger. So,
5 that's the absolute key assumption that you want to get
6 your arms around.

7 They use one year before the merger and two
8 years after the merger for comparison and they look at
9 the difference between Louisville and Chicago for
10 wholesale prices, retail prices and margins. They don't
11 see any significant change in the retail prices; but find
12 that the Louisville wholesale price has increased
13 significantly about 15 months after JV. They then do a
14 regression approach using DID, and the important
15 assumption, again, is that they need to assume that the
16 time effects, whatever the demand and supply shocks, are
17 common across Louisville and the control cities.

18 So, again, what they have to assume is that, to
19 the extent that the market structure changed in
20 Louisville, whatever happens in terms of non-merger
21 supply and demand effects, those can be removed by what
22 happened in Chicago. I'm just going to say Chicago
23 rather than just mentioning all three markets.

24 So, again, I wasn't involved in this merger,
25 but around Boston, there were a lot of Exxon and there

1 were a lot of Mobil stations, and let's just assume when
2 Exxon and Mobil combined -- this isn't actually what
3 happened -- when Exxon and Mobil combined, all of the
4 Mobil stations disappeared. They're still there, but --
5 the market structure is going to change and then the
6 question is, if I look at some place which wasn't
7 affected by the Exxon/Mobil merger and the gas stations
8 stayed in the same and the racks and everything else
9 stayed the same, does that provide a sufficient control?
10 That's the question at issue.

11 So, they take a difference between cities and
12 that eliminates the time effect. This is very easy to
13 think about. After the merger, they compare before and
14 after in Chicago and see how prices changed. And they
15 say, well, the merger had no effect in Chicago, or not
16 enough to worry about, and so however the retail price
17 changed relative to the crude price, let's say, or the
18 wholesale price changed relative to the crude price, in
19 Chicago in the two years after the merger and the year
20 before, we're going to assume that this same effect would
21 have happened in the but-for world in Louisville. It's
22 no more complicated than that.

23 So, you can think about that as an economist.
24 You know, is it reasonable to say whatever happened in
25 Chicago would have happened in Louisville? So, the

1 crucial assumptions are that the time-indicated variables
2 are the same across Louisville and the control cities,
3 and two, the differences in the time-indicated variables
4 are orthogonal to the other right-hand side variables.
5 The second one is less likely to hold, but I put it up
6 because it's always possible.

7 Well, the findings are they don't find
8 statistically significant effects of the JV on retail
9 prices comparing against one city at a time. They might
10 have pooled the data because it's the stochastic
11 disturbances that are likely correlated across city pairs
12 and that would allow them to get more efficient
13 estimates. So, they said that the joint venture wasn't a
14 big problem. The finding is the retail prices did not
15 change in the post-merger period but wholesale prices did
16 so that the retail margins contracted, and they conclude
17 the change in the wholesale price is due to the demand
18 shift from St. Louis entering the RPG program.

19 The St. Louis explanation is plausible but not
20 part of the model. So, it would have been better, in my
21 view, to model St. Louis and to estimate what happened
22 there before and see how comparable it was. Instead,
23 they use an ex post explanation of results. Now, I used
24 to write a lot of papers with a world-renowned
25 econometrician, who unfortunately died a few years ago,

1 named Zvi Griliches, and I used to always kid Zvi that no
2 matter what came out of the computer, he could always
3 rationalize it, ex post.

4 So, I would say to him, "Zvi, what do you
5 expect to happen?" before we ran this next maximum
6 likelihood, and then get him on the record as far as to
7 what his beliefs were. If you like to do economic
8 theory, you need to do the same thing or otherwise people
9 will say afterwards, oh, that theorem was obvious.

10 But there was always this worry that ex post
11 rationalization of the results, especially when you only
12 have one sample point. Zvi could always explain the
13 result for one sample point. The question is, if you
14 have a lot more sample points and you have to ex post
15 rationalize it, then it becomes a little bit more
16 difficult.

17 So, that is the reason then that structural
18 model that specifically controls supply and demand may be
19 superior to the difference-in-difference or event study
20 approach because that approach wouldn't have to give
21 this, bringing St. Louis in afterwards to explain things.
22 It could say, when St. Louis shifted to this, this is
23 what will happen and this is what happened in Louisville.
24 I'm not criticizing things, I'm just saying that this
25 would be another approach.

1 But one economic surprise I got was that the
2 retail margins contracted given the degree of expected
3 competition among retail outlets. I didn't quite
4 understand the explanation given in the paper, I must
5 say, because if you expect prices to be set at the margin
6 and these other stations remained open, unless they were
7 selling, you know, lotto tickets or other things that I
8 shouldn't know that they were selling, it's surprising
9 that they were able to remain open after their margins
10 had contracted so much. And I say the explanation of
11 company-owned versus other stations may not be consistent
12 with profit maximization. But, again, that's an
13 explanation for another time.

14 Okay, now, we turn to the GAO study. It turns
15 out that I did these in the same order as Dr. Taylor's
16 presentation. So, I want to look at the identification
17 (ID) strategy for the individual mergers, and as I said,
18 I'm not going to look at the increased concentration.
19 I'll leave that to the later panel.

20 So, GAO used data from '94 to 2000 on wholesale
21 gasoline prices. I have some worries about how the data
22 was constructed, but since I don't have the data and I
23 couldn't test it, I'm just going to mention that in
24 passing and move on. So, they built a reduced form
25 econometric model. The left-hand side variable is the

1 wholesale gasoline price minus the crude price. This
2 assumes a constant relationship between the two, which
3 can create problems. They tested it to some extent. On
4 the other hand, it's a good idea because you would expect
5 the gasoline prices to be integrated of order one, but I
6 would expect wholesale and crude prices to be co-
7 integrated, so subtracting them off this way removes a
8 potential problem. So, to that extent, it's a good idea.

9 Okay, so the right-hand side variables are
10 city-fixed effects and time-fixed effects. I certainly
11 think that city-fixed effects should be used here, so I
12 agree with the GAO. You should not use a random effects
13 approach here. I mean, you can test for it with the
14 Hausman Specification Test, but I'm virtually sure you
15 would reject. They have the indicator or dummy variables
16 for the merger and then they have -- and these are the
17 important variables, gasoline inventory ratio, refinery
18 capacity utilization rates. Those are the two economic
19 variables and they have dummies for the supply
20 disruptions.

21 So, these gasoline inventory ratio and the
22 refinery capacity utilization rate, this has to be what
23 controls for what differed in Boston and New Haven after
24 the merger of The Globe and The Times, okay? So, these
25 two economic variables, you know, they compare to

1 different PADDs or a lot of different markets, if you
2 want to call them that, and, you know, after the mergers,
3 things change over time, and these two variables have to
4 capture it and have to control for it. So, that's very
5 important.

6 Okay, initially, they assume that all right-
7 hand side variables are exogenous, although they also do
8 IV estimation. They assume that the variables are
9 measured without error, although they realize that this
10 assumption may not be true, and then they do fixed
11 effects estimation. So, the fixed effects allows the
12 right-hand side variables to be correlated with the city
13 component, so I think it's a very good thing. However,
14 the fixed effects can exacerbate the errors-in-variables
15 problem. That's what the IV stands for. And I think
16 they might have wanted to test this using the log
17 difference approach of Hausman and Griliches of '86,
18 because it seems to me that, especially since they
19 couldn't get data on the right-hand side variables for
20 each market. At best there are sort of proxies, which is
21 an errors-in-variables problem. So, you might want to
22 test for that.

23 They assume that the merger effects for a given
24 merger are the same across racks. It seems to me that
25 that is an assumption which may not be true -- which they

1 may also have wanted to test. And then they assume that
2 the coefficients are a constant across racks, so the
3 effects of the inventory ratio and capacity utilization
4 are constant across cities or areas, whatever you want to
5 call them. And, again, I think this is a problematic
6 assumption that they might want to test, and in
7 particular, the difference in the coefficients might be
8 correlated with the stochastic disturbances.

9 Again, I'm just saying these are things -- I'm
10 not saying that there's anything wrong with the models,
11 because I don't have the data, I can't test these, but I
12 thought it was worth, at least, thinking about.

13 They do a Hausman Specification Test for joint
14 endogeneity. If I understood what they did correctly and
15 I wasn't sure that I was -- but they seemed to just use
16 time and weekly dummies as excluded instruments. It
17 seems to me that weekly dummies might -- well, maybe they
18 should be in the regression to start with, so it wasn't
19 clear to me that they meet the exogeneity assumption that
20 you need for instruments. It seems to me there's likely
21 to be a seasonal effect in wholesale prices for gasoline
22 in places like California, although, again, that's just
23 me assuming things, not actually testing them.

24 They also did a test of over-identifying
25 restrictions, so that was good. It did not reject.

1 They used feasible generalized least squares to
2 get more efficient estimates. Now, here it's important
3 because FGLS assumes that you know the covariance matrix,
4 and since you're testing things here and you want to test
5 dummies, this can create possible bias in the true size
6 of the test. I will put up a paper that I wrote this
7 year on that, doing second order correction. But, again,
8 most of the results were so significant, I don't think
9 this would have changed this, but I think their
10 t-statistics are probably pretty much upward biased.
11 Alternatively, you can just do straight fixed effects and
12 correct for the estimated standard errors.

13 Okay, now, to finish up, what I want to do is
14 to compare the two approaches. So, I think this is an
15 interesting contrast between the two approaches and this
16 has been seen in the context of program evaluation for
17 the last 50 years. So, everybody who wants to do some
18 program evaluation, which, of course, Washington runs off
19 -- we're going to give school vouchers, we're going to do
20 no child left behind, we're going to put computers in
21 classrooms, we're going to have a negative income tax
22 experiment and on and on and on and on -- does it have an
23 effect. So, this has been thought of, you know, and
24 investigated a lot by econometricians and statisticians.

25 Well, I would say that Taylor/Hosken is what's

1 traditionally called a matching model. So, there what
2 you want to do is you want to find nearly identical
3 control units not affected by the intervention and do
4 DID. So, this is matching. So, we have some effect. We
5 send -- we give -- Luke will like this. We give Luke a
6 million dollars, it lands on him and we use me as a
7 control and we see how the difference in our expenditure
8 patterns change when Luke -- you know, Tobin Helicopter,
9 puts a million dollars in his backyard, I don't get the
10 million dollars and we see how the behavior changes for
11 income effects, okay? So, the assumption has to be that
12 after Luke gets a million dollars that in the but-for
13 world, he and I would have behaved similarly without it.

14 So, then the question is, are the controls good
15 enough? Well, they have to be not affected by the
16 intervention and then you do DID. So, the critical
17 assumption is the control units are not affected by the
18 event and, of course, the GAO claims -- and I don't know
19 one way or the other if this is true -- that MAP merger
20 affected some of the control areas. I think it said
21 Northern Virginia and it might have said Chicago. If
22 this is true, of course, you then contaminate your
23 result.

24 The assumption of the control units is
25 fundamentally non-testable since it's based on a priori

1 assumption. So, again, I just want to keep returning to
2 this is that, you know, Taylor-Hosken say, we think
3 Chicago is good enough; the GAO says, we think Chicago's
4 not good enough because it was affected by the merger,
5 and there you are, and then you go see Judge Walker.

6 Now, the GAO takes a regression approach. It's
7 a reduced form that they could have fit a structural
8 model, as well, and they try to control for these other
9 factors. Their crucial assumption is that the included
10 right-hand side variables control for the economic
11 factors that affect prices. So, let me put the supply
12 disruptions off to one side. They're basically saying
13 that the inventory ratio and capacity utilization --
14 they're making two assumptions. First is that these
15 variables control for everything and, secondly, that the
16 coefficients are the same in the merger and non-merger
17 areas, and thirdly -- I guess they make three assumptions
18 -- in the merger areas, those coefficients don't change
19 after the merger, which you might well think that they
20 would. The inventory ratio and the capacity utilization,
21 if you have a merger, those coefficients could well
22 change.

23 So, the crucial assumption is that the included
24 right-hand side variables control for other economic
25 factors, and the assumption is that the left-out factors

1 are not correlated. Now, I would think coefficients
2 changing is the same as a left-out variable because you
3 should have had an interaction. You don't have it. So,
4 it's really just the same thing. So, you have these
5 crucial non-testable assumptions and this is the basis of
6 many of the FTC staff comments, in their technical
7 report.

8 However, the one thing that, I think, could
9 have been done, and the FTC staff's technical report
10 mentioned it, is when you do ordinary least squares and
11 generalized least squares, the estimates should be quite
12 close. The FTC technical report is not quite correct.
13 It says that they're both unbiased. They're both
14 consistent, but not unbiased. But the idea is right.

15 So, there's this approach from the Hausman-
16 White-Newey, which all comes down to pretty much the same
17 thing in different guises, but it says that when you do
18 GLS, you have the efficient estimate; when you do
19 ordinary least squares, you have a consistent, but
20 inefficient estimate. If you take the difference between
21 the GLS and the ordinary least square coefficients and
22 take the quadratic form of that with the variance/co-
23 variance matrix, then the difference in the estimate,
24 which is the difference in the variances from my '78
25 paper, you can actually do a test. And the FTC staff

1 technical report on page 20 states that the estimates are
2 sensitive to the use of GLS. This is actually a
3 generalized way to test your orthogonality, to test these
4 crucial assumptions of the GAO model.

5 So, the FTC makes the point -- they have
6 slightly different data, but I mean to the extent their
7 point is right -- that things are sensitive to GLS, that
8 says to me that almost surely you have a potential
9 orthogonality problem and that should be tested. And
10 this, you know, could be tested using any statistics
11 package going, including STATA or whatever was used by
12 the GAO researchers.

13 Okay, so, to conclude, the Taylor/Hosken
14 approach must assume that the control areas are not
15 affected by the merger, so the time effects and the
16 control areas must be the same as in the merger areas,
17 which means that the control areas are similar to the
18 merger areas, may be difficult to find valid control
19 areas. Then the time effects from these control cities
20 or areas are used to eliminate the time effects in the
21 merger cities.

22 The GAO approach must assume that the right-
23 hand side regression variables control for the economic
24 effects after the merger, so they must control for both
25 changes over time and over market, and it may be

1 difficult to specify a robust single model with the same
2 coefficient that works across multiple mergers since a
3 merger changes a market structure.

4 So, I think it would have been interesting in
5 the GAO report, perhaps, to break apart the various
6 mergers and look at them one at a time or two at a time
7 rather than trying to do them altogether. But in my
8 final conclusion, I thought that both reports were quite
9 carefully done. You know, when you've been around as
10 long as I have, you always have ideas how to test things
11 and all. I think that you really need to focus, and I
12 wouldn't draw a conclusion that either approach is
13 better. I think that both of them have difficulties and
14 in a given situation, you actually may want to look at
15 both approaches. But I don't think that either approach,
16 in a typical situation, can solve all the problems. But,
17 of course, that's what makes econometrics interesting.

18 Okay, and I'll stop at that point. So, I guess
19 what we're going to do is just go through the panel
20 people. So, I'll first turn things over to Ken since
21 he's the first person here.

22 PROFESSOR HENDRICKS: I would like to reiterate
23 what Jerry has said. If you look at the Taylor/Hosken
24 study, the crucial assumption is that the merger or joint
25 venture, did not affect the control areas. In this case

1 they tried three different types of control.

2 With respect to that question, I found it a
3 little bit disconcerting that they found that the change
4 in demand in St. Louis, the change in regulatory
5 standards, had a big impact, according to their
6 explanation, on rack prices in Louisville. One wonders
7 to what extent the geographical, to what extent is the
8 rack market in Louisville really isolated from the rack
9 market in Chicago?

10 You can think about the situation of refiners
11 in the Gulf. They have to make decisions on where to
12 supply their gasoline to different markets, and there may
13 well be a lot of arbitrage possibilities across the
14 geographical markets in wholesale gasoline.

15 You can imagine retail markets being much more
16 geographically isolated. There is a follow up point I
17 would like to make on retail markets. The results of the
18 TH study are quite different from the result of Justine
19 Hastings studies. In the one study involving Tosco and
20 Unocal, she finds, using essentially the same kind of DID
21 approach, that wholesale prices did increase as a result
22 of the merger, ranging from one to two cents in various
23 rack markets throughout California.

24 In another study she looked at retail markets
25 and studied the impact of ARCO taking over some Thrifty

1 stations. Using essentially the same DID estimation
2 approach, with pretty satisfactory controls, fixed
3 effects at the station level, she finds that the takeover
4 did result in an increase in retail prices at competing
5 stations to Thrifty.

6 So, there is a puzzle as to why the results for
7 Louisville seem so different from the results that
8 Hastings got in both wholesale markets and in retail
9 markets in California. And one potential explanation
10 might well be that California is much more geographically
11 isolated than Louisville.

12 So the question here is what is the right
13 geographical definition of the rack market? Because, if
14 prices are arbitrated across different geographical rack
15 markets, then you're not going to pick up much of a price
16 effect at the wholesale level and you may not at the
17 retail level either. Well, you may pick up some effects
18 at the retail level if the merger makes these markets
19 more concentrated.

20 I also found it somewhat puzzling that the rack
21 price increase that occurred about 10, 15 months after
22 the merger was not passed on to retail prices. That fact
23 does not seem consistent with economic reasoning. You
24 would think that the opportunity cost of selling gas to
25 your own stations is going to be what you can get for it

1 in the rack market selling it to an independent. It may
2 be true that the independent competes against you, but if
3 you see a big increase in the wholesale price, you may
4 think that it would be more advantageous or more
5 profitable to shift some of your supply away from your
6 own stations and sell it to jobbers who are servicing
7 your rival stations.

8 With regard to the GAO study, my big concern
9 with that study was with the assumption that the error
10 terms in those regressions were uncorrelated with the
11 merger dummies, because the regressions did not include
12 much in the way of explanatory variables, at least at the
13 level of the geographic market. The explanatory
14 variables were either at the PADD level or they were at
15 the national level like the utilization rate. The only
16 thing that is varying at the city level to explain
17 changes in the city rack market price are the merger
18 dummies.

19 So, a lot of the economic forces are not being
20 measured and they are sitting in the error term. You
21 have to wonder whether those market forces are not being
22 picked up by the merger dummies. I would worry a lot
23 about this issue of unobserved heterogeneity, because
24 throughout this period there were a lot changes in
25 regulations. These changes, in particular the changes in

1 gasoline standards, have made arbitrage across markets
2 much more difficult. Once you make it more difficult to
3 arbitrage price differences across markets, prices are
4 likely to rise. That's a trend throughout this sample
5 period, proceeding in different ways in different markets
6 but possibly correlated with the trend in mergers.

7 Another feature of the regulatory standards
8 that have evolved over this period has been the increased
9 incidence of outages, of refineries breaking down and
10 causing price spikes. Just recently, an MIT student, who
11 is on the market this year, studied the impact of the
12 changes in regulatory standards. If you look at this
13 list of 44 outrages that he's examined, a lot of those
14 outrages have occurred in the latter part of '90s. So not
15 only are changes in regulations causing prices to trend
16 up, they are also causing price volatility to rise over
17 the latter part of '90s.

