The Apple-Cinnamon Cheerios War:

Valuing New Goods, Identifying Market Power, and Economic Measurement

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The contribution of new goods to economic welfare is a first order question for understanding the growth of modern economies. New attention to this question has improved both our understanding of the economic issues and the depth of our measurement toolkit. My comment on Jerry Hausman's paper opened up a debate about where new goods may make particularly large welfare contributions and about measuring those contributions.[1] Hausman has circulated a sharply worded rejoinder to my comment.[2] While Hausman's rejoinder is a jumble of non-sequiturs, the economic issues are sufficiently important, and the measurement issues sufficiently general, to continue the exchange.[3]

Narrowly, the issues concern the contribution to consumer welfare of a single new good, Apple-Cinnamon Cheerios. Hausman's paper in The Economics of New Goods contains a

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[1] The paper by Hausman "Valuation of New Goods under Perfect and Imperfect Competition" and my comment on it can both be found in The Economics of New Goods, Timothy Bresnahan and Robert J. Gordon, eds., NBER Studies in Income and Wealth Number 58, The University of Chicago Press, 1997.

[2] See "Reply to Prof. Bresnahan" by Jerry Hausman, distributed on July 21, 1997, and the scanned version on my website.

[3] Regrettably, the issues have also been personalized. Hausman has reacted very sharply to the way he has been treated procedurally as well as substantively. Hausman never received the copy of my comments I sent him, first seeing them in the published volume. This is a regrettable miscommunication. I bear more than the usual fraction of blame for the miscommunication, as I served not only as a discussant but also as co-editor of the volume. The harm to society is bounded by that associated with a professor first seeing criticism of his work in print, however. I will attempt to confine the personal controversy to footnotes in this essay so that (a) those interested in the substance can confine their attention to text and (b) thrill seekers can find the controversy quickly.

quantification of that contribution. Like several earlier analysts, he correctly attacks the problem as one of measuring the degree to which consumers treat a new variety of ready-to-eat breakfast cereals as a distinct good. A new variety's contribution will be larger as the single product demand curve is steeper, i.e., as consumers treat existing products as less than perfect substitutes for the new product. Quantifying the contribution involves measuring the single-product demand elasticities. Quantification is the tricky bit, taking us into the econometrics of differentiated product oligopoly.

Hausman found that the single-product demand curves for ready-to-eat breakfast cereals were quite steep. This led to a surprisingly large calculation of the welfare contribution of Apple Cinnamon Cheerios -- over sixty million dollars a year. Extrapolating to other new varieties, Hausman suggested that the CPI for cereals might be overstated by as much as 20 percent. Both of these figures would be a good bit higher if introducing Apple Cinnamon Cheerios had not led General Mills to raise prices on its other Cheerios products, as (still paraphrasing his results) systematically we should expect a multiproduct firm to raise prices on its other products after an introduction. These results are surprising enough that they seemed to call for a high standard of proof. Yet Hausman's main identifying assumption cannot survive restatement in plain English, and his use of imperfect competition theory is dicey. I was, and am, unconvinced.

Before an Apple-Cinnamon Cheerios war reminds everyone of Kissinger's remark about academic disputes, let me put it in economic context. Hausman's are potentially very important findings. We already have good reason to believe that new goods make a large contribution to economic welfare through the founding of whole new markets and industries, and through systematic improvements in product quality. Hausman's results suggest that new goods may make another important contribution through increasing variety in mature branded consumer products industries like cereals. That would imply that the failure of price indexes like the CPI to function as cost of living indexes is far more widespread than we thought. Economic growth in the rich, consumerist societies would be even more understated by official statistics than we thought. Findings like this are important enough and surprising enough to examine the methods behind them carefully. The methods used to determine the slope of the single-product demand curve and perform the counterfactuals are themselves quite general and important.

Product Differentiation and the Value of New Products

My first difference with Hausman, and the bulk of the discussion in his rejoinder, arises in connection with a critical and only partially solved problem in measurement method for product differentiated industries. To assess the value of a new good, or to measure monopoly power, we want to know the extent to which new product(s), or the product(s) of an alleged monopolist, etc., are treated by consumers as unique. That means that we need to measure the system of demand elasticities at the individual product level. Quantitative methods for differentiated products industries have been an active research area lately. (Most of this work looks at the role of product differentiation as a source of seller market power, not buyer value, but the issues are the same.) This has produced a body of distinct tools, not a single best method.

Table 1: Econometric Issues and Assumptions in Product Differentiated Industries

The analyst may:

I1. Be given observable cost shifters for each of the products in an industry. Rare and lucky.

I2. Have a few supply shifters from the cost function, but far fewer than the number of endogenous prices. More common. Calls for further assumptions.

