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The Concentration-Margins Relationship Reconsidered

THIS PAPER DISCUSSES an old and much-maligned topic: the cross-sectional relationship between the concentration of firms in the marketplace and price-cost margins.[1] Because it is hard to imagine a literature for which modern graduate students in economics are taught to have more contempt, some immediate justifications are in order. I have two. First, despite the well-known problems with this literature, it continues to affect antitrust policy. The inappropriate inferences used to justify an active antitrust policy have given way to equally incorrect inferences that have been used to justify a relaxed merger policy. Second, the alternative to cross-industry studies is to study specific industries. Indeed, the econometric analysis of individual industries has been labeled the "new empirical industrial organization."[2] Although this development is a healthy one, it is important to recall that it was the failure of studies of individual industries to yield general insights that made cross-industry studies popular. One might argue that the primary lesson from three decades of cross-sectional studies is that general principles based on simple indicators are not to be had. Nevertheless, the imprecisions

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1. For a recent review of the literature, see Schmalensee (1989).
2. Bresnahan (1989).

in single-industry studies make it useful to have some benchmark against which to judge the results.[3]

Whether it is market concentration or a firm's market share that determines a firm's profitability has been a focal point of the literature. As will be discussed later, this test fails to distinguish between the competing interpretations of the relationship between concentration and profits. To gain a partial understanding of how the literature became sidetracked on this issue, it is useful to review the history of the relationship between the literature and the policy implications drawn from it. Through the late 1960s there was a consensus that concentration increased profitability and facilitated collusion. In 1969 the so-called Neal report cited the findings of this literature in recommending an active policy of deconcentration.[4] One of the report's recommendations was to force the dissolution of companies with shares greater than 15 percent of markets in which the four-firm concentration ratio exceeded 70 percent. Although such legislation was never enacted, the Justice Department and the Federal Trade Commission brought several major monopolization suits in the late 1960s and early 1970s. These included the IBM case and the FTC's ready-to-eat breakfast cereal and titanium dioxide cases.[5] In all three cases the government alleged that firms had come to dominate their respective markets through means other than providing products that were better or less expensive or both than those sold by their rivals. In each case, however, a serious argument could

3. In a sense, cross-industry and single-industry studies can be thought of as Bayesian priors and experiments, respectively.

4. Bureau of National Affairs (1969).

5. The precise definition of the market that IBM was alleged to have monopolized changed during the case, but it was essentially the mainframe computer market. See Fisher, McGowan, and Greenwood (1983) for a discussion of the case; and McAdams (1982) for a different perspective.

The most important allegation in the breakfast cereal litigation was that the cereal companies had pursued a policy of brand proliferation to deter entry. The initial decision in the case is printed in *Federal Trade Commission Decisions* 16-269, vol. 99 (1982). For descriptions of the theory underlying the FTC's complaint, see Schmalensee (1978); and Scherer (1979).

As for titanium dioxide, DuPont developed a new technology for producing it that, with the changes in input costs in the 1960s, became the low-cost technology. DuPont thus increased its share of the market substantially. The FTC's allegation was that DuPont monopolized the market by pricing too low and failing to license its technology to other firms. See Commerce Clearing House (1980) for the dismissal of the complaint. See Holt (1988) for a description of the case.

be made that the dominance had been attained simply by superior performance.

Given the strong policy conclusions drawn from this literature, it is not surprising that objections were raised. If the correlation between measured profits and concentration does not reflect oligopoly profits, then either the difference between measured and true profits is correlated with concentration or the profits correlated with concentration are something other than oligopoly profits. The three other types of profits that they might be are short-term profits arising out of deviations from long-term equilibrium, Ricardian rents, or returns to innovative activity.[6] Although each explanation has appeared in the literature, the critique based on Ricardian rents has been the most influential.[7] The argument goes as follows. Suppose that all markets are perfectly competitive and that all firms have diminishing returns. In some markets, some firms have large cost advantages, and those firms are both profitable and large. Their size makes the market concentrated, which in turn creates a correlation between market concentration and market profitability. Harold Demsetz argued that this hypothesis implies only normal returns to small firms in concentrated markets, whereas oligopoly theories imply above-normal returns for such firms.[8] Demsetz presented evidence that it was only the large firms in concentrated markets that had high returns. When the Federal Trade Commission's Line of Business data became available, the standard test of the revisionist versus the traditional interpretation of the concentration-profits relationship was to in-

6. One might argue that the last source of "profits" is really measurement error. There are two levels to this argument. First, measured profits might simply be a normal return to past research and development expenditures. Second, they might represent the difference between expected profits ex ante and realized profits ex post. If one observes all firms that ever tried to enter the industry in question, this difference must be 0 on average. Because unsuccessful firms might exit, however, the difference for the firms in the industry at any given time might be positive. Whether one calls this phenomenon measurement error or the returns to successful innovation is, however, largely a semantic distinction.

7. For the argument that the correlation reflects short-term rents, see Brozen (1970, 1971a, 1971b). The argument that the relationship reflects Ricardian rents is generally associated with Demsetz (1973). Although he does not articulate the argument in quite this fashion, Peltzman (1977) makes the case that the relationship reflects a return to innovative activity. For the argument that profits are so badly measured that essentially nothing can be inferred from cross-sectional studies of profitability, see Fisher and McGowan (1985); Benston (1985); and Fisher (1987).

