3-1987

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The Validity of Studies with Line of Business Data: Comment

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In the March 1985 issue of this *Review*, George Benston found fault with Federal Trade Commission Line of Business (*LB*) data generally and singled out for extended criticism thirteen *LB* data-based papers written by the authors of this comment. Even by the pre-Queensberry rules governing economic disputation, Benston's article is one-sided and negative. Moreover, it is marred by numerous errors in characterizing our work. We wish to set the record straight.

I. The Line of Business Research Program

The data on which Benston focuses are financial performance statistics collected by the FTC for the years 1974–77 from some 437 to 471 U.S. corporations. The data are disaggregated to "lines of business" defined at a level of manufacturing industry detail ranging between the 3- and 4-digit divisions of the Standard Industrial Classification.

Benston's main theme is his doubt "whether meaningful empirical work on the relationships between [market] structure and performance is feasible" (p. 64). However, most of his critique could pertain to any systematic use of accounting data, and the studies he explicitly attacks sweep a much wider range of economic phenomena. The data have been used to analyze research and development (*R&D*)—productivity links, transfer pricing by multinational enterprises, business diversification strategies, the profitability of mergers and sell-offs, the relationship between profitability and stock market risk, how federal *R&D* contracts affect private *R&D* expenditures, and much else. As of February 1986, research with the disaggregated data had yielded some 63 papers, nearly half of which were published or accepted for publication in refereed journals and compendia.[1] In addition, an unknown but substantial number of papers have used the published *LB* industry aggregates.

Benston correctly observes that one motivation for the Line of Business program was the perceived inadequacy of existing data for ascertaining how market structure, and, in particular, seller concentration, affected profitability (taken as an index of industry performance). However, he fails to recognize the impact *LB* data analyses have had in modifying views on such structure-performance relationships. At the time the *LB* program was initiated, the state of knowledge was characterized in an exchange between Harold Demsetz (1974) and Weiss (1974). Weiss pointed to the large number of empirical studies demonstrating that profitability rose systematically with seller concentration, while Demsetz questioned whether the chain of causation reflected the ability of the larger firms in concentrated markets to maintain elevated prices, or their greater success in reducing unit costs or securing other advantages. The contending schools of thought were deadlocked; only with better data could the impasse be resolved. Subsequent simulations (Ravenscraft, 1984) confirmed Demsetz' intuition that when leading sellers have unit cost advantages over smaller firms, regression analyses at the industry level could, because

*Scherer: Swarthmore College; Long, Pascoe, and Ravenscraft, Federal Trade Commission Bureau of Economics; Martin, Michigan State University; Mueller, University of Maryland; Scott, Dartmouth College; Weiss, University of Wisconsin. All have been affiliated with the FTC Line of Business program as staff economists or consultants. A review by FTC staff has determined that individual company Line of Business data are not disclosed in this comment. The views here are our own and not necessarily those of the FTC.

[1]Some of the papers referred to by Benston, and others cited here, are not published. All that are listed as FTC working papers can be obtained upon request from the FTC Line of Business office.

of aggregation biases, yield positive profit-concentration coefficients even when no price-raising effect was present. To avoid the biases, it is necessary to analyze performance at the individual line of business level and to include market share as well as industry concentration variables.

The research with *LB* data has gone forward.[2] It has shown that individual market share effects are indeed much more powerful than the traditionally emphasized concentration effects in explaining profitability. With most specifications, concentration coefficients turn out to be *negative*, not positive, in conjunction with market share variables. The positive and significant market share relationships alone cannot discriminate between monopoly power and efficiency or cost advantage hypotheses. However, Ravenscraft (1983) has shown that the market share measures interact with capital intensity, implying scale economies, and advertising, which reflects product differentiation effects.

A more recent analysis of *LB* data by John Kwoka and Ravenscraft (1986) reveals that the profit advantage of larger sellers depends upon structural variables which in turn influence how the market leader chooses to exploit its strategic advantages. Analyses by Bradley Gale and Ben Branch (1982) of non-FTC data disaggregated to the line of business level show the market share-profit relationships to follow from a mixture of cost, first mover, and subjectively determined quality advantages enjoyed by larger sellers. Work by Scott (1982) and Scott and Pascoe (1986) has suggested that industrywide seller concentration operates in more subtle ways dependent inter alia upon relative bargaining position and capital intensity.

II. Accounting Biases?

Although Benston does not acknowledge how much economists' understanding of structure-profit relationships has changed, he is aware that strongly positive market share effects have been a prominent feature of the new analyses. He attempts to dismiss them (pp. 40–41 and 56–57) as the possible consequence of accounting biases or other anomalies correlated with market share. To the extent that evidence is cited, it is done selectively. Thus (at pp. 39–40) he cites empirical studies showing systematic relationships between the choice of profit-altering accounting methods and firm size, company control form, and the presence of debt covenants. He fails to note that one of his cited studies (Robert Hagerman and Mark Zmijewski, 1979) found that firms in more concentrated markets tended to choose accounting policies that *reduced* their reported profits[3]—the opposite of what one would expect if the positive coefficients estimated for concentration (earlier) and market share (more recently) stemmed from accounting bias. And to the extent that firm size is related to market share (which need not occur in a multi-industry company), the observed tendency for larger corporations to prefer profit-reducing accounting policies must weaken, not strengthen, the relationships obtained in structure-profitability studies.