I3. Restrict the demand system so that there are only a few free elasticities, perhaps by having price have the same coefficient in each demand equation. Very popular in the context of demand for expensive commodities such as cars, houses, and so on, where price of each good can be interpreted as a lump sum "income not available for other goods."

I4. Use individual consumer micro data, and permit the observable variation between consumers to trace out much of the demand curve for products. Great idea, but calls for data.

I5. Assume a specific model of product differentiated supply, such as Bertrand competition, and use the restrictions between supply and demand to identify the model.

I6. (Variant) Use supply instruments, like the number of products in a category or their "closeness" in product space, loosely derived from a specific model a la I5. This is a popular method

I7. Make a variance-components argument. This is Hausman's approach. He observes the price of the same good in a variety of cities, and puts restrictions on the statistical process driving prices.

I8. Assume that the error in the reduced form for price is uncorrelated with the demand error and estimate by OLS.

In industries with many product varieties, the demand for each product in principle depends on the prices of all products. (And on all their advertising and other marketing mix variables, though that is not the focus here.) So, this is econometric estimation with dozens of endogenous variables. Where the literature in general hits a rough patch is finding enough instruments for all the prices in the multiproduct demand system. Only very rarely will we see econometric identification of the most familiar sort, with as many instruments as there are prices. (That would call for an observable cost shifter for each product, which happens more in econometricians' dreams than in reality.) The open research question is: How steep is the demand curve for Apple Cinnamon Cheerios holding the price of Cheerios, Honey Nut Cheerios, and all other products fixed? So we really need to find an estimation strategy which leaves us convinced that we have seen the equivalent of a supply-side shock to each individual product.

This situation has produced a variety of econometric approaches. I outline them in Table 1. None of these are perfect, each involving assumptions. Of course, in any particular application and industry, these different approaches may be used in combination, buttressed by interviews with company officials, by readings of marketing documents, etc. Since are estimating demand, the key assumptions for identification will be about how supply works. Choosing from this menu is choosing the economic assumption about supply that underlies the conclusions about demand.

Econometric Identification in Hausman's Work

Now that we see that the problem of identifying an econometric model with dozens or hundreds of endogenous prices is general and important, let us turn to Hausman's application, my doubts about his identifying assumption, and his rejoinder.

Hausman's data cover the RTE cereals market in a panel of cities over time. He exploits the panel nature of the data to make a variance-components argument of type I7. While the technicalities of this argument matter for the efficiency of the estimators, non-specialists can understand it by recognizing that he estimates demand in Boston using prices in Chicago, Detroit, LA, and other cities as instruments for prices in Boston. He writes (cf. Eqn. (8) on p. 219) the reduced form equation for the price of product j in city n at time t as

$$\log p_{jnt} = \alpha_{jn} + \delta_j \log c_{jt} + w_{jnt},$$

Identification is to be found in the subscripts. The term α_{jn} captures anything that is city (j) and product (n) specific but not time varying (no t). Cost shocks for each product, log c_{jt}, are common across cities. The cost shock has no city subscript, i.e., product cost shocks enter prices in each city through a nationwide component. The error w_{jnt} contains both demand and cost shocks and is assumed to be independent across cities. There is an economic assumption in this restriction, in that the demand errors are restricted to be independent across cities (enter only through w_{jnt}) whereas cost errors can be common across cities (enter through log c_{jt}). In economic language, any demand shock that affects prices is drawn independently in each city rather than being a nationwide effect. It is this assumed asymmetry of the geographical structure of cost vs. demand errors that drives identification. Hausman explains this economic assumption by saying that c_{jt} is

Quote 1

"… the cost which is assumed not to have a city-specific time-shifting component (which is consistent with the national shipments and advertising of most differentiated products)"

Hausman did not further explain the economic argument behind his identifying assumption, except as reported in Quote 1. It is clear, however, that the price of product n in city j will be a good instrument for estimating the demand for product n in city k if most of the nationwide component of price shocks comes from unobserved movements in cost.

The question for any discussant in these circumstances is whether to believe the identifying assumption. I wasn't convinced, thinking that there might be unobserved shocks to demand which were common across cities, wondering what the unobserved shocks to cost might be, and wondering why Hausman thought advertising shocked cost rather than demand.

Before we go on, let me point out that there is no controversy about what this would mean. If the error in prices that is correlated across cities is largely cost, then the estimates are fine and Hausman is right that the value of the new variety he studies is high. If, on the other hand, I am right and the nationwide component of the error contains a substantial demand component, then the direction of the bias is clear. (See my argument on pp. 241--242.) Hausman has then overestimated the value of the new good.

My argument was simple, and consisted of a list of things I thought might cause the demand errors for a nationally branded consumer product to be correlated across cities.[4] Both Hausman and I agree that the most important issue is related to my point about advertising.[5] Let us focus on it here, since it is general to many consumer-product industries. I argued that consumers' response to advertising is a likely source of unobservable nationwide shocks to demand. For Hausman's assumption to be true, I wrote (p. 241), . . .