8. Demsetz (1973).

clude both market share and concentration in a profitability regression. The result from doing so was that the coefficient on market share was positive and statistically significant, and the coefficient on concentration was negative and significant but small.[9]

This idea has been extremely influential. For example, in a defense of the FTC's Line of Business program, F. M. Scherer and others consider the finding that market share rather than concentration determines firm profitability the most important result that has emerged from those data.[10] Although they emphasize that their conclusions do not by themselves resolve the traditionalist-revisionist debate, it is hard to imagine why else the findings would be deemed so significant. In his book on the effect of concentration on prices, Leonard Weiss cites Demsetz's critique as a "crucial criticism" of the concentration-profits literature and David Ravenscraft's finding of the relative importance of market share and concentration as the "clincher" in negating the force of these studies to provide evidence of oligopolistic behavior.[11] His only refuge was to suggest that perhaps 1974–76, the only years for which the FTC Line of Business data were available, were unusual. To cite just one more example, Richard Schmalensee decomposed Line of Business returns into industry effects, firm effects, and share effects.[12] While he was careful not to present his results as supporting one interpretation or another, a large coefficient on share effects and a small coefficient on industry effects would have been taken as evidence for Demsetz's hypothesis.

It is not clear why Demsetz's critique has been so influential. Perhaps it is because the paper was well written and it was thought that a simple test distinguished the critique from the traditional interpretation. Nevertheless, the various revisionist critiques have different implications for merger policy, which has supplanted deconcentration policy as the key issue in antitrust. If the concentration-profits relationship is caused by short-term or Ricardian rents, then even concentrated markets can be viewed as perfectly competitive, and mergers do not facilitate collusion and higher prices. If profits are returns to innovative activity, however, a short-term oligopolistic equilibrium supports the dynamic competitive

9. Ravenscraft (1983).
10. Scherer and others (1987).
11. Weiss (1989, pp. 7, 8).
12. Schmalensee (1985).

process.[13] Under such an assumption, an increase in concentration can be expected to cause prices to increase at least temporarily.[14] I will argue that the most sensible interpretation is that markets are dynamically but not perfectly competitive.

The remainder of this paper reviews methodological issues in more detail, then examines what has happened to the relationship between concentration and price-cost margins over time.[15] It is motivated by previous results that the strength of the relationship declined from 1972 to 1981.[16] From 1982 to 1984, however, the relationship was strong by historical standards. Next I examine the effects of changes in levels of concentration on prices, costs, and margins. The analysis closely resembles and updates work by Sam Peltzman, whose analysis covered the period 1954–72.[17] My analysis covers 1972–82 and reaches two main conclusions. The first, which is consistent with Peltzman's findings, is that increases in concentration are associated with cost and price decreases. This result is the heart of the claim that even if the relationship between concentration and profit margins reflects oligopolistic pricing, the process by which markets become concentrated is part of a dynamically competitive process that is beneficial to the economy. The second is that high levels of concentration were associated with cost and price increases from 1972 to 1982. This finding is consistent with other evidence concerning rent-seeking by workers.[18] Finally, I discuss the implications of the results for antitrust policy.

13. The underlying model I am suggesting might be labeled Schumpeterian. For a more modern and formal reference, see Dasgupta and Stiglitz (1980).

14. The welfare implications of allowing mergers that increase prices are ambiguous. If firms are allowed to merge, innovation may be more attractive. (On the other hand, it is not clear that more innovation would improve welfare.) Whether or not such a policy is appropriate, however, any merger that would be known to increase prices would probably be illegal under current enforcement standards.

15. Census of Manufacturers price-cost margins are usually estimated as (revenues − payroll − materials cost)/revenues. The difference between this measure and measures of profits is discussed later.

16. See Domowitz, Hubbard, and Petersen (1986a, 1986b).

17. Peltzman (1977).

18. See Salinger (1984); Rose (1985, 1987); Katz and Summers (1989); and Bartel and Thomas (1987). Also see the discussion and references in Schmalensee (1989).

Methodological Issues

The "plain vanilla" structure-performance regression has some measure of profitability as a dependent variable and some measure of concentration as an independent variable. In general, the rationale for a regression is to test some specific theory or to measure some parameter within a model. For example, one might imagine using such a regression to test the Cournot model of oligopoly. In a Cournot equilibrium,

$$L = H/\eta. \tag{1}$$

where L is the market Lerner index, H is the Herfindahl index, and η is the elasticity of demand.[19] Assuming that price-cost margins (PCM) approximate the Lerner index,[20] and that the four-firm concentration ratio ($C4$) is approximately a constant multiple, k, of the Herfindahl index,[21] one could test this relationship by obtaining estimates of demand elasticities and running the regression,

$$\ln PCM = \alpha_0 + \alpha_1 \ln C4 + \alpha_2 \ln \eta + \epsilon_1, \tag{2}$$

where ϵ_1 is an error term that is assumed to be orthogonal to $\ln H$ and $\ln \eta$. One could reject Cournot by rejecting the joint hypothesis $\alpha_0 = 0$, $\alpha_1 = 1/k$, and $\alpha_2 = 1$. Alternatively, if estimates of demand elasticities were not available, one could estimate

$$PCM = \beta_0 + \beta_1 C4 + \epsilon_2 \tag{3}$$

and take β_1 as an average value of $1/k\eta$ (assuming that $\beta_0 = 0$ cannot be rejected).