18 Since the mergers are occurring mostly in the
19 latter part of the '90s, it is important to control for
20 the changes in regulatory standards. If not, the merger
21 dummies are likely to be picking up some of the effects
22 of the changes in regulation. These kinds of controls
23 are not included in the GAO study which leads me to
24 question some of their results, or at least their
25 interpretation of the results. Let me stop here.

1 PROFESSOR HAUSMAN: Okay, let's move on to
2 Dennis Carlton.

3 PROFESSOR CARLTON: Okay, since there are
4 several other sessions, one of which I'm in charge of and
5 one in which Hal is in charge of, I don't want my
6 comments to be too duplicative, so I think I'll keep
7 these comments brief and expand on them more in my
8 presentation which is the next one.

9 Let me first start out by making a slight
10 correction to the introduction. Luke indicated that I
11 have not been involved in energy mergers. That's not
12 true. I have been involved in energy mergers.

13 Now, as Jerry said, we don't have the data, so
14 it's easy to poke holes in other people's studies, so I
15 do want to say that I thought that both studies were
16 very carefully done. It was clear that everybody was
17 trying to do a good job.

18 Let me first talk about the FTC study, just
19 very briefly. I think the central question is, where is
20 the marginal supply of RFG gas for Louisville? And I
21 think if you read the very beginning of the report, you
22 kind of get an indication of what the answer is and I'm
23 not sure that squares with the findings. If there's a
24 pipeline between X and Louisville, you have to explain
25 why X's price is different from Louisville's price, when

1 presumably the pipeline is still operating. I think
2 that's the central question, and I can leave some of my
3 other comments for later.

4 Picking up on something that Ken just said
5 there have been some other studies that find the result
6 that there may be an effect on the rack price, but not an
7 effect on retail. I think that's an unusual result and I
8 think I'll talk a little bit more about that in the next
9 session.

10 Let me turn briefly to the GAO study. Like I
11 say, whenever you do an empirical study, it's easy for
12 someone to say why didn't you measure the variable this
13 way or that way. I think the best answer to that
14 question is to recognize that the purpose of a data
15 analysis is to use the data to make inferences and not to
16 get overwhelmed with criticisms. On the other hand, you
17 have to be aware that there are criticisms of any data
18 set, and that goes both to the FTC and the GAO study.

19 I guess this would apply to both studies, the
20 way you deal with that is, I think, to do a sequence of
21 robustness tests to show that no matter how you estimate
22 things, no matter what econometric technique you use, or
23 more importantly, no matter how you measure your
24 variables -- because people can always question what's
25 the market, is it big, is it small, is it this, is it

1 that, and no one's ever going to get it exactly right.
2 The best way then to have confidence in your findings is
3 to measure variables in a whole variety of different ways
4 that are plausible and show that your results survive.

5 I'll talk more about that in my comments, but
6 it seems to me that is something that, especially when I
7 read some of the FTC staff technical report's re-
8 estimations, raised some fundamental questions about the
9 GAO results. And on the flip side, the GAO has raised
10 questions, I think, that are valid about the FTC study.

11 The one question I do want to specifically ask
12 about mergers, because I was not sure of exactly how it
13 was done in the GAO study, is I wasn't quite sure I
14 understood the discussion of the timing of how the window
15 was chosen. It appeared to me, maybe I just misread it
16 or I didn't understand it, that it wasn't just zero one;
17 in other words, premerger, zero; post-merger, you'd think
18 it would be one forever. But there are subsequent
19 mergers, but that just means the effects, presumably,
20 should cumulate unless you want -- you have some
21 hypothesis that the effect of the second merger is less
22 than the first merger or something like that, or maybe
23 the effects of a merger differ across geographic areas.

24 That I understand, but I couldn't quite
25 understand whether they were turning off a merger

1 variable, the first merger variable when the second
2 merger occurred or whether they were cumulating. It was
3 just a little unclear when I read it. Maybe -- I assume
4 there's no one from the GAO in the audience who could
5 answer that question.

6 **(Participant does not speak with a microphone**
7 **causing parts of his statement to be inaudible.)**

8 DR. SCOTT FARROW (OF GAO): Actually, I can
9 answer that one. They're not turned off, they continue
10 consistently throughout. It's just an attempt to explain
11 that the actual (inaudible) identification of the merger
12 effects was (inaudible) other mergers (inaudible)
13 basically coded zero-one.

14 PROFESSOR CARLTON: So, it is zero-one, it's
15 one forever and therefore it cumulates.

16 DR. FARROW: Right.

17 PROFESSOR CARLTON: So, the assumption would be
18 that the effect of a merger, the second merger is the
19 same as the first merger and the effect of the third
20 merger is the same -- I'm sorry, the effect of the -- a
21 rack where there are two mergers is the sum of two
22 effects and the effect of the second merger is the same
23 in that area where there was a previous merger as in an
24 area where there was no merger.

25 DR. FARROW: Your last question is going

1 further than I can respond to. Just in terms of the
2 coding, it was sort of zero and then one forever.

3 PROFESSOR CARLTON: Yeah, I think what I said
4 is then an accurate summary, okay. Why don't I stop
5 here.

6 PROFESSOR HAUSMAN: Okay, we'll move on to Hal
7 then.

8 PROFESSOR WHITE: Well, I'd just like to
9 reinforce a couple of the points that Jerry made and then
10 add a comment or two, and this has to do with robustness
11 that arises or may not be there because of the way that
12 the models are specified, and in particular, the fact
13 that we have two specifications, each of which has the
14 imposition of constant coefficient for all the other
15 explanatory variables, pre and post-merger, or in the
16 unaffected cities and in the affected cities.

17 That's a very strong assumption, and it's
18 something that actually I wouldn't even go so far as to
19 test. I would just let the data estimate those
20 coefficients. But, you know, if you think it might help
21 gain efficiency, I suppose it would be worth a test. But
22 my inclination would be to include interactions with
23 those dummy variables from the outset. So, that's not
24 only pre and post-merger, but also potentially across
25 cities or, at least, across groups of cities where the

1 effects might be different, especially in a situation
2 where we don't have a lot of different variables
3 controlling for different effects across cities, or at
4 least across groups of cities, where the effects might be
5 different.

6 Now, if those restrictions are, in fact, not
7 true, but they're imposed, then basically the effect of
8 the merger that's being measured is comparing the average
9 price that happened post-merger in those areas that were
10 affected by the merger to something which is a blend of
11 what the price would be expected to be in the absence of
12 the merger, but that blend is taking into account both
13 merger and non-merger observations. By putting in the
14 interaction, you're essentially excluding from the but-
15 for price observations which come from the merger
16 observations and, therefore, incorporate some of those
17 effects.

18 Now, it's, in my view, critical to be very
19 careful about the specification, and in that view, it
20 makes it even more important if you're going to do
21 something like generalized least squares because
22 generalized least squares is intended as an efficiency
23 enhancement to something which is already giving you a
24 consistent estimate. And if the model is inconsistent to
25 start with, for example, because you've imposed these

1 restrictions, then doing generalized least squares is
2 going to, as Jerry pointed out, give you an estimate of
3 something else, not the same thing. And, in fact, if I
4 remember correctly, as I was looking through, I think it
5 was the GAO study, I was comparing the OLS and the GLS
6 coefficients and I was going, gee, you know, they did a
7 Hausman Test on these other aspects, I would have liked
8 to have seen a Hausman Test comparing the ordinary least
9 squares and the GLS, and I think that may be indicative
10 that there is something to have a concern about there.

11 Of course, the GAO is doing something very
12 ambitious with eight mergers and all of those different
13 racks, but it does make me want to, perhaps, do also what
14 Jerry was suggesting, which was to break the studies up
15 and to compare the mergers or to compare subsets of the
16 cities so that their particular effects might be better
17 measured.

18 Then there's one other comment just at this
19 level that I'd like to make, and that has to do with
20 corrections for autocorrelation. I think in both studies
21 we see that there are corrections for autocorrelation
22 after an initial test of some kind has revealed that
23 there might be, you know, a Durbin-Watson statistic
24 different than two, and I'd just like to comment that
25 just the appearance of autocorrelation doesn't mean that,

1 in fact, a first order auto-regressive model is the right
2 thing to do and that by doing the pseudo difference and
3 proceeding that one is necessarily solving the problem
4 that's being indicated.

5 I've often seen just the inclusion of a lagged,
6 dependent variable to completely solve the problem or
7 maybe two lags. So, whenever I see a Durbin-Watson
8 statistic or some other diagnostic of serial correlation
9 that might suggest doing some sort of GLS, I have to ask
10 myself, well, is that really just indicating some sort of
11 dynamic mis-specification that could be better addressed
12 by including a lag-dependent variable or, perhaps, lags
13 of judiciously chosen other explanatory variables?

14 And I saw that we're beginning to run up
15 against our time constraints, so I'll stop with those
16 comments.

17 PROFESSOR HAUSMAN: Thanks. We'll turn to
18 Scott then.

19 DR. THOMPSON: Well, first of all, I need to
20 preface my remarks, that my views are not purported to be
21 those of the U.S. Department of Justice.

22 It's hard to go last, especially when the
23 relevant test statistics are named after people sitting
24 to my right.

25 **(Laughter.)**

1 DR. THOMPSON: So, let me back up since the
2 title of this session was supposed to be General
3 Identification Issues in Merger Events. Let me take
4 maybe a longer view in order to comment on identification
5 from the point of view of something that will come up
6 later this afternoon, namely thinking about merger
7 enforcement.

8 One thing that has not been discussed yet, I
9 think, is what exactly is the effect itself? How do we
10 define the effect and what effect is of interest? Now,
11 there are different ways to think about effects. I
12 believe during the presentation we heard about, you know,
13 "before the merger" versus "after the merger." We've
14 obviously seen estimators that are comparing "here"
15 versus "there," where the "here" is where the merger is,
16 and "there" is where it's not. The conceptual but
17 impossible to measure effect that economists tend to want
18 to see, compares the actual results versus what would
19 have happened in the but-for world, which does not
20 actually occur.

21 There are further distinctions that we might
22 think about that matter a lot from the point of view of
23 merger enforcement. What was the actual effect measured
24 ex post, versus what reasonable expectation you could
25 have for an effect ex ante in the merger review process?

1 How you go about measuring these things depends, to some
2 extent, on what exactly you think the effect is that's of
3 interest. I think implicitly in both the studies, the
4 effect that they attempt to get at is a comparison of the
5 actual prices versus what they would have been in the
6 but-for world. But from the point of view of thinking
7 about what merger policy ought to be, that's not
8 necessarily the most interesting thing to look at.

9 Related to that is the fact that the mergers
10 themselves are potentially endogenous -- something that
11 has basically been assumed away in these studies, and
12 rightly so if you're trying to look at the effect of
13 particular mergers. But from the point of view of
14 enforcement policies, it might be quite important to take
15 into account the fact that whether or not a merger gets
16 proposed in the first place may have something to do with
17 expectations about price effects. And those, in turn,
18 are possibly dependent on expectations about prices that
19 the parties themselves have when they propose the merger.

20 The other thing to think about related to the
21 definition of the effect gets back to Jerry's point about
22 thinking about these things as natural experiments. One
23 feature of natural experiments is that treatments are
24 randomized, something which is explicitly not true here,
25 although it may have been in the Vietnam lottery examples

1 that he referred to. And the other is repeated trials,
2 namely that the same experiment is run over and over and
3 you look at the average effects over repeated trials.

4 These are both quite important for knowing that
5 when you look at the results of one of these empirical
6 studies, that you really are capturing the effect of the
7 treatment directly and not confounding it with other
8 things that are changing simultaneously to it.

9 I saw that Hal has a paper he's passing out in
10 the back that I think touches on this, so we may hear
11 more about it.

12 One obvious question to ask is, if we could run
13 a real experiment, is the result that you would get from
14 it something that you would actually want to see? It
15 reminds me of a paper that I had to referee once where
16 someone asked about the effect of divorce on the
17 educational attainment of children. Defining the effect
18 in terms of a thought experiment. I was thinking, well,
19 is there any policy question for which we would want to
20 randomly assign divorces and see what happened to the
21 children. I think a comparable question should be asked
22 here, whether or not the randomized merger is actually
23 what we care about. To the extent that mergers are
24 endogenous, it really matters what you think of in the
25 alternative as the right way to define the effect, if you

1 aren't going to think about it in terms of an experiment.

2 I also have some thoughts on some of the other
3 things that the other speakers touched on, but for the
4 most part, everything I was going to say, they've already
5 said. So, I'll stop here.

6 MR. FROEB: Thank you, Scott.

7 The next panel will run for an hour and it is
8 on Special Issues Involving the Price Concentration
9 Estimation. Dennis?

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1 **PANEL TWO: SPECIAL ISSUES INVOLVING PRICE-CONCENTRATION**
2 **ESTIMATION**

3 **MODERATOR: DENNIS CARLTON, Ph.D.**

4 **PANEL MEMBERS: JERRY HAUSMAN, Ph.D.**

5 **HAL WHITE, Ph.D.**

6 **KEN HENDRICKS, Ph.D.**

7 **SCOTT THOMPSON, Ph.D.**

8

9 PROFESSOR CARLTON: First, I want to repeat
10 what I just said, which was that it's easy to criticize
11 empirical studies, and I think both studies were very
12 carefully done. It was clear that a great deal of effort
13 went into each of them, and really I think each group
14 should be commended for trying their best to do a good
15 job.

16 It's also not obvious to me a seminar like this
17 necessarily will resolve any issues as to who is right
18 and who is wrong. I think it would be wrong, therefore,
19 to think of us as the arbiters as to which study is right
20 and which is wrong. And I think what we can best do is
21 raise, through our comments, additional avenues that each
22 study may want to explore in order that, hopefully,
23 they'll be able to converge and that the two agencies,
24 who obviously have spent a lot more time on each of these
25 topics than anyone on this panel, will be able to

1 reconcile any differing views they have.

2 This is a panel on price versus concentration.
3 Specifically in the GAO study, there is a regression in
4 which there is an attempt to determine if there is a
5 relationship between price and concentration. And what I
6 wanted to do in my time here is give a brief overview of
7 what we know about the relationship between price and
8 concentration. In general, what are the pitfalls, and
9 then what do we do in light of the pitfalls. Then I'll
10 have specific comments on each study.

11 So, why are we interested in running
12 regressions between price and concentration? Is there a
13 relationship? What does economic theory tell us about
14 whether there's a relationship? If you pick up any
15 textbook in industrial organization -- and I can suggest
16 a good one for you to pick up -- you'll see diagrams that
17 look something like this, that if you're a cartel, you
18 set the price at the monopoly level. N is the number of
19 firms. It doesn't matter how many firms are in the
20 cartel. If it's a perfect cartel, they always set price
21 at the monopoly level. If you have a Cournot model,
22 price falls as the number of firms increases, and a
23 Cournot model is a model in which each firm takes the
24 output of the other firm as given.

25 If you have a Bertrand model, that's where you

1 take the price of your rivals as given, you get a
2 different relationship. But generally price falls with
3 N. Bertrand is more competitive than Cournot.

4 Now, why do we use Cournot and Bertrand as
5 examples? Because they're easy to work out. Can you
6 have other things? Sure. But, in general, the intuition
7 is no matter what competitive game people are playing,
8 price falls as the number of firms increases. Okay, so
9 that sounds like a pretty good intuition. And, in fact,
10 it gets even better if you say, look at Cournot, because
11 if you look at Cournot, let's write down a simple profit
12 maximization for Cournot, the profits equal price, which
13 depends on total output, which is what I have in the
14 parenthesis, times output minus C times Q. And in a
15 Cournot model, each firm maximizes profits. If you
16 maximize profits, you set the derivative equal to zero.
17 You get the first order conditions, and after some
18 manipulation, lo and behold you get an equation, it looks
19 great.

20 The mark-up, price minus cost over price,
21 equals minus one over the elasticity of demand, times the
22 HHI. That really seems to fit right into the merger
23 guidelines that use the HHI.

24 Now, there's a "but" here, and I'll come back
25 to the "but", but this looks pretty good. So, there's a

1 clear relationship from what I've just done between price
2 and the HHI, the price and the number of firms, and in a
3 Cournot model, that turns out to be between price and the
4 HHI.

5 So, now, the real hard question is, you're
6 looking at price, say, in different cities as a function
7 of the number of firms competing or concentration in the
8 cities, okay?

9 So, you have to ask yourself a very fundamental
10 question before you go any further. Why is it that in
11 some cities you have more firms than in others? Because
12 if I'm going to use cities as controls for each other or
13 I'm going to somehow ask that there be a relationship
14 between price and concentration, I have to explain why
15 the number of firms is different. There must be
16 something different going on from an economic point of
17 view, since something as fundamental as the number of
18 firms differs from city to city. So, maybe I should ask
19 why N varies?

20 Well, one possible answer is, well, free entry.
21 There's free entry. Free entry implies profit of zero
22 and that's what's going to determine the number of firms.
23 So, for example, if there's a big city, there's more
24 opportunity for firms to enter than in a little city and
25 you're going to get more entry. And that's really the

1 important insight. It's more sophisticated than what
2 I've just said, but that's basically the central theme
3 that Sutton has pursued in his papers over the last two
4 decades, summarized best by two books he's written.

5 And if you push that relationship and you say,
6 okay, the number of firms is determined by entry
7 conditions, you get a relationship that looks kind of
8 like the one I just drew up above, that if you have
9 concentration on the left axis and city size on the
10 bottom, you get diagrams that look like this. The cities
11 get bigger, you can fit more firms in, okay?

12 And here's what's kind of interesting, just
13 hold the city size constant and let your eye run up a
14 vertical line. So, let's suppose we look at a city of a
15 particular size, we have a vertical line, what do you
16 notice? You notice that cities that have the least
17 competitive market structure, a cartel, will have the
18 lowest concentration. Cities that have the most vicious
19 competition, Bertrand, will have the highest
20 concentration. Now, initially, that may sound a little
21 odd, but there's actually a good intuitive reason.

22 If competition is vicious, there's not much
23 margin and not much incentive to enter. On the other
24 hand, if I'm a cartel and I'm really overcharging
25 everybody, there are huge profits, and as long as people

1 can keep entering the room, if we had a cartel -- God
2 forbid the FTC should be involved in a cartel, but just
3 for a moment, let's make believe we're a cartel, okay?
4 If anyone off the street can enter for some price of
5 entry, then I will get a lot of people in the room.

6 So, that means that the -- you have to be very
7 wary in drawing an inference between the vigor of
8 competition and concentration. Highly concentrated
9 industries can be very competitive and industries with
10 low concentration can be very non-competitive. Well,
11 that means that it can often be quite dangerous to
12 compare across industries or cities.

13 So, if you're looking at price versus HHI
14 across industries, you can be looking for trouble because
15 across industries, one industry may be a very
16 competitive, Bertrand, one industry may be much less
17 competitive and like a cartel, and you won't know which
18 line you're on. You'll be comparing, you know, industry
19 number one that may be Cournot to industry number two
20 that's Bertrand, and you could get an inverse
21 relationship between the two.