"… there can be no successful nationwide campaigns which shift the demand for individual brands or products. … Adding advertising stocks or flows to the demand system does not necessarily solve this problem. The issue of the econometrician not observing the success of competitive advertising campaigns remains."

In his rejoinder, Hausman counters that he has included in-store display regressors which proxy for advertising:

Quote 2

"I believe that he [me] fails to understand the econometric specification because both Tables 5.3 and 5.4 include display variables which typically capture period specific advertising campaigns by cereal manufacturers."

This is a non sequitur, not an answer. Hausman's response, that he has included a variable that might be a proxy for advertising, is simply not an answer given that the issue raised was errors in the demand system even when advertising is included. More generally, his response like his paper fails to address the obvious and important econometric issue. He is using the price in Chicago as an instrument for the price in Boston. Isn't it likely that there is <u>something</u> omitted from the demand system that causes correlation across cities? Or are the omitted nationwide demand shocks trivial when compared to the nationwide product cost shocks (whatever those are)? Given that these are nationally branded consumer products, it seems to me that Hausman

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[4] I also argued that such errors will be reflected in prices, another uncontroversial point. Imperfectly competitive supply "curves" slope up because of the marginal revenue term.

[5] The other points were "There can be no fads shifting demand temporarily to a particular product, or if there

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bears something more of a burden than to say that he has included a regressor that might be correlated with one possible criticism[6].

Let me revisit this question of whether Hausman's identifying assumption might be believable after examining some of the new evidence he brings to bear in his rejoinder.

New Evidence on the Demand Elasticities

Continuing to rebut the identification complaint, Hausman writes that he has estimated a new version of his model and…

Quote 3

"…included month specific indicator variables to capture potential shocks not captured by the time trends with no change in the main result of the slope of the demand curves."

Given that the data are weekly, Hausman must now be assuming that cost shocks affect prices in the one-week short run, but demand shocks only matter at monthly frequencies. He doesn't explain why he is prepared to assume this, still offering no economic explanation of his identification strategy. It is clear, however, that Hausman's agenda here is to include more regressors that might soak up the criticism, i.e., regressors that might capture unobserved shocks to demand.

Adding regressors to the demand equations will reduce the demand error, and adding the same regressors (like those time dummies) to the demand equations in all cities will reduce the extent to which there is a nationwide component in the demand error. So far, so good. The criticism, however, was that there might well be a shock to demand in the weekly nationwide component of prices. I don't see how this argument is particularly undercut by including the month dummies. For the criticism to be right, retailers in the various cities must tend to respond

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are fads, they must be geographically local" and "there can be no slow acceptance of new brands."

[6] Especially a criticism nobody made. The pattern of non sequitur responses is repeated throughout his

to unobserved demand shocks in the same week. What are the main ingredients? Well, national advertising campaigns hit consumers in all the different cities at the same time. Consumers take however long they take to change their shopping and brand-choice behavior. Retailers take however long they take to change their pricing behavior. As long as these processes have about the same delays in all the different cities, there will be demand errors in the nationwide price component in the weekly data. Hausman might someday bring an argument to the table that would plausibly show that this kind of demand variance component is small.[7] I don't yet see what that argument might be. These are, not to put to fine a point on it, nationally branded consumer goods.

More importantly, the form of his argument is distinctly odd. Adding monthly dummies removes not only the monthly part of the unobserved shocks to demand, but also the monthly part of the unobserved shocks to cost. What is missing is any -- any -- explanation of what shocks to cost might have weekly frequency yet be orthogonal to the month dummies. Hausman offers no discussion of the cost side at all, so we have no information about the economics underlying his identification strategy. I am sure that there is some story of the time-series behavior of consumers and producers in this industry that will make Hausman be right. The identification strategy only becomes convincing, however, when that economic argument is made explicit.

In this regard, Hausman's rejoinder parallels his original paper. Hausman has still not advanced any economic story of what the shocks to cost might be.[8] What production story puts cost shocks into the nationwide component of prices? How do they make cereals, anyway? Are they made in nationwide plants? Does each plant make only one product? How often do individual plants' production processes break down? (Cheerios have the look of something pretty hard to manufacture to me.) More generally, are the cost shocks big enough to make a plausible

rejoinder. So is the pattern of nonsense assertions that the discussant does not understand the issues.

[7] He should avoid all of the series of obviously false arguments one could try, e.g., pointing to the institutions by which retailers set prices each week, arguing that MC is flat in the relevant run, and so on, that he tried on p. 219 of the paper.