Regressions such as equation 3 have neither of these purposes. Because a wide variety of oligopolistic outcomes is possible, it would not

19. The Lerner index is $(P - MC)/P$, where P is price and MC is marginal cost. In what I have called the market Lerner index, marginal cost is a weighted average of the marginal cost of each firm, where the weights are proportional to each firm's output.

The Herfindahl index is the sum of the squares of each firm's share. It ranges from 0 to 1 (if shares are measured as fractions). If all firms are of equal size, the Herfindahl index reduces to the share of each firm.

20. For the long-term (short-term) Lerner index, this approximation is based on the implicit assumption that average cost equals marginal cost (short-term marginal cost equals average variable cost).

21. If all firms were of equal size, the ratio would be 4. For the ratio to be constant across markets, the distribution of firm sizes relative to the largest firm in the market must also be constant across markets. Salinger (1984, p. 165) reports a ratio of about 7.

be very surprising if any one of them were rejected. A general conjectural-variations model embodies all possible outcomes. Unless one makes very strong assumptions that essentially eliminate the generality of the model, however, β_1 cannot be interpreted as a parameter within that model. The parameters in a more complicated model could be given structural interpretations. Because these data are crude, however, complicating the regressions is a questionable strategy.

The rationale for a regression such as equation 3 is that a correlation between concentration and price-cost margins is at least roughly consistent with a broad class of oligopolistic models. As such, it provides some support for thinking of the markets in question as oligopolistic. That evidence would be stronger if the regressions could be given a structural interpretation, and it would be stronger still if a model of perfect competition implied $\beta_1 = 0$.

Demsetz's critique is that perfect competition might imply $\beta_1 > 0$ as well. Thus even though the findings of the literature are consistent with viewing markets as oligopolistic, they do not constitute a rejection of the natural alternative to oligopoly, perfect competition.

Even though a positive correlation between concentration and profitability could emerge if all markets were perfectly competitive and firms had diminishing returns, this interpretation of the findings is implausible. Its problem lies in the necessity of reconciling high market shares with the assumption of price-taking behavior. Under Demsetz's approach, firms must believe that if they reduce their output, some other firm will increase its output by an equal amount. Because upward-sloping supply curves are an essential part of Demsetz's argument, however, such an assumption is illogical even if it is assumed that other firms behave competitively. The only logical reconciliation is that at some price the market supply curve changes from upward sloping to flat. Such an assumption is difficult to refute, but it is hardly standard.[22]

As was mentioned in the introduction, the finding that a regression of firm profitability on concentration and market share yields a positive

22. The more typical approach to reconciling large market shares with competitive behavior is to assume contestability. Contestability results are not, however, robust to minor changes in the assumption that sunk costs are literally zero. See Stiglitz (1988). Moreover, the evidence from the airline market, which was the prototypical economist's example of a contestable market, tends to refute the contestability hypothesis. See Morrison and Winston (1987).

coefficient on concentration and a negative coefficient on market share has been taken as evidence in favor of Demsetz's hypothesis and against the traditional interpretation. This conclusion simply is not warranted. In a Cournot equilibrium, the Lerner index for an individual firm, L, is given by

$$L_i = S_i/\eta, \tag{4}$$

where S_i is the market share of firm i. That is, in a Cournot equilibrium, profitability is a function of market share, not market concentration. In fact, virtually any oligopoly model in which firm size is endogenous must have the feature that small firms earn low margins. A small firm earning a high margin would typically be able to increase its output without having a substantial effect on the market price.[23] Of course, the Cournot model does not explain the negative coefficient on concentration that David Ravenscraft found. The magnitude of that coefficient is, however, small. Moreover, the negative coefficient is not predicted by the differential-efficiency view either.

The Dependent Variable

In the early work in this literature the dependent variable was the accounting return on assets. Starting with N. R. Collins and L. E. Preston, however, the use of Census of Manufacturers price-cost margins became popular.[24] Price-cost margins are a proxy for the return on sales. Depending on the precise way in which they are measured, they might be interpreted as short-term or long-term returns on sales. Several recent papers have used Tobin's q, the ratio of market value to replacement value, as the measure of profitability.[25]

In choosing among the alternatives, one must first decide whether the objective is to measure the determinants of profitability or the nature

23. If all firms had equal costs and market share was simply a matter of historical accident, then small firms would have the same profitability as large firms. Alternatively, small firms may be those that have found small, profitable niches that are arguably understood as separate markets. A related possibility is that small firms might commit to being small to make it irrational for large firms to prey upon them. See Gelman and Salop (1983). It is implausible, however, that these cases are typical.

24. Collins and Preston (1969).

25. See Lindenberg and Ross (1981); Salinger (1984); Smirlock, Gilligan, and Marshall (1984); and Montgomery and Wernerfelt (1988). The first use of Tobin's q in a structure-performance study seems to be in Stigler (1964).

of short-term equilibria. The former interpretation of this literature is standard, but the latter may be the more relevant consideration for some policy issues. To ascertain the determinants of profitability, some normalization is necessary. If the cost measure excludes a normal return to investment, then assets are the appropriate normalization.[26] If a normal return to investment is included in costs, then either sales or assets can be used, but relationships such as equations 1 and 4 might dictate the use of sales. The use of Tobin's q is also appropriate for such studies: the general rationale is that it is measured more precisely than the return on assets.