22 So, therefore, if you do want to run a
23 regression of price on concentration, it's best to assume
24 I'm underlining the word "assume." It's best to assume
25 that you're in the short run. You have to test whether

1 it's a reasonable assumption. Why? Because if you think
2 that you're in the long run when entry can set profits
3 equal to zero, having a merger in an industry won't tell
4 you very much, okay? Because in the long run, you know
5 profits are going to be zero and there's a natural level
6 that the industry keeps coming back to.

7 So, perhaps the best thing to do is assume
8 you're in the short run and, therefore, you are actually
9 on one of these lines. It doesn't matter which one.
10 Let's suppose you're on the Cournot line, and what you're
11 observing is that N is changing. So, that's the best
12 assumption you can make to justify these price
13 concentration assumptions. And if it's a merger you're
14 examining, you can assume that the N is changing because
15 the merger eliminates a rival. So, that sounds pretty
16 good as a way to examine the merger's effect on price.

17 But there's a problem. If you remember, I had
18 a "but" on one of my charts when I did the Cournot model
19 and derived the relationship between price and HHI. If
20 you recall my discussion of the Cournot model, which,
21 again, was the one that had price related to HHI, it
22 sounded pretty good as a basis to justify a regression of
23 price on HHI. If you actually work out a Cournot model,
24 and I work out a very simple one here, you will see the
25 problem. Here, the demand curve is 12 minus output.

1 $q_1+q_2+q_3$. There are three firms. Suppose cost is zero
2 to keep it simple. If you calculate the equilibrium when
3 there are three firms, you get that price will equal
4 three and quantity per firm will equal three. So, each
5 firm makes \$9 in revenue. Okay? Firm one makes \$9; firm
6 two makes \$9; firm three makes \$9.

7 Let's have firm one and two merge. If firm one
8 and two merge into firm one, so now you only have firm
9 one and firm three, so there are only two firms and they
10 play Cournot, what happens? What happens is that output
11 is now four per firm, so output per firm goes up, profits
12 will equal 16 for each firm. So, the merged firm makes
13 16, and the unmerged firm makes 16.

14 How does that compare to the premerger
15 situation? Premerger, each firm made \$9. So the firms
16 that merged, firm one plus firm two made \$18 premerger.
17 In other words, there's no incentive for a merger in this
18 Cournot model. So, you can use these assumptions that,
19 you know, you're playing Cournot, so I can estimate price
20 against the HHI, but then you just have this peculiar
21 implication that it's very hard to figure out why there
22 is a merger.

23 So, obviously, if you start assuming, for
24 example, that an efficiency is generated as a result of
25 the merger, you can start getting an incentive for

1 merger, and then you have two offsetting effects.
2 There's the efficiency effect at a lower price and then
3 there's a price increase from the merger.

4 But the key thing has to do with -- and Jerry
5 talked about it and the other panelists talked about
6 it -- the endogeneity of concentration. We didn't talk
7 about concentration. We talked about the endogeneity of
8 the other variables, but it's the same point. How
9 endogenous is concentration? Is it so endogenous that
10 you have to worry about it and do something special about
11 it or not?

12 Now, there have been a lot of studies,
13 generally in the literature, between price and
14 concentration. There's a boatload of studies, and I'm
15 not going to summarize them. They've been done across
16 industries and they've been done for a particular
17 industry over time. It's summarized in my textbook. I
18 would just say that these price concentration studies, if
19 you, for example, compare their findings to something
20 like the Merger Guidelines where you have one HHI cut-off
21 of 1,000, and another HHI cut-off of 1,800, you might ask
22 how closely supported are those cut-offs by the empirical
23 literature? I think it would be a stretch to say that the
24 precise HHI numbers in the Guidelines are supported by
25 the empirical price concentration literature.

1 Okay, let me now turn to the individual
2 studies. Let me talk about the GAO study first. It's a
3 very ambitious study because it's studying many mergers,
4 and the advantage of studying many mergers is that you
5 can see whether there is a systematic pattern that's
6 occurring. That's one of the advantages of studying lots
7 of things. Is there something similar going on when I
8 study each merger?

9 Now, the downside is, if you study lots of
10 different mergers, they are different, and you have to
11 worry that you've not studied each one individually and
12 the question is, if you had studied each one individually
13 in great depth would there have been variables that are
14 important to understand each but that you have failed to
15 account for? As a general matter, I think that, you
16 know, you can go either way, but it is a relevant
17 question, and I think one response they could have is,
18 well, we haven't done a separate study where we've
19 focused on each merger. We could do that. We'd have to
20 get a lot more variables, and, obviously, it would be
21 much more intensive an effort. But the fact that we're
22 getting similar results across all these mergers, or many
23 similar results, might give them some comfort.

24 Well, like I say, any time you do an empirical
25 study, it's easy to come up and criticize, and the

1 criticisms don't mean that the results aren't there.
2 Rather the point of the criticisms is to ask whether you
3 can respond in a way that gives me some comfort that
4 these criticisms don't matter materially to your results.

5 And what I said in my earlier comment, I
6 actually think the safest response is, I don't want to
7 get into details as to whether you're exactly right or
8 I'm exactly right, as to whether the market is one mile
9 bigger or 10 miles bigger. I can't possibly measure
10 everything exactly. I'm going to do things in very many
11 different ways. I'm going to measure variables
12 differently. I'm going to estimate the equation
13 differently and I'm going to see if any of these
14 differences matter. And the less it matters, the more
15 differently I do things, the more confidence I have in my
16 results.

17 If you look at either their merger equation
18 that was put up or their HHI equation, they have
19 endogenous variables. They have a demand indicator, they
20 have a supply indicator.

21 Now, that's not quite right what I just said.
22 It's not really that they have a demand variable and a
23 supply variable. That's not what they have, because a
24 demand variable would be a measure of demand, the supply
25 variable would be a measure of cost, marginal cost. What

1 they have instead are two endogenous variables that are
2 the outcome of decisions that the firm has made, the
3 amount of inventory the firm wants, and the capacity
4 utilization. When you run a refinery how much capacity
5 utilization to have over time it is a dynamic problem.
6 The refiners make these decisions, they're forward-
7 looking.

8 So, you should ask yourself the question, why
9 do you want to write down a structural model with
10 endogenous variables? It's not a demand equation, it's
11 not a supply equation. It's kind of like a mixture of
12 both.

13 Now, in general, there is, you know, an upsurge
14 of interest in industrial organization in estimating
15 structural models, and that means you estimate the demand
16 side, the marginal cost curve, and you can try and figure
17 out the game that's going on. You can get a lot of
18 insights into how markets are operating.

19 The real question is whether you want to go
20 that route when you're trying to answer a question like,
21 did price go up? I mean, that's a very defined question.
22 You might be interested in why price went up, what's
23 going on, and then you might want a structural model.
24 But if the real question, which is really a question
25 posed by both studies, is simply did price go up? it's

1 not so obvious to me you want a structural model. When
2 GAO does the structural model, what they do is -- as I
3 understand it, they hold constant those endogenous
4 variables and ask but for the merger, what would price
5 be? I don't know what it means to hold constant
6 endogenous variables. I just can't figure out why that's
7 the relevant experiment.

8 Moreover, what they use as instruments are time,
9 time squared, and a lot of time dummies. Those time
10 dummies, as Jerry said, it seems to me, might belong in
11 your original model. And if that's what you're doing,
12 why even worry about those inventory and capacity
13 variables? Why not just stick to, you know, the time
14 dummies, time and time squared and then see what you get.
15 My suspicion is it won't change the results that you
16 report very much.

17 Choice of the deflator? That struck me as a
18 little odd, but that's not likely to be a big deal. But
19 if you used other deflators, what would happen? And my
20 concern is that the FTC (and again, since I don't have
21 access to the data, I can't check this) claims that if
22 you use a different deflator, many of the GAO results
23 vanish. If that's true, that obviously would be a
24 robustness check should raise serious concerns.

25 The real question, the central question, is

1 whether the rack price that you're explaining is a
2 market. If it is a market, then you obviously want all
3 the other variables to be relevant to that market. This
4 probably applies more to the merger event study with
5 variables that are turned on and off, than to the HHI
6 study. But as I understand it, let's suppose two racks
7 are close together, but let's suppose both of the two
8 merging firms are in one of the racks, but only one firm
9 is in the other. I'm a little worried if the racks are
10 located close together that what you've done is you've
11 turned on the merger variable for one rack, but not for
12 the other, and that wouldn't be sensible if these two
13 racks are very close together.

14 Therefore, what I'm questioning is whether it
15 is true that racks define a market for all of the racks
16 in your sample. What you might want to do is aggregate
17 up to a larger level. But at least it's a question that
18 came up. I don't know how often that is a problem.

19 The price you're explaining is the price at the
20 rack. The variables you have, aside from the dummy
21 variables, are, as I understand it, the HHI. But the HHI
22 isn't at the rack. As I understand it, the HHI is at a
23 much larger geographic level. It's, I think, PADD level.
24 If you believe in most of the analysis, the HHI is the
25 same over time during the year for all racks in the PADD.

1 I'm not sure if that's right, some of the people are
2 nodding their heads, so I think that's right. But it
3 does seem to me a disconnect between what you think the
4 market is and what you want concentration to be
5 measuring. So, if you think about what you're doing,
6 let's just look at one PADD. You have annual data on the
7 HHI. I forget how many years of data. I was thinking
8 '94 to -- well, about six or seven years, something like
9 that. So, you have, say, five to ten observations on the
10 HHI and that's what you're using to identify the HHI
11 coefficient.

12 You're measuring the price, I think, weekly.
13 That raises the question, how much are your results being
14 driven by the fact that -- are you fooling yourself into
15 thinking that you have more independent observations than
16 you really have when you calculate standard errors? Now,
17 I understand if you do everything right with the right
18 serial correlation correction you'll get it right. But
19 another way of handling this is suppose you aggregate up
20 to different levels. Suppose you did it, instead of
21 daily, you used monthly data on prices, monthly averages,
22 would you get the same result? You know, 60-day
23 averages, would you get the same result? You better be
24 careful about creating serial correlation. So, you might
25 want to avoid having overlap. But at least it raised in

1 my mind that question, how robust the results would be if
2 you did that estimation differently.

3 Now, GAO reports results on the HHI and it
4 appears that they're statistically significant. Now, the
5 HHI is taken to be, if I remember right, exogenous, so
6 some of the endogeneity questions I discussed earlier --
7 worrying about how concentration changes are not dealt
8 with. But putting that aside, if you look at the
9 magnitudes of the HHIs, they're relatively low in the
10 scheme of things. If I recall there was one case where
11 the HHI increases from the mid-800s by actually 200
12 points or so. In the scheme of things, that's relatively
13 low. So, my prior would be to be surprised to get
14 anything -- but you do.

15 Now, I should point out, if you have a poorly
16 measured variable, an HHI, and I think Jerry alluded to
17 this, basically you have errors-in-variables in the stuff
18 on the right-hand side and you're mis-measuring the
19 market. Generally, that will cause you to avoid finding
20 a relationship. That is, this problem I'm describing
21 doesn't mean you will find the relationship. It's
22 actually surprising that in light of this, you do find a
23 relationship. At least that was my reaction.

24 However, if you measured the HHI in a slightly
25 different way, would things change? Now, again, the FTC

1 staff in their technical report has done an experiment in
2 which they calculate the HHI differently, not based on
3 the particular method that the GAO uses, but a slightly
4 different method that, at least at first blush -- and
5 I'll defer to the GAO and the FTC to figure out what's
6 the better way of measuring the HHI, but at least when I
7 look at it, the FTC approach seemed reasonable.

8 According to what I saw in the FTC staff technical
9 report, use of the FTC HHI caused the GAO results to
10 vanish. So, that makes me nervous about the reliability
11 of the GAO results.

12 There is an assumption that there's serial
13 correlation across the racks. That means that the error
14 term in rack one is serially correlated with the error
15 term in rack two. I believe that's probably true.
16 You've got to be real careful here. If the correlation
17 is coming about because the markets are linked, then this
18 could be an indication that you shouldn't be regarding
19 the rack as a separate market.

20 During the course of the GAO study, there were
21 several crises. Now, the problem with a crisis is it's
22 an unusual situation. So, it does make me a bit nervous
23 to have a time period that we know has an unusual
24 situation being used to estimate a merger effect.

25 Now, that's life, I understand, you can't

1 rerun the world. But it would be nice to know what
2 happens to the results if you took out those crises. In
3 particular, during crises, some of these relationships
4 amongst branded, unbranded, and I think there was an
5 allusion earlier in the first presentation to DTW versus
6 rack price, can sometimes get inverted. We do know that
7 rack for unbranded is much more volatile than rack for
8 branded. So, you might want to see whether your results
9 are robust to handling the crises differently.

10 I talked about the deflator already. I'm not
11 going to mention that again.

12 Let me talk a bit about econometric technique
13 in the GAO study. I thought they were quite
14 sophisticated in the techniques they used and they were
15 trying to be careful in correcting for serial correlation
16 and using these routines, I think, in STATA. The only
17 other thing I would say is that the fancier the technique
18 you use, in a sense, if you get different results,
19 depending on how you estimate things, that can be a test
20 of robustness. It always makes me a bit suspicious of
21 the results when you correct in fancy ways for the error
22 and it has a big effect on the coefficients.

23 So, I would like to be convinced that even
24 though correcting for serial correlation would be
25 efficient with a correctly specified model, if you don't

1 have the correct specification and you have some errors-
2 in-variables, some of the econometric techniques the GAO
3 uses could actually make things worse.

4 In my earlier comments, I talked about this
5 concern I have about the merger window. I know I'm only
6 supposed to talk about concentration, but I'll just
7 briefly repeat the point. My understanding of what the
8 GAO study does is that it says that where there are two
9 racks, merger one affects rack one, not rack two. And
10 then I have a second merger. The effect in the GAO model
11 of the second merger is the same at both rack one and
12 rack two, if those two are affected. You might just want
13 to ask whether that is sensible. In particular, when
14 you're doing the merger study, you're ignoring the HHI
15 effect. If you really think the HHI belongs in there and
16 that the effect of concentration matters, then the level
17 the industry concentration will be important.

18 So, the standard diagram, which would be in my
19 textbook, would be that, you know, initially you don't
20 expect much to happen from a merger in a very competitive
21 industry, and then maybe something will happen in a
22 moderately concentrated industry and then the merger
23 effect will level off in a highly concentrated industry.
24 So, whether I'm in this region, this region or this
25 region will have a big effect on what I think the effect

1 of incremental concentration from a merger has.

2 Now, I don't know exactly what those curves
3 look like for energy, but it's at least something you
4 might want to consider. So, when you run the HHI
5 equation, another way of doing this would be price on HHI
6 where HHI is divided into three categories, low, medium
7 and high concentration, or just low and high, and see if
8 you get something.

9 Okay, let me now turn to the FTC study which,
10 as I thought was clearly pointed out in the presentation
11 and also in Jerry's discussion, is a slightly different
12 approach. Also, I thought it was a very thorough and
13 careful approach in which they're focusing on one merger,
14 not multiple mergers. And what I said in my comment is
15 the key question is, where is the marginal supply for
16 Louisville coming from? Now, it has to be that the
17 marginal supply for Louisville is coming from Kentucky.
18 If, instead it really comes from Chicago, if there's a
19 pipeline, then there's no effect of the merger.

20 And you would want to know whether the marginal
21 supply for Chicago is different than the marginal supply
22 for Louisville, because after the merger it appears we
23 have this peculiar result that rack prices go up for RFG
24 in Louisville, but not apparently in Chicago.

25 So, the question is why? Now, it could well be

1 plausible that this demand for RFG in St. Louis comes on
2 and it's huge. But was that a surprise? When was it
3 known that St. Louis was going to switch to RFG? Was it
4 known with sufficient advance notice that refineries
5 could adjust their refinery, and I know you can't adjust
6 the refinery with the touch of a button? It takes a
7 while. You have to work off a certain inventory so
8 there's an optimal way to transition. I would like to
9 see more about what that time is and why St. Louis wasn't
10 anticipated.

11 There is a sense that what's going on in
12 Louisville is not explained in the model. That's clear.
13 It doesn't come out of the model and, therefore, there is
14 this sense that it's sort of after-the-fact explaining
15 the result. I'd like to see a little bit more on what's
16 the relationship between the price in St. Louis now and
17 the price in Louisville and whether they go in sync. I
18 think they should if I understand correctly the FTC
19 argument.

20 What's the marginal supply for St. Louis,
21 what's the marginal supply for Louisville? I guess they
22 are the same if you believe the argument in the FTC
23 working paper. It seems like a plausible explanation for
24 what's going on. But if it's true, then I should be able
25 to follow that all over time.

1 Now, whenever you have a study like the FTC's,
2 the real question is are the control cities adequate?
3 Jerry talked about this, so I'll just be very brief here.
4 The criticism that GAO staff raised about the control
5 cities is that they, too, were affected by mergers. Now,
6 if I understand the import of that, though, that would
7 raise the control base. So, that would mean that if you
8 found nothing, well, maybe it doesn't tell you very much
9 because Chicago was going up and because it was affected
10 by other mergers that don't affect Louisville, so you
11 don't have a good control. That I understand.

12 On the other hand, the main finding I'm getting
13 out of the FTC paper is that 15 months after a merger,
14 price went up relative to Chicago. So, you know, there's
15 no question it went up. If Chicago's price was affected
16 by the mergers that are raising it, if you had corrected
17 for that, it would have gone up even more. So, it
18 doesn't really explain what the FTC found. But
19 obviously, it's a relevant question to ask, are the
20 controls adequate?

21 Now, I know something about adjustment time. I
22 know something about the adjustment time between a shock
23 in crude oil and the rack price. There have been several
24 studies of this, you know, maybe they're out-of-date, I
25 don't know. But my understanding is there's a very rapid

1 adjustment of rack price to crude oil prices. And if I
2 remember right, there's a Borenstein-Shepherd study where
3 they're trying to specifically look at adjustment costs
4 and they show -- I can't remember exactly, but I thought
5 it was that within 60 days there's almost a complete
6 price adjustment. By the way, I should have mentioned
7 this earlier when I talked about the GAO study, they use
8 the price minus -- the rack price minus the crude oil
9 price. If those don't move in sync and there's a 60-day
10 lag, you should really fool a little bit around with the
11 lag structure. That's why another way of doing that
12 would be to aggregate up to a longer dimension other than
13 weekly, a longer dimension.

14 Anyway, I think the FTC gets a slightly odd
15 result. Rack price goes up, but the retail price
16 doesn't. That is somewhat puzzling.

17 So, the real question is, how do they explain
18 that? Well, they have this argument, there's
19 conventional gas outside of Louisville, maybe that's
20 keeping it down. And they also have this explanation
21 that the DTW price didn't go up. You know, maybe that
22 explains it. Is that a long run phenomenon? It would be
23 a little odd that it's a long run phenomenon that the
24 rack price goes up and it has no effect on retail. One
25 or two things would have to happen. Either retail prices

1 are going to go up or some of these gas stations in
2 Kentucky, are going to go out of business because their
3 margin has been lowered.