[8] This involves a curious reversal of the roles of author and discussant. Hausman, the author of this paper, is silent on the economic story behind his identifying assumption. I, the discussant, have advanced an economic story of what the shocks to demand might be. Since the only economic story on the table is that of the discussant, Hausman is

demand side theory look small? Do the cost shocks have the right kind of behavior over time? Etc.

I am sure that there is an industry in which Hausman's unverified assumptions are correct. There isn't much evidence that RTE cereals might be such an industry. Hausman has responded to a criticism that <u>demand</u> is not identified by adding variables to his <u>demand</u> equation. We need something in <u>supply</u>! The identifying strategy can only become convincing after there is an explicit economic statement of why the supply / cost story might be true.

Outside Evidence on the Demand Elasticities (and the Instruments)

Hausman cites the excellent work of Aviv Nevo[9] on the demand and imperfectly competitive supply of cereals as filling in the gaps in his argument. Nevo should sue for libel.[10]

In contrast to Hausman's approach, Nevo attempts to systematically examine the amount of information in the data about the demand hypotheses.

One Nevo innovation is to include three instruments that might plausibly proxy for cost at the city level,

> "regional dummies, which pick up transportation costs; city density
> (defined as the number of people per square mile), which is a proxy for the
> difference in the cost of space; and average city earning[s] in the supermarket
> sector computed from the CPS Monthly Earnings Files." (p. 44)

These instruments, unfortunately, do not vary across products. With about fifty products in the industry, even three widely varying instruments would not satisfy the rank conditions for identification. So, instead of estimating an unrestricted model of cereal demand with these instruments, Nevo estimates an extremely restricted one, a logit with the same coefficient on price in the indirect utility function for each brand of cereal. The model is identified with only three instruments because, far from having approximately fifty free coefficients on price in each of fifty

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content to add regressors to the demand curve that might soak up the offending correlations. Nice work if you can get it.

[9] His May, 1997, Ph.D. thesis at Harvard: <u>Demand for Ready-to-Eat Cereal and its Implications for Price Competition, Merger Analysis, and Valuation of New Brands</u>.

[10] He will have to get the court to distinguish between the innocent "I agree with Nevo" and the libelous

equations of the demand system, it has <u>one</u>. Thus Nevo combines identifying principle I3, highly restricted price coefficients, with the available plausible cost instruments, I2. Nevo is careful to warn us that this model is "inadequate for measuring market power" but is "a useful tool in getting a feel for the data." This is sensible given the highly restrictive demand system.

Nevo compares these estimates to some that are related to Hausman's. Nevo uses a variance-component argument of type I7, using prices of the product at other cities in the same region as the same instruments. These instruments are related to but different from Hausman's instruments, as now the identifying assumption is that regional shocks to cost (not national ones) drive prices. These instruments are more immune than Hausman's to the criticism that there are nationwide shocks to the demand curve. Nevo estimates with both the regional price and city cost estimates and finds two things: first, as Hausman reports, the estimated price elasticities under the two methods are similar. Second

> "The tests of over-identification are all rejected, suggesting the identifying
> restrictions are not valid." (p. 62).

This is a long way from being an endorsement of Hausman's instrumenting strategy.

As part of the same investigation, Nevo also examines the usefulness of "degree of competition" instruments of an I5 form. This approach, which has been highly successful in other industries, fails utterly here for the obvious reason that there is little variation in the instrument along the dimensions that might predict prices.

Nevo examines his own estimates and Hausman's to see what is going on in the cross-price elasticities as well as the own price elasticities. He is quite concerned that models, specifically including Hausman's, routinely find different cereal products to be complements not substitutes. Nevo's diagnosis is on p. 95: "This is consistent with the instrumental variables not being exogenous."

The other Nevo results adduced by Hausman change the functional form of the demand system. Nevo uses a random-coefficients model to avoid having to make assumptions about the way in which the cereal industry is divided into segments. This model is distinct from Hausman's

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"Nevo agrees with me," but that should not be too difficult.

in the way the elasticities of substitution are modeled. It is not, however, distinct in the identifying principle. Nevo still uses a variance-components argument of type I7. He has to, there are many more endogenous prices than there are cost shifters.

When the dust settles, and this is what draws Hausman to bring Nevo into this argument, Nevo's preferred results have fairly steep demand curves for individual cereal products. Yet the guarded tone of Nevo's discussion of the econometric support for these findings stands in stark contrast to the blithe tone of Hausman's paper and the rejoinder. And these results are not, it turns out, identified by a different principle than that advanced by Hausman.

Nevo provides two things that Hausman might have found useful. (1) Very guarded support for Hausman's substantive results. (2) Plenty not to like about Hausman's methods. Since my gripe with Hausman was over whether he had proved his case, I am happy to embrace Nevo's analysis. There are plenty of other reasons to like it, too, notably the care and craft of the econometric work and the attempt to be careful about the degree of support for the main conclusions in the data.