Several recent papers, most notably one by Franklin Fisher and John McGowan, have argued that the problems with measuring economic depreciation are so severe that accounting measures of the return on assets cannot be assumed to bear any resemblance to the true return.[27] Even though Tobin's q is likely to be measured more precisely than the return on assets, the measurement of the denominator suffers from the same problem raised by Fisher and McGowan. In another paper Fisher has argued that accounting measures of the return on sales are similarly flawed, but the argument there is weaker.[28] Errors in measuring depreciation taint both the numerator and the denominator of the return on assets; they taint only the numerator in the return on sales. Moreover, because capital costs are typically a relatively small fraction of the total costs of production, the magnitudes of the errors are probably small.

The arguments about the errors in measuring profitability are based entirely on hypothetical examples. Whether the measures are so flawed that they render invalid any conclusions that might be drawn from cross-sectional studies of profitability, or whether they simply weaken results that would have been stronger had the measurement problems been less severe, is not known. The key to an appropriate test was put succinctly by Fisher and McGowan: "It is an economic rate of return (after risk adjustment) above the cost of capital that promotes expansion under competition and is produced by output restriction under monopoly."[29]

26. The rationale is that it is the return to assets, not the return to sales, that is equalized across industries (on the margin and after adjustment for risk).

27. Fisher and McGowan (1985).

28. Fisher (1987).

29. Fisher and McGowan (1985, p. 82).

The key issue in assessing a measure of profitability is that it is associated with the flow of resources. Thus one might judge the value of a measure of profitability by regressing some measure of the flow of resources, such as investment or output changes, on a hypothesized measure of profits. In his comprehensive study of rates of return in manufacturing, George Stigler found that the return on assets was not positively correlated with investment when a measure of cash flow was included in the regression.[30] Tobin's q is usually positively and statistically significantly correlated with investment. In general, however, the estimated coefficients are small and the fits of the equations are poor.[31] In some exploratory work for this paper, I found that Census of Manufacturers price-cost margins are statistically significantly correlated with investment. As with the results for q, however, the explanatory power of the regression was low. If one were to focus on the statistical significance of the results rather than the goodness of fit, then one could take these results as support for using either q or price-cost margins as measures of profitability.[32]

Even if long-term profitability cannot be measured, price-cost margins can arguably be interpreted as measures of the short-term return on sales, which in turn can be interpreted as the short-term Lerner index. The intractable problems in measuring profitability concern the treatment of durable assets. Insofar as these assets are sunk, they do not affect and therefore do not prevent the measurement of short-term

30. Stigler (1963, chap. 4).

31. In contrast to Stigler's results for return on assets, Fazzari, Hubbard, and Petersen (1988) find that q is correlated with investment even when measures of cash flow are taken into account.

32. One might object that such a test would be appropriate if all markets were competitive but that the presence of rents (either monopoly or Ricardian) implies that economic profits will not always be associated with expansion. This argument suggests a joint approach to testing for the validity of a profit measure and assessing the effects of concentration. Rather than correlating profitability with measures of market structure, let some measure of the flow of resources be the dependent variable in a regression and the hypothesized measure of profitability and concentration be explanatory variables. The combination of a positive coefficient on the profitability measure and a negative coefficient on concentration would suggest that in concentrated industries, high profits do not signal an expansion of output, which in turn would ultimately result in a price reduction. I tried this approach without much success for price-cost margins in exploratory work for this paper and for q in Salinger (1983).

marginal cost.[33] The relationship between concentration and the short-term Lerner index sheds light on the nature of short-term oligopolistic equilibria regardless of whether firms are earning true economic profits. That is, such an exercise is consistent with markets' being monopolistically competitive as well as truly oligopolistic.

Additional Variables

In equation 1 the only other determinant of the price-cost margin is the elasticity of demand. That equation is based on Cournot behavior. A more general treatment would include a measure of conjectural variations.[34] Finally, equation 1 assumes that potential entrants do not alter the behavior of the incumbents.

In testing or gauging equation 1, an obvious variable, other than concentration, to include is the demand elasticity for the product itself. This approach was tried by Dennis Mueller. However, he reports that independent estimates of demand elasticities seem to be uncorrelated with each other, which generates serious doubts about their reliability and usefulness.[35]

33. Domowitz, Hubbard, and Petersen (1987) also take the position that Census price-cost margins should be thought of as the short-term Lerner index. The major costs not deducted from Census of Manufacturers price-cost margins are general and administrative expenses, advertising, and capital expenses. While one might try to argue that general and administrative expenses are fixed, doing so would confuse the economic and accounting senses of that term. These expenses vary with the size of an organization in the long run. Whether they are sunk or variable is less clear. The exclusion of advertising expense is appropriate. Even when firms advertise, the costs that enter conditions such as equations 1 and 4 are production costs. See Dorfman and Steiner (1954). Capital costs are somewhat trickier. If the production function is continuously differentiable, then short-term marginal cost equals long-term marginal cost when the firm is in long-term equilibrium. Even if capital is sunk in these circumstances, average variable cost understates short-term marginal cost while average cost measures it correctly. If the production function is characterized by fixed coefficients, then average variable cost measures short-term marginal cost, provided that the firm operates below capacity. If the firm is at capacity, marginal cost is infinite. Although capital costs rarely enter the estimation of price-cost margins directly, a capital-sales ratio is sometimes included as an explanatory variable to adjust for the exclusion. In the estimation for this paper, tests for the sensitivity of the results to the treatment of capital and general and administrative expenses were performed.