4 Did that happen? We can look right now. In
5 other words, what I interpret the FTC explanation to be
6 is that there was a blip in demand in St. Louis that was
7 unanticipated because of a supply shock. That's what
8 caused the rack price to go up in Louisville. But now
9 it's anticipated. Now, they've seen it happen, and
10 presumably, they should adjust to it, and therefore, if I
11 look right now, what is the relationship between the rack
12 price of RFG in Louisville compared to the rack price in
13 Chicago compared to the rack price in St. Louis? In
14 other words, there is something that I would suggest the
15 FTC do to figure out whether what they're claiming is the
16 explanation for the increase is born out by what has
17 subsequently happened.

18 You know, having said that, I thought the FTC
19 explanation sounded pretty reasonable, subject to further
20 investigation. Let's suppose a merger is going to raise
21 the rack price, I'd be a little surprised if it took 15
22 months to do so. So, there seems like there's clearly
23 something going on. It's an interesting point, but I'll
24 talk about it to the FTC. They have a futures price in
25 there. There's a literature on what the coefficient

1 should be, so you might want to -- I'll talk to the FTC
2 about that. It's just a simple point.

3 In the interest of saving time and to let
4 others speak, I'll just end.

5 There have been other studies in this market
6 and I think what is emerging is that the notion that
7 there are hints of market power in certain parts of the
8 chain comes out of some of these studies. In the
9 Borenstein-Shepherd study, they did find an effect of
10 concentration on adjustment time. That is, it does
11 appear that concentration is mattering sometimes. There
12 are these Hastings studies and Hastings and Gilbert
13 studies. Now, my general impression is that these
14 studies that are finding effects are in the relative
15 minority of studies, but at least I think it's indicating
16 that there may be something in the data that makes a
17 conference like this worthwhile so that we get to the
18 bottom of it. In particular, what a number of these
19 studies seem to indicate is that the vertical
20 relationship can sometimes matter, and by vertical
21 relationship what I mean is the output of independent gas
22 stations versus branded stations, that that quantity can
23 sometimes be affected by refinery concentration or at
24 least that's the suggestion of these studies. Although
25 it's far from definitively established, that possibility,

1 given the importance of this industry, you know,
2 justifies having intensive investigations like the FTC
3 and GAO have performed. So, I'll stop here and thank
4 you.

5 PROFESSOR HAUSMAN: Do you want to reverse the
6 order or do you want me to go next?

7 DR. THOMPSON: I can probably go quickly, so
8 maybe you can let me do that. I really have just two
9 comments to make on the price correlation studies, and as
10 a caveat, I should add I've never done one myself. So,
11 this is speaking as someone who looks at these things
12 occasionally, but has always been scratching his head a
13 little bit when looking at them to try to figure out
14 what's going on.

15 First of all, the market definition question
16 seems to me to be quite critical in the sense that if you
17 use the HHI measure of concentration, the HHI is
18 inversely proportional to the square of the total size of
19 the markets. That's what's in the denominator of each of
20 the terms that you sum. What this means is that
21 relatively small errors in measuring the size of the
22 market potentially lead to fairly large and serious
23 measurement errors, and that's been commented on already.
24 But the main point I want to get across is that those are
25 potentially large errors, not small ones.

1 The second point related to the use of the HHI
2 is that, in general, we should take into account when
3 there are substantial sales by firms not physically
4 producing within a relevant market, the presence of
5 imports into the market. That could change the
6 concentration measures substantially in ways that I'm not
7 sure were taken into account in the GAO study. I'm
8 afraid that it's a little bit unfair here in that the GAO
9 did a price concentration study and the FTC did not. So,
10 the comments about this pertain only to the GAO.

11 But in any event, if you were to take into
12 account the inflows and look at the actual sales coming
13 in from other areas, you might get a better feel for some
14 of the issues that Dennis mentioned, namely whether or
15 not racks nearby are linked, whether or not there's
16 substantial arbitrage taking place between geographic
17 areas that could discipline price increases. And that,
18 of course, is very closely tied to the question of market
19 definition, something that is essentially not dealt with
20 directly in either one of the studies.

21 The second point that I want to make has to do
22 with the interpretation of a price concentration study.
23 There are quite a few things that might be driving
24 concentration, and in particular, driving the differences
25 in concentration across time or across geography, which

1 are obviously the dimensions being exploited here. You
2 can have differences in the input cost, differences in
3 the size of the markets. Mergers, obviously, are one
4 possibility. Entry and exit and internal growth of
5 firms. All of these things are going to affect
6 concentration.

7 From the point of view of a public policy
8 question, it's difficult to know what to make of a price
9 concentration study if what's driving the variation in
10 the explanatory variable (or if you want to think of this
11 in terms of simultaneity, in the concentration study, in
12 the concentration measure itself) is not something that's
13 directly affected by some public policy decision.

14 From the point of view of the antitrust agency,
15 the merger is obviously one thing that might be going on.
16 But to the extent the concentration in these markets --
17 to the extent that the interest in concentration in these
18 markets is driven by considerations of mergers, it
19 strikes me that looking at the merger studies is really
20 getting much more directly at the public policy question
21 of interest here.

22 PROFESSOR HAUSMAN: Again, I'll just keep my
23 comments fairly brief. What you have to understand that
24 is going on here is that the HHI is being put in, as it
25 were, as a proxy for market structure and that, depending

1 on the type of oligopoly model you have, you can get very
2 different results and somehow that the HHI has to be the
3 same across markets, so it has the same relationship and,
4 again, as I emphasized earlier, when you have changes
5 over time and the market structure changes, it has to
6 pick that up.

7 Now, in the GAO models, they have the inventory
8 ratio and capacity utilization variables and this has
9 been mentioned, but I think this is a very important
10 point, those are at very different levels of aggregation.
11 You have at the PADD level and at the rack level and it's
12 not clear how that matches up. In that type of
13 situation, and more broadly, I have grave doubts about
14 whether the changes in HHI can control for changes in
15 market structure. So, I pretty much agree with Scott's
16 last point. If you want to take a look at what happened
17 with mergers, I think it's better to take a more direct
18 approach, which is the first GAO approach of using merger
19 indicated variables, rather than trying to use the HHI.

20 But let's say that you do want to use the HHI.
21 In my view, you have two main problems. One is this
22 joint endogeneity problem, about which I don't have much
23 to say, but it's a serious problem. But the second is
24 that the HHI, I think, really doesn't change very much in
25 markets apart from mergers. So, let me assume that

1 that's true during this time period. Now, I could be
2 wrong, but if the HHI doesn't change very much in markets
3 apart from mergers, then it's very difficult to sort out
4 the effect of the HHI.

5 So, in my view, because you have fixed effects
6 in the model, in other words, you're explaining the price
7 with the dummy variable for each rack, the only way you
8 can really then find out the effect of the HHI is to see
9 what happens when it changes. In other words, if you ran
10 a cross section regression here on panel data and you
11 didn't have mergers or any reason for the HHI to change
12 over time or just change a tiny bit, you couldn't tell it
13 apart from the fixed effect. You know, what's going on
14 in the rack in San Diego is explained by its fixed effect
15 and what's going on in the rack in San Francisco would be
16 explained by its fixed effect. So, it's only changes in
17 the HHI.

18 And so, then what it seems to me that you have
19 to do, if you really want to believe these models or to
20 really estimate these models is that you have to have
21 markets in your sample in which the HHI changed for some
22 reason other than a merger. Now, that could have
23 happened in this data. I think Gulf Oil sort of
24 disappeared from the Northeast and Cumberland Farms took
25 over its stations and I don't understand exactly how

1 those are sourced. But I mean, it's certainly the case
2 that somebody could have exited from these markets during
3 this time apart from the merger.

4 Otherwise, it seems to me that when you're
5 looking at the changes in the HHIs, it really is just
6 like using a merger-indicated variable, and then you're
7 scaling it by the HHI, and as I started off to say, it
8 seems to me that there is no reason to think that you're
9 going to have this constant linear relationship. So, the
10 way to test this would be to have other markets where you
11 don't have a merger where the HHI changes, if you had
12 that in your data and then you'd be able to have much
13 more confidence in your results if they were robust to
14 that type of change.

15 PROFESSOR WHITE: Well, let me pick on
16 something that Jerry was just saying and that is the
17 concern with the endogeneity of the HHI and, as it's
18 usually defined, that's a market share measure, but here
19 we actually have a capacity measure. So, perhaps there's
20 a sense in which the capacity was intended as a proxy,
21 but because capacity is usually determined over a longer
22 time cycle, perhaps it's being considered as
23 predetermined here. But I'm not sure that that's really
24 capturing necessarily the concentration measure that one
25 would want. Capacity can have strategic considerations

1 that may have a role to play and so a capacity HHI is
2 like being one step removed with some extra things going
3 on and I'm not sure necessarily that even if one wanted
4 that sort of concentration measure it would be something
5 that would be really telling me what I would want to
6 know.

7 Now, there's another issue and it's been
8 touched on briefly, but I'd just like to highlight it.
9 We have basically five PADDs and we've got seven years,
10 but only five years of concentration data. So, there's
11 really only 25 observations where changes happen, and
12 we're regressing basically monthly -- or is it weekly --
13 weekly cross section panel data on these basically 25
14 observations. It's a concern to me that there's not
15 nearly enough variation in this HHI measure, whatever it
16 is, to really identify any kind of effect that's going
17 on.

18 Now, sometimes when you measure explanatory
19 variables at a lower frequency than the dependent
20 variable, you can do some kind of instrumenting, but in
21 this case, you have sufficiently few data points that
22 even that sort of instrumenting is not going to be
23 productive. That would lead me to try to go to some sort
24 of a reduced form, and where that reduced form would take
25 me would be back to a model that would basically have

1 some merger dummies because that's the thing that would
2 be driving this, perhaps along with some entry and exit
3 information.

4 So, I think that that more direct analysis is
5 the one that I would prefer.

6 PROFESSOR HENDRICKS: As an editor of the
7 Journal of Industrial Economics, one of the IO field
8 journals, I get a lot of these price concentration
9 studies. My first test is the following. If the model
10 is trying to explain the variation in prices across a
11 cross section of industries using the variation in HHI or
12 some other measure of market concentration, I will reject
13 the paper. The reason is that there are lots of economic
14 forces not being measured, market characteristics not
15 being measured, which are driving both the concentration
16 index and prices. So, it's not clear that the
17 coefficient on the HHI is picking anything up except
18 correlation.

19 If the data in paper has a panel structure, I
20 can change the question. Instead of asking how does the
21 variation in HHI across markets explain the variation in
22 prices across markets, I can use fixed effects and ask
23 how does the variation in HHI over time within each
24 market systematically affect the prices in that market?
25 This is what the GAO study did, and for that, I commend

1 them, because I think that is the right way to think
2 about this question.

3 My problem with the GAO study, though, is one
4 that two of my colleagues on the panel have alluded to,
5 namely, that the market prices and the fixed effects are
6 at the city rack level but the HHI is at the PADD level.
7 The way that I would have thought about the regression is
8 to aggregate prices up to the PADD level, include a PADD
9 fixed effect and then look at how changes in the HHI
10 within the PADD affect changes in average prices in the
11 PADD across time.

12 But if you think of the model in this way then,
13 as Hal said, it's actually less than 25 price changes
14 because two of the years are interpolated.

15 PROFESSOR WHITE: Well, I was subtracting those
16 out. There were seven.

17 PROFESSOR HENDRICKS: Yeah. I mean, there
18 are really few changes to estimate the coefficient on
19 HHI. That is why I like the merger regression more than
20 the price concentration regression because in the merger
21 regression the merger dummy is being turned on and off at
22 the city rack market level. Hence, you could take
23 advantage of some of the variation in that variable
24 within the PADD because presumably, it is not turning on
25 for all of the city rack markets in the PADD. When I

1 think about running the price concentration regression at
2 the PADD level, it effectively reduces to the merger
3 equation, except that it is at a more aggregate level and
4 you're not using some of the variation within the PADDs.

5 One final point I'd like to make is that from a
6 theoretical perspective, IO economists make a big
7 distinction between horizontal mergers and vertical
8 mergers. If it is a horizontal merger in the wholesale
9 market on the supply side, then the theory is
10 unambiguous: prices are going to go up. The merging
11 parties are going to restrict quantity and raise price.
12 If it is a vertical merger, the theory is ambiguous.

13 I ran some simulations on the Hendricks-McAfee
14 simulator last night. The simulator allows you to study
15 vertical mergers along with horizontal mergers. It is
16 not hard to get wholesale prices increasing and retail
17 prices decreasing, because of the elimination of the
18 double mark-up problem associated with a vertical merger.

19 So, that leaves an open question that needs to
20 be thought about more carefully. The GAO study says that
21 the mergers are changes in market structure that raised
22 wholesale prices. But the study is silent on the issue
23 of whether or not retail prices actually went up as a
24 result of those increases. That is an open question.
25 I'll finish on that point

1 MR. FROEB: Okay, yeah, thanks very much. Now,
2 we have a lunch break until 1:45 and we'll reconvene at
3 that time with a robustness panel. Thank you very much.

4 **(Whereupon, at 12:34 p.m., a luncheon recess**
5 **was taken.)**

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1 it's something that I'm actually quite pleased about,
2 because this is addressing issues that are near and dear
3 to my heart.

4 Robustness of any sort of econometric analysis
5 is something that I have spent a lot of time thinking
6 about. Most recently, I've been thinking about
7 measuring effects of natural experiments like the sorts
8 of natural experiments that are presented to us by
9 trying to understand what happens looking back when a
10 merger has occurred and in particular what the price
11 effects are.

12 Now, today I'm going to talk to you about
13 traditional regression methods, which is how I would
14 characterize the studies that we see from the FTC and
15 the GAO, but also I want to discuss why there may be
16 some robustness concerns with those approaches. I also
17 will present to you what I call a treatment effect
18 approach, which can achieve robustness against some of
19 the things that the traditional approach may be sensitive
20 to. This is therefore message of hope. Something that
21 may give us some methods that I hope will be interesting
22 for those of you who are involved in this work to
23 explore.

24 So just to give you a big picture story here,
25 robust approaches are those that deliver results that

1 are not sensitive to changes in the different aspects of
2 the analysis, for example, the assumptions we make or
3 the statistical techniques. You've heard Professor
4 Hausman and Professor Carlton talk about various
5 different, important types of sensitivity analysis, and
6 the obvious concern is that if you have an analysis that
7 is somehow sensitive to the methods that are used,
8 either basic assumptions or the statistical techniques
9 or their variations, that raises questions about whether
10 those analyses are sound bases for policy analysis.

11 Here, we're focusing specifically on the
12 question of: Well, what about the robustness of the FTC
13 and the GAO studies? Do we have concerns there? To
14 what extent might we take those studies as a basis for
15 policy decisions?

16 We're going to be talking about what we can learn from
17 them, but I also want to talk about how we might proceed
18 from those studies and get some additional insight as to
19 what the effects of these petroleum mergers might be.

20 Now, both the FTC and the GAO studies are what
21 I would call traditional dummy variable approaches. They
22 use different explanatory variables. The observations
23 are different to a degree, but what I want to concentrate
24 on for the moment is the similarity between these two,
25 because in both of these studies, the effects are

1 measured by including dummy variables for the merger
2 observations of interest.

3 Certainly, these kinds of approaches can
4 deliver useful results of merger effects, but there are
5 stringent conditions, and we've heard both Professor
6 Hausman and Professor Carlton talk about some of those
7 conditions, which are required for those useful
8 estimates to be obtained, and I want to present a
9 certain emphasis on those and ask some tough questions
10 about, Well, how comfortable are we making those kind of
11 assumptions?

12 I also want to show you a way that it may be
13 possible to improve on these traditional approaches by
14 exploiting methods from the treatment effects
15 literature. In fact, I would like to recommend that you
16 consider a treatment effects approach, whenever doing
17 these sorts of ex-post studies. In fact, it may also be
18 possible to do it ex-ante, but my focus for right now is
19 going to be on looking post-merger and seeing what
20 might have happened.

21 So in the treatment effects literature, applied
22 to the analysis of merger events, we're going to consider
23 the merger as a treatment that's being applied to the
24 post-merger observations.

25 Now, this treatment effect approach is a

1 standard approach to analyzing the effects of treatments
2 in clinical trials. There we can randomize, but even
3 when it's not possible to randomize, as when we're
4 dealing with these natural experiments, it's possible in
5 principle with identifying assumptions of the sort that
6 Professor Hausman was talking about to achieve the
7 effects or the benefits of randomization without having
8 to have experimental control, and that's by making use
9 of certain conditional impediment assumptions.

10 I have a paper that is, or was, available out
11 at the table, so maybe some of you picked that up. I'll
12 also give you a website at the end of my remarks where
13 you can download that paper. It goes into the technical
14 details of this. I don't want to get too far into the
15 weeds, but the results of this treatment effect
16 literature, is that even with non-randomized experiments,
17 we can infer effects of interest.

18 Now, the interesting thing about this approach
19 is that it has robustness to certain of the sorts of
20 issues that would arise in a traditional approach. In
21 particular, it explicitly allows for imperfect data. It
22 can handle errors-in-variables problems directly, and,
23 in fact, it embraces the use of proxies in attempting to
24 account for, and I'll make a distinction between
25 accounting for and controlling for, other factors which

1 will have an impact on the response variable, price.

2 It also can be more robust in that it is not as
3 dependent upon having a correct specification for the
4 regression model, and I'll talk more about that.

5 So these two features, being able to have
6 imprecisely measured data, and not having necessarily to
7 have an absolutely correctly specified regression model,
8 are the benefits, along with the fact that despite this
9 greater flexibility, it is possible to isolate the
10 effective interest, that is the total effect of the
11 merger ex-post.

12 So let me be a little bit more precise, and
13 tell you what I mean by the effects of the merger, and
14 what I mean is, what I think the common understanding is
15 and in all of the discussion that we've had today that I
16 find it useful to be exclusive -- merger effect is the
17 difference between what we actually observe post merger,
18 and what's been called the "but-for" price. The but-for
19 price is the price that we would expect to observe at
20 that time in that market in the absence of a merger, and
21 the word "expect" is an important aspect of this.

22 I could also use the word "predict", and I want
23 to emphasize that what I'm talking about are predictions
24 and expectations because that understanding is the key
25 to gaining these different robustness properties that

1 I'm talking about.

2 Now, it's possible to talk about the effects
3 period by period. I can ask what's the observed price
4 now, and what would I have expected it to be in the
5 absence of the merger, so that's a period by period
6 notion of effect. But I can also talk informatively and
7 will be interested in average effects, so I might be
8 interested in the average effect overall of the post-
9 merger observations, or as in the FTC study, I might be
10 interested in the effects of the merger in different sub-
11 periods.

12 So in the FTC study, we see the merger analyzed
13 for both 1998, what's the average effect for '98, what's
14 the average effect for '99, so using the period by
15 period effects, I can build up interesting averages.

16 The traditional approach is in fact estimating
17 these average effects of the merger by virtue of the use
18 of the dummy variable. These regression methods
19 basically include the merger dummy, along with relevant
20 explanatory variables, and the purpose of the merger
21 dummy variable is to account for and to measure the
22 effect of the merger. The relevant explanatory variables
23 are to control for all of the other things which might
24 affect the price and that we want to avoid confounding
25 with the effects of the merger.