New Arguments

Hausman brings a new economic argument to bear in his rejoinder, based on the high price-cost margins (in the sense of P-MC) seen in many consumer products industries. If Hausman had actually provided an examination of profit data at the product level it would be very welcome. An examination of supply as well as demand behavior would increase the amount of evidence on the table. But he does not do this. Instead, he offers the general argument that new products are very valuable to consumers in product differentiated industries with high price-cost margins. The argument is quite simple. High price-cost margins are evidence of product uniqueness, as firms will only be able to sustain high price-cost margins if there are steep single-product demand curves. Steep single-product demand curves mean, also, a large increase in consumer surplus from new product introduction.

Quote 4

> ". .. . we know that the price elasticity is not extremely high for
> most differentiated products given the well known high values of price to
> marginal cost ratios found in differentiated product markets. Thus, in most
> situations, new differentiated products which sell significant quantities will
> lead to a significant increase in consumer welfare."

Should we take the high price-cost margins in that industry as evidence that consumers gain substantially from new products? In general there are two different theories of high price-cost margins in a product-differentiated industry, steep single-product demand curves and explicitly or tacitly collusive price setting by sellers. (With multiproduct firms, it is the single-firm rather than the single-product elasticity that matters.) Hausman's logic about the relationship between price cost margins and elasticities breaks down because of this distinction. Is this a purely theoretical argument? The long literature on ready to eat breakfast cereals, never cited by Hausman, has arguments for both classes of theories of its high prices. Much of this literature was inspired by the FTC case against the industry, in which the Commission argued that the industry was characterized by tacit collusion. Now, only the steep single-product demand curves theory of the high price works for Hausman. If $P>>>>MC$ because the firms do not act very competitively in pricing, then there is no evidence in the profits data for Hausman's argument. Of course, Hausman might have argued that the scholars who think that the high cereal P-MC come from steep single product demand are right, and that the scholars and antitrust enforcers who think that the industry is partially collusive are wrong, but he does not make this argument.

History has handed Hausman a piece of bad luck. After he wrote his original paper, but before he wrote his rejoinder to my comment, cereal industry trade sources reported a large price war. Prices and price cost margins fell dramatically. This would lead any sensible person to put more weight on the possibility of a tacitly collusive theory as against the steep single firm demand

curve theory. Of course, Hausman might rebut this evidence[11] but he doesn't even allude to it.

Even absent collusion, the profit argument cannot succeed in general. The theory of product differentiated industries tells us that there is not necessarily any direct alignment of sellers' incentives to add products and buyers' gains from those products. This has been one of the most successful applications of formal economic theory in recent times. Let me simply remind you about a few of the most important arguments. Sellers don't necessarily internalize the value of variety correctly -- it depends not only on the elasticities but on the specifics of the pricing game. Sellers' incentives to change the demand system by advertising drive a wedge between their incentives and social welfare. Further, conclusions about sellers' incentives to introduce new products and welfare depend on the nature of strategic competition in entry -- and on all the elasticities.

On the price cost margins evidence Hausman writes

Quote 5

"Indeed, Prof. Bresnahan's main contention that successful new goods in differentiated product markets may not create significant consumer welfare is easily seen to be incorrect, given an understanding of basic economics and the data."

How lightly Hausman discards the contribution of his own paper in the course of making this argument! For the Hausman of the rejoinder, we don't need econometrics to learn about the demand elasticities in product differentiated industries. We can examine the accounting profits data and learn about the value of new products that way. The Structure-Conduct-Performance paradigm lives, revived by Hausman. If he were right, this would save a lot of research work. As with any other strategy that saves research work, it involves making unverified assumptions, ones that might be contradicted by other kinds of evidence, as they are by the price war, the FTC's

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[11] Some of the stories I read in my brief time in the trade press of what is, after all, an industry I don't follow might be interpreted to mean a change in the form of pricing (the role of coupons) rather than a price war.

collusion case, etc., in this industry.

I'm happy to be associated with the contention that "successful new goods in differentiated product markets *may* not create significant consumer welfare." He should be, too. The lack of general theoretical results linking sellers' incentives and buyers' valuation of new goods is the main reason to like Hausman's paper and others like it. The paper attempts to illuminate buyers' payoffs and seller's motives by measuring some of the important determinants of the social and private return to a new good. It is a mistake for Hausman to discard econometric analysis in favor of a broad-brush argument that sellers' profitability and success is all you need to know.

Imperfect Competition in PD Industries

Introduction of a new good into a product-differentiated industry will typically change the prices of other products, sometimes with non-trivial welfare consequences. To assess these consequences calls for a counterfactual calculation in which (imperfectly competitive) supply changes along with demand. Hausman focussed a good bit of attention on this, even mentioning it in his title.