34. Although some argue that conjectural-variations models are logically flawed, they are observationally equivalent to the behavioral factors that are universally considered important features of oligopolistic equilibria. See the discussion in Bresnahan (1989).

35. Mueller (1986, p. 66).

The demand elasticity that enters equation 1 is the elasticity of residual demand facing the firms included in the market, not the elasticity of demand for the product itself. That elasticity is likely to depend at least as much on entry barriers as on the pure demand elasticity.

As with the choice of dependent variable, the treatment of entry barriers turns at least in part on whether one is testing for the determinants of profitability or for the nature of short-term equilibrium. The appropriate definition of entry barriers differs in these two cases. For the former an entry barrier is a condition that allows firms in the market to charge a price above average cost without inviting entry. This definition was the one suggested by Joe Bain.[36] For the latter it is a condition that allows firms to price above short-term marginal cost without inviting entry.

The measurement of entry barriers has, however, proved elusive. Consider, first, the barriers relevant for ascertaining the determinants of profitability. Bain used subjective measures. Not only does such an approach necessarily generate irreconcilable questions of judgment, but it runs the risk of being tautologous. An entry barrier is defined as a condition that allows firms to earn abnormally high profits without attracting entry. Any researcher who is familiar enough with an industry to classify the level of entry barriers sensibly will also know its measured profitability. Such knowledge is bound to affect the entry barrier classification.

Objective measures have different but equally serious problems. If there are sunk costs, scale economies that are large relative to the market are an entry barrier. The standard measure of minimum efficient scale from Census of Manufacturers data is the size class of firms containing the median shipment. At best this measure captures plant-level scale economies, and it does not do that very well. The standard measure of product-differentiation entry barriers is advertising intensity. That measure is clearly endogenous and likely to be correlated with errors in measuring profitability.[37] Any credible theory of entry barriers (other than those imposed by the government) has sunk costs as one of its

36. Bain (1956).

37. See Dorfman and Steiner (1954). For attempts to account for the endogeneity of advertising econometrically, see Comanor and Wilson (1974); Martin (1979); Schmalensee (1972); and Strickland and Weiss (1976).

components. The measurement of sunk costs remains difficult, although the work of Ioannis Kessides represents a major advance.[38]

Any entry barrier that is relevant for the determinants of profitability is also an entry barrier for testing the nature of short-term equilibrium. In addition, scale economies alone (even without sunk costs) are an entry barrier for this purpose. As mentioned earlier, however, the cross-sectional measures of scale economies are at best crude.

Although a better treatment of entry would vastly improve this literature, it is not clear how the failure to treat it effectively or to treat it at all could create a spurious correlation between concentration and profitability. It seems plausible that concentration is correlated with entry barriers, so one might suspect omitted variable bias. Still, it would be surprising if exogenous increases in concentration did not result in price increases in those markets in which significant entry barriers exist.

Simultaneity Issues

That advertising intensity is endogenous in a profitability regression raises a general point that virtually all variables put into profitability regressions are endogenous. With only a handful of possible exceptions, the variables used are choice variables for the firm.[39] These variables are chosen to maximize a firm's profits. This problem is most acute when the firm or line of business is the unit of observation. Since a firm's profitability is a component of industry profitability, the problem also exists when the industry is the unit of observation.

A potentially major concern about the proper interpretation of structure-performance regressions is that concentration itself is endogenous.[40] Concentration depends on the output decisions of individual firms, which in turn affect prices. If a large firm chooses a higher output than is predicted by the underlying (implicit) model, concentration will be higher and profits will be lower than expected. Thus output errors

38. Kessides (1986).

39. Measures of the type of customer are arguably exogenous.

40. Because Demsetz's critique highlights the importance of the process that determines concentration, one might suspect that it implies that concentration is endogenous. In fact, however, Demsetz's argument is that more than one model is consistent with the standard structure-performance regression, not that the regression is misspecified, given either of the underlying models.

by large firms reduce the correlation between concentration and profitability. By the same line of reasoning, output errors by small firms increase the correlation between concentration and profitability. It is plausible that large firms have greater discretion over their output and that the magnitude of these errors for them is consequently greater. If so, the endogeneity of concentration causes a reduction in the measured correlation between concentration and profitability.

The endogeneity of concentration notwithstanding, it is likely that technology generates a substantial amount of exogenous cross-sectional variation in concentration.[41] Although there may be some technologically linked change in concentration within industries, much more of the intertemporal variation is likely to be caused by changes in conduct. Consider an industry with a group of dominant firms and a competitive fringe. Suppose the dominant firms sometimes collude to keep prices high and that the fringe expands during such periods. Periodically, however, the dominant firms engage in price wars, in which case the fringe shrinks. Prices and (probably) profits will be lower and concentration will be higher during the price wars. Thus the correlation between concentration and profitability can be negative. Such a finding would not suggest, however, that a merger between two of the dominant firms would result in lower prices and profits.

