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ABSTRACT

The relationship between profits and other accounting-based numbers, concentration, and other variables has been measured extensively. Because increasingly diversified companies do not publish data on consistently defined products, the FTC recently required some 450 of the largest firms to report detailed data by specified lines of business. These presumably greatly improved data were used to produce several statistical reports and at least twelve research studies. This paper analyzes the extent to which accounting data can provide valid economic measures, the likelihood that the FTC data are meaningful, and the extent to which the studies were able to overcome the data's shortcomings.

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The simple proposition that consumer damaging collusion is more likely to occur when there are fewer competitors has given rise not only to restrictions on mergers but to an enormous amount of empirical work.[1] Most such studies have measured competition by an index of concentration (e.g., four-firm concentration or Herfindahl) and performance by accounting profit (e.g., net profit divided by equity, or sales less direct costs divided by sales). From the positive, statistically significant correlation often found between greater concentration and profit (with other factors presumably accounted for), the researchers conclude that many markets are non-competitive and that concentration, therefore, is bad.

The concentration-profits studies have been criticized essentially on two grounds. One is that the positive relationship is not indicative of collusive behavior. In particular, Williamson (1968), McGee (1971), Demsetz (1973 and 1974), and Peltzman (1977) point out that higher profits could result from efficiencies experienced by large firms, which resulted both in greater market shares and high levels of concentration. Analytical arguments (cogently presented in Peltzman's paper) show that higher profits logically follow as much from lower costs that are associated with higher levels of concentration, as from collusion-determined higher prices. Some empirical evidence that supports this belief is presented by Demsetz, Peltzman, and Gale and Branch (1983). However, their findings have been contested on the grounds that the data used are biased or inadequate or that the researchers have not demonstrated that the observed higher profits-greater concentration relationship was caused by lower costs.[2]

The second criticism is that the only available data are inadequate. The concentration numbers are based, usually, on industries defined by the Commerce Department's Standard Industrial Classification (SIC). Most SIC definitions are supply rather than demand determined, include non-homogeneous products, and exclude

sales of similar products that are included in different SIC groups or are imported. The profit data are taken from accounting reports that provide poor measures of economic values. Weiss (1974) describes many of these problems. However, he does not believe that these shortcomings invalidate the studies. As Weiss (1979, pp. 1106-7) later summarizes his conclusions:

> I argued that the crudeness of the concentration data, the increasing diversification of firms, and many distortions in accounting profits all bias the observed relationship between concentration and profits towards zero. Because of these biases, I argued that the effect [of a significant positive relationship] was probably understated when observed, and that it might well have been present when it was not detected.

Largely as a consequence of these problems (particularly the increasing diversification of large companies that made their reported profits and other data difficult, if not impossible, to link with measures of market concentration), the Federal Trade Commission (FTC) designed and implemented a large scale program to gather detailed data from large companies by lines of business (LBs).[3]

Originally, the program sought to collect data from some 2000 companies on their revenues, expenses, and assets, allocated to 357 lines of business. After vigorous protests, culminating in an unsuccessful 1975 challenge in the Washington, D.C. District Court, the program was scaled back somewhat, and 450 of the largest industrial companies were required to file reports on 267 FTC designated lines of business (that follow the SICs) for 1974 through 1977. (Partial reports also were filed for 1973). These data are much more extensive and detailed than have ever been collected by a public agency. With them, the FTC has published several Statistical Reports and more than a dozen papers based on the data have been written by its staff and outside economists. Thus, there is a substantial basis on which the usefulness of the program and of profit-structure studies in general can be judged. Considering the detail, large number of observations and cost of the data and the qualifications of the economists who used them, an analysis of the inputs and outputs of the LB program can serve as

a general critique of empirical work that is based on company accounting numbers and data classified by SICs. If the studies using the FTC's LB data are not valid, the very large number of papers using similar methodologies and based on inferior data are also likely to be invalid.

The analysis is presented in two major sections. In section I the extent to which LB and other company-derived accounting data are reliable indicia of economic performance is delineated. Studies that use the LB data are analyzed in section II, with emphasis on the usefulness of the data for testing the hypotheses posed.

## I. The Usefulness of the FTC's Line of Business (LB) and Similar Data for Evaluating Economic Performance

### A. The LB Program's Purposes

In the first Statistical Report published under the FTC's LB Program, the specific purposes for it was established and for which LB data are to be used are reiterated.[4] According to the FTC staff, "The Line of Business Program is designed to elicit information vitally needed in evaluating industry performance." (Federal Trade Commission, 1981A, p. 2) These data, they say, will serve the following purposes:

1. Antitrust Enforcement: "Knowledge of industry performance is essential to efforts of the Commission to direct its activities towards industries where poor performance suggests the need for more detailed investigation and possible enforcement action." (p. 3)

2. Allocation of Private Sector Resources: (a) "Managers and directors of corporations will be able to evaluate the performance of their own enterprises against industry averages"; (b) "Investment analysts and investors are likely to find LB reports useful in evaluating the prospects of particular industries" (p. 4).

3. Aid for Economic Research in Industrial Organization: "And, like economists within the FTC, outside scholars will use the Line of Business aggregates as a basic data source for advancing the frontiers of knowledge in the field of industrial organization." (pp. 4-5)[5]

It is clear that the LB Program requires numbers that adequately reflect the desired economic market values.

The extent to which accounting data can provide unbiased measures of the required economic values and an evaluation of the extent to which the biases can be

overcome or assessed is investigated next, in sub-section B. The degree to which the data are contaminated by the FTC's (SIC) designation of "industries" is considered in sub-section C.[6] Evidence supporting these criticisms is presented in sub-section D.

B. Accounting Data As Approximations of Economic Market Values

1. The Usefulness of Accounting Data. For several reasons, a firm's accounting data are useful to company managers and outside observers of company acitivities even if these data do not provide good measures of economic market values appropriate to other issues. These reasons include audits of company affairs for investors and internal control for managers. When the reported figures are attested to by a certified public accountant, affirmation is made that an audit was conducted by an independent expert who verified the figures and tested the adequacy of the company's financial control system. The accounting numbers also can be evaluated in the context of the manager's or analyst's knowledge about the company's activities and past performance. Thus, the user of the numbers can formally or informally adjust them to account for idiosyncracies in the reported accounting procedures, their company-specified deviation from economic measures, and their relevance to particular questions being asked (e.g., what is the probability that the company can repay debt as promised?). Furthermore, changes in the reported numbers over time, within the context of the past and expected future economic conditions in which the company operates, may provide the user with useful information.

However, none of these purposes are served by the reported LB or other aggregated accounting data, and, even if they were, individual firm data may not legally be disclosed under the LB and Census programs. Hence, whatever the utility of accounting data for company-specific purposes, their use as measurements of industry-wide economic market values takes them far out of context. As shown in sub-section C, this aggregation introduces substantial biases in the LB data.

Of course, imperfection characterizes most measurements. This virtual truism

has been used to dismiss all empirical work ("useless at best and misleading at worst"). Alternatively, it has been used to accept almost any numbers as better than none ("it may be a crooked wheel, but it's the only wheel in town"). The issue should be resolved by reference to the question asked and the effect of relying on the available information. In some contexts, some information is worse than none, and in others reliance on bad data is preferable to choices made randomly. As is discussed above, the purposes for which the LB and similar data are gathered can be served only if the reported numbers are reasonably meaningful estimates of economic values. The question, then, is what "reasonably meaningful"? While this question can be answered completely only by reference to specific uses to which the data are put (some of which are analyzed in section II), some insights can be gained by considering the inherent divergences of accounting numbers from economic market values.

For present purposes, only some obviously important sources of divergences are considered.[7] These include: (1) the values of long-lived tanible assets and their depreciation or depletion over time; (2) the value and amortization of intangible assets (e.g., advertising and research and development); (3) inventory values and the cost of goods sold; (4) allocation of common and joint costs to lines of business; and (5) intrafirm transfers among lines of business. For each, the reasons for the divergences are delineated first, followed by an evaluation of the extent to which the reported numbers can be adjusted to approximate economic values and the effect of the remaining biases on two commonly used measures of economic performance, profit/sales and profit/assets. The conclusions of the analysis are summarized in Table 1.

1. Long-Lived Tangible Assets and Their Depreciation or Depletion Over Time

Accounting Procedures. Accountants do not attempt to record, initially, an asset's economic value to a firm, but, rather, the amount paid for it as measured in objectively determined monetary units (market value). As a rule, an asset's

economic or present value (value in use) to the firm is greater than or equal to its market value (value in exchange), since otherwise it would not have been purchased. Where an asset is constructed rather than purchased, the recorded amount is even more likely to understate the asset's value in use, for two reasons. First, when the asset could have been purchased at less than the amount expended to make it, accountants charge the excess to expense (the conservative accounting bias). Second, the recorded amounts rarely include a factor for the seller's selling and administrative costs and total cost of capital. But an asset is never capitalized (recorded as investments) at more than its recorded cost.

These understatements of initial asset values are particularly serious for extractive assets. When these assets are fortuitously discovered, at most only the amounts expended for the successful discovery are capitalized. In other cases, the successful efforts method of accounting (used by most larger natural resource producers) capitalizes only the direct cost of discovering and developing successful wells and mines. The amounts spent for unsuccessful efforts are "charged-off" as current expenses, despite the fact that successful ventures usually are accompanied by unsuccessful ones. Even companies that use the full-cost method (under which amounts incurred for all discovery efforts are capitalized) charge off many development costs.

Once the tangible assets have been acquired, the amounts recorded as asset values are written off (expensed) over the expected economic life of the assets. The tax regulations usually permit a faster write off, which may also be used for book (reporting) purposes. But a book method may be adopted that yields currently higher reported net income. In any event, the depreciation method chosen is not designed to measure the user cost of the assets -- the change in the asset's present values -- in part because these calculations are too difficult to make, and in part because the accountants who must attest to the numbers would find such calculations uncomfortably subject to manipulation.

The recorded numbers also are not adjusted to reflect changes in the capital values of the tangible assets. Such changes could occur as shifts take place in the supply and demand for the assets, the factors used with the assets for production, and the goods produced.[8] But the accounting values are not changed.

The monetary values of assets also change as the purchasing power of money changes. While general price level changes presently are used in the published statements of large companies, usually by the application of indices, these adjustments are not often made a part of a company's accounting system and individual asset accounting numbers are rarely changed. Furthermore, current values are not recorded, largely because the cost of such adjustments to the recorded numbers is greater than value of the adjustments to most companies.

The magnitudes of the divergences between accounting numbers and economic value cannot be determined. An estimate, perhaps, could be made if one could obtain the information on individual assets and estimates of their current market values if sold as separate assets. Even then, the assets' economic (present) values to the firm would not be measured. Furthermore, market-revealed values are unlikely to occur for most user-constructed assets. Even general adjustments in the magnitudes, perhaps with some index, would require detailed information about purchase dates, current equivalents in terms of production, and the economically correct depreciation procedure to be applied. Such knowledge is impossible or very expensive to obtain.