I complained about an important issue and an unimportant one. The important issue concerns correct interpretation of imperfectly competitive supply models such as Bertrand for purposes of counterfactual analysis. Hausman's words and formulae showed he only calculated supply changes for the other products of the same firm that introduced AC Cheerios. Since these changes led to higher prices for those products, Hausman reports that the effect of imperfect competition is to partially offset the positive welfare contribution of new good.[12]

Hausman's explanation of the supply counterfactual botched the definition of Bertrand competition:

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[12] He also implied the other prices of the product introducing firm should rise systematically, which is obviously false. His rejoinder weakens this to the claim that they may rise, which is obviously true, and to the claim that this is novel to him, which is obviously false. Hausman still offers no explanation of why they do rise in his estimates (e.g., did his demand assumptions or his demand estimates drive this?) This whole issue is unimportant except for his odd claim of having made a contribution with these calculations.

Quote 6

"[I] calculate the markups over marginal cost that are profit maximizing for General Mills under the Nash-Bertrand assumption that other firms, such as Kellogg, will not change their prices in response to the introduction of Apple-Cinnamon Cheerios."

In my comment I pointed out that Hausman is simply wrong, technically, in what he understands under Bertrand competition in Quote 6. In Bertrand competition, the <u>firm</u> assumes the other firms' prices stay constant, but the <u>economist</u> does not. In equilibrium, the other firms' prices change. This is not a purely semantic objection, for it raises an important conceptual point. Hausman's treatment leaves out a very important part of what new products do in imperfectly competitive industries, i.e., their impact on <u>competitors'</u> pricing. (My somewhat long discussion of this may have been tedious, with a little chart, etc.) To this conceptual point Hausman rejoins:

Quote 7

"To compute the effect, I used a first order linear approximation, equation (15), since the overall effect is likely to be small. Prof. Bresnahan does not appear to understand that a first order linear approximation which is the first step of the Gauss-Seidel method for solving a system of nonlinear equations will give quite accurate results for small changes. … Here I have recomputed the results …under the assumed Nash-Bertrand behavior of my paper while allowing the elasticities to change when moving to the new equilibrium."

This is another non sequitur.[13] True, the new results presented in the rejoinder are very close to the old results. But Hausman does not respond at all to the conceptual query. (Readers

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[13] To my conceptual query, Hausman offers no response. To the non sequitur, he adds a baseless remark that I don't understand the technicalities. This is perfectly parallel to the econometric part of his rejoinder. The only charitable explanation is that Hausman has responded to my comment without reading it, presumably out of anger over not seeing my comment prior to book publication. In this section of his rejoinder, he remarks again that he did not see my comment and offers that he could have set me straight on the technicalities if only he had seen my comments. That would have

who think he has responded will please provide a translation of "the assumed Nash-Bertrand behavior of my paper" in Quote 7.)   It is possible, of course, that Hausman actually made a correct calculation and that only the words and symbols of his original paper were simply sloppy. I discard this unlikely hypothesis on the evidence from his rejoinder. Hausman didn't say that I had read him incorrectly as to the conceptual basis of the calculation.

This is not just a technical or semantic difference. Competitor's product introductions tend to erode market power, not to increase it. This point is one of the most important ones in the analysis of product differentiated industries. Surely the competitive impact of a new product, not its impact on the prices of other products offered by the multiproduct firm, is the important point in "valuation of new goods under imperfect competition." Surely we think a new product's effect will more typically be to lower, not raise, the prices of pre-existing substitutes.

How Economists Argue

Hausman's argument in his paper and in his rejoinder serves to illustrate three important failings in the way economists argue today. They are,

- Structuralism: Technique Replacing Thinking
- Factism: The Absence of Technique Replacing Thinking
- Burden of Proof Games.

Structuralism: Technique Replacing Thinking

A new way for empirical economists to argue is growing very popular, so popular that it deserves to be called a movement and labeled, perhaps as "structuralism." Structuralists' rigorous and probing linkage of econometric method and economic theory is useful on many problems. Yet structuralism has a very strong element of research taste, so strong that structuralists can sometimes be blind to the weaknesses of their approach.

There is a great distinction between structural econometric methods and the emerging methodological religion of structuralism. Structural methods involve the linkage of econometric

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worked only if (a) I were technically wrong and (b) he read my comment. I'm not and he hasn't.

techniques to economic assumptions and economic parameters in empirical work. These methods are very useful. Structuralist econometricians, however, sometimes forget that methodologies, methods, and techniques are not the output of the economics profession, but an often-useful intermediate input. Mixed with a heady dose of religious fervor, this leads to two systematic mistakes. Hausman makes both.