One variable that is likely to create serious endogeneity bias is import intensity. Although an exogenous increase in imports typically reduces profits of domestic firms, high profits create an incentive for greater import penetration. Thus a positive coefficient on import penetration in a profitability regression should not be particularly surprising.

Market Definition

The other major measurement problem that plagues this literature concerns market definition and, in turn, concentration. Because 4-digit standard industrial classification codes do not measure economically meaningful markets, concentration is measured with error. In some cases two or more SIC codes cover products that are such close sub-

41. The technological sources of concentration are both economies of scale and intraindustry variations in costs. The variations affect concentration under perfect competition, which was Demsetz's point, and under oligopoly, a point that Demsetz and his followers have overlooked.

stitutes for each other that they should be aggregated into one market. But some SIC codes cover more than one economic market. Because reported concentration is domestic national concentration, it can understate concentration when relevant markets are local and overstate it when they are global.[42]

What Happened to the Concentration-Margins Relationship?

As was mentioned in the introduction, the positive correlation between concentration and Census of Manufacturers price-cost margins is reported to have disappeared in the 1970s. Before addressing the proper interpretation of the concentration-margins relationship, it is important to know whether it still exists.

This study is based on a panel that contains annual data on shipments, costs, inventories, value added, and capital. For some of the variables, including shipments, the data set contains both nominal and real values. Thus it is possible to estimate the rate of price increase by comparing the ratio of the nominal value of shipments to the real value of shipments in different years. It also contains data on hours worked by production workers and total production wages, from which a production wage rate can be calculated. See the appendix for a more complete description.

The panel contains data for 1954–84, but most of the analysis in this paper is restricted to 1972–84. Standard industrial classifications were changed significantly in 1972. Although the Census of Manufacturers panel is apparently consistent in its treatment of industrial classifications, comparable concentration data are not available for the entire period. Thus one is left with the choice of using only data for 1972 and thereafter or restricting the study to those industries for which the SIC codes remained unchanged. Because the original objective in this paper was to address the experience of the 1970s and 1980s and because changes in industrial classifications are likely to reflect the types of important structural changes that might be of interest, I have chosen to use only data since 1972.

42. See Weiss and Pascoe (1986).

Table 1. Sample Means and Standard Deviations

	Levels			*Changes*[a]		
Variable	*1972*	*1977*	*1982*	*1972–77*	*1972–82*	*1977–82*
PCM	.268	.275	.281	.007	.013	.006
	(.089)	(.085)	(.099)	(.055)	(.071)	(.057)
C4	.372	.373	.372	.002	.003	.001
	(.202)	(.203)	(.201)	(.050)	(.073)	(.053)
C4AN (Weiss and Pascoe, 1986; not adjusted for imports)	.412	.414	.412	.001	.002	.001
	(.198)	(.198)	(.196)	(.048)	(.072)	(.053)
C4AI (adjusted for imports)	.395	.399	.387	−.005	−.012	−.007
	(.185)	(.186)	(.183)	(.054)	(.071)	(.054)
Wages	3.80	5.55	8.35	.456	1.18	.498
	(.876)	(1.42)	(2.35)	(.092)	(.217)	(.095)
Prices	. . .	. . .	. . .	.556	1.23	.445
	. . .	. . .	. . .	(.495)	(.549)	(.215)

Sources: Weiss and Pascoe (1986); and author's calculations based on data described in the appendix to this paper. Values in parentheses are *t*-statistics.

a. Absolute changes for price-cost margins and concentration measures, and percentage changes for wages and prices.

Table 1 contains means and standard deviations of price-cost margins, various concentration measures, wages, and price changes. The first concentration measure reported (*C4*) is the simple four-firm concentration ratio reported in the *Census of Manufacturers*. The second measure (*AC4N*) comes from Leonard Weiss and George Pascoe, who report concentration ratios adjusted for inappropriate industry classifications and localization of markets.[43] In the third measure (*AC4I*), *AC4N* is adjusted for imports.[44]

Evidence that the concentration-margins relationship weakened in the 1970s is provided in the first three columns of table 2. They contain the results of Ian Domowitz, R. Glenn Hubbard, and Bruce Petersen from annual regressions of price-cost margins on the unadjusted four-

43. Weiss and Pascoe (1986) also estimate the concentration ratios suppressed by the Census Bureau to avoid disclosure.

44. Specifically, adjusted domestic concentration ratio is multiplied by the imports-sales ratio. Thus the measure used here is not the import-adjusted concentration ratio reported by Weiss and Pascoe. Doing the adjustment directly makes it possible to take advantage of the annual variation in import intensity.

firm concentration ratio and the capital-sales ratio.[45] The authors emphasize a business-cycle interpretation of the differences over time. Examination of those results makes it clear that their interpretation is consistent with the data. The coefficient on concentration is lowest in 1974, 1975, 1980, and 1981, which are all years when the unemployment rate was high. Nonetheless, it would seem difficult to rule out the possibility that the decline simply represented a secular trend. The second group ($C4$) in table 2 reports comparable regressions from the data set used for this paper. They are reported to demonstrate that in the period of overlap, the results are similar to those of Domowitz, Hubbard, and Petersen. Although the drop is not quite as dramatic, the coefficients from 1973 to 1981 are generally lower than those from 1971 and 1972. The regressions also show that the concentration-margins relationship revived after 1981. Indeed, the coefficient is larger from 1982 to 1984 than it is in 1971 and 1972.