Benston's main methodology, however, is worst-case analysis. After reciting a litany of well-known problems that can infiltrate accounting data, he assumes without evidence that if anything can go wrong, it will. He thus ignores a corollary to Murphy's Law that might be called Leamer's Lemma (1985): If something can go wrong, one has an obligation to perform sensitivity tests to see whether it actually has, and if so, what the range of effects is. Indeed, for exploring the impact of accounting convention choices on structural relationships, few vehicles are more suitable than the *LB* data base, which contains explicit information on the amount and method of common cost and asset allocations, transfer pricing methods and magnitudes, inventory accounting methods, depreciation conventions, depreciable asset ages, merger accounting methods,[4] and much else.

[2] To be sure, other recent research using more aggregated data has helped clarify the debate over structure-performance relationships. For a wider-ranging survey, see Ravenscraft (1984).

[3] See also Hagerman and Zmijewski (1981).

[4] The information on merger accounting methods was not in the original *LB* data base, but was linked to it from outside sources, contradicting Benston's asser-

A. *Profit-Structure Sensitivity Tests*

The richest set of sensitivity tests thus far has been carried out in the context of the profit-market structure models Benston criticizes. Table 1 provides a summary, focusing on the coefficients estimated for market share and (in a more limited set of cases) four-firm seller concentration ratios across 15 studies varying widely in profit variable definition, sample year, sample composition, model structure, and control for accounting method choices.[5] It excludes models like Martin's (1983), in which the market share variable is endogenous, but continues to be significantly positive, or those in which the structure variables interact with other variables, yielding quite different interpretations of profit function partial derivatives. It encompasses, however, a diversity of approaches to the extreme value problems encountered in using *LB* data—for example, when profit ratios in the minus 100 percent range occur because of new business startups, accidents, or impending exit.

The first four equations report the market share and concentration coefficient changes for different years using Ravenscraft's (1983) basic operating income/sales regression with the most inclusive *LB* sample manufacturing line coverage. Earlier research at the industry level showed seller concentration coefficients to vary with the business cycle, being most strongly positive in the late 1950's and early 1960's and weakest in the inflationary 1970's. See Weiss (pp. 200–03, 221). Mild business cycle effects are evident here too. The smallest market share effect is for 1974, when price controls gave way to soaring inflation and then the start of a recession. The largest effect is for recovery year 1977. In every year, the market share coefficients show that profitability doubled or even trebled with increases in market share over the range of observed values. Four-firm seller concentration coefficients remain negative, small in comparison to analogously scaled market share effects, and hovering near statistical insignificance.

Equations (5) and (6) compare essentially similar regressions, except that in equation (6), Ravenscraft (1981) adjusted all inter-*LB* transfers made at nonmarket prices to a basis consistent with those determined for *LB*s in the same industry transferring at market prices. Glejser heteroskedasticity corrections were also implemented. The market share and concentration coefficients do not change appreciably. Benston acknowledged this result (p. 49), but forgets it in arguing that the strongly positive market share coefficients obtained by Ravenscraft in an analogue of Table 1's equation (2) might have resulted because "possibly transfers to those units were made at less than market prices" (p. 57).

Equation (7), which has not been reported previously, shows how the Ravenscraft results for 1975 (equation (2)) change when common or "nontraceable" costs are reallocated from the corporate pool on the basis of "market" rates—that is, reflecting the cost burdens of otherwise comparable lines with *no* nontraceable cost allocations. The market share coefficient drops by a third but remains strong and significant; the con-

tion "that firm-specific information from other sources cannot be brought in" (p. 51). Many other external data linkages—for example, involving patents received, input-output statistics, stock prices, merger activity, company internal organization and management turnover, etc.—have been accomplished.

[5] The implications of our Table 1 are strikingly different from those of Benston's Table 3 (p. 57), in which he reports estimates for some coefficients, including those for market structure. Most of the large differences shown in his table (whose year headings are erroneously reversed) stem from Benston's failure correctly to adjust variable scalings—for example, when market shares or concentration indices were measured as ratios by one author and as percentages by another. The sign on the Weiss-Pascoe market share coefficient is incorrectly reported; when this error is corrected, all signs are identical. Many of the variables in the table are not in fact identically defined, contrary to the table's implication. For example, Ravenscraft's asset/sales variables were adjusted for capacity utilization, whereas Martin's were not. Other coefficients differ because of the inclusion or exclusion of additional terms interacting with them (which alters their economic interpretation) and collinear variables (such as a cost disadvantage ratio, coupled with the minimum efficient scale variable by Martin, but not by Ravenscraft). Benston incorrectly identifies the source of Martin's estimates; the paper he cites presents a five-equation simultaneous model; the coefficients Benston reproduces are from an earlier four-equation model.