The Effect of the Biases on Measures of Economic Performance. Although there is no practical way to convert accounting asset values and depreciation to economic values, those who want to use these numbers claim that the numbers could be useful if the direction and rough magnitude of the biases could be stated. But, as the following analysis shows, the effect of the biases on two commonly used ratios -- operating profit to sales (P/S) and operating profit to total assets (P/A) -- is not determinable.

As noted, the initially recorded amounts for tangible long-lived assets almost always understate economic values, as do shifts in relative and general prices that increase the value of assets. In both cases, the economically correct adjustment would increase the asset's value and increase income from capital gains. Since this is not the accounting practice, profit and asset values are understated. Consequently P/S is understated, and as long as P/A is less than 1.0 (an assumption followed hereafter), P/A also is understated. The effect on P/S is greater than on P/A, since both assets and profits are understated. Subsequently, profit and P/S are overstated, since depreciation on the higher unrecorded asset value is not recorded. But since assets still are understated, the overstatement of P/A is even greater than the overstatement of P/S. (Unrecorded decreases in the present value of assets have the opposite effect.)

The effects become more complicated if the initial events and changes in asset values occur more than once, as is likely. If these events occur uniformly over time, the effects on profit might conceivably wash out, so that the unrecorded capital gain equals the unrecorded depreciation. (Capital gains are assumed here and below; the conclusions are reversed for unrecorded capital losses.) In this event, P/S would not be biased. But assets would be understated and P/A would be understated and P/A would be overstated. If these events were diminishing over time, perhaps because of a decline in growth of the company or in relative prices, unrecorded depreciation probably would exceed unrecorded capital gains. Hence profit would be overstated. The cumulative understatement of assets (which would not be removed until disposal of the assets) would result in a greater overstatement of P/A. But if the company were growing or relative prices were increasing, unrecorded capital gains probably would exceed the unrecorded depreciation of past unrecorded capital gains. Thus P/S would be understated. The effect on P/A is uncertain, since both the numerator and the denominator of the ratio are understated.

The effect of general price level changes is different because expected changes do not give rise to economic income or expense. But if the recorded nonmonetary asset amounts are not changed to reflect the current purchasing power of the dollar, an expected inflation results in understated assets and overstated profit (since depreciation is understated). Therefore, with respect to nonmonetary assets, P/S is overstated and P/A even more so. Interest income and expense also are overstated since they include a return of capital. (If the interest expense overstatement were large enough, which is doubtful , P/S could be understated.) Where the inflation is unexpected, though, the company realizes an unrecorded capital loss if it holds nominally denominated assets (such as bonds, notes and accounts receivable), and an unrecorded capital gain if it holds similarly denominated liabilities (such as loans and bonds payable). Thereafter, its interest income, interest expense, depreciation, and assets are understated. The effects on P/S and P/A thus depend on the extent to which it holds nominally denominated assets and liabilities. If the unrecorded capital gains exceed the capital losses, the first period P/S is understated; P/A would be understated if the unrecorded gains were large enough. In subsequent periods, P/S is overstated (unless the unrecorded interest income is large enough) and P/A is even more overstated.

While these biases are generally as specified, the magnitude of the effect varies with several factors. Profit rates and rates of return of companies with longer-lived assets are more affected than those with shorter-lived assets. The amounts invested in long-lived compared to other assets similarly affects the bias. The methods used by the company to depreciate or deplete its assets (e.g., straight line, sum-of-the-years' digits, double declining balance, or units of production) also affects the bias. The time over which the cash flows are expected to flow from the assets, and the discount rate that is relevant to these cash flows, also affect the difference between the economic profit and rate of return and the accounting numbers. Furthermore, the extent to which the initial period

effects occur discontinuously affects the bias. As is discussed above (and summarized in Table 1), except in growth situations the bias in the initial period is the reverse of the bias in subsequent periods. Hence, a large enough change in one period can reverse the sign of the continuing effects of previous periods. In general, then, there is no way to know how badly the accounting numbers measure economic market values.[9] And the differences are likely to be quite large unless the amount of long-lived tangible assets is relatively small, even in the absence of inflation. Given inflation and the non-adjustment of accounting numbers (which particularly affect long-lived assets purchased at different price levels), the biases are likely to be considerable. Indeed, the effect of a sufficiently high inflation probably swamps the other sources of divergence between accounting numbers and economic values. At the least this situation can lead to the conclusion that companies with relatively more tangible assets and with assets that are longer-lived are likely to have overstated profits and rates of return on assets compared to other companies. In the absence of this overwhelming inflationary effect, the direction of the bias cannot be stated a priori, nor does there appear to be any practical way to adjust the reported LB and other company accounting data to the desired economic values.

2. The Value and Amortization of Intangible Assets

Accounting Procedures. The amounts expended on intangible assets -- such as advertising, goodwill, research and development, and employee training -- are charged-off as current period expenses in accordance with the generally accepted accounting principles adopted by the Financial Accounting Standards Board. Although a large portion of these amounts clearly result in assets, they are not recorded as such because the present value from these expenditures is very difficult to measure objectively. Consequently, managers would be able to use the estimates to manipulate the reported profit and assets of their corporations. Even when estimates were objectively made, the likelihood that they would turn out to be wrong is considerable.

Subsequently, it could appear that the amounts were deliberately misestimated to fool the users of the company's financial statements. Then the independent accountants who attested to the statements would be faced with defending their acceptance of the numbers. So, to protect their positions and as a means of demonstrating that auditable estimates of the present values of intangible assets cannot be made, all expenditures on intangibles are expensed as a matter of accounting fiat.

The Effect of the Biases on Measurments of Economic Performance. The effect on recorded profit of not capitalizing intangibles depends on the magnitudes of past and current expenditures and the rate at which they depreciate. Several economists have attempted to estimate the numbers, an exercise that requires some heroic assumptions. For example, Weiss (1969) developed a model that requires an assumed constant annual growth rate of advertising and a constant rate at which past advertising expenditures depreciate. Ayanian (1975) adapted Weiss' model and shows that the rate at which advertising is assumed to depreciate is critical for calculating the economically correct rate of return. Ayanian attempted to calculate the depreciation rate by estimating an assumed multivariable Cobb-Douglas production function for advertising which required additional assumptions. To estimate the parameters, ten years' data were required. Though such procedures probably yield more meaningful results than assuming that the current year's expenditures on intangibles equals the expense, it is doubtful that the required assumptions are valid or that the data needed are available. Indeed, a basic characteristic of intangibles such as advertising and research and development is that the value of expenditures, future as well as current, is very difficult to estimate even when one has complete knowledge about the company, its environment, and the specific projects and products to which the expenditures pertain.

Given the caveats noted above, the following effects of the divergence between accounting numbers and economic values on P/S and P/A can be specified. If the company is in a stable, zero growth state, such that the amount expended on intangibles

exactly equals the depreciation of past, implicitly capitalized amounts, P/S is not affected but P/A is overstated since past expenditures were not capitalized. If the company's expenditures on intangibles is declining steadily and the depreciation rate of the intangible is constant, profit is overstated since the decline in value of past written-off expenditures is greater than the amounts currently charged to expense; thus P/S is overstated. Since assets are understated as long as the past expenditures on intangibles have any present value, the overstatment of P/A is even greater. When expenditures on intangibles are increasing and the depreciation rate is constant, P/S is understated. But, since assets also are understated, the effect on P/A is not determinable a priori without additional information. Finally, when the growth rate of intangibles equals the real rate of return, P/A equals the real rate of return.[10]

The relative amounts of intangible and other assets affect the magnitudes of these biases. Importantly, when the assumption of a constant rate of depreciation is removed, neither the magnitudes nor the direction of the bias can be determined.

3. Inventory Values and Cost of Goods Sold

Accounting Procedures. Accountants assign values to inventories according to several rigidly applied formulas that need not depend on the physical movement of the goods. The first-in, first-out method (FIFO) assumes that the first goods purchased are the first sold or used in production; last-in, first-out (LIFO) makes the opposite assumption. An average of the prices paid also is an acceptable method. Or, when the items in inventory are relatively few and unique, the acquisition prices of the specific goods sold may be charged to the cost of goods sold expense. For all methods, the value of the goods in inventory should not exceed their market values, net of disposal cost. Thus, the amounts considered current expenses and the amounts capitalized as the asset, inventory, is a function of the accounting method employed and the pattern of prices paid for the goods. None of the acceptable accounting methods records the cost of goods sold at opportunity costs -- the amounts foregone as a consequence of having sold the goods. In most (though not

all) situations, this is the replacement cost of the goods. Nor, except possibly in supplementary schedules, are the goods in inventory valued at their market prices.

Several accounting procedures determine the amounts recorded for goods that a company manufactures. In addition to the alternative conventions governing the cost of materials discussed above, some manufacturers use standard costing to assign numbers to manufactured inventories. The standards applied differ among companies. Overhead, especially, is allocated to products according to several alternative, acceptable bases. These allocation procedures usually are arbitrary either because there is no conceptually meaningful way to assign costs that do not vary with output or are joint among outputs, or because the cost of determining the association between cost and output exceeds the benefits thereof. Furthermore, to the extent that overheads or direct costs include such accounting numbers as depreciation, the amounts charged to inventories and then to cost of goods sold diverge further from economic values.

Company managers can "live with" these accounting numbers because the numbers are used to control operations (rather than as determinants of prices and outputs, for example.) As long as the cost assignment and allocation procedures are applied consistently, the managers can interpret the numbers meaningfully. But such interpretation also requires knowledge about the operations from which the numbers arise and the physical and economic conditions that attended the production of the goods and the numbers. The outside analyst, however, does not have this knowledge and, therefore, cannot adjust for the biases.

The Effect of the Biases on Measurements of Economic Performance. Short of a revaluation of inventories at current prices and of cost to goods sold at opportunity costs, there is no way that the reported numbers can be adjusted to economic values. However, if one assumes that prices of the materials and other costs of production are and have been stable, the only important problems are the divergence of accounting numbers from economic values (e.g., depreciation) and the arbitrary allocation of overhead to specific goods and processes (unless a company produces only one good

or group of products in a plant.)

When prices are changing, the method of assigning values to the goods sold and remaining in inventories becomes important. If prices are increasing steadily, the LIFO method will come the closest to recording costs of goods sold at economic values. But the inventory will be understated. Hence, as long as the physical inventory does not decrease, P/S may be reasonably unbiased; but P/A is overstated. The FIFO method tends to value the asset at current prices while profit is overstated. Thus both P/S and P/A are overstated. When prices are decreasing steadily, the reverse situation occurs -- the overstatements are then understatements. However, since companies are legally required to use the same inventory costing method for tax returns as they do for their books, they are likely to use LIFO in periods of steadily rising prices and then switch to FIFO if they believe that prices will steadily decline. (Taxpayers are permitted to make on unchallenged switch; they need not use the same costing method for all inventories.) But when prices fluctuate, the direction of the biases is no longer certain. Nor is the effect of overhead allocation procedures determinable. This is particularly troublesome for the FTC's line of business data, since information on plant level allocations is not reported.