1 So the FTC study is a little bit more complex
2 because it actually has two merger dummies, as I
3 mentioned, the 1998 and '99 dummies, so it's looking at
4 isolating two average effects. The GAO study is
5 quite ambitious, because it has dummies for eight
6 different mergers. But just to keep the ideas simple,
7 let's just suppose that we are interested in a simple
8 single post-effect average of the effect of a merger.

9 Now, in the treatment literature, they are also
10 interested in the effects of interventions, mergers are
11 an intervention for this purpose. In the treatment
12 literature, you'll see some different jargon used. If
13 you want an entry into the treatment effect literature,
14 you'll have to go looking for something called the
15 treatment effect on the treated.

16 So we translate that into the merger terms,
17 that's the effect of the merger on post-merger
18 observations, so that's the effect that we're interested
19 in, and if you want to link up what I'm talking about
20 here to the treatment literature, whenever they're
21 talking about the treatment effect on the treated,
22 that's what we're interested in.

23 In that literature, there are a number of
24 different methods that can be used to estimate those
25 effects robustly in using suitable covariate, so this is

1 the literature on natural experiments pioneered by
2 Angrist that Professor Hausman was talking about.
3 There's been recent work by Hahn and econometric done
4 in 1998 by Heckman and his colleagues and by Heron
5 Embans and their colleagues and by Hirano, Imbens, and
6 their colleagues on and different ways of doing
7 this in a cross-section framework.

8 Now, here we have a before and an after, but
9 we're in a time series framework. Actually, in the GAO
10 study we're in a time-series, cross-section framework,
11 so the approach there needs to be extended, and in the
12 paper that I've made available, I'm doing that just
13 extending the time series. I'm not doing it per panel,
14 and that's going to take some further thought, but it's
15 possible to straightforwardly extend those ideas and
16 make them relevant for use for our purposes.

17 So let me begin by just reminding you about
18 what a traditional regression attempts to do, and then
19 talk
20 about the different things that may cause concern in
21 attempting to achieve these goals.

22 What the traditional regression approach
23 attempts to do is to measure the effect of every variable
24 in the regression on the variable of interest, here
25 price, holding all of the other explanatory factors

1 constant.

2 So that's called the ceteris paribus, other
3 things equal, approach or interpretation. When we look
4 at the coefficients in a regression situation, for
5 example, we look at the merger dummy coefficient, we
6 will interpret that in the standard way as the effect on
7 price of the merger, but holding everything else constant
8 as if we were able to conduct a laboratory experiment.
9 Or in a price concentration study, the coefficient on
10 that variable is intended to represent the effect on
11 price of that concentration variable holding everything
12 else constant.

13 Now, that is an ambitious goal because we're
14 attempting to learn about all of these different effects
15 as they operate effectively simultaneously. This can be
16 done, but the conditions to achieve that are stringent,
17 and it's possible that some of these conditions may not
18 be plausible in particular situations.

19 So here are three of the most important
20 conditions for my purposes here today. First of all, the
21 regression equation must be a truly causal relation. It
22 must be the case even if we can't conduct this experiment
23 in actuality, we ought to be able to perform a thought
24 experiment where we vary one of the factors on the
25 right-hand side and hold all of the others constant, and

1 that will result in the corresponding change to the
2 dependent variable.

3 In our discussions of the price concentration
4 equations, basically a lot of the concerns that we were
5 raising were concerns about whether that equation is
6 truly causal.

7 Now, it's also important that the relevant
8 variables be included and measured accurately. When I
9 say accurately, what I mean is that we can't
10 legitimately be using proxies because proxies inherently
11 have errors in them. This creates an errors-in-variables
12 problems that Professor Hausman was referring to. There
13 are ways of dealing with errors-in-variables problems,
14 for example, instrumental variables, but those are often
15 not explicitly implemented or if they are, we end up
16 using instruments whose appropriate instrument properties
17 we may have questions about as well.

18 But even if we have managed to include the
19 relevant variables and measure them accurately, the
20 functional form that we specify has to be correct, so if
21 we've got a linear equation, it must be the case that
22 the effect of the merger is identical in each period and
23 in various market conditions. It must also be the case
24 that the effects of all the other variables must be
25 precisely as specified by the model, so that if we have

1 a variable that enters in usual linear fashion, it's
2 effects cannot depend upon its value. This is one of
3 the points that Professor Carlton was making when he was
4 talking about the concentration index, that it's effect
5 might differ depending on its value.

6 Also among the issues that Professor Hausman
7 was talking about was that when a merger happens, the
8 structure of the market changes, and this may change the
9 way that the prices respond to the underlying cost and
10 demand variables, so if we impose a restriction that
11 those effects are the same, pre- and post-merger, then we
12 may be getting ourselves into trouble.

13 Now, there are other requirements. They're the
14 sorts of things that you can find in the econometric
15 textbooks so I won't go into them here, but these are
16 the ones that are certainly serious concerns so that if
17 they're called into question, then may make it difficult
18 for us to know how much reliance to place on the
19 resulting estimates. Another way of saying that is
20 that this is treating a robustness problem.

21 So here are the consequences. The primary
22 consequence is that if we have any of these problems, if
23 we've got errors-in-variables, or the functional form
24 isn't correctly specified, or the relationship isn't a
25 causal relationship, then the estimated coefficients no

1 longer measure the ceteris paribus effects. In
2 particular, the estimated merger effect is misleading,
3 and all of the other estimated effects are also
4 misleading.

5 In fact, what you have is a prediction
6 equation. You can't predict conditional on things that
7 you observed, but you're not necessarily measuring the
8 effects of interest.

9 Another consequence of this is that the results
10 can be highly sensitive to the inclusion or exclusion of
11 particular variables. If you put in one set of
12 variables you may get one result. If you put in a
13 different set of variables, you get a different result.
14 Both of these different choices can't necessarily be
15 right, and so researchers are going to arrive at
16 different conclusions. You have a process which is not
17 robust and which, of course, is productive in generating
18 debate but may not generate insight.

19 So that's to set the stage and raise the
20 possible concerns that can happen with a traditional
21 dummy variable approach.

22 Let me now tell you about a treatment effect
23 approach and how one might go about doing that, what it
24 delivers, what it doesn't deliver and hopefully
25 highlight the ways in which this might be useful in

1 attaining the goal of estimating these effects of the
2 mergers without necessarily running afoul of some of the
3 things that a traditional approach might give us.

4 Here's what we need to proceed with the
5 treatment effect approach. We need a list of the
6 driving factors, the determining variables that we can
7 actually observe. This is going to come from an
8 economic theory carefully applied to the problem of
9 interest.

10 In price determination, this is going to tell
11 us things about cost and demand shifters, and our
12 knowledge of the particular industry is going to tell us
13 about which particular cost is of interest, oil, which
14 particular demand shifters, maybe weather, maybe income.
15 It may be a variety of different things, but our economic
16 insight is going to tell us what it is that we would like
17 to be measuring as driving and determining prices.

18 I talked about the observable true cost and
19 demand shifters. What I mean by that is that these
20 should be those cost and demand shifters that we can
21 really measure accurately.

22 Now, once you really start to think carefully
23 about whether we can accurately measure the true cost and
24 demand shifters, I think it's more likely the case that
25 we would feel somewhat uncomfortable thinking that we

1 had really got our hands on the true cost or demand
2 shifters. For example, both of these studies, the FTC
3 and GAO include oil prices.

4 Certainly oil prices account for a huge amount,
5 maybe 60 percent, of the raw materials cost of gasoline,
6 but now let's think carefully. The prices that are
7 included in these regressions are the prices measured
8 contemporaneously in the spot market, in the case of GAO,
9 or the prices measured in the future markets in the FTC
10 contemporaneously with the gasoline price: That's likely
11 not to be the purchase price of petroleum that went into
12 the production of the gasoline
13 that's being sold on a particular day.

14 Nevertheless, it is capturing something
15 important about the cost determinants, and so I would
16 say that such a variable is really a proxy for an
17 unobservable true petroleum price that we would like to
18 have our hands on but can't get.

19 So this gets to my second list of things that
20 we would like to have, observable proxies for
21 unobservable determining variables, so these are the
22 things that we as economists naturally look to find. We
23 get an oil price that might be spot price. It might be
24 futures price. It might be several lags of spot or
25 futures prices.

1 We also know that there are costs and demand
2 shifters that have an impact but that we can't measure
3 directly. This is where things like the price of
4 gasoline in Chicago can come into play because the
5 Chicago gasoline prices are being driven by costs and
6 demand shifters perhaps similar, if not exactly
7 identical, to what's going on in Louisville as is the
8 case in the FTC study.

9 So the price of gasoline in Chicago can act as
10 a proxy for cost and demand shifters. The prices in
11 Houston, the prices in northern Virginia, may also be
12 potential proxies, and we have to think about whether or
13 not they're going to satisfy the core requirements,
14 which I'm going to discuss when I get to the end of this
15 page here, for being useful proxies. But, I want to
16 think of these variables not as controlled variables,
17 that is control observations, but rather as proxies for
18 observable cost and demand shifters.

19 The things we can observe, whether they are the
20 true underlying driving variables, or whether they're
21 just proxies for those things that are acting together as
22 predictive proxies for the true unobservable determining
23 variables.

24 So the things that are really driving prices
25 are omitted. We can't really observe them. Instead, we

1 can get our hands on things which we think are going to
2 be correlated with those, so there's correlations between
3 the proxies and the things which are really driving
4 prices, and that's how I want to think of these
5 observables, X.

6 In the treatment effect literature, this
7 collection X is what is called a set of covariates, and
8 the key requirement for these covariates is that they
9 can't be affected by the treatment. That's why we have
10 double blind studies in the clinical trial literature.
11 That's why Chicago prices are a useful proxy provided
12 that Chicago is not impacted by the Marathon Ashland
13 merger.

14 So as long as the proxies aren't causally
15 impacted by the treatment, then they can be usefully
16 included, and they should be linked by some compelling
17 economic theory to whatever the things are that we would
18 wish we could measure rather than just trying to throw in
19 the kitchen sink. So in fact, if Chicago is a useful
20 proxy and Houston is a useful proxy, then those can go
21 into the set of Xs that one uses in this approach.

22 It's also the case that we can decide whether
23 or not to include proxies based upon whether or not their
24 behavior changes pre- and post-merger, so obviously oil
25 is an important factor. If its distribution was the

1 same pre- and post-merger, we wouldn't have to control
2 for it because it wouldn't be a confounded variable.

3 So that says, Well, we don't necessarily have
4 to have observations on everything. We just have to have
5 observations on those things whose distributions may be
6 changing pre- and post-merger and which can therefore
7 possibly be confounded with the merger effect. That
8 means that we have to include proxies for any
9 unobservable whose behavior changes pre- and post-merger.
10 That's why these variables X have to go in there.

11 Now, what I just said is true, that you don't
12 have to include proxies for things whose distributions
13 don't change, but if you happen to have those, you can
14 reduce effectively the error of residual in the equation
15 that we're estimating and get more precise estimates on
16 the merger effect, but it doesn't bias the merger effect
17 estimate to leave those out.

18 How do you perform this treatment effect
19 estimation? Well, the thing that we're really trying to
20 get at as far as an average effect is the difference
21 between 1) the average that actually occurred post-merger
22 and 2) what we would predict to have occurred given the
23 market conditions post-merger, but if the pre-merger
24 predictive relationship were to hold. I want to stress
25 the fact that I've said predictive relationship.

1 I didn't say causal relationship. I said predictive
2 relationship, and it's that fact which is going to give
3 us the robustness that we are interested in.

4 There is a way to do that by running a
5 regression, thanks to the algebra of linear regression.
6 You can achieve that by a simple regression which is
7 very similar to the standard regression that we're used
8 to running, and that is to include a merger dummy. We
9 include the covariates X, that is, all of our observable
10 proxies and determining variables, and we also include
11 flexible transformations of X. So not necessarily to say
12 that this would be a good idea, but just to give you an
13 idea what I mean by a flexible transformation, maybe the
14 squares of cross products of the Xs. There are other
15 things perhaps that will perform better statistically
16 that one can do, but that's the basic idea.

17 We're trying to achieve a flexible functional
18 form that will include these covariates because we don't
19 know what the true predictive relationship is, and
20 that's what we're out there for. We also want to
21 include one more thing, and this is to achieve the
22 recommendation that Professor Hausman made in his
23 remarks earlier, we don't want to necessarily impose the
24 restriction that the way the market responds to its cost
25 and demand shifters is the same after the merger as it is

1 before the merger. This can be achieved through
2 the managing of the regression equation by including in
3 the regression the merger dummy multiplied by the
4 flexible transformations.

5 So this is very close to what we had before,
6 but it's also different. We've got the merger dummy like
7 we had before, but now you don't just have X, intending
8 those Xs to be the true perfectly measured causal
9 variables but rather we view the Xs as predictors. As
10 predictive proxies, they're acting as predictors of the
11 things that we can't really observe, and then we have
12 the interactions of those Xs with the merger dummy.

13 And these permit us to circumvent the
14 specification problem. We're dealing with prediction,
15 not with causal relations. The flexibility gives us the
16 ability not to have to have this linear form be exactly
17 the way the world is, and as it turns out, this,
18 nevertheless, delivers a consistent estimate of the
19 total effect on average ex-post of the merger. It's the
20 difference between the average price that we actually
21 observe ex-post and what we would predict on average but
22 for the merger, so we're comparing what actually happened
23 with a prediction.

24 So we get the effect of interest. We get the
25 baby. Now, there is some bath water that gets thrown

1 out, and that bath water is the other ceteris paribus
2 effects. This approach does not deliver consistent
3 estimates of any of the other effects that one would
4 typically interpret from a regression equation. Instead
5 what you have is a mix -- these coefficients on all of
6 the other variables of the model are a mix. They're a
7 mix of the causal effects of the variables that you
8 should have included but couldn't because you can't
9 measure them, and the predictive relations between the
10 proxies which you have included and those causal
11 effects.

12 So what that means is that you can look at
13 those remaining coefficients. Understand that they're
14 capturing this mix of complicated, predictive and causal
15 relationships, and that means that they don't have to
16 have the expected signs or magnitudes, that economic
17 theory would tell you should be there if what you were
18 talking about was a causal relation. They're observing
19 all of these predictive relations so the fact that the
20 sign is wrong might be a concern if you really cared
21 about the effect of oil prices, but if our main focus of
22 interest is the effect of the merger, we can still get
23 there, despite having used a proxy for oil prices.

24 So this, in effect, sort of liberates us
25 because I don't know how many of you, like I have, tried

1 to run a regression equations and get the darn sign right
2 and have it not happen. Well, it's because what's in the
3 equation is a proxy is capturing causal and predictive
4 effects all jumbled together, but the thing that we care
5 about, the effect of the merger, comes through all of
6 this relatively unscathed.

7 Now, let's just line up the traditional
8 approach and the treatment effect approach that I'm
9 advocating here. The treatment effect approach
10 explicitly permits the use of proxies in our covariates.

11 Now, with the traditional approach, we use
12 proxies and then pretend we don't, so here what we get
13 to do is at least to be honest, but now we have to think
14 carefully about the proxies. They should be things that
15 are proxying for variables that we have left out, but
16 they also should be things that are not themselves
17 impacted by the merger.

18 The treatment effect approach by having a
19 flexible functional form avoids misspecification by not
20 imposing the linear straight jacket on the predictive
21 relationship. Instead we can let the data speak and
22 guide us to what might be a useful predictive
23 relationship, and we also have the interactions, so we
24 avoided imposing the restrictions that Professor Hausman
25 was telling us to avoid imposing.

1 Now, we've lowered the bar in terms of the
2 requirements of what we have to do, and this is
3 naturally going to mean that we don't get everything
4 that we would have gotten with a traditional approach.

5 We don't get the ceteris paribus effects of
6 the non-merger variables. But if what we care about is
7 the effect of the merger, then we still have what we
8 want because the treatment effect approach does deliver
9 robust estimates of the merger effects. It's robust to
10 errors-in-variables issues. We use proxies. It's
11 robust to misspecification issues by using flexible
12 predictive forms, and we can avoid those issues. And it
13 is also robust to the fact that we're not really
14 estimating causal relations, we're estimating predictive
15 relations, and that's the basis for the estimation of
16 the effective interest.

17 And if I've peaked your imagination, the slides
18 and the paper, if you couldn't find it outside are
19 available on this web site.

20 Is there a question?

21 MR. GEORGE ROZANSKI: Actually there is. Just
22 to be clear, when you're trying to then go back and
23 interpret the effects of the merger, are you just
24 looking at the estimated coefficient on that merger
25 dummy, and not the coefficients on the interactions

1 between the merger dummy and the other observable
2 proxies.

3 PROFESSOR WHITE: Right, yes. The result of
4 these interactions is basically to isolate the post
5 merger predictive relationships from the pre merger
6 predictive relationships, and by including those
7 interactive terms, it performs that isolation.

8 So basically only the pre merger observations
9 are operating to construct the coefficients for the
10 prediction equation when we're doing the predicted but
11 for crisis, so the effect is only the effect on the
12 merger coefficient.

13 If there are no other questions from the floor,
14 let me turn it over to the panel and ask for their
15 comments, please.

16 PROFESSOR HENDRICKS: How robust are these
17 treatment effects towards the independent variable
18 problems? Suppose you're trying to predict the "but-for"
19 price when you really don't have a very good list of
20 demand and cost shifters?

21 PROFESSOR WHITE: Okay. So first it's
22 important to have at least the guidance of theory in
23 trying to get a good list.

24 Now, let me clarify. Were we talking about
25 when theory doesn't even help us, or are we talking about

1 when theory might at least give us some good guidance.

2 PROFESSOR HENDRICKS: No, I was thinking in
3 terms of these two studies. We don't really have good
4 measures of demand or cost factors except for have the
5 crude oil price.

6 PROFESSOR WHITE: Yes, okay. So we have
7 important costs --

8 PROFESSOR HENDRICKS: For example, some of
9 these environmental regulations in certain areas of the
10 country, particularly in Illinois, Wisconsin, and
11 California, have had a major impact on refining costs.

12 PROFESSOR WHITE: Yes.

13 PROFESSOR HENDRICKS: And you would think that
14 an increase in refining costs is going to cause prices
15 to increase, and also at the same time have a major
16 impact on concentration because a lot of refiners, small
17 refineries that were previously serving the market, they
18 effectively exited the market.

19 PROFESSOR WHITE: Sure.

20 PROFESSOR HENDRICKS: So we've seen an increase
21 in concentration, and that was one of the issues that I
22 was struggling with with the GAO study. Some of these
23 environmental regulations which cause costs to increase
24 within a market were correlated with the changes in
25 concentration or price.

1 PROFESSOR WHITE: Sure. So if there are
2 variables that are changing pre and post and you have
3 not proxied them, you're not going to avoid
4 deconfounding. The specifics of the proxy are to avoid
5 deconfounding, so that's why it's paramount to identify
6 all the different things, think about whether there are
7 either theoretical or empirical reasons why their
8 distributions may change pre and post-merger and at
9 least proxy those.