TABLE 1—EFFECTS OF YEAR DIFFERENCES AND ACCOUNTING METHODS ON STRUCTURE-PERFORMANCE RESULTS

Equations	Special Emphasis	No. of Obs.[a]	Year	Dependent Variable[b]	Market Share	Concentration	Other Variables	R^2
(1) Ravenscraft (1983)[c]		3030[d]	1974	*OI/S* [.073]	.1793 (4.71) [.038]	−.0370 (−2.22) [.387]	19 other *LB*, firm and industry variables	.223
(2) Ravenscraft (1983)		3186	1975	*OI/S* [.065]	.1833 (4.90) [.037]	−.0218 (−1.34) [.387]	21 other *LB*, firm and industry variables	.208
(3) Ravenscraft (1983)[c]		3185	1976	*OI/S* [.071]	.2162 (5.13) [.036]	−.0350 (−1.94) [.387]	21 other *LB*, firm and industry variables	.160
(4) New regression for this comment		2955[e]	1977	*OI/S* [.078]	.2335 (7.60) [.037]	−.0343 (−2.60) [.391]	15 other *LB*, firm and industry variables	.067
(5) Ravenscraft (1983)	Heteroskedasticity correction	3186	1975	*OI/S* [.065]	.1476 (5.51) [.037]	−.0222 (−1.77) [.387]	21 other *LB*, firm and industry variables	.128
(6) Ravenscraft (1981)	All transfers at market prices, heteroskedasticity correction	3004[d]	1975	*OI/S* [.066]	.1432 (5.33) [.038]	−.0195 (−1.54) [.388]	29 other *LB*, firm, and ind. variables, including transfer method controls	.154
(7) New regression for this comment	All common costs allocated at market rates	3186	1975	*OI/S* [.068]	.1234 (3.18) [.037]	−.0240 (−1.40) [.387]	21 other *LB*, firm and industry variables	.150
(8) Long and Ravenscraft (1984)[c]	Depreciation charges not subtracted from numerator	3014[d]	1975	*OI + Dep/S* [.092]	.2000 (5.39) [.038]	−.0220 (−1.34) [.390]	21 other *LB*, firm and industry variables	.168
(9) Benvignati (1986)	Estimated capital costs subtracted from numerator	2635[f]	1975	*OI-KCst/S* [.018]	.2620 (5.95) [.037]	–[g]	16 other *LB*, firm and industry variables	.282
(10) Long and Ravenscraft (1984)[c]	Assets instead of sales used in denominator	3014[d]	1975	*OI/A* [.092]	.1720 (2.73) [.038]	.0015 (0.05) [.390]	21 other *LB*, firm and industry variables	.134
(11) Schmalensee (1985)	Relative effects of firm, industry, and share	1775[h]	1975	*OI/A* [.137]	.2359 –[i] [.061]	–[j]	455 firm effect and 241 industry industry effect dummy variables	.496
(12) Ravenscraft and Scherer (1986a)	Merger analysis	2955[e]	1977	*OI/A* [.139]	.3925 (6.34) [.037]	–[j]	257 industry effect dummy and 4 merger variables	.155
(13) Ravenscraft and Scherer (1986a)	Merger analysis, effects of accounting methods	2955[e]	1977	*OI/A* [.139]	.3691 (5.91) [.037]	–[j]	257 industry effect dummy and 4 merger and 2 accounting variables	.159
(14) Marshall (1986)	Comparison of PIMS and *LB*: *LB* data used	2450[k]	1974–77	*TrOI/TrA* [.190]	.2709 (5.14) [.041]	.0127 (0.63) [.390]	7 other *LB* and industry variables	.131
(15) Marshall (1986)	Comparison of PIMS and *LB*: PIMS data used	837[k]	1974–77	*TrOI/TrA* [.192]	.3249 (9.82) [.246]	−.0185 (−0.73) [.559]	7 other *LB* and industry variables	.263

Sources: For all equations except (4) and (7), see the References. For equation (4) see Ravenscraft and Scherer (1986a). For equation (7), see Ravenscraft (1983) and Long et. al. (1982).

Notes: Means are in square brackets [ ], and t-values are shown in parentheses ( ).

[a]For an explanation for minor differences in number of observations among studies, see individual studies. For major differences, see fnn. d, e, f, and h below.

[b]Variables used to describe profitability measures: *OI*-Operating Income; *TrOI-OI*, traceable, i.e., only traceable operating costs are subtracted; *OI + Dep-OI* + depreciation charges; *OI-KCst-OI* less estimated capital costs; *S*-Sales; *A*-Assets; *TrA*-Assets, traceable.

[c]Discussed, but not presented.

[d]Excludes *LB*s from companies added in 1975.

[e]Excludes *LB*s with inadequate merger data; outliers.

[f]Excludes *LB*s with missing trade data.

[g]Not comparable, since interaction terms with concentration are used.

[h]Excludes *LB*s in industries designated as "miscellaneous" or "not elsewhere classified"; *LB*s with market share less than 1 percent.

[i]Significant at the 5 percent level.

[j]Cannot be included because of 4-digit industry effect dummies.

[k]Excludes *LB*s not in all four years; *LB*s with zero traceable assets; outliers.

centration coefficient is hardly affected. Equation (8) estimates the basic equation (2) model with depreciation added back into operating income. The effects on market share and concentration coefficients are small.