4. Allocation of Joint and Common Costs and Assets to Lines of Business.

Accounting Procedures. Many companies charge costs incurred above the plant level (company-wide costs) to current expense. Others allocate these costs to divisions or plants (rarely to products), using such bases as sales, direct labor cost, labor plus materials (prime) cost, and contribution margin (sales less plant costs).

When the company-wide costs do not vary as a consequence of output aggregated by lines of business, any allocation method necessarily is arbitrary and profit by product line cannot be determined meaningfully. In addition, the accounting numbers for company-wide costs (such as depreciation of central facilities, general company advertising and basic research and development) are unlikely to provide valid measures of economic costs (as discussed above). These biases can be

ignored only if the magnitudes of the company-wide costs are so small that their amounts and allocations would not alter the answer to the question asked.

Effect of the Bias on Measures of Economic Performance. The effect and direction of the divergence between the profit and asset amounts that are reported after allocations of company-wide costs and assets and the economically valid amounts is not determinable. Most enterprises are not merely mutual funds in which individual companies, each of which produces a line of products specified by the FTC or the Department of Commerce, are joined only by common ownership of their shares. Rather, products usually are produced together because they share joint demands, costs, or both. There is no conceptually valid way (with respect to economics) to allocate these joint revenues and costs to the individual lines of business. And, unless an analysis was made of how common costs varied with individual products or product lines and could be avoided were production abandoned, there is no way of knowing how closely allocations made via some base, such as sales or direct expenses, approximate the correct amounts. Nor is there any way of knowing what portion of the company-wide costs and assets are joint or common and how much the accounting numbers diverge from economic values. Hence the effect of the bias of allocating or not allocating company-wide costs cannot be determined.

However, if the common and joint costs are relatively small, they could be ignored for some purposes. Data on the extent to which these nontraceable items are present is given in the FTC's 1974 Statistical Report (1981A, pp. 62-70.)[11] Nontraceable items were reported by 86 percent of the companies. Of these, 62 percent used a single base for making allocations (over half used sales), 28 percent used two bases, and 10 percent used three bases. For the entire sample of company LBs (each of the FTC-defined lines of business of each reporting company) with more than one line of business, the average ratios of nontraceable to the sum of nontraceable and traceable amounts (weighted by the amounts) are 14 percent for total selling and general and administration expenses and 12 percent for total assets. Nontrace-

able ratios over 20 percent are reported by 26 percent of the company LBs for expenses and 18 percent for assets. By LB industry aggregates, the weighted average nontraceable ratios are 15 percent for expenses and 13 percent for assets. Ratios over 20 percent are reported for 31 percent of the industries for expenses and 19 percent for assets. Thus, allocated expenses and assets are not negligible.

Long (1981A) assessed the effect of allocating common costs and assets via alternative bases on a regression of profit/sales on seventeen independent variables plus the intercept, using company LB data as observations. Profit originally was calculated as gross profit (sales plus transfers less cost of sales) less direct and nontraceable expenses as allocated by the reporting companies.[12] When nontraceable selling and administrative expenses were reallocated according to sales or a sales dominated base, the coefficients were not altered much, a result which is not surprising since 69 percent of the companies allocating common costs used sales as their allocation base and most of the balance included sales in their bases. However, Long reports that the effect of allocating these costs randomly (which has about as much validity as the other methods) "is large and disasterous. The F-ratio plummets. Coefficients change signs. Significance levels are altered." (p. 21)[13] Considering that there is no way that is consistent with economic theory to allocate common and joint costs and that company-wide costs are not trivial in magnitudes, one cannot conclude that these data yield results that are relatively insensitive to allocations of these expenses.

5. Intrafirm Transfers Among Lines of Business

Accounting Procedures. Intrafirm transfers that are not priced at the opportunity value of the goods impart a mismeasurement of the sales of the sending business unit and the expenses of the receiving unit. Thus, when transfers are priced at, say, cost rather than market, the sending unit's sales and the receiving unit's costs are understated. Because transfers at other than market prices could only be repriced at very great expense, the FTC permitted respondents to report data as per their books, but asked them to state the pricing method used. Transfers from one FTC-defined line of business to another are reported by 80 percent of the

companies.[14] Of those transferring among lines of business, the transferred amounts (weighted by sales plus transfers, hereafter denoted as sales) average 8.9 percent of sales. Transfers above 9.9 percent of sales are reported for 24 percent of the lines of business. Goods transferred to foreign activities (which are not included in the LB data) average 3.5 percent of sales. (Ravenscraft, 1982, Table I) Thus transfers, while not great on average, are not negligible.

Less than half the companies priced transfers at market -- a weighted average of 39.3 percent. Cost plus markup averages 25.6 percent of the transfers, cost 13.5 percent, and other 21.6 percent.[15] About half the companies used more than one transfer pricing method. Thus transfers at other than market prices potentially can seriously bias the reported profit revenues and costs, and hence profit.

Effect of the Bias on Measures of Performance. The bias imparted by intrafirm transfers at other than market prices is analyzed by Ravenscraft (1981). He calculated the percentage to sales of transfers made and received by each company LB and the percentages of these that are priced at cost, cost plus markup, and other. These percentages are included as independent variables in his regressions of LB profitability (profit/sales) on market structure and other variables. He finds that where transfers are at other than market the profitability of the sending LBs is significantly lower and the profitability of the receiving LBs is significantly higher. Thus the transfer pricing method does matter. But he also finds that few of the coefficients of the other variables are affected; for all but two (rather unimportant) variables, the differences are negligible.

Ravenscraft also attempted to correct the LB profits by revaluing sales and costs priced at other than market to an estimate of market.[16] This adjustment is only partially successful, since several of the variables that measure the extent of transfers still are statistically significant; had the method been sufficient these variables would no longer have been statistically associated with profitability.[17] The regression run with revalued profitability compared to Ravenscraft's original

regressions shows significant changes in three coefficients and several fairly large changes in the other coefficients' magnitudes. However, the general conclusions drawn from the original specification are not changed much.

Ravenscraft (1981, p. 10) reports that the profit/sales measure (which the FTC claims would be useful to analysts) is changed as follows:

> The average absolute change in operating income to sales for the 702 LBs is .0259, with a maximum change of .2540. In percentage terms, the average change in operating income to sales is 445%, with a maximum change of 17,595%. [This is an average of the fifteen largest changes - so the actual maximum is higher.] The correlations between the original and reallocated profits is .8936.

On the industry level, the two measures of profitability are highly correlated -- .9942. The average absolute change is .0033, an average absolute percentage change of 11.5. But among the 237 industries reported on by the FTC, there are some dramatic changes. The ten industries with the largest changes show an average absolute percentage change of 170 percent, with a maximum of 590 percent. Thus individual profitability numbers can be considerably affected by transfers at other than market prices.

6. The Cumulative Impact of Accounting Biases

The extent to which accounting numbers diverge from economic values cannot be determined, either as to magnitude or sign, but the effect of divergences of specified percentage amounts can be determined analytically.[18] Table 2 shows the reported profit rate on sales (ps') or assets (pa') that results from a given percentage error in costs (e), compared to the "actual" profit rates. Divergences due to nonmarket priced transfers and arbitrary allocations and mismeasurements of assets are ignored. Even with these potentially important measurement errors implicitly assumed to be zero, the effect of an error as small as ± .10 changes an actual profit rate of .15 to .07 or .24. A ± .20 error results in a stated profit rate of -.04 or .32 rather than .15. Consequently, it seems evident that data measurement problems are likely to swamp any inferences that otherwise might be drawn from the

reported numbers.

C. The Contamination of Lines of Business

The LB data are designed to provide useful numbers that would otherwise be hidden by the published financed reports of diversified, multiproduct companies. But companies normally do not aggregate data according to the FTC-SIC-defined industries. Hence, in order to make the program feasible, the FTC requires that all the data reported by each "basic component" of the respondent companies be assigned to a single LB.[19] Consequently, data properly classified in one LB are reported as belonging to another LB, resulting in an overstatement of data in the reported LB and an understatement in the other LBs. Although the FTC requires that a basic component be at least 85 percent specialized in terms of sales,[20] this rule does not apply to expenses and assets, which need not and often do not vary directly with sales. Moreover, while the program yields data with which the extent of overstatements and understatements in sales can be measured somewhat, the FTC has no information on the magnitude of distortions in the LB expenses and assets data or, therefore, in the reported profit (sales less costs) or profit ratios (profit/sales and profit/assets).

The FTC has sought to measure the extent by which reported sales data are contaminated with a "specialization ratio" and a "coverage ratio." The "specialization ratio" is the ratio of primary LB product sales to the sum of primary and secondary product sales. These ratios average .97 (weighted by total revenues) for the 1975 data; none can be below .85.[21] Thus, this form of revenue contamination does not appear to be serious, except for relatively few industries. The "coverage ratio" is calculated as the ratio of sales of primary LB product sales reported in an industry category to the total sales of those products reported in other industry categories. The weighted average over industries is .96 for the 1975 data. But 29 percent of the industries have percentages between .80 and .91, and 14 percent have percentages below .80. Thus, for a substantial number of industries, this measure shows a

fairly high level of sales contamination. Moreover, figures for the measures of contamination are not given for individual companies, only for industries, which are averages of the companies' lines of business. Consequently, analyses that use company LB data (as do several of the studies discussed below) are likely to be much more seriously affected by uncorrectable biases.

But of even greater significance is the FTC's candid acknowledgment that "whether there is a significant relation between the degree of sales contamination for other variables [e.g., expenses and assets] has not been established."[22] Nor can it be established with the reported data. Hence, there is no way of knowing how serious the contamination is for such measures of performance as profit/sales and profit/assets. The FTC's express warning that the data should therefore be used with "caution" is honest but not very helpful, for there is no way to exercise caution except by not using the data for the purposes advocated by the program.

Nor are these the only types of contamination. The FTC permits vertically integrated companies to include data from an upstream (earlier) operation that other companies report as an LB in a downstream (later) different LB. This contamination is permitted if more than 50 percent of the upstream LB's net operating revenues come from transfers to the downstream LB. A similar combination and contamination of data is permitted for backward transfers (e.g., wholesaling combined with manufacturing.) As a consequence of these permitted contaminations, additional overstatements and understatements of LB data will result.[23] Thus, the potential for misleading data being reported and published is considerable.