(1) Structuralists confuse precision with correctness. A structural econometric model states all of its assumptions precisely, but this is no guarantee that the assumptions are correct. Precision about assumptions can help us move forward to correctness in the long run, but in any particular paper the most precise methods may be deployed to state an incorrect assumption.

Hausman states the identifying assumption behind his variance-components estimating method with complete precision. The statement includes, for example, telling us how the unobserved nationwide shocks enter the system -- through cost not demand. Yet use of the Hausman-Taylor methods is not a technical cure-all. A variance-components identification argument frees the analyst from the confines of cost-driver observability, but not from cost-driver existence. The argument will be correct if there are some underlying nationwide shocks to product cost that can not be measured easily and no nationwide had shocks get in trouble when a structuralist author makes no effort to convince the reader that the unverified assumption is correct. Hausman does not attempt, in either his paper or rejoinder, to convince us that he is correct about the nationwide cost shocks.

(2) A second systematic structuralist error is the belief that all arguments are technical. This belief is privately useful to structuralists but socially costly to the advancement of economics. The belief lets structuralist econometricians focus on the comfortable certainties of technique instead of the scary issue of whether the technique is useful in the application. The belief also creates an in-group of technically fluent scholars who dismiss criticisms from the out-group. Thus Hausman's rejoinder is full of remarks that I don't understand the econometric technicalities, don't understand the relevant economic theory, don't understand what he did precisely. It is a

pure in-group power play.[14] It slows the advancement of science by deflecting attention from a real issue -- what economic assumptions should underlie our estimation of these important models -- with technical noise.

Many scholars with strong research tastes for econometric method disagree whole focus of this part of this essay. They think that arguments over identification are a wasted effort. We should focus research attention on getting the technique right, this argument continues. "Attacking his identification strategy is like attacking a man's religion" one scholar told me.[15] I could not disagree more. One purpose of high quality econometric method is to permit discussion of untested assumptions in economic terms. Transparency is an important feature of econometric method. If an empirical paper uses method well, we can understand all of what has been assumed and what has been proved in economic terms, not econometric ones.

Factism: The Absence of Technique Replacing Thinking

The other contemporary methodological religious movement is factism. We almost certainly need more facts in Economics, so I want to offer every encouragement to careful documentation and measurement, even without theory. Factist scholars, however, present facts alone as if they resolved economic questions. This usually involves incomplete or informal economic arguments.

Hausman's use of the profit data in his rejoinder is the purest factism. His economic interpretation of the fact that RTE cereals firms are profitable involves a number of logical leaps, mostly dubious and all unstated. In general, factist sloppiness introduces a dangerous gap between what scholarship actually accomplishes and its claims. It does in the present instance as

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[14] And the in group power play is literally hilarious. Let me remind readers that I took up the problem of structural estimation of product differentiated oligopoly models over a decade before Hausman (or almost anybody else) and that I invented a good deal of the relevant analysis and many of the identifying assumptions in Table 1. As to religious affiliation, I am a thoroughly lapsed structuralist. But that doesn't mean I have forgotten the technique, only that I refuse to make the ritual bows.

[15] This remark may be penetrating. The strong *ad hominem* element of Hausman's rejoinder has the flavor of religious war. He writes that my points my points "do not have any importance in reality" and are "easily seen to be incorrect", that "[he, me] fails to understand the econometric specification" and "[he, me] does not appear to understand. …", and that my contention is "incorrect, given an understanding of basic economics and the data." Using language like that before reading the other side's argument betrays an author.

well.

Most scholars have either factist or structuralist research tastes, not both. It is a curious tribute to the power of Hausman's mind that he can make both factist and structuralist errors in the same rejoinder.

Perhaps the explanation lies not in the opposition between structuralism and factism but in their commonalties. Both involve carelessness about economic theory. Structuralism takes formulae, but not always ideas, from economic theory. Structuralists write down a model with all of its economic assumptions explicit. The models typically are complete and precise in the way they implement a narrow range of economic theories. Analysis is "conditional" on the narrow range. The use of the methods does not guarantee that the most interesting or important elements of the wide range of theories one might apply are on the table. In structuralists' defense, I would say that the problem of using careful methods with very explicit theoretical roots is that they, paradoxically, make it very expensive for the analyst to consider a wide range of theories.

Where the structuralist may miss theoretical points by being too careful, the factist may miss them by being too sloppy. Factists typically offer half-baked theoretical arguments, often by not offering any explicit theory at all. Factists and structuralists reinforce one another's worst tendencies. Factists grow ever more cynical about all precision and formalization because of the way that structuralists argue. Structuralists grow cynical about the very possibility of economic conclusions or the utility of facts. We could use less religious fervor and more reasoned judgment.

Burden of Proof Shifting

A final, particularly alarming trend in applied economics is the use of rhetorical burden of proof shifting stategics. The trick here is to move attention from one's own argument to the other side's. Since economists, especially applied microeconomists, are hypercritical about evidence, this strategy can be very effective in argument.