An even more stringent profit variable redefinition is covered by equation (9). Anita Benvignati (1987) subtracted from the operating income/sales ratios estimates by Scott and Pascoe (1984) of securities market-based capital costs, in effect specifying the dependent variable as a measure of excess returns. The market share coefficient remains robust.

Regressions (10)–(12) relate the ratio of operating income to assets, rather than sales, with a diverse assortment of additional controls. The market share coefficients are larger than in the sales regressions because mean operating income/assets ratios (reported in brackets) are larger than the comparable sales-deflated ratios. They are also more variable over the business cycle, although they remain powerful at all stages. Regression (10) is most similar to the earlier equations; there, for the first time, the seller concentration coefficient turns positive but insignificant.

Richard Schmalensee's (1985) regression (11) is the most different, culling out from the sample all lines in "miscellaneous" manufacturing categories and those with market shares of less than 1 percent, and controlling by means of fixed effect dummy variables for both company effects (with respect to which accounting choices are believed to differ) and industry effects. The market share coefficient remains in the same range as in quite differently specified models.[6]

Regressions (12) and (13) control identically for industry and merger accounting effects on 1977 profitability. They differ in the addition to equation (13) of variables measuring the extent to which LIFO inventory accounting and straightline (as contrasted to accelerated) depreciation methods were adopted. The straightline variable was significantly related to profitability, but the market share coefficient changes by only 6 percent with the added accounting method controls.

Equations (14) and (15) introduce what is without doubt the most glaring oversight in Benston's critique. The FTC's Line of Business program, on which his analysis focuses exclusively, has a private sector counterpart —the PIMS (Profit Impact of Market Strategies) program. Financed by several hundred participating companies, it too collects profit-and-loss and other accounting data at the line of business (i.e., "business unit") level. Thus, the kind of data collection Benston attacks as meaningless has passed a clear market test. Early analyses using PIMS data, notably, by Gale and Branch, yielded market share and seller concentration coefficient effects quite similar to those reported in our Table 1.

Equations (14) and (15) come from a study by Cheri Marshall (1986) estimating profit-structure relationships for Line of Business *and* PIMS data sets matched as exactly as possible with respect to business cycle phase, coverage of manufacturing lines only, and model specification. Even though the PIMS data tend to oversample leading sellers with high market shares and to define markets more narrowly than the *LB* program does, the market share coefficients estimated with these two data sets turn out to be quite similar. The concentration coefficients have different signs, but fall far short of statistical significance.

To sum up, contrary to the implications drawn by Benston, the basic structural relationships estimated using *LB* (and PIMS) data turn out to be robust across a wide range of variable definitions, sampling frames, and controls for accounting method variations.

B. *Other Accounting Impact Analyses*

Two further tests of how accounting choices matter were reviewed by Benston. He draws negative implications mainly by

[6] No industry concentration coefficient could be estimated, since it would be perfectly collinear with the fixed industry effect dummies.

quoting the authors out of context or by ignoring results that run contrary to his conclusions. Thus, summarizing Long's discussion (1982) of how the coefficients of a structure-performance model were changed by applying alternative cost allocation formulae, he emphasizes Long's observation that the effect of "random" allocation was "disasterous" (p. 48).[7] He fails to note the concluding sentence of the same paragraph, in which Long observes that the random allocation method had "serious problems." Contrary to Benston's assertion that it "has about as much validity as other methods," it places on average the same absolute burden on lines with 2 percent of a firm's sales as on those with 40 percent—a situation that is neither realistic nor competitively sustainable.[8] Likewise, despite citing Long's follow-up analysis (Long et al., 1982) in his references, he fails to acknowledge the insights from market-oriented cost allocation tests conducted by Long. Those tests showed the random allocation method to be by far the *least* consistent of several methods with a market-based procedure.

Benston correctly summarizes most of Ravenscraft's research (1981) on the effects of alternative transfer pricing methods. However, he singles out for quotation a remark by Ravenscraft indicating that the shift from nonmarket to market transfer methods changed operating income-sales ratios by as much as 17,595 percent, with an average of 445 percent. He fails to mention Ravenscraft's footnote, which observes that since most of the nonmarket transfers were at cost, the profitability denominator with respect to which these high percentage changes are calculated was typically close to zero. And of course, any finite change, however modest, divided by zero or something close to zero leads to very large numbers of the sort Ravenscraft reported.[9]

An accounting choice variable not explicitly considered by Benston is the determination of what asset values will be recorded after a merger occurs. There are two main alternatives: purchase accounting, under which acquired assets are revalued to reflect the actual transaction price paid, and pooling of interests, under which premerger book values are retained. Studies by Ravenscraft and Scherer (1986a) show that postmerger profit rates of acquisition-prone lines of business are strongly affected by this choice. Lines with substantial amounts of purchase accounting assets report significantly lower profitability postmerger than lines using pooling of interests accounting. However, this bias has been identified and broken down into components associated with the write-up of asset values, increased depreciation (typically modest), and a selection bias reflecting the tendency for purchase mergers to have poorer premerger profitability prospects. Thus, with the appropriate analytic effort, differences in accounting method choices can be turned into a lever for understanding better the economics of merger.