D. The Stability of the FTC's Line of Business Data

Although there is no way to determine how badly the FTC's LB data misrepresent economic values or are contaminated among LBs with respect to expenses and assets, some insight into their validity may be provided by considering the stability of the two key ratios -- profit/assets and profit/sales. (Profit is defined as net operating income before interest and taxes.) Table 3 lists the FTC industry LBs (aggre-

gates of company LBs) with the ten highest, ten median, and ten lowest profit/assets or profit/sales in either 1974 or 1975. While some of the ratios are reasonably stable, others change considerably between years, often going from negative to positive or the reverse. The mean absolute difference in profit/assets between years is 14.0 percent for the highest, 3.4 percent for the median, and 11.3 percent for the lowest. The mean absolute percentage change (1975/1974) is 56.6 percent for the highest, 40.8 percent for the median, and 11.3 percent for the lowest. For the profit/sales measure, the mean absolute difference is 5.3 percent for the highest, 2.8 percent for the median, and 6.5 percent for the lowest. The mean absolute percentage change (1975/1976) is 43.3 percent for the highest (excluding the change from 0.0%), 38.6 percent for the median, and 171.1 percent for the lowest. Unless it is reasonable to believe that these "industries" really change this much, the FTC, corporate managers, investors, economists and other users would not be well advised to use numbers such as these for evaluating or comparing performance.

The data reveal an even greater range with respect to the individual company LBs that are aggregated to obtain the industry averages and that are used as the dependent variables for several of the statistical studies discussed below. Profitability, measured as the difference between sales and traceable expenses divided by total sales (1975 data, 2297 observations) averages 7.9 percent; the standard deviation is 14.1 percent. But the individual percentages range from less than -125.8 percent to over 58.9 percent (these are averages of the six lowest and highest percentages). (Martin, 1981, Table 1) Of these, 16.4 percent are negative and 3.0 percent indicate profitability rates of more than 30 percent. Profitability measured as sales less traceable and nontraceable expenses divided by sales (1975 data, 3186 observations) have a mean value of 6.5 percent; the standard deviation is 12.9 percent. (Ravenscraft, 1983, Table A1) This measure of profitability ranges from less than -110.7 percent to more than 53.7 percent (averages of the ten most extreme numbers). It seems very doubtful that such data as these can possibly reflect meaning-

ful economic annual profit rates on sales. It appears even more doubtful that they measure long-run or equilibrium profit rates.

The rate of return on assets shows an even greater range of values. The rate, measured as sales less tranceable expenses divided by traceable total assets (plant and equipment net of depreciation, inventory and other assets), has a mean of 23.6 percent (1975 data, 2297 observations); the standard deviation of 170.0 percent. (Martin, 1981, Table II, PRA575) The percentages range from less than -227.8 percent to more than 2399.6 percent (averages of the six extreme values). Other definitions of assets show similar wide variations. Can it be that companies operate lines of business with such extreme economic returns? The range of these numbers and the number of LBs with negative and very high positive rates of return on sales and assets appear to reflect the generic biases in the LB data and cast doubt on the validity of analyses that use them.

## II. Analysis of Economic Studies Based on the FTC LB Data.

Though I believe that the biases and shortcomings of reported company accounting data and the FTC's LB data delineated above are overwhelming, there are those who strongly believe that these data are valuable if adjusted properly and used effectively. Therefore, the extent to which the reported studies prepared by the FTC's economics staff and consultants can fulfill the purposes specified by the agency are considered explicitly in this section. The working paper status of the studies[24] reviewed should not shield them from public scrutiny, for several reasons. The LB program was proposed 13 years ago -- in 1970. The costs of the program on the respondents has been considerable.[25] The nature of the data collected has been known since 1974. Hence, it is reasonable to expect the FTC's economists to have had well-structured hypotheses to which the data would be applied. Indeed, the papers reviewed address the issues to which the data collected appear to be directed. Furthermore, it is useful to analyze working papers because they are more likely to present unsatisfactory and anomalous as well as satisfactory and expected findings than are published papers.

Unfortunately for the sake of knowledge, papers tend not to be published (or even submitted) unless they reveal statistically significant results that are in accord with accepted hypotheses.[26] In addition, working papers often contain informative descriptions of the data that are not included in the shortened published revisions. Finally, in this case working papers can influence policy decisions.

A. Economies of Scale

Absent an analysis of scale economies, there is no way of determining whether a measured positive relationship between profit and concentration was due to some sort of anti-competitive behavior or to lower costs.[27] Obviously, the policy implications of these alternative explanations are diametrically opposed. Unfortunately, neither the paper reviewed next (the only one to address this question), nor any other study based on the LB data, can provide a meaningful measurement of economies of scale.

In Economies of Scale, Concentration, and Collusion. Mueller (1980, p. 5) states his objective as follows: "What we seek to explore in this [61 page] study, therefore, is the extent to which the efficiency-concentration-collusion interrelationships can be disentangled using data as disaggregated as the LB data are." To disentangle the efficiency-concentration-collusion interrelationship, Mueller first specified a Cobb-Douglas production function and estimated its parameters by regressing value added (sales less materials) on payrolls, net plant and equipment/payrolls and (in some equations) advertising/payrolls, all in logarithms. He had to "assume that labor and capital stock are somehow pre-determined and exogenously given for any cross-section year." (p. 8) As an alternative measure of economies of scale he estimated a Cobb-Douglas cost function, regressing total cost on sales, both in logarithms. Both sets of equations were run for each of 108 LB industries, the observations being company LBs, 1974 data. The estimated alternative measures of scale economies are weakly and negatively correlated, which is an indication that the models, the data, or both are poorly specified. But a much more important criticism is that neither of these forms is or could be meaningfully specified. The

dependent variables, sales less materials and total costs, are determined by the interaction of supply and demand (and, importantly, by the companies' accounting systems). Therefore, even if the numbers used were meaningful measures of economic market values, there is no basis for determining whether the regressions trace out cost curves, demand curves, or the interaction of individual demand and cost curves. Thus there is no valid way to interpret the coefficients estimated.

Despite this serious, indeed crippling, limitation, Mueller correlates the mislabeled "scale economies estimates" with several measures of concentration, stating: "If scale economies account for high concentration we expect a positive correlation between [the measures of scale economies] and the various concentration measures." (p. 25) Since the measures could reflect demand as well as supply conditions or simply bad data and poorly structured models, the small and generally negative correlations that he finds cannot be meaningfully interpreted.

In the penultimate section of his paper (Chapter 4), Mueller attempts to distinguish collusion and economies of scale by constructing a model based on the assumptions that "each firm uses the same technology, each has the same cost function, C(x). We assume homogeneous products, and so each charges the same price, P." (p. 28) Given the broad FTC-SIC definitions of industries, these assumptions surely do not apply to the LB data set. After some mathematical manipulations, he concludes: "Thus, while efficiency and collusion factors affect the pattern of costs and sales in a predictable way, their effects cannot be separated without imposing further restrictions and looking at more data than observations on firms within a single line of business." (p. 33) He then imposes several additional assumptions that, in effect, specify all firms in each LB industry as homogeneous with respect to economies of scale, degree of collusion, and elasticity of demand. After further manipulations and assumptions about the values of various parameters, Mueller concludes:

> Attempts to estimate [the key equations] yielded disappointing results. Convergence was seldom achieved and even when achieved,

> the asymptotic standard errors often exceeded the regression coefficients. Moreover, whether or not congruence was achieved, the implications of the coefficients obtained were found to vary with the choice of deflator. (p. 47)

Thus the LB data are not even amenable to estimating what might be labeled (incorrectly) as economies of scale, without which the structure-performance studies cannot be used for policy purposes. Mueller's study, though, is useful because it illustrates the limits to which a well-motivated, imaginative and technically able investigator can put the LB data.

B. Structure - Performance Studies

In Structure-Profit Relationships at the Line of Business and Industry Levels, Ravenscraft (1982, p. 17) seeks to determine "whether profits rise with industry concentration when other structural variables, such as market share, are appropriately held constant. Also, what economic phenomena underlie the observed profit-market share associations?" Profit is defined as sales less direct (traceable) and allocated expenses divided by sales, as taken from the 1975 LB data for 3186 companies LBs. Profit at the industry level is a similarly measured number derived from the 1975 Annual Survey of Manufacturers for 258 industries.[28] Those dependent variables are regressed on the fourteen variables listed in Table 4 plus nine additional variables for the company LB analysis. The industry regressions exclude the variables that relate only to company LBs.

Ravenscraft's carefully constructed study is seriously marred by the conceptual and empirical shortcomings of the dependent variable, profit/sales, which also is used in several of the papers reviewed below as well as scores of others. This ratio is not a substitute for the conceptually meaningful price-cost margin (price less marginal cost/price -- the Lerner Index).[29] Aside from the problem that total recorded expense does not equal marginal cost, profit/sales importantly differs among companies according to their degree of vertical integration and the extent to which they employ capital (since the factor price of capital is not included as an

expense.) Including sales/assets (or assets) as an independent variable to account for the bias is likely to lead to inefficient and biased estimators because the accounting number for assets usually is a very poor measure of the economic value of capital.[30] Indeed, in Ravencraft's regressions (as in the other researchers' regressions), the coefficient of sales/assets is inexplicably significantly negative. It is likely that this coefficient, and probably many, if not most, of the others, is due to accounting-based biases.[31]

Aside from (or perhaps because of) the biased numbers used for the analysis, the meaning of the coefficients is difficult to assess. For the company LB regression, Ravenscraft (p. 11) finds that "statistically, the most important variables are the positive effect of higher capacity utilization (LBCU) and industry growth (GRO), with the positive effect of market share (MS) running a close third." LBCU is measured as the smaller of one or company LB 1975 sales/1974 sales; GRO is industry change in the value of shipments, 1976/1972. Thus, the greater the increase in company LB sales or in industry shipments, the greater the increase in measured profit per dollar of sales. Does this mean that the profit margin increased because expenses are less than proportionately variable? Or, were companies in expanding industries able to increase their sales price more? Or were industries with higher profit margins on sales able to grow faster?

Interpretation of MS is more important because this presumably bears on the profit-structure relationship in which Ravenscraft and others are interested. He expects a positive association of market share with profits for three reasons. "First, LBs with a large market share may have higher quality products or market power. ... Second, LBs with larger shares may be more efficient because of scale economies or because efficient LBs tend to grow more rapidly. Third, LBs with large market shares may be more innovative, or better able to develop innovations." (pp. 2-3) Clearly, these alternative explanations not only have diametrically opposed policy implications but they cannot be disentangled. Furthermore, why

should profits per dollar of sales provide a meaningful measure of any of these hypotheses?