10 The distinction I like to make is that we may
11 not be able to control for these limited variables, but
12 at least we can account for them, and the use of the
13 Chicago price as a proxy for cost and demand shifters may
14 achieve that.

15 To the extent that there are other regions that
16 are similarly impacted by the environmental costs, I
17 think it may be plausible that Chicago is, I'm not an
18 expert on that -- but to the extent that they are, then
19 they will help serve as a proxy.

20 It's also the case that you don't need to think
21 of the proxies as one-to-one. Some proxies like the
22 Chicago gasoline price may act as proxies for several
23 different things simultaneously. Moreover, the oil price
24 and the Chicago price jointly operate as proxies for the
25 things that are left out. So if there are correlations

1 between oil prices and other demand shifters, let us say,
2 or other cost shifters, those are going to be picked up
3 by oil prices and the Chicago gasoline price jointly.

4 That's one of the reasons why the coefficients
5 on the other variables don't tell you ceteris paribus
6 effects. They're picking up all these other things with
7 which they may be corollary.

8 PROFESSOR CARLTON: I just wanted to make a few
9 comments just to -- I think you'll agree with all of
10 them, but I just want to say them, that the approach
11 that Professor white has described is not structural.

12 PROFESSOR WHITE: Absolutely.

13 PROFESSOR CARLTON: And therefore just to go
14 back to your previous comment, the thought experiment of
15 using what the GAO said was a quasi structural equation,
16 with endogenous variables, I want to repeat something
17 that Professor White had on one of his slides, the
18 experiments you're doing is quote, if all else is held
19 constant what happens to the price merger and the
20 question I have for the GAO is: What is the thought
21 experiment you're doing in holding all else constant?

22 If it is an endogenous variable you have in
23 your equation and you're holding it constant, that's a
24 very odd conceptual experiment because, by assumption,
25 the endogenous variable will be altered by whether

1 there's a merger or not. So therefore there's a logical
2 problem when you use these quasi structural equations to
3 estimate something with endogenous variables, and that's
4 why I was saying the reasons sometimes instead of a
5 structural estimation you may want to do a more reduced
6 form estimation. This is definitely such a case. I
7 think this is what Professor White was saying, a case in
8 which more of a type reduced form estimation is done.

9 I think this is the correct way to think about
10 it. Suppose you have past data on price and variable X
11 and suppose price equals some function of X. If you
12 don't have data on all the Xs, but you have some of them,
13 you can still make predictions of price. You want to
14 make a prediction into the future and then you look at
15 the difference between that prediction and what happens,
16 so that seems exactly right. That's the spirit of a
17 reduced form.

18 Now, when Professor White talked about using
19 cost and demand shifters, I want to emphasize I'm pretty
20 sure what he means is exogenous cost and demand
21 shifters, not endogenous.

22 PROFESSOR WHITE: Not jointly determined,
23 absolutely.

24 PROFESSOR CARLTON: That I think is critical.
25 Now, when Hal talked about the advantage of this

1 approach is that you're not in a linear straight jacket
2 and Professor Hausman said you can allow the
3 specification to change post-merger, that's all
4 something that you could do either in a structural or a
5 non-structural set up. So, as a matter of course, you
6 should be adjusting your functional form assumptions to
7 test for robustness of your results.

8 So those I think were just confirmatory
9 comments.

10 PROFESSOR WHITE: That's correct.

11 PROFESSOR CARLTON: This is a slight question.
12 I think this is a question. You didn't describe whether
13 you thought the merger was endogenous or not. Now,
14 let's suppose within the structure of the model you can
15 predict whether the merger will occur or not, and it
16 seems to me whether a merger occurs or not could alter
17 the functional specification.

18 I wasn't sure. I think what you were saying is
19 you estimate the but-for effect of the merger
20 conditioned on what would have happened absent the
21 merger from the predictive equation.

22 PROFESSOR WHITE: Yes, pre-merger.

23 PROFESSOR CARLTON: Okay. And again it's just
24 a question of what the conditioning experiment is. What
25 you're conditioning on is a merger would happen.

1 PROFESSOR WHITE: Right.

2 PROFESSOR CARLTON: You're saying suppose I
3 could have prevented that merger but the economic
4 factors that would otherwise have worked themselves out
5 would work themselves out.

6 PROFESSOR WHITE: In the same way.

7 PROFESSOR CARLTON: In the same way, and the
8 only question is: In the same way absent the merger?
9 In other words, if there is a relationship between a
10 merger and probability of a merger and the economic
11 factors, I think it gets a little more complicated.

12 PROFESSOR WHITE: Right, it is complicated but
13 if we look at these key requirements this will help
14 clarify. The predictive proxies I'm talking about,
15 and this is a point that you just made which is
16 absolutely right, these are the costs and demand shifters
17 that are not causally impacted by the merger itself, so
18 these are removed either in time or by levels of market
19 structure from the impact of the merger.

20 Now, the underlying condition that makes all of
21 this work is what's known in the literature as Rubins
22 unconfoundedness condition, and in this context, what
23 that condition means is that given the predictive
24 proxies that we have available, the merger is independent
25 of the unobservable drivers. That's the key condition,

1 and that's talked about in the paper.

2 Now, that doesn't mean that the probability of
3 merger isn't impacted by these proxies. In fact it is,
4 and that probability is in the treatment literature
5 called a propensity score, and that actually has an
6 important role to play in properly handling the non
7 randomness of the treatment.

8 PROFESSOR CARLTON: Okay.

9 PROFESSOR HAUSMAN: I'll just make a few
10 comments. Again as with every method, I think this has
11 some advantages and some potential disadvantages. Let's
12 go back and talk about regression. In the literature
13 this goes back to -- in the statistics literature you'll
14 see a description of there are two interpretations of
15 regressions and it's again one of the things that I'm not
16 sure everybody understands what's going on. Let's do the
17 following thing. Let's say that we have something, I
18 don't know if I should make this serious or not. Let me
19 make this something serious.

20 It's a price of some product in supermarkets,
21 price of cereal in supermarkets and the price of beer in
22 supermarkets. If we can use that in most cases,
23 Massachusetts allows beer to be sold in supermarkets

24 PROFESSOR WHITE: I don't advise putting it on
25 cereal though.

1 PROFESSOR HAUSMAN: There's a tax change. Now,
2 one of my other hats, I used to do a lot of work in
3 public finance, we like to say, Well, what's the impact
4 of this tax, how is going to affect the retail price of
5 beer? There are two ways you can do this, two
6 interpretations of regressions. There is one which is a
7 structural model so we get the cost and demand shifters.

8 The other thing is you could have just a
9 predictive equation which says, Look I know what goes
10 into the cost and demand for beer, it's hops,
11 electricity, it's service, and the structural models are
12 almost too complicated, so what I'm going to do is I'm
13 just going to write this as a regression model. It
14 could be nonlinear but people are often using linear.
15 That gets the conditional expectation as I said this
16 morning on all these factors.

17 And indeed it can be structural, the signs,
18 they can be complicated interactions but it does have
19 some properties. It's a prediction. It's a minimal mean
20 square error predictor.

21 So now let's say that we just had one city. We
22 had San Diego, where I know they sell beer in
23 supermarkets so we did that, and the San Diego city
24 council for whatever reason puts a tax on beer in year
25 two, so what are we going to do in this type of model?

1 We're going to take the predictor from period one. We're
2 going to put all these factors in. I'm going to assume
3 the price of hops and sugar are unaffected by this tax.
4 Therefore I'm going to take the prediction in period two.
5 I'm going to take the price, and I'm going to subtract,
6 and I'm going to say that's the effect of tax, okay?

7 Now, there are some important assumptions that
8 are going on in this model, and that is to say that in
9 period two, whatever that functional form was in period
10 one that you've chosen are those coefficients are going
11 to work in period two if you have --

12 PROFESSOR WHITE: Actually, no, that's not an
13 assumption.

14 PROFESSOR HAUSMAN: I'm going to only have two
15 observations.

16 PROFESSOR WHITE: Two observations.

17 PROFESSOR HAUSMAN: I have all the observations
18 before that allows me to fit the model but I only have
19 one time period after.

20 PROFESSOR WHITE: Okay.

21 PROFESSOR HAUSMAN: So what I'm going to do is
22 I'm going to subtract and I'm going to say that's the
23 effect of the tax.

24 PROFESSOR WHITE: Let me be sure I understand.
25 I've got one period post-tax.

1 PROFESSOR HAUSMAN: Right.

2 PROFESSOR WHITE: And so I've got some
3 observation on that, and then I've got a prediction
4 built up from let's say 50 weeks of data pre-tax, so I'm
5 going to make a forecast of what I would have expected
6 the outcome to be

7 PROFESSOR HAUSMAN: That would be the but-for
8 price, but for the tax, and then you subtract it.

9 PROFESSOR WHITE: Right.

10 PROFESSOR HAUSMAN: So that's really the
11 methodology.

12 Now, that has some potential problems, and that
13 is that if anything else changed. If other things change
14 besides the tax in the period when you're going to
15 measure, it's going to be confounded.

16 PROFESSOR WHITE: Yes, if you didn't include a
17 proxy for something that changed between those periods,
18 then you're absolutely right.

19 PROFESSOR HAUSMAN: But the technology could
20 change for making beer. There could be a new entrant of
21 another beer company. There could be all sorts of
22 things. I'm not saying anything is wrong with this.
23 It's something you have to assume.

24 PROFESSOR WHITE: You're making a very
25 important point.

1 PROFESSOR HAUSMAN: It's something that you
2 have to assume could happen.

3 So now let's say instead of just having San
4 Diego, we have San Diego and we have LA, or let's not
5 choose LA because we don't want some city too close. We
6 have San Diego and I will use San Francisco and San
7 Francisco has not imposed a tax, so now you're in better
8 shape because what you can do is you can take the
9 predictive equation for San Francisco, to the extent
10 you're going to assume they have the same technology now
11 as San Diego, and you're going to say, Can I predict San
12 Francisco in period two and can I do that well. And if I
13 can do that well, that then gives me confidence that what
14 I'm observing in San Diego is actually true.

15 But if things change in San Francisco, what I
16 have to do is to assume that the way they change in San
17 Francisco is going to be similar to the way they changed
18 in San Diego to allow me to separate out the
19 experimental effect and the -- let me just finish up.

20 PROFESSOR WHITE: Go ahead.

21 PROFESSOR HAUSMAN: As you have more cities,
22 then of course this becomes better and better because
23 you can test this.

24 Now, you don't have to do this within the
25 cities. You can do this over time as well. I took two

1 cities because I think it's easy to think about, but of
2 course you can do San Diego 50 periods before the merger
3 and 50 periods after the merger, but this kind of
4 stability test that I was talking about with San
5 Francisco is really saying that you could do after the
6 merger and say are things stable as well.

7 What really worries me about this is the
8 following: The good thing about structural models, and
9 Dennis's point about what you're holding constant is a
10 good point, I mean I think that's on a different point.
11 The good thing about structural models is when you
12 estimate them, you can say, Do these elasticity estimates
13 really make sense?

14 So I don't know whether people from GAO are
15 here, but I did not make this point and now I'll make it.
16 The thing I find most surprising about the GAO study is
17 not necessarily that they found positive effects but
18 that they found large and significantly negative merger
19 effects for a couple of the mergers. It's hard for
20 me to think of an economic model of mergers, apart from
21 vertical ones, but if these were actually horizontal
22 mergers it's hard for me to think of a theory that will
23 give a negative effect. I'm not an expert I'll say on
24 the oil or gas industry, but to the extent that I
25 understand it, I think going back in the '70s or '80s, I

1 just don't see how the marginal supply -- these guys are
2 not the marginal suppliers, how you get a significantly
3 negative effect, so that really starts to worry me, and
4 this sort of goes back to Hal's thing.

5 If you're closer to a structural model you can
6 sort of take a look at the estimates of the elasticity
7 and say, Do these really sort of make sense to me a
8 priori. I think for most people to the extent that
9 they do, you feel more confident with this, and you can
10 do it both on a before and after basis. If the
11 elasticity changes for beer the market elasticity is
12 probably about one -- say it's 1.2, if it changes to six
13 after the merger, I would be really worried because I
14 just can't believe all these Bud drinkers became much
15 more price sensitive. That's something you can actually
16 benchmark and worry about.

17 What I'm worried about sort of in this effects
18 approach is when you have everything interacting
19 together. I've done this as well. It's much more
20 difficult to sit down and scratch your head and say, Does
21 this make a lot of sense. And the worry is you'll start
22 putting in more and more interactions to fit things
23 better and better and better and better, and then that
24 leads to a whole other set of problems which I don't have
25 time to talk about.

1 But it's just not being able to -- so what Hal
2 is emphasizing is absolutely right, is you want to use
3 you're a priori knowledge to say what variables you want
4 to put in, but we also have some a priori knowledge
5 about what reasonable coefficients are and that's really
6 what we lose in this approach. Every approach has
7 advantages. Every approach has disadvantages.

8 This approach has a lot of advantages. I
9 actually happen to like it, but I think that's a
10 disadvantage that we may have left out some variables
11 because we didn't think about them, and that if we had
12 done a structural model, when we looked at the elasticity
13 aspects, we would say this model is screwed up because
14 you can't get that as an elasticity of beer. But in
15 Hal's approach, you could have left out some variables
16 that you should have put in and you may not catch them.
17 You may get lucky. If you have other data you will
18 catch them, but you can't look at the coefficient and
19 know that you missed something that you should have
20 included, so you can't use as much a priori knowledge as
21 you can in the more traditional approaches.

22 PROFESSOR WHITE: Let me just respond briefly
23 because I know we have tied together as a panel.

24 I agree with probably most of what you just
25 said. There is one thing that you can look at and

1 understand whether it makes sense or not and that is the
2 effects of the merger, whereas in the alternative world
3 you may not be able to even do that. But there is also a
4 false sense of security that you can gain by looking at
5 coefficients that all seem to have pretty much the right
6 sign. Just because a coefficient has the right sign
7 doesn't mean the model is necessarily correctly
8 specified just as those of you who have run a lot of
9 regressions to get correct science may suspect. So I
10 would actually advocate doing a Hausman test, even if
11 the signs look right, because even if everything looks
12 right to me, that may be a story that's too good to be
13 true, and I might be suspicious that there may be other
14 subtle specifications that the right signs may be
15 lulling me into.

16 I also want to agree with what Jerry was saying
17 about the importance of having ways to test the validity
18 of these underlying assumptions.

19 Now, as Professor Hausman very carefully
20 articulated in his studies, there were these underlying
21 untestable assumptions, but there are also ways to test
22 certain overhead assumptions. In the last section of
23 the paper, I discuss a test which can be applied to see
24 whether or not we really have this conditional
25 independence that's necessary to think that we really

1 have identified the effects of interest.

2 So if you've got those other identifying
3 variables, predictive proxies actually, then you can at
4 least test to that degree and let me stop with that.

5 PROFESSOR HAUSMAN: I do have one last point
6 and that is I think there's a little bit of a difference
7 between doing merger estimation and doing something in
8 labor economics, okay? It's somewhat a subtle point.

9 In labor economics you put a bunch of
10 coefficients in. You want to get the right signs, but
11 actually in merger analysis, a lot of the coefficients
12 that you estimate, go back to my beer example, are
13 actually elasticity estimates, so it's not only that you
14 know what the sign is but you actually have a pretty good
15 knowledge about the magnitude. So that's different from
16 a lot of regression work people do, in labor economics.

17 So I'm just saying that you're going to lose
18 that priority information which can actually be useful.

19 PROFESSOR WHITE: Provided that you've got all
20 of those other things specified properly.

21 PROFESSOR HAUSMAN: Yes.

22 DR. THOMPSON: I only have a minute, so I'll
23 ask you a question instead. There's one thing that
24 puzzled me in your presentation, and that is what exactly
25 do you mean by a proxy?

1 Proxies appear to have to have certain
2 properties. But it's not clear to me that they have to
3 have any properties that would actually cause them to be
4 very informative about the underlying things for which
5 they are proxying. So I'm wondering if there are some
6 conditions unstated in your presentations that we should
7 know about?

8 PROFESSOR WHITE: This slide here is actually
9 the one where I say the ingredients, and it's really
10 economic theory, which is driving this. The things that
11 you start with are the list of things that economic
12 theory tells us should matter.

13 Then, for example, there are going to be some
14 things that we can't observe, and maybe it's let's say a
15 wage for a worker or a collection of workers on the cost
16 side in the industry that we're interested in.

17 Well, maybe we can observe a wage index for
18 those things, so what I have in mind are sort of errors
19 in-variables, error latent measurements for the things
20 that we would like to observe. But economic theory may
21 also tell us that, for example, as in the FTC study the
22 price of Chicago is impacted by similar supply and
23 demand shocks. Maybe they're not identical but at least
24 they're driven in a similar way, and as long as the
25 errors in measurement are not affected by the merger, and

1 as long as the relationship between the things that we
2 can measure and the things that we aren't satisfy this
3 conditional independence condition, then they can act as
4 useful proxies.

5 Now, it may be that we've included a variable
6 that actually has no predictive value, and that's a
7 question that actually can be addressed by hypothesis
8 testing for the prediction equation in the pre-merger
9 observations.

10 DR. THOMPSON: The conditional independence
11 that you referred to is quite important.

12 PROFESSOR WHITE: It's the key.

13 DR. THOMPSON: And it's roughly equivalent to
14 the zero correlation condition between the instruments
15 and the error in a more conventional structure. Is that
16 right?

17 PROFESSOR WHITE: Yes, it is. It's playing a
18 very similar role. It's just that what these things are
19 is not instruments, which are supposed to be uncorrelated
20 over the errors, but rather proxies which are supposed to
21 be highly correlated with the things that we have.

22 PROFESSOR CARLTON: Can I re-ask a question?
23 It's subtle. I may not have asked it well. If the
24 merger is endogenous --

25 PROFESSOR WHITE: Oh, yes.

1 PROFESSOR CARLTON: If the merger is endogenous
2 and predictable from the data, and you ask the
3 question, What was the effect of the merger. You could
4 also ask the flip question, what would have happened had
5 I stopped the merger, which I think is what a lot of
6 people are asking. There's a subtlety in this that I
7 don't think -- I tried to bring up and I think I failed
8 to, and that is if you stop a merger that otherwise would
9 have occurred and been predicted to have occurred, then
10 you have interfered with economic forces that were in
11 place during the period of estimation and now in the
12 but-for world without the merger, you will be in a
13 different situation.

14 That was my subtlety. That is, if mergers are
15 endogenous, I think the interpretation of the merger
16 variable is quite subtle and especially if you're a
17 government agency and asking what is the effect of
18 letting a merger go through or not, there's a very
19 subtle condition as to exactly what question you're
20 answering. It all has to do with what you're
21 conditioning on remaining constant.

22 PROFESSOR WHITE: So let me respond briefly
23 with two components of an answer. First, whatever the
24 answer is it applies equally to the traditional approach
25 or to this approach, so that's an important question for

1 both contexts, and if there are misleading answers that
2 arise by not properly treating the endogeneity, they will
3 impact both the traditional and this approach.