In sum, accounting method choices do make a difference in reported profit figures. However, it is possible to go well beyond Benston's speculation on what *might* happen, analyzing both the magnitude of their effects on reported profits and any systematic biases they might impart to structural coefficient estimates. The work done thus far refutes Benston's claim that the biases in structure-performance and other analyses are so serious as to vitiate the results. After elaborate and diverse controls, the basic relationships persist. As always, this cannot be the last word; more remains to be done.

[7]The misspelling was not in Long's original. It was enclosed in quotation marks in Benston's original (1982, p. 31) and hence survived subsequent editing.

[8]Even with the large changes in some profit-sales ratios induced by the random allocation procedure, structure-performance hypothesis tests were little affected. Of 8 coefficients that were positive and significant at the 10 percent level, 7 remained positive and significant after the reallocations. Of 6 that were negative and significant, 4 remained negative and significant after the change.

[9]The same "divide by zero" problem strongly influences the alternative common cost allocation results reported by Robert Mautz and Fred Skousen (1968), and cited approvingly by Benston (p. 48).

III. Too Much Variability?

Benston criticizes the Line of Business data among other reasons because reported profit figures exhibit too much variability. Observing that 16 percent of the lines in Martin's sample had negative profitability in (recession) year 1975, he suggests that the data reflect "very limited entry and exit into a substantial number of markets" or "substantial annual random variation in returns, which implies that the data measurement period should be longer than a year," or "the effects of accounting practices" (p. 53). Without intervening clarification, he later expresses his summary view that "these data reflect the accounting biases present in the numbers" and that "it is doubtful whether analyses using these data would yield valid findings" (p. 64). Similarly, Benston expresses concern over large year-to-year changes in profitability ratios for data aggregated to the industry level, suggesting that "...changes in the environment in which the reporting companies operated or changes in the sample caused these differences. The errors of measurement described above could also be responsible for the differences between years" (p. 52).

A. *A Prediction Test*

Recognizing that the measures of "true" economic value against which *LB* data might ideally be judged are not available, Benston argues that "The validity of company accounting data for economic analyses... would be best determined with tests of prediction" (p. 39).[10] He attempts no such test. However, a strong test is possible.

A fundamental proposition of economic theory is that firms exit a market in response to negative profits—that is, losses. Benston finds implausible the high incidence of losses in individual lines, but makes no attempt to test whether there is in fact "very limited entry and exit."

The characteristic form of exit in large manufacturing corporations is sell-off; few units are simply shut down. Among the roughly 4,000 manufacturing *LB*s operated by companies in the FTC sample, 436 lines were sold off totally, and at least 455 more experienced partial sell-off, between 1974 and 1981. For lines with no recorded sell-off activity, operating income (i.e., profit before deduction of interest charges, income taxes, and extraordinary items) averaged 13.93 percent of assets. For the lines that were totally sold off, the trend in operating income-assets ratios (in percentage terms) over the six years before sell-off (in year T) is shown in Table 2. The pattern is what one would expect if *LB* profitability figures were valid decision-making indicia. About three years before sell-off, profitability declines markedly (and statistically significantly), dropping further two years before sell-off and turning *negative* on average in the year before sell-off.

TABLE 2—PREDIVESTITURE PROFITABILITY TRENDS

Year[a]	Operating Income / Assets[b]
$T-6$	9.3
$T-5$	8.3
$T-4$	7.1
$T-3$	3.5
$T-2$	2.9
$T-1$	−0.3

Source: Ravenscraft and Scherer (1986b).
[a] T here is the year in which sell-off was initiated.
[b] Shown in percent.

The *LB* profitability variable was the most powerful single predictor in a logit regression equation whose dependent variable equals unity when a business unit was fully divested during the 1976–81 period.[11] Also, holding *LB* profitability constant, sell-off was significantly more likely, the lower overall *company* profitability was. It seems clear

[10] See also Milton Friedman (1953).

[11] See Ravenscraft and Scherer (1987, ch. 6.). Other statistically significant variables included past merger history, market share, *R&D* intensity, and whether there was a change in the chief executive officer in the two years preceding sell-off. An advertising intensity variable was not significant, presumably because advertising, unlike *R&D* and contrary to Benston's statement (p. 43), is known to depreciate rapidly in most instances. Compare Darral Clarke (1976).

TABLE 3—RANK AND PROFITABILITY CHANGES OF INDUSTRIES AMONG THE TEN MOST PROFITABLE IN AGGREGATED *LB* REPORTS FOR 1974, 1975, AND 1976