Hausman carries the burden shifting game to extraordinary lengths. He undertakes a remarkable reversal of the role of discussant and author. He offers no economic story of why his regression is identified by his techniques. He is content to snipe at my, the discussant's, economic

story of what is going on.

There is a lost opportunity in this. If Hausman were to offer an economic explanation of his identifying assumption, this exchange might be more useful. Certainly none of the classes of identifying assumptions in Table 1 is a cure-all for product differentiation industry estimation. It would advance our understanding of important issues if we had a broader toolkit. Perhaps there are variance-components assumptions about costs and demand shocks over time that would work in particular industries.

There is an important methodological principle that we should put criticisms of papers on the same evidentiary basis as papers. The discussant cannot simply say, "I think P is endogenous in the demand equation", or "I think you have the theory wrong." Discussants must say why they think this and what evidence would show that their interpretation is correct. I think I have met that standard in my comment and here.

Should we go along with Hausman and assume that price in Chicago is a good instrument when estimating demand in Boston? His own (useful!) variance components framework tells us the issue is whether the nationwide component of the price error is influenced by demand factors. It seems to me very unlikely that you could ever control out all the nationwide demand error by adding regressors, his strategy. Consumer responses to consumer product marketing are notoriously noisy. Advertising agencies' ability to turn any given budget into a specific amount of consumer response is notoriously imprecise and uncertain. Both the timing and the size of responses to ad campaigns are hard to predict. In general, even conditioning on the advertising budget, or messages sent, or other measures, there will still be serially complicated, large, geographically correlated errors in the demand curves for consumer products.

We may have to be realistic about consumer products industries with a large number of similar products. Aggregate data like Hausman's may simply not contain the amount of information needed to tie down the large number of unknown elasticities. There may simply not be enough information in the data to convince reasonable skeptics.

New Goods in Modern Economies

Why, beside the entertainment value, should anyone care about this exchange? We are in

the middle of an extensive debate about measured productivity, price index measurement, and a set of related questions about how rapidly the rich countries are getting richer. Hausman and I are both on the same side (tough luck, entertainment-seeking readers.) We both think that conventional productivity measures miss substantial increases in output associated with new goods and other quality change. Where we differ is on methodology and on where to look for substantial new-goods welfare contributions.

The substantive difference concerns the importance of incremental product introduction in mature consumer products industries, like ready to eat breakfast cereals. Hausman thinks their cumulative impact on welfare is large. I conjecture that we need to look elsewhere for big social welfare gains to new goods.[16]

And I think we will find it. When will a new good be part of an economic process generating substantial social welfare? The short answer is when it is the outcome, or one of the outcomes, of an important downward shift in the LR supply curve. There are a number of areas of the contemporary economy in which very substantial economic change, driven by technological progress, has shown up as improvements in product quality or as new goods. I would include new drugs, medical devices, and procedures; new services introduced in the finance and other sectors based on information technology; and the new organization of retail trade as important cases. There are many more. If we look to the newer and more dynamic parts of the economy, we will find plenty of change-inducing new goods. The issue is to do careful valuation measurement -- not trivial in the areas of contemporary economic change. I conjecture we will also find that existing government statistics are particularly bad welfare measures for the newer and more dynamic industries.

If Hausman and I are on the same side, why am I taking such care to be his critic? First, I think the methods issues are very important. That is what the bulk of this essay has been about. There is another consideration.

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[16] Not all of my reasons for doubt about the mature categories are related to my specific criticisms of Hausman. See Bresnahan and Gordon ("Introduction" to the New Goods volume) for a discussion of the broader issues in valuing new goods.

The discussion of new goods and quality change has been politicized. There is widespread view, on the left, that the point of the Boskin Commission report on the CPI was to slow transfers to traditional left-wing constituencies. Our critics are once again positioning economists as apologists for capital. The charge is false, and the view that there is substantial unmeasured quality change and uncounted value from new goods is fundamentally technocratic in origin. Yet our findings about much more important classes of new goods are opened to mockery -- economists think that Apple Cinnamon Cheerios is the greatest thing since sliced bread. The situation reminds me of that following Michael Boskin's famous remark that the government shouldn't decide between investments in computer chips and potato chips. Protectionist interest groups pounced on the remark, misquoting it systematically. Twisted into "investments in computer chips and potato chips are equally good for society" the remark served two political purposes. It painted the center-right Bush Administration as the hard-right "Reagan-Bush Administration." It positioned economic analysis as the handmaiden of ideological extremism. These critics of Economics should have waited for Hausman. We never thought Ruffles with Ridges was comparable to the microprocessor. I have never met an economist who strayed that far from reality by ideology -- only by arrogance.