Ravenscraft attempts to analyze the MS-profit/sales relationship further by regressing profit/sales on MS and MS times the other LB variables. With respect to advertising, he finds a negative coefficient for LB advertising/sales, a positive coefficient for industry advertising/sales, and a positive coefficient for the product of LB advertising and MS. Taking partial derivatives and assuming that the LB companies make up 47 percent of all industries, he calculated that profit/sales increases when MS is above 4 percent. (p. 17) Assuming that these numbers are valid economic measures, should they be interpreted as meaning that firms with more than trivial market shares charge higher prices, offer higher quality products, economize more on other selling expenses, have a greater investment in capital, or what? Interpretation of the other measured relationships are subject to similar uncertainties.

Thus, despite Ravenscraft's careful and imaginative econometric work, there is little that can be learned from his exercise, with one exception. He finds that when the company LB data are aggregated into industries, the results are quite different. The coefficient of the four-firm concentration ratio changes from significantly negative to significantly positive (probably because company LB market share is not included in the industry regression). Five of the coefficients of the fifteen other common variables change from significance to insignificance (three also change sign), and all but two of the balance increase in magnitude by from two to five times. It appears, therefore, that analyses using industry data may have produced misleading results. Or do the regressions using company LB data produce misleading results? Or are the companies' allocations to FTC-defined LBs causing the differences? Or perhaps the changes in the coefficients are due to differences in the independent variables specified in the company LB and in the industry regressions? At the least, these alternatives should lead one to wonder about the usefulness of the output of such exercises, no matter how well done, for public policy or business decisions.

Market, Firm, and Economic Performance is a very ambitious, sophisticated econometric study that attempts to analyze the determinants of the price-cost margins of company LBs, where the profits/sales dependent variable was measured as reported sales (including transfers) less only traceable expenses divided by sales. Martin (1981A) specified thirty-eight independent variables. Though he considers fifteen of these variables to be endogenous, he estimated simultaneous equations only for the LB market share, firm market share, and LB selling efforts variables. The two-stage least squares estimates were corrected for heteroscedasticity; the corrected and uncorrected coefficients are presented. The observations are 2297 company LBs for 1975. Excluded are LBs in industries described as "miscellaneous" or "not elsewhere classified," LBs that were not surveyed in 1974, and observations with nonpositive values for assets (since assets were converted to logarithms).

In the profit/sales regression, twenty-six of the thirty-nine coefficients (including the intercept) are statistically significant at the .05 level or better and five more at the .10 level.[32] Among the findings that might be of interest to the regulatory authorities, Martin finds a statistically significant (t over 3.1) positive coefficient for LB market share and a statistically significant (t over 5.7) negative coefficient for the Herfindahl concentration index. He concludes that "the positive impact of market share on profitability may reflect either market power at the LB level or the realization of scale economies." (p. 33) Thus, without further information (which is not obtainable from the LB data) this finding is of little public policy value. Martin explains the unexpected negative coefficient of the Herfindahl index with a variety of rationalizations (e.g., sub-optimal or excess capacity, X-inefficiency, increases in prices during an inflation, restrictions on exit following restrictions on entry, and market power expressed as reduced risk.)[33] But he does not consider the possibility that the significant negative coefficient also may indicated misspecification of the model or the use of data that do not reflect economic values or markets. The significant (t over 5.2) negative

coefficient of assets/sales is consistent with this explanation, since the sign of this variable should be positive.

Two additional facts support the inappropriate model or bad data hypotheses. One is the values of the dependent variable, sales less traceable expenses/sales, which range from at least -125.8 percent to more than 58.9 percent (each are averages of six extreme values). Negative returns are present in 16.5 percent of the observations. It seems very doubtful that these numbers could represent meaningful economic profit rates. Second, as shown in Table 4, many of the coefficients and t-statistics reported by Martin differ considerably from those reported by Ravenscraft (1982) even though they based their analyses on subsets of the same 1975 LB data. (Their dependent variables are somewhat different, since Ravenscraft deducted allocated expenses from sales while Martin did not; but they used thirteen independent variables that are almost identical.) These differences make descriptive statements about the profit-structure relationship of doubtful validity.

Some Early Results on the Concentration - Profits Relationship from the FTC's Line of Business Data seeks to test Demsetz's (1974) hypothesis that concentration-related economies of scale (efficiency), superior products or management, or luck is responsible for the often reported positive relationship between concentration and profit. To this end, Weiss and Pascoe (1981) regressed profits/sales on the variables listed in Table 4 plus an adjusted four firm concentration ratio (adjusted "to correct for non-competing sub-products, inter-industry competition, local or regional markets, and imports") (p. 2), an interactive concentration ratio equal to the concentration ratio times a ratio of consumer demand to total demand, imports as a percentage of domestic output plus imports less exports, and LB market share. The regressions were run with over 3000 observations, each a company LB, with 1974 and 1975 data separately. LBs that were new (not reported in 1974) and discontinued (not reported in 1975 or 1976) were excluded, as were extreme outlyers. The reported $R^2$s range between .04 and .07. The coefficients of market share are statistically

significant (t statistics over 5.6) and positive. Those for concentration are insignificant, except for the 1975 data when market share is excluded. The coefficients of the interactive concentration ratio is significant and negative in 1974 and not significant (though negative) in 1975. Thus market share dominates which, the authors state, "provides some support for the Demsetz-Mancke hypothesis, but it is not as compelling as it might have been if the relationship of margin with concentration excluding market share had been stronger." (p. 8)[34]

But Weiss and Pascoe do not state why their dependent variable, profit per dollar of sales of companies in different industries, is a relevant measure of any hypothesis. Nor can they explain why the coefficients of assets/sales, which is included to correct for the exclusion of the factor cost of capital is significantly (t statistic over 5.6) negative. In an effort to explain it, they regress assets/sales on the other independent variables. A negative statistically significant coefficient was found for the interactive concentration-consumer demand ratio and positive significant coefficients for the concentration ratio, distance shipped, import intensity, and advertising/sales. They offer some speculations, but state: "These are at most hypotheses to be tested on other data and, hopefully, with some theoretical basis." (p. 11) I would suggest that the unexplained negative relationship between profit/sales and assets/sales is a measure of the divergence between economic values and accounting numbers.

Multimarket Contact and Economic Performance is concerned with measuring whether the coincidence of firms in several markets (multimarket grouping) is associated with higher profits and, if so, whether these higher profits result from coordinated behavior, lower costs, or both. To answer these question, Scott (1981) constructed a "new method of assessing multimarket contact." (p. 3) For each of twenty-four lines of business (1974 data) he paired 492 competing firms (246 pairs), based on the unverified assumption that firms with sales in the same FTC-defined line of business necessarily were in competitive contact. Then he recorded the

number of other lines of business each reported and the number they reported in common. This "frequency of contact" was compared to the number expected if common LBs were randomly determined. He finds "significant multimarket contact among sellers" (p. 8), as evidenced from a greater number of observed common LBs (37 percent of the pairs) than are expected by chance (.10 level). But considering that firms are more likely than not to produce related products, this result is not surprising. Also, since fewer than the expected number of common LBs (.10 level) were found for 31 percent of the pairs, the distribution is strongly bi-modal. This result, I suggest, is more surprising.

The fifty-one pairs of firms that had more contacts than expected by random selection at the .01 level were then subjected to further tests. He finds "significantly less advertising and R&D intensities [ratios to sales] for lines of business where significant contact occurred." (p. 13) Apparently, firms that operated in related LBs experienced economies for some reason. In another test, Scott regressed operating profit/sales on the variables listed in Table 4 plus a measure of industry minimum efficient scale, a one-zero dummy variable for the probability of greater multimarket contact the sample median, and a one-zero dummy variable for a four firm concentration ratio above or below the sample median. The product of the dummy variables also was included as a variable to measure the interaction of higher contact and higher concentration. The 492 selected firms (246 pairs) are the observations. With respect to his hypothesis, Scott finds significantly (.10 level) greater profitability (profit per dollar of sales) that is about 3 percent higher for the higher contact-higher concentration variable.[35] But whether this is due to greater efficiencies, barriers to mobility, collusion, accounting and classification biases, a relationship between vertical integration and number of "contacts, or some other factor cannot be determined from the analysis. Furthermore, as Table 4 shows, there are considerable differences between several of the t statistics and signs of the coefficients Scott estimates and those estimated by

Weiss and Pascoe (1981), which renders his findings suspect.[36]

C. Research and Development

In Using Linked Patent and R&D Data to Measure Inter-Industry Technology Flows, Scherer (1981A) describes in detail the procedures he and his assistants used in gathering data on 15,062 patents issued to 443 large industrial companies during a ten-month period (June 1976 through March 1977) and in assigning each patent to one or more lines of business. The companies' 1974 R&D expenditures per the FTC's LB reports then were associated with the inventions. The paper is very clear and honest in describing the great difficulties that were experienced in making the required associations. Though he was most imaginative in attempting to solve the problems, Scherer's expression of them makes it clear that they were "solved" only by making some heroic assumptions that severely compromise the meaningfulness of the resulting numbers.

Aside from questions of the meaningfulness of the data, the essential question is why the analysis was undertaken in the first place. The introduction of the paper (pp. 1-2) Scherer states: "The motivation for developing these new data was straightforward. [Productivity declined in the 1970s followed by the previous decline in R&D.] ... The key questions remain, what quantitative links exist between R&D and productivity growth, and did the parameters of any such relationships shift between the 1960s and the 1970s?" The matrix of patents and R&D expenditures by LB that is presented at the close of the paper cannot answer this question, even if there were no questions about the validity of the data. Nothing in the paper relates expenditures on R&D to changes in productivity. Indeed, at best all that is learned is that expenditures on R&D were made in some FTC-defined industries and that these expenditures may have been related to the output of these industries and to other industries.

The Propensity to Patent uses the data just described because, Scherer (1980, p. 1) says, "they are a rich source of quantitative and qualitative infor-

mation on technological change," and, to a lesser degree, on monopoly power. To these ends, he regressed the number of patents issued to a company LB from June 1976 through March 1977 on eight technology class dummy variables, five variables measuring the type and scope of use, two variables designed to correct for some LB data reporting characteristics, the percentage of federal funding to total R&D expenditures, and a variable described only as "compositions of matter." All of the variables are multiplied by research and development expenditures and a constant term was added. The regressions were run with 1819 observations.

How this specification and the reported findings speak to the questions that presumably motivated the study is not clear. If the question asked is whether greater expenditures on research and development are positively related to a greater number of patents issued, an answer would hardly require a rigorous empirical study. It seems obvious that if more is spent on research and development, more patents are likely to be applied for and granted. Another unremarkable major finding is that there are differences among industry groups. None of the empirical data provide "information on technological change." Monopoly power is measured by the concentration ratio, which was used to test the hypothesis that firms in more concentrated industries use patents as a means of securing their positions. The statistically significant, negatively signed coefficient would seem to disprove this hypothesis. Or, a critic of business behavior could argue that more highly concentrated industries have erected such great barriers to entry that they need not engage in further patenting. But Scherer dismisses this finding as being due to the highly concentrated automobile industry, which has few inventions. Or there could be, and, given the quality of the data, are likely to be, other biases present, as noted above.