FTC Industry Code	Industry Description	Operating Income/Assets Rank (and Percentage)[b]		
		1974	1975	1976
33.04[a]	Primary lead	1 (41.8)	76 (15.0)	30 (21.3)
20.17[a]	Beet sugar	2 (41.5)	15 (25.5)	210 (5.8)
28.14[a]	Fertilizers	3 (40.3)	4 (36.4)	151 (11.6)
20.09	Cereal breakfast foods	4 (39.8)	3 (38.1)	3 (38.3)
28.15[a]	Pesticides and agricultural chemicals	5 (34.1)	7 (29.5)	49 (19.0)
28.08	Proprietary drugs	6 (31.0	30 (22.2)	9 (23.3)
29.03[a]	Misc. petroleum and coal products	7 (29.6)	39 (20.2)	192 (8.5)
26.01[a]	Pulp mills	8 (29.1)	47 (19.0)	66 (17.5)
27.06[a]	Manifold business forms	9 (28.9)	17 (24.1)	58 (18.1)
33.05[a]	Primary zinc	10 (27.3)	191 (5.8)	205 (6.2)
34.09[a]	Fabricated structural metal	223 (0.0)	1 (39.2)	1 (41.9)
34.03	Cutlery	20 (24.5)	2 (38.2)	2 (38.5)
20.13[a]	Wet corn milling	16 (25.0)	5 (35.0)	152 (11.5)
20.02[a]	Poultry and egg processing	232 (−11.6)	6 (29.5)	226 (1.1)
26.08	Stationery, tablets, etc.	24 (22.5)	8 (28.9)	42 (19.8)
20.15	Cookies and crackers	41 (18.7)	9 (28.7)	4 (33.9)
35.26	Speed changers and industrial drives	46 (18.2)	10 (28.0)	8 (27.9)
21.03	Chewing and smoking tobacco	11 (27.2)	13 (26.0)	5 (31.0)
20.27	Flavoring extracts and syrups	32 (20.8)	11 (27.3)	6 (30.2)
36.26	Primary batteries	–	23 (23.1)	7 (29.6)
36.11	Household vacuum cleaners	27 (22.0)	42 (20.1)	10 (25.6)
All Industries—Median		(12.1)	(12.1)	(13.4)
Top Quintile Bound		47 (18.0)	47 (19.0)	47 (19.1)

Source: U.S. Federal Trade Commission.
[a]Quintile leavers—see text for full information.
[b]Percentages are shown in parentheses.

that accounting data at the line of business and company levels contain strong signals to which important economic decisions are related.

B. *Industry-Level Profitability Changes*

Benston's assertion that year-to-year variations in reported profitability, aggregated to the industry level, are implausibly high (p. 52) can also be confronted with external evidence. Table 3 lists the 21 industries that were among the top 10 industries in terms of operating income-assets ratios in at least one of the reporting years 1974, 1975, and 1976. It reveals some dramatic changes in profitability rank (out of 234 industries for 1974 and 1976 and 237 for 1975). The question Benston poses but fails to answer is, do those moves correspond to "changes in the environment," or must one accept Benston's more negative view that sample changes or measurement errors were to blame? We focus here only on the 11 industries that moved out of the most profitable industry quintile in one or more of the years 1974–76. Those were, to repeat, years of extraordinary turbulence, beginning under price controls (until April 30, 1974), progressing into a price explosion and then a sharp recession, followed in 1976 by a strong recovery.

Industry 33.04. Annual average New York lead prices rose 37 percent from 16.3 cents per pound in 1973 to a record 22.5 cents per pound in 1974.[12] They fell slightly to 21.5 cents in 1975 and rose to 23.1 cents

[12]Unless otherwise indicated, all price information is drawn from various issues of the U.S. Bureau of the Census *Statistical Abstract of the United States* and from the U.S. Department of Agriculture annual, *Agricultural Statistics*.

in 1976. The pattern is fully consistent with the pattern of profitability changes.

Industry 20.17. When price controls were relaxed, the distortions they and sugar import quotas had caused led to a price explosion. Refined sugar prices (at New York) soared from 13.8 cents per pound in 1973 to 33.7 cents in 1974. Refiners had entered fixed proportion revenue-sharing contracts with growers, and so as refined sugar prices climbed, refiner profits rose sharply. See Keith Anderson et al. (1975, pp. 67–76). With expanded output in 1975, prices, like profits, dropped modestly to 30.8 cents. In 1976, following a relaxation of sugar import quotas and greatly increased imports, prices fell sharply to 18.85 cents. The evidence is completely consistent, although we supplement it by jumping out of order to a related industry.

Industry 20.13. In the early 1970's, the wet corn milling industry introduced a new product, high fructose corn syrup (HFCS), which is a close substitute for cane and beet sugar in many applications. For the rest of the story, we can do no better than quote Michael Porter and Michael Spence:

> Demand growth was aided by a tremendous surge in sugar prices in 1974.... However, at the end of 1974 the sugar price support legislation in the United States lapsed and was not renewed because of high sugar prices. The latter then tumbled to the eight cent per pound level. This adversely affected the HFCS market. By 1976 the capacity planned in 1973–74 was coming on-stream, while demand was falling off. By late 1976 industry capacity utilization was low...and the profits had been squeezed out of the margins in the industry. [1982, p. 281]

Industry 28.14. Price controls also distorted fertilizer supply, while sharply higher grain prices increased plantings and hence the derived demand for fertilizers. See Marvin Kosters (1975, pp. 85–89), and Milton David et al. (1976). Fertilizer makers promised capacity expansions and diversion of output from export to domestic markets in exchange for early relief from price controls. At first, prices soared. But when the new capacity came on stream, prices declined, as the two representative price series reported in Table 4 show. Meanwhile the costs of important inputs, especially natural gas, were rising rapidly. The mild and then sudden drop in fertilizer industry profitability is consistent with these movements in output, cost, and prices.