4 Now, off the top of my head, since I haven't
5 carefully considered this, but I know that this is
6 worthy of a very careful consideration, it strikes me
7 that it may be possible to instrument the merger. If
8 might have to be in a situation where you have a lot of
9 different mergers so you properly instrument, but if you
10 can somehow be predicting its probability in some
11 suitable way instead of treating it as a zero-one, treat
12 it as some predicted value, there may be a way to extend
13 this framework so that that endogeneity might somehow be
14 incorporated. But that, at this point, is a projection.

15 MR. FROEB: Thank you very much, and we're
16 going to to a break until 3:00 p.m., at which time Ken
17 and Scott will come back and tell us what this all means
18 for merger policy. It's the last panel.

19 **(Whereupon, a brief recess was taken.)**

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1 **PANEL FOUR: IMPLICATIONS OF CURRENT LEARNING AND**
2 **ANTITRUST MERGER POLICY IN THE PETROLEUM INDUSTRY**

3 **MODERATORS: KEN HENDRICKS, Ph.D.**

4 **SCOTT THOMPSON, Ph.D.**

5 **PANEL MEMBERS: HAL WHITE, Ph.D.**

6 **JERRY HAUSMAN, Ph.D.**

7 **DENNIS CARLTON, Ph.D.**

8

9 MR. FROEB: We're ready for the last panel.

10 Ken Hendricks and Scott Thompson will be co-moderating
11 the last panel which will hopefully tell us what this all
12 means for antitrust policy. We're going to be out of
13 here precisely at 4:00. Thank you.

14 PROFESSOR HENDRICKS: In contrast to previous
15 speakers, I don't have any presentation to exhibit on the
16 screen, but I do have some comments and thoughts.

17 The first question I would like to focus on is
18 the value of the treatment approach versus the
19 traditional regression approach.

20 If we look at the FTC study, the narrow
21 question that's being asked is: What was the impact of
22 the merger on prices in a specific market, in this case
23 Louisville? The treatment approach requires a control
24 group, which in this case was Chicago. If prices in
25 Chicago are an adequate control, that is, they were not

1 affected by the merger that took place between Ashland
2 and Marathon, what we're doing is taking the relationship
3 between the Chicago price and the Louisville prices pre-
4 merger, and using that relationship to project the "but-
5 for" price in Louisville if the merger had not occurred.
6 You take the difference between actual and but-for prices
7 and that indeed would be a measure of the impact of the
8 merger. The finding is that there wasn't much of an
9 impact.

10 In the case of the GAO study, essentially a
11 similar approach was taken with merger dummies. Only in
12 this case, the treatment effect was obtained by averaging
13 across the markets in which the two firms in the merger
14 were participating. Hence the control group is markets
15 in which only one of the merging parties was operating or
16 neither party was operating. Thus, in this case the
17 coefficient of the merger dummy is essentially an average
18 across a set of markets in which the merger had an
19 impact. A further complication in the GAO study is that
20 they study the impact of different mergers in the same
21 regression, which makes it difficult to determine what
22 the control group is for each merger.

23 In neither study do you really get a handle of
24 what is determining the impact of the merger. So, for
25 example, if you thought about the FTC study, you would

1 have to think about running that kind of treatment
2 analysis separately for every market that is impacted by
3 the merger and finding an appropriate control for that
4 market. At the end of the day, you would a list of
5 numbers which measure the impact of the merger in
6 different markets.

7 What I would like to know is: what is
8 explaining the variation in the impact of the merger
9 across markets? The FTC study focuses on one market and
10 does not produce this variation across markets, so I
11 can't answer that question. The GAO study assumes away
12 that variation because it turns on the dummy in every
13 affected market and computes an average across the
14 markets. So I can't answer this questions from that
15 study either.

16 In the GAO study, you do have some variation
17 across mergers because they're doing a multi-merger
18 analysis. So you can ask the questions, what is
19 explaining the variation in the average impact of the
20 different mergers?

21 But I would be happier if we could first
22 measure the impact of a single merger in different city
23 markets and then try to explain this variation across
24 markets rather than trying to explain the variation in
25 average impacts across mergers.

1 And the reason why I think that this issue is
2 important is because if you are at the FTC or DOJ
3 thinking about whether or not to okay a merger or contest
4 it, the question that you really want to ask is: what are
5 the market conditions under which this merger going
6 forward is likely to have a big impact on prices? Which
7 markets would it not have a very big impact?

8 In particular, what conditions are warning
9 signs? I think the treatment approach doesn't really get
10 at that question because it simply measures what the
11 impact of the merger was in a particular market without
12 asking why the impact is what it is.

13 I'm not saying that it can't be done, but in
14 principle what you would have to do I think is estimate
15 the magnitude of the merger impact for different markets
16 and then ask the question, what factors explain the
17 variation? What are the market conditions under which
18 this merger's impact is large, what are the conditions
19 under which the impact is small?

20 And that ultimately is the policy question that
21 sits before the FTC and the DOJ. Theirs is not an ex
22 post evaluation; it is an ex-ante evaluation. So what I
23 always thought was valuable about measuring the impact of
24 mergers ex post is that the results should help us decide
25 ex ante whether we should let the merger go forward, or

1 contest it, or what kind of divestiture should we be
2 asking for.

3 I think that is the perspective that I have
4 always had when I was thinking about this program, of the
5 FTC going out and getting data after the merger,
6 measuring what the impact was, and determining whether
7 the predicted impact matches up with what actually
8 occurred.

9 I think that the exercise of comparing what
10 happened ex post to what we predicted should happen ex
11 ante is something that has not been addressed in either
12 of these two studies as far as I can tell from reading
13 these studies. They have simply focused on the ex post
14 analysis which is essentially a measurement problem. I
15 think the treatment approach does a very good job on the
16 measurement problem. It's very flexible. I'm using it
17 myself in studying the impact of new album release on
18 catalogue sales, but in that study, I can't explain nor
19 can I predict at this point which new release is going to
20 have a big impact on catalogue sales and which will not,
21 and that's the kind of question we want to know ex ante.

22 I'm more of a theorist by nature. The way I
23 think about the ex ante perspective is that I want to
24 know the value of certain key structural parameters,
25 e.g., demand elasticity. We know that the impact of a

1 merger is likely to be very low when demand is highly
2 elastic. The treatment approach is not going to give you
3 any information on structural parameters like demand
4 elasticity or marginal costs. That is the kind of
5 information that I would like to have in studying a
6 merger ex ante to help predict which is likely to happen.

7 Theory provides models. The Cournot model has
8 been described and is the basis for the Merger
9 Guidelines. I would like the ex post analysis to help me
10 understand which theoretical models should we be using to
11 try and predict whether or not a particular merger is
12 going to have a big impact or not. For example, is the
13 reason why the FTC study finds that the Ashland-Marathon
14 had no impact in Louisville despite large changes in the
15 HHI because their definition of the market is too narrow
16 or because competition among firms is more competitive
17 than is assumed in the Cournot model?

18 This is the kind of feedback I would like to
19 see from an ex post analysis. In this way, the results
20 will feed back into policy-making and help us think about
21 mergers from an ex ante perspective.

22 DR. THOMPSON: Well, I just have a couple of
23 comments. First of all, I want to thank everybody for
24 having us here today. I think this has been quite
25 educational for me, and hopefully for other people as

1 well.

2 I'm going to highlight a few things that I take
3 away from this about the econometric exercises themselves
4 in conducting these ex-post analysis of mergers.

5 The first one is the obvious one. This is not
6 easy to get right. There is plenty of room for
7 disagreement. A lot of hard work can go into it,
8 and nevertheless reasonable people can disagree about
9 what the answer is.

10 Secondly, I was struck by Professor Hausman's
11 emphasis of what he called the bedrock assumptions things
12 you can't question. This is something that, back when I
13 was teaching, I tried to hammer home to my students, and
14 I still do with my colleagues at EAG today. It may have
15 actually come from Professor Hausman's chapter on
16 identification in the Handbook of econometrics, but I
17 can't be sure.

18 The main thing I take away from the importance
19 of bedrock assumptions is that it really facilitates
20 discussions about the disagreements, about the
21 differences of opinion, to try to explicitly identify
22 what they are. I think that this is actually one of the
23 difficulties in trying to compare the two studies that
24 are the subject of our discussions today.

25 I don't think either study is as explicit as it

1 could be about these kinds of assumptions.

2 I guess I found the FTC study perhaps a bit
3 more transparent on this, in part because it's a less
4 complex study, and it doesn't try to do quite as many
5 things. It's much harder to draw general conclusions
6 from it as a result. But at the same time it's a lot
7 easier to understand what's going on and what has been
8 assumed and what hasn't been assumed.

9 The fact that you can put up a graph, a plot of
10 the data of these differences, and get a pretty good idea
11 of what the econometrics is going to tell you is a huge
12 virtue. There's nothing quite like putting a clear
13 picture in front of a Judge if you're going to go in and
14 challenge a merger.

15 Now, if you look at that picture, it shows more
16 or less flat impact on retail prices for -- I'll get it
17 wrong now since I didn't bring it up here, let's just
18 say retail prices.

19 It's always possible of course that there
20 actually was a merger effect in that flat line. It could
21 be that the prices would have gone down but for the
22 merger. But that's a story that's extraordinarily hard
23 to tell if you're going to go into the courtroom and try
24 to say that a particular merger was bad or is going to be
25 bad.

1 And so one of the things that I took away here
2 is that, in some sense, pictures are better than any kind
3 of econometrics you can do in terms of actually making a
4 convincing case about how you should be thinking about
5 one of these transactions.

6 A third lesson that I would take away from this
7 in terms of the econometrics is that you may be able to
8 do quite a bit with imperfect data and uncertainties
9 about the functional form of the underlying reduced form
10 expressions. But one thing you can't live without is a
11 pretty good idea as to which variables ought to be there
12 and which ones ought not to be there. During Hal White's
13 discussion of the treatment approach, I think somebody
14 said that this was a non-structural approach. I'm
15 actually inclined to disagree.

16 I think you really need to have a pretty good
17 idea what the underlying economic structure is in order
18 to be able to make reasonable decisions on which
19 variables are important and which ones aren't. I believe
20 the language used in the presentation was asking which
21 variables are determining of the outcomes that you're
22 measuring.

23 Now, that doesn't necessarily mean you need a
24 structural model, but it does mean you need to be
25 thinking structurally about the economics. And in

1 particular it emphasizes something that I try to bring
2 home to my colleagues when they come to me with
3 econometric questions, namely that you better be a good
4 economist first. If you're not ready to make hard
5 choices about the bedrock assumptions, as they were
6 called today, then you're not really ready to start doing
7 econometrics.

8 It's an absolute prerequisite. Everything you
9 are going to do is going to depend on these, and you
10 better feel that each one it really is a bedrock
11 assumption as opposed
12 to something you're just pulling out of the air.

13 A fourth, and probably the least important
14 point, but one I see a lot so I want to mention it, has
15 to do with dealing with the correlation structure of the
16 errors in equations that you might be estimating. Both
17 studies included various kind of GLS corrections to try
18 to deal with correlations of the errors.

19 My experience, and I think I heard the
20 panelists today confirm this, has been that trying to
21 correct for that kind of structure is an extraordinarily
22 difficult thing to do well. It requires much stronger
23 bedrock assumptions to do correctly, because you're
24 explicitly mixing the data. That changes the kind of
25 assumptions you're making about which things are

1 correlated with which other things.

2 It's been my experience that a far preferable
3 approach is to not do the correction. Try instead to
4 calculate appropriately robust standard errors and report
5 those. You probably ought to do that even if you do do
6 these adjustments.

7 So those are various things that I've taken
8 away from the econometrics, from the discussions of the
9 econometrics today.

10 But I think more generally the question that we
11 hope to get answered is whether or not there are any
12 implications in all of this for the merger review process
13 and for how the agencies ought to be conducting their
14 merger investigations.

15 I'm having a hard time hearing any strong
16 lessons along those lines: Be a good economist; Think
17 hard about what you're doing and do the best job you can
18 in terms of getting the economics right; Double-check
19 all your work; Make sure that you don't make silly
20 mistakes when you do your work; and all the usual common
21 sense things that you hope any economist would do, not
22 just somebody reviewing mergers.

23 I think Ken was absolutely right that there
24 seems to be a distressing variety in the measured
25 outcomes when you take these at face value. It's

1 difficult to know what to do with those. How am I
2 -- well, I probably won't have anything do with it, but
3 in the next gasoline merger that comes along, how do we
4 decide if it's the plus \$0.05 or the minus \$0.02 result
5 that we should be projecting for that particular case?
6 I'm not sure we've actually heard much today that lets us
7 answer those questions, and that's due in large part to
8 the non-structural approach that both of these papers
9 take.

10 Now, there's a related question which is, What
11 if you don't have all the data you need in order to make
12 a projection based on any one of these models? That's a
13 separate issue. It's a difficult question, but I'm
14 really abstracting from that a little bit and trying
15 to get at the more fundamental problem, which is that
16 there is a distribution of outcomes in the world. You
17 see that in these results, and the problem that we're
18 faced with is how to sort them out, not to find the
19 average effect, or the worst possible effect, or the best
20 possible effect, but to find out what is the likely
21 impact of this particular merger?

22 In a broader view of the world where the
23 mergers are endogenous, we have an even more difficult
24 problem because, as a couple of people have mentioned, if
25 the mergers themselves are endogenous, then talking about

1 the effect of the merger is a little bit like talking
2 about the effects in that example that I mentioned
3 earlier. It's something that's jointly determined with
4 other things that we care about, and what you really have
5 to do is go back into the economic structure and figure
6 out how the merger is endogenous, and what determines the
7 merger process. Of course that's a much harder thing to
8 do in terms of coming up with reasonable answers.

9 So I'll just wrap up what I say by posing just
10 a few questions for the other people on the panel. These
11 are just a few thing things that occurred to me while I
12 was hearing the presentation this morning. First of
13 all, I would be interested to know if anybody does see
14 any prospects for prospective merger review from
15 retrospective studies, however good.

16 A related question to that, I think, is the
17 extent to which we learn anything by looking at
18 individual mergers that has implications for the merger
19 review process overall. I'm not sure that I do see any.
20 Finally, I'll finish up with the question of what people
21 feel about the lack of structural modelling in these
22 studies. Is it still possible to say
23 something about causation in the mergers that we might
24 actually be facing? There seems to be a bit of a
25 disconnect between explaining what would happen using a

1 treatment effects concept and actually going in to argue
2 to a Judge that the treatment effects concept is the
3 right way to approach it, and that the merger really is
4 the cause, as opposed to a correlation, of something you
5 might care about.

6 PROFESSOR HENDRICKS: Can I say one thing before
7 you go on? If you think back to the price concentration
8 type of regression and contrast that to the treatment
9 approach, you can get some idea of how much more
10 ambitious a price concentration regression is than a
11 treatment regression. That is why, if you simply want to
12 measure the impact of a merger, I think the treatment
13 approach is far more flexible, far more robust as
14 Professor White has indicated for all of the reasons that
15 he has cited.

16 But if you think about the price concentration
17 regression, the goal of this approach is to use the
18 coefficient on the HHI variable to explain all of the
19 variation of the impact of a merger across different
20 markets. You will never believe it, but that is the
21 goal, and that is the sense in which the price
22 concentration regression is trying to answer not only the
23 question of what is the impact but also, coming back to
24 the ex ante perspective, to help policy makers to predict
25 the impact of a proposed merger. These are the changes

1 in the market conditions that we care about; that's why
2 the numbers 1800 and 1000 show up in the Merger
3 Guidelines. The problem, as Professor White and I think
4 most of us on this panel feel, is that it is difficult to
5 believe in a price concentration regression as a casual
6 relation, to believe that the coefficient in front of the
7 HHI variable is really measuring something. There are a
8 lot of assumptions that go into the model and a lot of
9 these assumptions are problematic in practice.

10 PROFESSOR HAUSMAN: Okay. I know many people
11 in the audience, but probably less than half, so what I'm
12 going to talk now about a lot of is going to be rather
13 cryptic vis-a-vis the Merger Guidelines, so if anybody
14 has questions, stop me and I'll try to answer them.

15 Just in terms of background the first thing is
16 that I would like to agree with Scott in terms of the
17 importance of getting the assumptions down so that they
18 can be discussed. I think if you look in the broader
19 scheme of things and you say, What happened to economic
20 theory or economics as a profession after World War II?
21 In pre World War II, apart from a few people who -- we
22 have a lot of people waving their hands telling stories,
23 but the whole thing about economic theory and having to
24 write down models is you have to make your assumptions
25 explicit.

1 Now there's something lost when you do that
2 because you have to simplify, you're waving your hand to
3 try to explain everything, but it sharpens the focus of
4 the discussion, and that's really what econometrics is
5 doing here too. It is saying, What do you have to assume
6 to believe the results and if you don't assume it, how
7 sensitive are those results.

8 Another thing I would like to mention briefly,
9 which I think is very important within the merger
10 context, and that's what I want to speak to now, is what
11 statistical inference is all about. So what statistical
12 inference is all about is you have some data and you're
13 trying to generalize from a sample to the population.
14 So we have done this empirical work and now we have
15 a new merger walk in the door and we want to be able to
16 generalize from our sample information. You want to
17 convince yourself and you want to convince others.
18 There's a huge literature on this of course on the
19 statistics literature.

20 The thing that I find disturbing is, of course,
21 when you have imperfect data, a lot of what you're going
22 to infer depends on your priors. I used to have a
23 colleague, who unfortunately died of cancer, named Fisher
24 Black, a very famous guy. He would come in to my office
25 and we would argue for days sometimes, and it turns out

1 that Fisher, I would always say, you have the tightest
2 priors of anyone I ever met. And, if you have tight
3 enough priors, no amount of data unless it's perfectly
4 ended to infinity, will ever change someone's mind.
5 Fisher had to be a very good economist, a little crazy
6 but a good economist, and sometimes he was very right and
7 sometimes he was very wrong, at least in my view.

8 I think within the agency and within merger
9 review, this gets just completely screwed up, because at
10 least in the old days -- I've been at this for 20 years,
11 it's gotten much worse recently -- people would come in
12 with priors and say based on my knowledge of this
13 industry or based on this or based on something, here
14 are my priors and you have the hurdle to overcome for
15 that second request if you're going to allow this to
16 happen.

17 What's gotten much worse now is that you have a
18 lot of arbs out there. Every time there's a merger, I
19 get three arbs calling me within two hours if I've worked
20 in the industry, and the arbs hire former FTC lawyers, I
21 can name names if you want who come and see their old
22 buddies in the FTC and tell them that they can get them
23 affidavits from customers who will tell them how horrible
24 this merger is going to be.

25 I think this came out ever so plainly in the

1 Oracle trial. Judge Walker just kicked the living
2 bejesus, if I can use the word, out of the Justice
3 Department because he didn't believe the witnesses.