One other point to note about this group of results is the coefficient on the capital intensity variable. As in the results of Domowitz, Hubbard, and Petersen, it is positive throughout the 1970s and significant in most years. That result changes dramatically in the 1980s, when the coefficient becomes significantly negative. This result is rather puzzling. It would not be particularly surprising if the return on assets were negatively correlated with capital intensity in downturns. Because most of the costs captured in the price-cost margin are variable, it is not clear why a similar result with price-cost margins should arise. Moreover, the coefficients are negative in some expansion years.[46]

One conceivable explanation of a decline in the measured relationship between concentration and margins is that the errors induced by in-

45. Domowitz, Hubbard, and Petersen (1986a).

46. The estimated coefficients on concentration and on capital intensity also seem to be negatively correlated with each other. One might suspect, therefore, that collinearity between the two makes the estimates of the individual coefficients unstable and is the source of much of the intertemporal variation in the coefficients. As it turns out, however, concentration and the capital-sales ratio are not that highly correlated. A regression of concentration on capital intensity yields R^2's of roughly .05. The coefficient on concentration remains virtually unchanged when capital intensity is excluded from the regressions altogether. When the coefficient on capital intensity is restricted to .1, which is roughly the largest of the estimated coefficients on capital intensity reported in table 1 and which is arguably a plausible adjustment for the cost of capital, the magnitude of the estimated coefficients drops somewhat. However, the pattern remains the same.

Table 2. Annual Price-Cost Margin Regressions, 1971–84[a]

	Domowitz, Hubbard, Petersen (1986)[b]			*Domestic concentration (C4)*			*Adjusted domestic concentration (C4AN)*			*Import-adjusted concentration (C4AI)*		
Year	*C4*	*Capital-sales ratio*	*R^2 and number*	*C4*	*Capital-sales ratio*	*R^2 and number*	*C4*	*Capital-sales ratio*	*R^2 and number*	*C4*	*Capital-sales ratio*	*R^2 and number*
1971	.112	.038	.09	.099	.040	.09	.133	.033	.13	.177	.012	.15
	(4.48)	(2.11)	284	(4.64)	(3.02)	366	(6.12)	(2.55)	366	(6.26)	(0.74)	242
1972	.092	.049	.08	.081	.021	.04	.125	.014	.08	.180	−.000	.13
	(3.68)	(2.72)	284	(3.47)	(1.53)	364	(5.29)	(1.05)	364	(5.90)	(−0.03)	240
1973	.081	.079	.12	.077	.060	.09	.112	.052	.13	.155	.026	.13
	(3.38)	(4.39)	284	(3.64)	(4.25)	364	(5.24)	(3.76)	364	(5.51)	(1.56)	245
1974	.050	.098	.10	.059	080	.09	.079	.074	.10	.121	.054	.10
	(2.08)	(4.67)	284	(2.70)	(4.85)	364	(3.53)	(4.44)	364	(3.97)	(2.48)	245
1975	.050	.071	.06	.076	.045	.06	.111	.038	.09	.166	.015	.12
	(2.00)	(3.38)	284	(3.29)	(3.01)	364	(4.77)	(2.57)	364	(5.33)	(0.81)	246
1976	.063	.059	.05	.073	.049	.07	.106	.041	.10	.166	.018	.13
	(2.52)	(2.57)	284	(3.42)	(3.12)	363	(4.87)	(2.64)	363	(5.69)	(0.91)	245

1977	.061	.042	.03	.052	.021	.02	.095	.014	.05	.155	−.016	.10
	(2.44)	(1.75)	284	(2.33)	(1.29)	365	(4.22)	(0.85)	365	(5.39)	(−0.85)	250
1978	.066	.020	.03	.065	.023	.03	.101	.017	.06	.160	.010	.11
	(2.54)	(0.83)	284	(2.86)	(1.40)	365	(4.38)	(1.03)	365	(5.47)	(0.50)	252
1979	.065	.042	.04	.080	.028	.05	.109	.022	.07	.154	.007	.10
	(2.60)	(1.75)	284	(3.60)	(1.79)	364	(4.78)	(1.39)	364	(5.25)	(0.38)	253
1980	.059	.028	.02	.065	.002	.02	.096	−.004	.04	.147	−.017	.08
	(2.27)	(1.12)	284	(2.75)	(0.14)	365	(3.95)	(−0.26)	365	(4.73)	(−0.89)	255
1981	.047	.021	.01	.071	−.014	.02	.106	−.021	.05	.161	−.031	.10
	(1.81)	(0.81)	284	(2.95)	(−0.86)	365	(4.31)	(−1.28)	365	(5.11)	(−1.59)	253
1982	n.a.	n.a.	n.a.	.108	−.064	.08	.147	−.070	.11	.218	−.066	.17
	n.a.	n.a.	n.a.	(4.23)	(−4.71)	365	(5.73)	(−4.71)	365	(6.69)	(−3.84)	255
1983	n.a.	n.a.	n.a.	.112	−.040	.07	.147	−.046	.10	.199	−.050	.16
	n.a.	n.a.	n.a.	(4.70)	(−2.75)	365	(6.14)	(−3.18)	365	(6.44)	(−2.96)	255
1984	n.a.	n.a.	n.a.	.104	−.033	.05	.133	−.039	.07	.189	−.041	.12
	n.a.	n.a.	n.a.	(4.14)	(−1.82)	363	(5.21)	(−2.20)	363	(5.81)	(−1.94)	254