TABLE 4—MOVEMENTS IN THE PRICE PER TON OF REPRESENTATIVE FERTILIZERS

Year	46 Percent Superphosphate	Ammonium Nitrate
1973	$ 91	$ 74
1974	169	155
1975	197	171
1976	152	136

Source: U.S. Department of Agriculture.

Industry 28.15. Price and output behavior is more heterogeneous for the complex mix of pesticides, herbicides, and other agricultural chemicals produced by this industry. The most that can be said is that the profitability changes are similar to those in the closely related fertilizers category, but strong product differentiation inhibited a sharper decline as capacity caught up to demand.

Industry 29.03. Any comprehensive industry reporting system must have some "miscellaneous" or "catch-all" categories, and this is one. Its products include lubricating oils blended outside refineries, petroleum coke, charcoal briquettes, and brake fluids. Only 5 or 6 companies reported in any given year, and we do not know (or would not be permitted to disclose) what products they emphasized, and hence what caused their profitability changes.

Industry 26.01. The producer price index for wood pulp rose from 128.3 in 1973 to 217.8 in 1974, 283.4 in 1975, and 286.0 in 1976. Falling profits despite high and (until 1976) rising prices can be attributed in part to a drop in capacity utilization from above 90 percent in 1974 to less than 85 percent in early 1975 (Harbridge House, 1976, p. 96). In addition, 22 percent of the U.S. timber supply came from federal forests, where the price of the most important pulp input,

TABLE 5—POULTRY INDUSTRY OUTPUT AND PRICES

	1973	1974	1975	1976
Poultry and Egg Output (1967=100)	106	106	103	110
Broiler Output (billion pounds)	11.22	11.32	11.10	12.52
Average Broiler Prices (cents per pound)	24.0	21.5	26.3	23.6
Corn Prices (per bushel)	$2.55	3.02	2.54	2.15

Source: U.S. Department of Agriculture.

stumpage, adjusts to downstream market changes only as multiyear tract leases are rebid. Thus, input costs probably followed pulp prices up only with a lag.

Industry 27.06. The manifold business forms industry supplies a wide array of custom-made forms. No adequate output price index is available. It seems reasonable to suppose that form producers found it difficult to pass along fully the rapidly rising prices of a key input, paper.

Industry 33.05. The average price of prime western zinc soared from 20.7 cents per pound in 1973 to a record 35.9 cents in 1974. The price continued a more gradual rise to 39 cents in recession year 1975, while domestic output fell 21 percent. The reason for price increases despite recession is not clear, but decreased profitability is consistent with significantly reduced capacity utilization and/or upward cost curve shifts. The average price fell to 37 cents in 1976 while output rose to 90 percent of its 1974 level.

Industry 34.09. Fabricated structural metals is a contracting industry, assembling bridge sections, joists, television towers, ship sections, and the like. Having written contracts anticipating something like a continuation of the 5.9 percent 1973 inflation rate on its principal input, structural steel, it was shocked by a 28 percent annual increase when price controls expired in April 1974. Wages also rose rapidly, and many bankruptcies occurred.[13] The high profits in 1975 and 1976 evidently came from some combination of capacity reductions (for example, Bethlehem Steel Corp. closed four fabricating plants) and the projection of high steel price inflation in bidding for new contracts. Materials costs fell from 44 percent of sales in 1974 to 35 percent in 1975. The volatility of industry earnings owing to changes in price-cost spreads is enhanced by low capital intensity. In 1974, fabricated structural metals ranked 206th among 234 industries in the ratio of total assets to sales.

Industry 20.02. The poultry and egg processing industry is clear winner in the "profit roller coaster" competition. Preliminary insight into its volatile profit behavior is gained by examining the four output and price series summarized in Table 5. Broiler chicken prices dropped by 10 percent in 1974, partly because cattle held back until meat price controls were lifted in late 1973 surged into the market, forcing down all meat prices. Meanwhile feed prices rose sharply. These two developments are sufficient to explain the poor 1974 profits.[14] The situation reversed in 1975, in part because of lagged output adjustments (broilers require two months to mature from the chick stage). The resulting profit margins were characterized by the *Wall Street Journal* as record breaking.[15] Another overreaction followed: output rose 13 percent in 1976 and broiler prices fell, confirming the *WSJ*'s July

[13] "No Physical Growth Ahead with 5% Dollar Gain," *Engineering News Record*, January 23, 1975, p. 38.

[14] "Poultry Industry Faces 'Rough Sledding,'" *Feedstuffs*, June 24, 1974, *46*, pp. 1, 64.

[15] "U.S. Chicken Industry Enjoying a Feast as Domestic and Foreign Demand Booms," *Wall Street Journal*, July 6, 1976, p. 28.

1976 prediction that the "gravy days may be ending."

To sum up, for most of the eleven industries that experienced quintile-crossing profit variability over the years 1974–76, it is possible with a modest amount of research to find a plausible explanation consistent with economic theory and contemporary published materials. Benston could have found the relevant materials as easily as we. It is, to be sure, easier to sit ex cathedra and conjure up imaginary sampling biases or data deficiencies. But it is also less constructive. Moreover, to anyone who enjoys economic detective work, the Line of Business data offer interesting challenges that can lead, we believe, to insights much more enlightening than those Benston offers.