In Inter-Industry Technology Flows and Productivity Growth Scherer (1981B, 1982 published version p. 1) says that he "exploits a new, uniquely rich data source to analyze the relationships between research and development (R&D) and productivity

growth." Using the FTC's 1974 LB data, he associated R&D expenditures (cleverly, but given the nature of the data, necessarily crudely) with originating and using industries. He then regressed two productivity measures on the 1974 R&D expenditures divided by value added and aggregated into two-digit manufacturing groups.[37] The productivity dependent variables are "total factor productivity growth" (not otherwise described) and sales/labor expense, expressed as changes over 1948-66, 1964-69, or 1973-78. Other analyses present simple correlations of various measures of productivity growth with R&D by industry of origin and industry of use, broken down into "well measured" and "poorly measured" industry aggregates.

Unfortunately, Scherer infers causation from the correlation of R&D expenditures and productivity without providing a supportive conceptual or empirical basis. For example, an industry could have higher measured productivity growth in a period which yields higher present or expected taxable profits. As a consequence, it might expend more on R&D because this is a tax deductible expense. Thus the higher productivity (and profits) could have "caused" the higher expenditures on R&D. Or its 1974 expenditures on R&D might have been financed from the fruits of past productivity, and directed towards reducing material and capital expenditures and developing new products, rather than towards further increasing labor productivity. In this event past productivity would be associated with present R&D expenditures, but these R&D expenditures would not be associated with future measured productivity. Or the regression coefficients simply may reflect mismeasurements. (The productivity measures are very crude.) And the amount expended on R&D is unlikely to measure the change in the stock, except in a steady state, at best. In addition, the industry aggregates are rather crudely defined. Therefore the regressions cannot provide evidence on the relationship of productivity and R&D or even on whether research productivity has declined over some time period. Nor is Scherer (1982 published version, p. 633) justified in concluding "that the social returns during the 1970s [on R&D investment] appear to have been quite high," since this

conclusion is drawn from the coefficients of the regression of the productivity measures on R&D expenditures. There is no way of knowing from these data whether there is a causal relationship or even what the social return might have been.

The Effects of Inter-Firm Cooperation and Economies of Scale on Product Improving Research and Deveopment Expenditures is introduced by Long (1981B, p. 1) as follows: "The purpose of this paper is to perform an empirical analysis of the determination of research and development expenditures for the improvement of the quality of manufactured products in the American economy for 1974 and 1975." For this purpose, he proposed a basic model of R&D behavior. After some simplifying assumptions, he derived several equations and ran several sets of regressions with data from 205 FTC-defined industries from 1974 and 1975 individually. It is difficult to evaluate the findings of this work, since it is incomplete. One thing that has been learned is that the relationship between R&D expenditures/sales and number of patents issued differs considerably among FTC industries and, remarkably, often is negative and differs in sign between years. It is not clear whether this is a function of some underlying structure or of the quality of the data. In any event, it seems clear that the stated purpose of the paper -- "the determination of research and development expenditures for the improvement of the quality of manufactured products" -- neither was nor, given the FTC LB data, could have been fulfilled.

D. Advertising and Selling

In The Size of Selling Costs, Weiss, Martin and Pascoe (1981, p. 5) seek "to explain selling expenses in manufacturing industries." For this purpose they regressed advertising/sales, other selling costs/sales, and total selling costs/sales (separately) on five independent variables. The observations are 260 FTC-defined industries (1975 data). The authors learn that all three measures of selling costs/sales are positive significant functions of consumer demand/total demand, the distance products are shipped, and profit/sales -- results that are not

surprising. But they also find that after about a 51 percent level of concentration for advertising and 39 percent level for other selling costs, the selling costs/sales ratios decline. The descriptive or public policy implications of this finding (assuming that it is valid) are unclear. Do companies in more concentrated LB industries incur less selling expense/sales because they have achieved efficiencies? Or do they compete less in terms of selling expenses? Or have they spent more on selling in the past and now need not maintain consumer acceptance, while companies in less concentrated industries have yet to inform (or possibly influence) consumers? These and other plausible hypotheses cannot be distinguished from the FTC LB data.

In Advertising Intensity, Market Share, Concentration and Degree of Cooperation, Long (1980, p. 1) wants to "assess the role that advertising plays in several explicit models of industrial organization and to formulate procedures for testing hypotheses which that assessment generates." To this end he developed a model which led him to estimate the following equation for each of 32 consumer goods "industries ": $\text{advertising}_i/\text{sales}_i = B_0 + B_1$ market share of i th firm, where i, ... n = firms in an industry. The regressions were run with 1974 company LB data and replicated in Long (1981) with 1975 data. While the $B_0$ estimated are quite stable and generally statistically significant, relatively few of the $B_1$ are significant, and many are inconsistent in magnitudes and even signs between years. Long (1980, p. 28) concludes: "The predicted relation between advertising intensity and market share shows up clearly in only 25% of the cases examined;" "some evidence concerning the presence and impact of cooperation was produced, but it is not clearly persuasive;" and "virtually no evidence concerning economies of scale in advertising can be gleaned from this study, given its assumptions." Whether the model tested has been largely disproved, was inadequately specified, or was unsuccessful due to the poor quality of the data used cannot be determined. I suggest that if one reads the SIC descriptions of the LB industries listed in Long's Appendix B,[38] considerable weight would be given to the last explanation.

## IV. Conclusion

Several important factors make aggregates of company accounting-determined data, such as those gathered by the FTC's Line of Business Program, useless, at best, for the purposes of economic analysis. Most important, perhaps, is the fact that the numbers reported are derived from the companies' accounting systems and do not reflect economic market values well. The biases caused by the divergences of accounting numbers from economic values not only are likely to be considerable, but also impart an unknown bias to most of the reported numbers, particularly profit and assets. Consequently, there is no reason to expect the errors to be randomly distributed or not yield measured, though invalid, relationships among the variables that purport to reflect economic performance of companies in markets.

An additional important error follows from the FTC's use of the SIC categories and from the procedures companies must or can follow in reporting their data. The SIC designated "industries" yield aggregates that are unlikely to be comprised of products that are substitutes in demand. The aggregation rules employed by the FTC for its Line of Business Program further bias the data, such that it is not clear how well the reported numbers relate even to the industry categories as described by the SIC manual. While some tests can be (and were) made of the extent to which the sales data are misallocated, there is no way for the FTC to determine the extent to which expenses and assets have been misallocated. Hence, the reported data are unlikely to be useful for any of the purposes delineated by the FTC or for any other purposes related to economic decisions.

A review of the reported underlying numbers reveals that the variance of such measures of profitability as profit/sales and profit/assets is great, much greater seems likely were these numbers meaningful economic measures of profitability. Improbably, a fairly high proportion of the numbers are negative, which indicates that companies are operating a substantial number of lines of business at a loss, in some cases at a very considerable loss. Furthermore, the profit rate reported

for a number of lines of business is measured as enormously high. I suggest that these data hardly are believable. It is doubtful, therefore, that analyses that use these data are likely to yield valid findings.

The twelve studies done by the FTC staff and consulting economists confirm this conclusion. Even aside from the data problems, the potentially important study on economies of scale is fatally flawed in concept. There is no way that the LB data can be used to estimate economies of scale, since supply and demand cannot be meaningfully separated. Without knowledge of economies of scale, there is no way for the profit-structure studies to separate the effects of efficiency from the effects of collusion on a measured positive relationship between profitability and market structure. But most of these studies use profit/sales as their measure of profitability. It is not clear at all why profit before interest and taxes per dollar of sales is a measure of "profitability" among companies in different industries (even if the products produced within the FTC-SIC designated industries were homogenous), nor why any relationship of this measure with market structure or other variables can have public policy or other implications. But these studies do provide some indication of the mismeasurement of the numbers, in that assets/sales are found to be strongly negatively related to profit/sales, while the reverse should be the situation. Those studies that relate current expenditures on research and development to patenting and to productivity infer causal relationships when there is no reason to expect them from the nature of the data that are or could be used. The studies concerned with selling expenditures are useful only in providing some descriptive data. Considering the limitations of the relatively well done economic studies that use the considerably improved FTC LB data, it seems likely that other studies using less detailed data and often less sophisticated statistical methods are likely to be worse.

A final question that should be confronted is whether meaningful empirical work on the relationship between structure and performance is feasible. The answer, I

believe, is that such work is useful, but not in the way much of it has been conducted. Bain's (1951) often cited paper has led many economists to undertake and publish a very large number of misleading studies. These papers present what purports to be evidence that concentrated markets are associated with abnormally high profits, when it is likely that the positive coefficients were simply the result of biases in the accounting data used. The consequence not only is a misunderstanding of the workings of the U.S. economy, but a possibly misguided antitrust policy that prohibits most horizontal and many other mergers.

If strong and convincing work on the relationships among profits, company size, market share, degree of competition and the like is to be conducted, it must take cognizance of the biases inherent in accounting and other data. I doubt, though, that the required adjustments to the data can be made, particularly when these are aggregated in ways that cannot take account of significant differences among supposedly homogeneous observations. Rather, I suspect that detailed company and industry studies, which require knowledge of the specifics and dynamics of events and institutions more than of econometric techniques, are needed.[39] In any event, continued sophisticated misuse of biased and irrelevant data is not the answer.