4 There have been two problems with those
5 witnesses for the last 20 years I bring up every time.
6 Number one, they're not marginal customers, and number
7 two, they're not randomly selected. In most mergers if
8 you can't price discriminate it only takes five to ten
9 percent of the people to shift to stop a price increase
10 in an industry with high fixed costs. How empirical work
11 has happened has gotten completely out of hand in my view
12 because you have lawyers, staff lawyers who are easily
13 convinced. They don't understand econometrics but they
14 have some customer brought in by their former friend, and
15 he will come up with a customer who says this is the
16 worst merger that I can think of.

17 And I was involved in a merger last year in
18 which this happened. Yet it turned out that that
19 customer's bank was negotiating with one of the providers
20 in that industry who the customer said in a sworn
21 affidavit they would never think about.

22 We had documents. He didn't know this, but we
23 had documents that his bosses were negotiating. So
24 then the final thing with that was in the Oracle trial
25 when the Justice Department in sworn answers to

1 interrogatories, they said, Nobody in their right mind
2 would buy this software from AMS, the stuff is
3 technologically outdated. Well, what happened of course
4 is the Justice Department two weeks later bought it from
5 them and spent \$24 million, a point not missed by Judge
6 Walker.

7 So the real question here is how this all
8 interacts. It's not that I'm going to fix it, but I
9 think in terms of using these models and how it needs to
10 work, that all has to be taken into account. But, of
11 course I have many jobs that I would like to have for
12 half an hour, and I like to be an academic a lot so a
13 half hour to an hour is my attention span. Being head of
14 the FTC it would be one hour for instance, but at some
15 point or another it needs to get fixed. It's not going
16 to get totally fixed but as I say, I think it's gotten
17 much worse in terms of how you can use this and how you
18 can do this.

19 Now, I want to turn with that as a background
20 to trying to answer the question, should merger policy
21 change based on the GAO study?

22 So one way to look at the GAO study is here we
23 have these people at the FTC. They're doing their
24 public duty and they did the best they could and they
25 let these mergers go through. The GAO turned around

1 and said they made some mistakes, the policy was too lax
2 and for whatever reason they let mergers go through and
3 this led to price increase. So should that lead to a
4 change in merger policy?

5 Well, surprising enough my answer is yes, so
6 Luke is going to fall off his chair. My answer is yes
7 which is unexpected. I draw two lessons from this:
8 number one, that merger policies should be changed and
9 number two, this demonstrates the HHIs can't be used. I
10 have believed this forever, if you think of it from an
11 economic point of view. But really what GAO has shown in
12 my view, to the extent that you take the results to be
13 valid and of course we do, we talked about that, is that
14 when you put in the dummy variables, the treatment effect
15 if you want to call it that way, you seem to get very
16 disproportionate results. You get some negative mergers,
17 you get some negative price changes and you get some
18 positive ones.

19 Now, if you fit all this into your regression
20 model, you can force it and get a coefficient but that's
21 the wrong way to look at it. As Scott just said, we
22 don't care about the average merger. We care about a

1 particular merger, so if I take the results as given and
2 I say, We find these results. Well, the HHI was
3 increasing in all of them, and this is yet another
4 reason that just reinforces my belief, that that section
5 of the Merger Guidelines should be removed. So, Section
6 1, gone. I mean, you can still define markets. I mean
7 I know you have to do that legally, but the whole thing
8 of calculating HHI hurts. I asked Larry White, who was
9 Baxter's assistant at the time, I challenged him one day
10 in a conference, Just show me where the thousand and
11 whatever the 1,800 or the 2,000 and how this works, and
12 if you can show it to me I'll believe it. That was six
13 or seven years ago, and I'm still waiting for the
14 definitive study that has anything to do with that.

15 So if you get rid of the HHIs, what this does
16 say though, and which I think the GAO study does
17 emphasize, is that you need to do a competitive effects
18 analysis of each merger. So, that's Section 2 of the
19 Guidelines. It's in the Guidelines. It's just a
20 question of emphasis.

21 And what this says is markets are going to
22 differ. Conditions are going to differ, and you need to
23 study each one uniquely, which is of course what the
24 agencies do to some extent, and something as simple as

1 the HHI is not going to do it.

2 Now, in certain situations if you want to build
3 differentiated good models, which Luke and I have done
4 over the years, you can have a merger prediction. But in
5 something like these models, where it's pretty much a
6 homogeneous good and if there is a problem it's going to
7 be coordinated interaction, we don't really have that
8 good of a model, I'll say this point blank. But my point
9 is that you have to get in and really dig in and do a
10 competitive effects study for a particular merger.

11 So in terms of how merger policy should change
12 with respect to oil mergers, but also how merger policy
13 should be changed in the large -- with respect to this,
14 I think what the GAO study, and I also think this is
15 true of the FTC study, has shown is the heterogeneity
16 that's involved in these various mergers. The FTC looked
17 at just Louisville by itself, the GAO were trying to lump
18 a bunch of mergers together. I would just completely
19 eliminate this idea that changes that we're usually
20 worried about that occur in the HHI ranges of 1,500 to
21 2,500 or -- I'm not saying that if you convince yourself
22 that it's a merger monopoly you shouldn't try to stop it,
23 but the hard mergers are where the HERF starts off at
24 1,500 and is going to end up at 2,500 and should we
25 stop that merger.

1 I think in those cases, it's my reading of the
2 GAO's results, which I'm saying conditional on me
3 accepting them, say that the using the HERF just is not
4 useful or predictive in that type of situation of what's
5 going to happen.

6 PROFESSOR WHITE: I would like to address a
7 couple of the questions that Professor Hendricks and Dr.
8 Thompson raised, and then also address just briefly some
9 of the things that Professor Hausman was just saying I
10 would like to start by reinforcing the message that Dr.
11 Thompson was making, which is the importance of economics
12 in doing the treatment effect approach and in particular,
13 but also -- and this underlies what Professor Hendricks
14 was saying, how important economics is to understanding
15 things like elasticities and marginal costs.

16 Maybe it's just nomenclature. The determining
17 factors that I was talking about do arise from as intense
18 an understanding of the economics as you can bring to
19 bear to the problem. So you end up with something which
20 is a reduced form in a sense, but it is a reduced form in
21 which the variables have been included and excluded with
22 excruciating care, which things belong and which things
23 don't, and it is the economics and the understanding of
24 the measurement, all of those economic variables which
25 determine which things properly belong and which things

1 properly don't.

2 The structure is designed so as to focus
3 attention on the one thing that you really care about.
4 Everything else gets left by the wayside, and that
5 either is a huge benefit or a huge liability. It's a
6 huge liability if what you want to do is to understand
7 all of the inner workings of what's going on in a
8 particular merger event.

9 On the other hand, it does provide, as
10 Professor Hendricks was suggesting, a piece of data to
11 single data point, and I think that if one can measure
12 effects of individual mergers and measure a lot of them,
13 then one can build up data from which further inferences
14 can be made. So that suggests that this sort of study
15 should proceed, and it's a brick by brick approach, but
16 only by putting these data together can we begin to
17 understand the variability and then try to sort out which
18 of these different effects really do make a difference.
19 I suppose we can put the Herfindahl index in there and
20 see if it has an impact, but my priors were pretty
21 closely aligned with Jerry's.

22 There's another aspect of the creation of this
23 data, and this addresses the question that Dr. Thompson
24 raised about whether we can learn anything about
25 prospective merger review from retrospective studies,

1 and this is also addressing the question, What do we
2 learn from individual mergers.

3 If it were possible to begin to keep or perhaps
4 we already have the beginnings of a database where the
5 predictions, and they may be heterogenous or they may be
6 a single prediction, that emerge from any prospective
7 study is recorded and put into a database, and then
8 retrospectively we can go back and look at what actually
9 happened and see whether there is a correspondence
10 between what was expected on the basis of whatever
11 analysis and what actually happened on the basis of a
12 relatively objective ex-post analysis. Maybe then we can
13 begin to understand perhaps first of all at gross levels
14 how accurate ex-ant- predictions can be, given the
15 processes that are currently used. And if there are
16 differences in those processes, perhaps we can understand
17 some of the variation and some of the factors that lead
18 to success or some of the factors which lead to
19 imprecision in our ex ante assessments of what may happen
20 in particular cases.

21 Now, to the question of whether merger policy
22 should change as a result of the GAO study, I actually
23 was originally thinking, Well, how can one study really
24 suggest any significant changes to the merger policy
25 guidelines, but I liked Jerry's answer. Now, I don't

1 necessarily think that it was this one study that led
2 him to those remarks that he gave, but I think that the
3 substance is very similar to how I'm feeling about it.

4 I've come very lately to this area, but I have
5 to tell you that my first impression when I heard about
6 the Herfindahl index and what it's used for was, Are you
7 kidding me? I haven't seen a lot that makes me change my
8 mind yet, so to the extent that my opinion as a newcomer
9 in this area should be weighed, well, perhaps it's from
10 outside. That could be either a benefit or a
11 disadvantage, but I find the characterization of the
12 concentration of an industry which is using the
13 Herfindahl index strange because, as we heard Dr. Carlton
14 say earlier, this changes across industries. As I think
15 of what concentration is, I think it must be a multiple
16 dimensional measure, not just a one dimensional measure,
17 and
18 so if there can be some movement in the direction of a
19 more considered approach, I would have to support that,
20 and I'll stop with this.

21 PROFESSOR CARLTON: All right. Let me try and
22 confine my remarks to answering the following question,
23 which I think someone posed. What change in policy, if
24 any, should we expect as a result of either these
25 particular papers or papers like these?

1 My own view is that looking at what happened to
2 mergers that were approved is certainly something you
3 want to do. You want to know whether price went up or
4 not. Now, that's what these studies are trying to
5 determine.

6 Now, suppose you did find that prices went up
7 in some mergers. That then raises the more serious
8 policy question, Should we alter our policy, not in
9 hindsight but prospectively. That is, I think
10 Professor Hendricks asked the question exactly right,
11 Prospectively, going forward, have I learned something
12 from these studies so that now if I faced exactly the
13 same situations could I have stopped a merger that that
14 would have the price go up? And that is a different
15 question than these studies are examining. I think
16 they're both important questions.

17 These studies are examining the first. There
18 have been studies that have examined the second. Not
19 many, though. The one study I'm familiar with is
20 actually done by someone in the Department of Justice I
21 think it's Dr. Pierson , and he asks the following
22 question: There may be other studies but that's the one
23 I'm most familiar with.

24 It says, We have a lot of ways to predict the
25 result of a merger. Some involve very structural

1 approaches, and then we do a merger simulation, and there
2 are a variety of different types of those. Another one
3 involved what I'll call a reduced form in which you just
4 make predictions. And, by the way, I don't mean to
5 quibble, but I'm fearful people were going to think the
6 treatment approach is very different from a reduced form.
7 They're very close in approach, so we're not really
8 talking about a huge difference, they're very similar.

9 So that seems to me a good thing to do, and if
10 I recall, the reduced form did pretty well compared to
11 the structural approach. So that's an important lesson
12 that I want to make when I want to make projections,
13 which model is more reliable? And I think that those
14 types of studies are very valuable. There was a question
15 about non-structural and what was meant. I think all
16 that was meant, certainly all I meant, was when you have
17 an equation with no endogenous variables, that's a non-
18 structural equation. I agree entirely that when you
19 write down such equations, you should have based it on
20 deep economic thinking about what the structure is, but I
21 think when people are using whether an equation is
22 structural or non-structural, they just mean whether you
23 have an endogenous variable.

24 Now, having said that both types of studies are
25 useful, a prospective study about predictions and asking

1 the question, What actually happened in mergers. Let me
2 just say something about the HHI. Although I did speak
3 and told you all the problems that can arise when you
4 have a regression between price and the HHI, it's not
5 always true that those problems exist, and sometimes you
6 can do such an equation.

7 Moreover, and obviously the FTC can defend
8 themselves, I think it's an oversimplification to the
9 extent that there was any impression left at least in my
10 experience that people at the government agencies look
11 only at the HHI. That's not been my experience. I think
12 they really do try and use their economic logic and
13 thinking about the individual circumstances. They try
14 and see if a different way of measuring concentration is
15 better, if using an index of the number of firms is
16 better, and I would agree with what Jerry said that the
17 HHI is too simplistic. That I agree with, but I think
18 everybody in the agency agrees too, they wouldn't make an
19 analysis just based on an HHI, and they would experiment
20 with different ways of analyzing the relationship of
21 pricing.

22 All right. So that answers the question, yes,
23 I think studies like the GAO and the FTC study are
24 valuable, and also I think studies as Professor
25 Hendricks described also would be valuable because

1 that's the one that informs policy.

2 Now, what can I say about this -- these two
3 specific studies, the GAO study and the FTC study?
4 Having read them, would I, if someone asked me, if
5 someone would listen to me, change merger policy as a
6 result of these two studies?

7 Well, I think it should have been clear from my
8 earlier remarks that I thought both of these two studies
9 made a lot of progress, but there's lot more progress
10 they have to make in order to convince me of their
11 findings. That's why, I'm not going to put up the slides
12 again, I had some studies that I thought actually the FTC
13 could do pretty simply to solidify their findings to
14 convince me of their validity, and similarly, I had a
15 longer laundry list of things to do, but a list of things
16 that I don't think would be very hard to implement that
17 the GAO should do.

18 Until they do that, I certainly wouldn't feel
19 comfortable saying that I'm convinced that either study
20 has convinced me of their validity, and therefore I
21 certainly, for example, wouldn't say, Oh, I'm going to
22 allow merger or not allow mergers both based on either
23 of those studies.

24 So my answer to the question is, yes, I think
25 studies like the ones we're discussing today are

1 extremely valuable. I think studies of the kind I
2 described that ask which types of models make
3 predictions that turn out to be correct and that are
4 implementable, therefore, as a matter of policy, think
5 those also are very important. In fact, I think they're
6 so important that they should be done. And, indeed, in
7 this very room yesterday, there was a meeting of the
8 Antitrust Modernization Commission, of which I'm a
9 member, and one of the topics we are considering is
10 things like exactly this, looking at what has happened as
11 the outcome of various antitrust decisions, looking both
12 prospectively and retrospectively, and trying to use that
13 information, difficult as it may be to do, but at least
14 trying to use it to try and decide, Are we on the right
15 track, and that seems like an appropriate use of
16 resources and will guarantee that we don't create
17 anything other than a very efficient antitrust policy.

18 Thank you. .

19 PROFESSOR HENDRICKS: I would like to make one
20 comment. I personally don't have as high a prior as
21 Professor Hausman has on the use of the HHI or
22 uselessness of the HHI.

23 I think the way to proceed is to do more
24 studies like the GAO report and the FTC study, do a
25 careful job of looking at the impact of mergers and

1 measuring those impacts on a market by market basis
2 across different mergers using essentially this reduced
3 form dummy variable for the merger, and build up to set
4 an understanding, sort of a data set and subsets of these
5 impacts, and then ask the question, Okay, let's explain
6 some of this variation, and the factors that explain the
7 variation across markets for a given merger, and across
8 mergers -- what are they telling us. I'm not going to
9 disregard the HHI as one potential explanatory variable.

10 In fact, if I look at a study that was done by
11 Hastings and Gilbert and on Tosco/Unocal, they had a set
12 of variables that sort of described the variation and the
13 impact of that merger across markets. The two factors
14 that had the most explanatory power were essentially the
15 downstream share that Tosco and Unocal the retail market.
16 And the other factor that mattered a lot in terms of
17 predicting -- the increase in the price at the wholesale
18 level was how many independents were close to their
19 stations. So those are all sort of market facts that
20 sort of maybe HHI doesn't measure them exactly right, but
21 they are the kinds of considerations that the HHI is
22 trying to measure.

23 So I think what I liked about the treatment
24 approach is it really is explicitly ex-post, and sort of
25 disentangles the problems of the ex-ante, and I sort of

1 always think of the price concentration regression sort
2 of confounding the ex-ante and the ex-post. The
3 treatment and prediction approach, it's really saying
4 what is the impact and we can be agnostic about what
5 underlying behavior is, what the underlying model is to
6 some extent, although what I mean by being agnostic is
7 with regard to the functional forms. The variables that
8 go in of course are the demand shifters, and the cost
9 shifters, but the model we can be agnostic about how they
10 enter in our measurement problem. Then, once we build up
11 that kind of data set, now we're in a position to sort of
12 ask the question, What are the things that matter.

13 MR. FROEB: Anyone else have anything else you
14 want to add? Go ahead.

15 PROFESSOR WHITE: I'll just go back and what
16 Professor Hendricks just said about the Hastings and
17 Gilbert study I could actually agree with, and that is
18 with a model of unilateral effects and limited or no
19 entry or expansion is -- the refinery market, we expect
20 something like the share that you have to matter. That's
21 in the Guidelines, and that's in Section 2 on competitive
22 effects, and I think it's potentially pretty useful. I
23 don't think it always works, but it is something that
24 makes a lot of sense.

25 And I don't disagree with your point that you

1 want to have a database and you don't include the HHI as
2 a separate variable, but my problem is that if you go
3 back and look at single shares, we actually have an
4 economic model that works with that, and so once we get
5 your ex-post coefficients and we've got some odds and
6 things, we probably could get some idea of how much in
7 accordance they are with economic theory.

8 My problem with the HHI is that it really comes
9 out of one model. It's dead set that that model predicts
10 you shouldn't have mergers and so to have Section 1 of
11 the Guidelines based on that, I've always found sort of a
12 conundrum, but maybe I need to think harder about it.

13 MR. FROEB: Okay. Well.

14 PROFESSOR HENDRICKS: I take your point. I'm
15 not too surprised that the model doesn't apply in a lot
16 of cases.

17 MR. FROEB: Well, the one thing that I'm sure
18 all our panelists can agree on is that we need more
19 economists doing antitrust policy.

20 PROFESSOR HAUSMAN: Fewer lawyers and more
21 economists.

22 MR. FROEB: Yeah, economists. But I certainly
23 want to thank the panel for coming here. The
24 opportunity costs of the time of the people on the
25 panel is well over six figures, and the benefit that we

1 get out of their presence here and their insights has
2 far exceeded that, so I want to sincerely thank them.

3 I want to also just get around applause for Liz
4 Callison who put this conference together almost
5 single-handedly, and I want to sincerely thank her for
6 her efforts. Just to let you know, we will put this
7 transcript up on the web. We will put the presentations
8 up on the web. I hope that this will be, one of many
9 ongoing kind of continuing studies in what we obliquely
10 call our enforcement R&D, our continuing to follow up on
11 what we're doing to try to make sure that we're doing the
12 right thing to see if we can learn from anything that
13 we're doing, to understand how difficult it is to
14 actually follow
15 up on these studies and to figure out what you're doing,
16 but it's certainly something that should be done by
17 everybody who is enforcing the laws.

18 It's as I said in the introduction, it's a
19 terrible conceit to think that you've got it right and
20 to think that you can't improve, and you think, Well,
21 how are you growing to improve? You have to set up some
22 sort of feedback mechanism to try to estimate what
23 you're doing and trying to learn from this.

24 Anyway I want to thank the panel and Liz
25 Callison too.

1 (Applause.)

2 (Whereupon, at 3:57 p.m. the workshop was
3 concluded.)

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1 C E R T I F I C A T I O N O F R E P O R T E R

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8 herein is a full and accurate transcript of the notes
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