IV. The Individual Studies

The lack of care with which Benston undertook his critique is demonstrated by the number of errors he makes. Certain errors have been pointed out already. In the Detailed Appendix Comments, we provide a selective addendum focusing on his critique of our papers. We proceed seriatim, identifying page number and column.

V. Conclusion

Data are fallible. So are scholars. Yet when an article is as consistently negative as Benston's, one suspects bias, and when it contains as many demonstrable errors as Benston's, one suspects a degree of carelessness incompatible with the burden a scholar must bear when he singles others' work out for criticism. We also have a more fundamental objection. It is easy enough to sit at one's desk and take pot shots, accurate or inaccurate, at others' empirical research. It is more difficult to augment an already complex data base and design sensitivity tests to ensure that results are robust and not afflicted by bias. Although constructive criticism is necessary and welcome, knowledge can scarcely advance without an emphasis on data base building, testing, and sensitivity analysis. Much has been done using the Line of Business data base to advance economic knowledge. Much remains to be done. From Benston's selective, inaccurate criticism of our work, readers of this *Review* may have gained the opposite impression. We hope to have corrected it at least in part, but we believe the best proof of our position must come from the reader's own unbiased perusal of the substantial literature emanating from *LB* data.

DETAILED APPENDIX COMMENTS

P. 54–2; Benston is correct in his observation that Mueller's (1980) study illustrates the difficulty in estimating production functions using the existing *LB* data. Unfortunately, the *LB* program did not collect the requisite physical output and employment measures. Mueller concluded that the data were insufficient and abandoned his effort. Where Benston errs is in insisting that without production function estimates, "structure-performance studies cannot be used" (p. 55–1) to illuminate such questions as economies of scale. Compare Ravenscraft (1984) and Long (1982), who develop techniques by which scale economy effects can be disentangled.

P. 55–1 (fn. 37, and also p. 58, fn. 43): A stepwise procedure was used by Ravenscraft not to eliminate variables from the analysis, as alleged by Benston, but to select variables for a heteroskedasticity correction.

P. 55–2 (fn. 38): Contrary to the implication drawn, Martin found significant differences between profit/sales and profit/assets regressions mainly when nontraceable assets were excluded from the denominator. And the Herfindahl index was significant in two cases, not one as alleged.

P. 56–1 (and also p. 59–1): Benston correctly notes that the negative and significant *LB* assets/sales coefficient was inconsistent with a priori expectations. However, accounting bias is not the only explanation. Ravenscraft (1983) shows that industry assets/sales and *LB* assets/sales, the latter interacting with market shares, have the anticipated positive signs. Thus, the negative coefficients on *LB* assets/sales imply low profits for low market share lines with high asset intensity.

P. 56–2: Citing Stanley Ornstein (1975), Benston argues that "the relationship between profit/sales and concentration (or market share) should be specified as log linear...." Ornstein's argument, however, is not based upon a profit-maximizing model, and it ignores the fact that profits can be meaningfully negative. Roger Clarke and Stephen Davies (1982) and Long (1982) use a standard oligopoly model to derive first-order equations in which profits/sales is a *linear interactive* function of concentration and market share. This form, rejected by Benston despite strong theoretical support, was estimated by Long (1982), Ravenscraft (1983, Table 2), and Kwoka and Ravenscraft.

P. 60–2: Scott's analysis covered 246 industry categories, not the "twenty-four lines" claimed by Benston.

P. 61–2: Benston claims that Scherer's methodological paper did not relate *R&D* expenditures to produc-

tivity. In fact, the published version cited in Benston's references did report such relationships.

P. 62–1: Benston claims that it "seems obvious that if more is spent on research and development, more patents will be applied for and granted." The earlier FTC version of his paper (1982, p. 79) contains nearly the same language and also a statement that "*R&D* expenditures should not be expected to relate materially to the number of patents granted" (p. 8). Obviously, something was not obvious. In fact, the paper's main claim to novelty was not that patents and *R&D* were related, but that the relationship was preponderantly linear once a modest size threshold is exceeded, and that other variables have little incremental explanatory power.

P. 62–2: Scherer's productivity growth analysis used three different productivity measures, not two. In the main analysis, the level of aggregation was 3- or 4-digit, not 2-digit. In most cases, the *R&D* outlays were divided by value of output, not value-added. No variable measuring changes in sales/labor expense was used, contrary to Benston's statement. Capital/labor variables were not omitted as alleged, nor did the bulk of the analysis rely upon simple correlations as alleged. A "wrong lag" analysis tested for timing effects, contradicting Benston's conjecture that greater productivity might have led to more intensive *R&D*.

P. 63–1: Long's analysis related patent/sales, not patents, to *R&D*/sales with quadratic terms. Benston's error here is important, since a negative quadratic coefficient sign is not an indication of poor data quality, as suggested, but of plausible diminishing marginal returns in the input-output relationship. There is no information in Long's paper from which Benston could draw the conclusion that the estimated relationships differ in sign between years. Rather, his conclusion reflects a misreading of differences in statistical significance as differences in sign.

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