Table 1

The Effect of Some Divergences Between Economic Values and Accounting Numbers on the Ratios of Profit to Sales (P/S)[a] and Profit to Total Assets (P/A)[a]

U = understated, UU = very understated, O = overstated, OO = very overstated, N = not affected, ? = bias not known

| | Initial Period | | Later Periods | |
|---|---|---|---|---|
| | P/S | P/A | P/S | P/A |
| Capital Gains - One Time | | | | |
| Initially recorded assets and increases in present values | UU | U | O | OO |
| Capital Gains and Overdepreciation - Multiple Events | | | | |
| Stable - uniform distribution of events | | | N | O |
| Declining | | | O | OO |
| Growing | | | U | ? |
| Expected Inflation | N | N | O | OO |
| Unexpected Inflation | ? | ? | O | OO |
| Intangible Assets | | | | |
| Stable - current expenditures equal depreciation | | | - | O |
| Declining | | | O | OO |
| Growing | | | U | ? |
| Cost of Goods Sold and Inventories | | | | |
| Prices increasing steadily: | | | | |
| Last-in, First-out | | | N | O |
| First-in, First-out | | | O | O |
| Prices decreasing steadily: | | | | |
| Last-in, First-out | | | N | U |
| First-in, First-out | | | U | U |
| Overhead allocations among products | | | ? | ? |
| Allocation of Joint and Common Costs and Assets to Lines of Business | | | ? | ? |
| Intrafirm Transfers Among Lines of Business | | | | |
| Contamination due to Assignments to LBs | | | ?(b) | ?(b) |
| Transfers priced at other than market | | | ?(b) | ?(b) |

[a]Profit is before interest and income taxes

[b]+ probably not very great on average

Table 2

Reported Profit Rate of Sales (ps' . x) When
Costs are Understated or Overstated (e)

| Understatement (-) or overstatement (+) of costs (e) | Actual profit rate on sales (ps) or assets (pa) | | | | |
|---|---|---|---|---|---|
| | 0.05 | 0.10 | 0.15 | 0.20 | 0.30 |
| -0.30 | 0.34 | 0.37 | 0.41 | 0.44 | 0.51 |
| -0.20 | 0.24 | 0.28 | 0.32 | 0.36 | 0.44 |
| -0.10 | 0.15 | 0.19 | 0.24 | 0.28 | 0.39 |
| -0.05 | 0.10 | 0.15 | 0.19 | 0.24 | 0.34 |
| +0.05 | 0.00 | 0.06 | 0.10 | 0.16 | 0.27 |
| +0.10 | -0.05 | 0.01 | 0.07 | 0.12 | 0.23 |
| +0.20 | -0.14 | -0.08 | -0.04 | 0.04 | 0.16 |
| +0.30 | -0.24 | -0.20 | -0.11 | -0.04 | 0.09 |

Source: Calculated with formula derived in footnote 18.

Table 3

Profit[a]/Assets and Profit[a]/Sales
Ten Highest, Median and Lowest LB Industries, 1974 or 1975

A. Ten Highest in 1974 or 1975

| Profit/Assets | | | | Profit/Sales | | | |
|---|---|---|---|---|---|---|---|
| 1974 | 1975 | Difference | 1975/1974 | 1974 | 1975 | Difference | 1975/1974 |
| 41.8% | 15.0% | -26.8% | -64.0% | 28.1% | 27.3% | - 0.8% | - 2.8% |
| 41.5 | 25.5 | -16.0 | -38.6 | 19.0 | 25.5 | 6.5 | 34.2 |
| 40.3 | 36.4 | - 3.9 | - 9.6 | 24.2 | 24.3 | 0.1 | 0.4 |
| 39.8 | 38.1 | - 1.7 | - 4.3 | 24.3 | 24.2 | - 0.1 | - 0.4 |
| 0.0 | 39.2 | 39.2 | - | 23.8 | 21.4 | - 2.4 | -10.1 |
| 24.5 | 38.2 | 13.7 | 55.9 | 22.6 | 22.2 | - 0.4 | - 1.8 |
| 25.0 | 35.0 | 10.0 | 40.0 | 22.1 | 15.4 | - 6.7 | -30.3 |
| 34.1 | 29.5 | - 4.6 | -13.5 | 19.7 | - 5.8 | -25.5 | -129.4 |
| 31.0 | 22.2 | - 8.8 | - 28.4 | 18.1 | 19.6 | 1.5 | 8.3 |
| 29.6 | 20.2 | - 9.4 | 31.8 | 19.1 | 16.9 | - 2.2 | -11.5 |
| -11.6 | 29.5 | 41.1 | 354.3 | 13.2 | 19.0 | 5.8 | 43.9 |
| 29.1 | 19.0 | -10.1 | -34.7 | 11.9 | 18.4 | 6.5 | 54.6 |
| 28.9 | 24.1 | - 4.8 | -16.6 | 6.3 | 18.3 | 12.0 | 190.5 |
| 22.5 | 28.9 | 6.4 | 28.4 | 18.1 | 14.2 | - 3.9 | -21.5 |
| 18.7 | 28.7 | 10.0 | 53.4 | | | | |
| 18.2 | 28.0 | 9.8 | 53.8 | | | | |
| 27.3 | 5.8 | -21.5 | -78.8 | | | | |
| Mean absolute | | 14.0 | 56.6[b] | Mean absolute | | 5.3 | 38.6 |

a = operating profit before interest and taxes

b = does not include missing value

Source: Federal Trade Commission, 1981A and 1981B.

Table 3 (continued)

B. Ten Median in 1974 or 1975

| Profit/Assets | | | | Profit/Sales | | | |
|---|---|---|---|---|---|---|---|
| 1974 | 1975 | Difference | 1975/1974 | 1974 | 1975 | Difference | 1975/1974 |
| 12.1% | 22.3% | 10.2% | 84.3% | 12.7% | 7.8% | -4.9% | -38.6% |
| 12.2 | 18.1 | 5.9 | 48.4 | 7.6 | 12.4 | 4.8 | 63.2 |
| 11.8 | 16.9 | 5.1 | 43.2 | 7.8 | 11.5 | 3.7 | 47.4 |
| 11.7 | 15.3 | 3.6 | 30.8 | 11.2 | 7.5 | - 3.7 | 33.0 |
| 14.4 | 11.9 | - 2.5 | -17.4 | 7.7 | 9.7 | 2.0 | 26.0 |
| 12.1 | 14.3 | 2.2 | 18.2 | 7.5 | 9.6 | 2.1 | 28.0 |
| 12.1 | 13.5 | 1.4 | 11.6 | 8.1 | 9.4 | 1.3 | 16.0 |
| 13.2 | 11.7 | - 1.5 | -11.4 | 8.0 | 9.4 | 1.4 | 17.5 |
| 12.7 | 11.7 | - 1.0 | - 7.9 | 7.9 | 9.1 | 1.2 | 15.2 |
| 12.1 | 12.2 | 0.1 | 0.8 | 7.7 | 8.7 | 1.0 | 13.0 |
| 6.9 | 12.2 | 5.3 | 76.8 | 8.6 | 7.5 | - 1.1 | -12.8 |
| 4.4 | 12.2 | 7.8 | 177.3 | 7.5 | 7.8 | 0.3 | 4.0 |
| 11.0 | 12.1 | 1.1 | 10.0 | 3.9 | 7.8 | 3.9 | 100.0 |
| 10.2 | 12.1 | 1.9 | 18.6 | 7.1 | 7.6 | 0.5 | 7.0 |
| 12.1 | 9.3 | - 2.8 | -23.1 | - 4.3 | 7.6 | 11.9 | 276.7 |
| 6.6 | 12.0 | 5.4 | 81.8 | 6.3 | 7.5 | 1.2 | 19.0 |
| 9.7 | 11.9 | 2.2 | 22.7 | c | 7.7 | - | - |
| 5.7 | 11.9 | 6.2 | 108.8 | c | 7.6 | - | - |
| 11.8 | 11.3 | - 0.5 | - 4.2 | | | | |
| 11.8 | 9.6 | - 2.2 | 18.6 | | | | |
| Mean absolute | | 3.4 | 40.8 | Mean absolute | | 2.8 | 43.0 |

c = not reported in 1974

Table 3 (continued)

C. Ten Lowest in 1974 or 1975

| Profit/Assets | | | | Profit/Sales | | | |
|---|---|---|---|---|---|---|---|
| 1974 | 1975 | Difference | 1975/1974 | 1974 | 1975 | Difference | 1975/1974 |
| 8.2% | -14.7% | -22.9% | -279.3% | - 9.0% | -1.9% | 8.1% | 9.0% |
| -12.4 | - 4.9 | 7.5 | 60.5 | - 2.6 | - 8.3 | 5.7 | 219.2 |
| -11.7 | - 2.6 | 9.1 | 77.8 | - 6.9 | - 2.9 | 4.0 | 58.0 |
| -11.6 | 29.5 | 41.1 | 354.3 | 3.0 | - 6.6 | - 9.6 | -320.0 |
| -10.1 | 1.5 | 11.6 | 114.9 | 19.7 | - 5.8 | -25.5 | -129.4 |
| - 2.8 | - 9.2 | - 6.4 | - 63.4 | - 4.3 | 0.8 | 5.1 | 118.6 |
| 4.5 | - 8.9 | -13.4 | -297.8 | - 4.3 | 7.6 | 11.9 | 276.7 |
| 4.5 | - 3.7 | - 8.2 | -182.2 | 3.7 | - 3.2 | - 6.9 | -186.5 |
| - 3.4 | 4.8 | 8.2 | 241.2 | - 2.0 | 0.2 | 2.2 | 110.0 |
| - 3.2 | 2.6 | 5.8 | 181.3 | 4.7 | - 1.9 | - 6.6 | -140.4 |
| 6.9 | -3.1 | -10.0 | -144.9 | - 1.8 | 2.7 | 4.5 | 250.0 |
| - 3.0 | 0.3 | 3.3 | 110.0 | 5.7 | - 1.8 | - 7.5 | -131.6 |
| 12.8 | - 2.8 | -15.6 | -121.9 | 5.0 | - 1.6 | - 6.6 | -132.0 |
| 8.0 | - 2.4 | -10.4 | -130.0 | - 1.3 | - 0.3 | 1.0 | 76.9 |
| - 2.0 | 2.7 | 4.7 | 235.0 | - 1.1 | 1.6 | 2.7 | 245.5 |
| 6.4 | - 1.8 | - 8.2 | -128.1 | - 1.0 | 2.3 | 3.3 | 330.0 |
| - 1.6 | 3.9 | 5.5 | 343.8 | | | | |
| Mean absolute | | 11.3 | 180.4 | Mean absolute | | 6.4 | 170.8 |

Table 4

Comparison of Coefficients and Absolute t Statistics (in parentheses)
Estimated in Four Studies using FTC-LB Data

| | 1974 Data | | 1975 Data | |
|---|---|---|---|---|
| Same Variables | Ravenscraft[a] | Martin[b] | Weiss/Pascoe[c] | Scott[d] |
| Market share | 14.76<br>( 5.51) | 10.09<br>( 2.98) | -0.20<br>( 6.47) | 0.11<br>( 1.8 ) |
| 4-Firm concentration ratio | -0.02[e]<br>( 1.77) | -0.11<br>( 5.77) | | |
| Diversification (firm) | -1.43[e]<br>( 1.65) | -1.99<br>( 1.30) | | |
| Minimum efficient scale (industry) | .18[e]<br>( 2.05) | 0.39<br>( 2.97) | | |
| Exports/Shipments (industry) | 6.51<br>( 1.72) | 11.25<br>( 2.32) | | |
| Imports/Shipments (industry) | -4.01<br>( 2.23) | -2.05<br>( 0.81) | | |
| Buyer concentration index (industry) | 5.52<br>( 4.48) | 0.08<br>( 3.60) | | |
| Supplier concentration index (industry) | -31.40[e]<br>( 1.39) | - .09<br>( 2.58) | | |
| Buyer dispersion | -0.46<br>( 0.64) | -6.13<br>( 4.00) | | |
| Supplier dispersion | -4.59<br>( 2.86) | -7.20<br>( 2.79) | | |
| R&D expenditures/sales (LB) | -0.47[e]<br>( 3.68) | -0.31<br>(( 5.62) | | |
| Assets/sales (LB) | -2.40<br>( 2.82) | -5.33<br>( 6.75) | -0.032<br>( 5.64) | -0.0082<br>( 0.59 ) |
| Assets/sales (industry) | 6.00<br>( 4.98) | 1.87<br>( 1.31) | | |
| Similar Variables | | | | |
| Selling expense (direct)/ sales (LB) | | -0.30<br>( 6.74) | | |
| Advertising/Sales (LB) | -0.02[e]<br>( 0.35) | | 0.123<br>( 1.90 ) | -0.083<br>( 0.54 ) |
| Growth in demand (shipments) | | | 0.061<br>(10.76 ) | 0.046<br>( 3.1 ) |
| Distance goods shipped | | | - 0.000018<br>( 3.07 ) | -0.000021<br>( 1.2 ) |

Table 4 (continued)

| | Ravenscraft | Martin | Weiss/Pascoe | Scott |
|---|---|---|---|---|
| Different Variables | | | | |
| Number of additional variables | 9 | 24 | 3 | 3 |

Notes:
a. Ravenscraft, 1982, Table I., 3186 observations
b. Martin, 1981A, Table 15, corrected for heteroscedasticity, 2297 observations
c. Weiss and Pascoe, 1981, Table 1, equation (4), 3043 observations, scale not given
d. Scott, 1981, Table 7, equation 7-2, 480 observations, scale not given
e. rescaled to Martin's magnitudes

## FOOTNOTES

1. See Weiss (1974) for a review of 46 papers, a presentation of a 47th, and a footnote mention of 8 more, which he notes excludes studies on banking markets. For a review of 15 additional studies on banking markets, see Benston (1973). Weiss (1970) also reviews many of these studies but does not find them largely inadequate, as I do. Since the time of these surveys, many more similar papers have been published, including some of those reviewed below.

2. See Scherer (1979B), who shows how the industry output and price statistics used by Peltzman are likely to be biased. Scherer argues that product innovations rather than production efficiencies are causally related to higher profit rates. Interestingly, he doesn't provide similar (or any) evidence showing that the higher profits are due to collusion.

3. A resolution of the alternative (efficiency) explanation of the data might also have been a goal, but it was not mentioned in the FTC's Statements of Purpose. See Benston (1979, pp. 61-65) for a review of these statements. Also see Federal Trade Commission (1981A, pp. 1-7), which repeats the purposes of the program and does not include mention of the efficiency-profits-structure hypothesis.

4. See Federal Trade Commission, 1981A, pp. 1-6.

5. In the FTC's previous releases, which also stress these purposes, mentioned additionally are improvements in the government's efforts to control inflation and unemployment, aids to small business firms and to buyers and sellers of goods, facilitation of labor unions' bargaining by providing them with information on profits, and information to farm groups that will help them in dealing with suppliers and processors. For references to the relevant documents and critique of the aims of the program, see Benston (1979). Also see Breit and Elzinger (1981). For an opposing view, see Scherer (1979A).

6. The poor conformance of four-digit SIC-defined markets (which the FTC uses to define lines of business) to homogeneous industries is discussed and demonstrated by Weiss (1974, pp. 194-196), Bock (1975) and Benston (1979, pp. 66-78), among others. Therefore, though this problem is severe, it is not analyzed further here. An additional bias is the restriction of the program to large firms. However, this concern is not too serious as long as readers realize that the generalizations derived from the analyses are limited to this sub-set of companies.

7. For a much more complete, but differently structured and directed analysis, see Benston (1982).

8. For example, the 1970s increase in the market price of oil increased the values of oil and gas reserves, substitutes such as coal, and fixed-price contracts to purchase these resources.

9. See Solomon (1970) and Stauffer (1971) for formal proofs.

10. Ayanian's (1975) discussion and illustrations show how difficult it is to make any a priori statements, even when constant rates of growth and decline of intangibles are assumed. Solomon (1970) also presents an excellent discussion.

11. The 1975 data are similar; see Federal Trade Commission (1981B), pp. 54-59.

12. The original regression was run by Ravenscraft (1982).

13. Long (1980) also reallocated nontraceable advertising and other selling expenses and recalculated the equations he had calculated for another paper. (Long, 1980). However, only an average of 3.6 percent of these costs are nontraceable and few (less than 10 percent) of the regression coefficients estimated are significant at the 5 percent level. Hence, though he found few significant changes in the coefficients and other statistics, the finding is not of much value for the present question. Martin (1981B) uses alternative definitions of total assets (e.g., traceable and traceable plus nontraceable) as variables in regressions. Since he does not employ alternative allocation methods for nontraceable items, his results are not relevant here.

14. Federal Trade Commission (1981A), pp. 46-53. The 1974 data are similar.

15. Ravenscraft (1982) reports weighted averages of 50.7%, 25.2%, 9.4% and 14.7%. He also states that "By far the two most common classifications in 'other' are negotiated value and market, list, or wholesale price less discount.") (Footnote 2). These methods apparently were used in about a third of the "other" lines of business. Though these methods seem to be close to market values, his regressions indicate that, in relation to market price, transfers at "other" prices were lower than transfers at cost or cost plus markup.

16. The revaluation factor was calculated by dividing those industry sales and transfers that were valued at market by the cost of goods sold plus general and administrative expenses and less the amounts transferred at other than market. By this method, the profits of 22 percent of the LBs were reallocated.

17. Curiously, the coefficients of transfers out at cost and cost plus markup change from significantly negative to significantly positive.

18. The following symbols are defined for this analysis:

S = sales

C = costs

P = S - C = net profits

ps = P/S = profit rate on sales

e = error in costs due to arbitrary allocations, etc., as a percentage of costs, C

ps' = reported profit rate on sales when costs are over- or understated by e

$$ps' = \frac{S - C - Ce}{S} = ps - e + eps$$

Since net profits/assets (pa) is equal to net profits/sales (ps) times sales over assets and neither sales nor assets are affected by errors in costs, the effect on pa is the same as the effect on ps. The effect on the profit rate on sales or assets also can be expressed as an amount, x', rather than as a percentage. In this event, the equation would be $e(ps - 1) = x'$.

19. A "basic component" is defined by the LB program as the organizational sub-unit which reports data. A corporate respondent will typically have a number of components, and in many cases they will cover products classified by the FTC in different LBs.

20. If at least 85 percent of a component's sales is not accounted for by products classified in the same LB, the component must be further refined until the 85 percent rule is met.

21. The ratios are described and the figures reported in Federal Trade Commission, 1981B, pp. 41-46. The 1974 figures are similar.

22. Federal Trade Commission, 1981B, p. 43.

23. For more detail on this issue, see Benston (1975) and Bock (1975).

24. Several of them have been published as noted in the references.

25. See Benston (1979, pp. 110-114) for an estimate of the prospective costs. Net of benefits (which were considered to be zero), the present value of the program's cost was estimated to be $210 million.

26. For example, Weiss (1979, p. 1122) reveals: "An empirical test by Hall and me [Hall and Weiss, 1970] apparently yielded a mild but significant positive relationship between firm size and profit rates among the few hundred largest industrial firms -- a relationship which would have implied a capital-requirements barrier. I am more skeptical now because in subsequent unpublished work I have often found the relationship not to be significant."

27. Even then, economies of scope are ignored due to the separation of company data into FTC-designated lines of business. Furthermore, other efficiencies could be responsible for higher profit rates, as Peltzman (1977) suggests. Or efficiencies in the development and/or marketing of new products could be responsible, as Scherer (1979B) suggests.

28. The regressions were replicated with 1974 and 1976 data. With a few exceptions, the coefficients are not significantly different among the samples, and almost all were statistically significant at the .10 level, in part because, as Ravenscraft (p. 8) states, "variables insignificant at the 10 percent level were eliminated by a stepwise procedure."

29. See Liebowitz (1982), who also shows empirically that the empirical price-cost margin is not a good proxy for either accounting profit over assets or equity or a constructed measure of economic profits over sales which ignores the accounting biases. Also see Scherer (1979B, pp. 200-205) for a demonstration of the biases introduced into industry numbers by the incorrect assumption of homogeneity.

30. For example, oil and gas producers severely undervalue their major asset, oil and gas reserves, because they do not revalue their assets to market values. Worse yet, large companies use successful efforts accounting, wherein only the exploration and development expenses of successful wells are capitalized. Their cost of sales, then, is understated if they use owned oil. But if they use imported oil (which, if purchased, is valued at market), these expenses are higher. If they are decreasing their exploration, drilling and development activities,

Footnote 30 (continued)

these expenses will be lower. Thus, how can the level of their profit rate on sales or, say, the coefficient of the variable sales/assets, be interpreted? Similar analyses could be made about any company or industry about which one is knowledgeable.

31. An additional indication of such biases is given in Table 4, which compares the coefficients estimated by Ravenscraft with those estimated by Martin (1981A), who used similar data.

32. Assets/sales has a (strongly significant) negative sign. Martin (1981B) explored this unexpected result more fully by running regressions with six alternative definitions of assets. Only the coefficient of log assets was affected much. Assets defined as all except inventories fit to the data best.

33. Martin also recognizes that the profitability measure (the dependent variable) does not reflect profit but the profit rate on sales; hence, the negative sign "may reflect a more than proportionate impact of market power on sales as compared to profit." (p. 33)

34. They also find that "distance shipped" (a measure of geographic market share) is statistically significant (and negative). This is curious since this variable is an SIC industry-wide index based on 1963 data, which should be only tenuously related to the company LB dependent variables.

35. High contact and high concentration alone are negative and not significant coefficients; both are positive at the .05 and .10 levels.

36. Differences in the coefficients' magnitudes could be due to the scaling of the variables; the means are not reported.

37. The model calls for percentage changes in the stock of R&D, capital/labor and materials/labor. The R&D expenditure was assumed to measure the first variable; the other two were omitted.

38. For example, the first five "industries" are the following:
    meat packing, sausages and other prepared meat products, including bacon, ham, canned meats, and smoked and fresh meats
    diary products excluding fluid milk, including butter, cheese, condensed and evaporated milk, and ice cream
    canned specialties, including baby food, baked beans, ethnic foods, health foods, and soup
    frozen specialties, including baked goods (except breads), dinners, pizza and waffles
    canned, dried, dehydrated, and pickled fruits and vegetables, including preserves, jam, jellies, dehydrated soup mixes, vegetable sauces and seasonings, and salad dressings.

39. Scherer's (1979B) delineation of the reasons for increased concentration both illustrates the type of analysis I have in mind and further supports the conclusion that statistical studies which associate variables aggregated by FTC-SIC defined industries are fraught with error and a considerable potential for misinterpretation.

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