Sources: Domowitz, Hubbard, and Petersen (1986a); and author's calculations.
n.a. Not available.
a. The dependent variable is price-cost margins. Values in parentheses are *t*-statistics. In the 1971 regression the concentration measure is for 1972.
b. Provides data through 1981 only.

appropriate market definition have become more severe. For example, SIC code 2834 (pharmaceutical preparations) is a frequently cited example of a classification that groups together more than one relevant market. As a result, measured concentration for that classification understates the true concentration in the markets encompassed by it. Suppose that the margins in pharmaceuticals had become larger over time, perhaps because the cost of getting new drugs approved had increased or because the increase in third-party reimbursement had made demand less elastic. Since true concentration in pharmaceutical markets is high, such a change would increase the correlation between margins and true concentration. At the same time, however, it would weaken the correlation between margins and measured concentration.

The third group of results in table 2 (*C4AN*) is based on Weiss and Pascoe's domestic adjusted concentration ratios. These adjustments are, of course, imperfect. For example, in those cases in which 4-digit SIC codes are deemed too broad, Weiss and Pascoe adjust by taking averages of concentration ratios for 5-digit SIC codes. Such an adjustment is incomplete when even 5-digit codes are too broad. Again, the case of pharmaceuticals is instructive. Their adjustment raises the measured 1972 four-firm concentration ratio from 25 percent to 43 percent. In correctly defined pharmaceutical markets, however, the true concentration ratio is likely to be close to 100 percent. Nonetheless, the data of Weiss and Pascoe are clearly improvements over unadjusted concentration ratios.

As should be expected with such an improvement, the adjustments strengthen the correlation between margins and concentration. Moreover, although the size of the coefficients is lower from 1973 to 1981 than in 1971 and 1972, the decline is not as great as with the unadjusted data. For example, the coefficient on the unadjusted concentration ratio is about half as great in 1977 as in 1971, whereas the coefficient on the adjusted concentration ratio is only 29 percent less.

Perhaps the most plausible explanation for the decline in the relationship between margins and measured concentration is the increasing importance of imports. The automobile industry is a frequently cited example in which import competition has reduced industry profitability. The direction of the result is consistent with the structure-performance paradigm; a reduction in true concentration is associated with a reduc-

tion in profitability.[47] For the last set of results in table 2 (*C4AI*) the concentration measure is adjusted for imports. Although there is some softening of the relationship in the 1970s, the coefficients on the concentration ratio appear much more stable. Except for 1974 all the coefficients on concentration are within 15 percent of the 1971 and 1972 coefficients. As was the case with the unadjusted concentration data, the coefficient is actually greater from 1982 to 1984 than in 1971 and 1972.

The results reported by Domowitz, Hubbard, and Petersen could suggest that the debates about interpreting the concentration-profits relationship had been rendered moot. The opposing camps could be viewed as arguing about the interpretation of a relationship that no longer existed. Taken as a whole, however, the results in table 2 suggest that the concentration-profits relationship has been remarkably stable. In short, the one-word answer to the question that heads this section is "nothing."[48]

47. A closer examination of the data suggests that the automobile industry does not illustrate this point as clearly as one might initially expect. First, even with an import adjustment, the industry is still concentrated. Second, margins in the industry are still high. The persistence of the high margins results in part from import restraints, but these affect concentration as well. Thus the industry is not an example in which margins have dropped dramatically because of a dramatic decrease in true concentration.

48. As is discussed in note 33, Census of Manufacturers price-cost margins contain no deductions for general and administrative expenses. Moreover, the coefficients on the capital-sales ratio in most years are too small to adjust for capital costs. Because it is not clear whether either of these costs is part of short-term marginal cost, some sensitivity checks were performed. The adjustments for capital intensity are discussed in note 33. For two years (1977 and 1982), a rough adjustment for general and administrative expenses was made as follows. The IRS *Source Book of Statistics of Income for Corporation Tax Returns*, 1977 and 1982, gives industry data on expenses. IRS minor industries correspond roughly to 3-digit SIC codes. Following Peltzman (1988), "cost of sales and operations" was assumed to correspond to labor expense plus materials cost, and "other deductions" were assumed to correspond to general and administrative expenses. General and administrative expenses for the Census data were then estimated by taking the ratio of "other deductions" to "cost of sales and operations" and multiplying the result by the sum of materials cost and labor expense. This adjustment had no quantitative impact on the regression of margins on concentration.

Concentration, Changes in Concentration, and Changes in Prices and Costs

Even if one accepts the proposition that the concentration-margins relationship reflects at least short-term oligopolistic behavior, the policy implications turn on how concentration is determined. The Neal report's recommendation to break up dominant firms would seem to be based on the implicit assumption that concentration can be viewed as exogenous.[49] A natural alternative hypothesis is that concentration is the by-product of a dynamically competitive process in which firms compete to produce better products and lower production costs. Those firms that succeed in the competition gain short-term monopoly power. An informal test of this hypothesis is to see whether increases in concentration are associated with increases or decreases in prices. This is the general approach suggested by Sam Peltzman.[50]

Peltzman examined this issue indirectly by combining results on the effects of changes in concentration on changes in margins and changes in unit costs. His model embodied features that seem rather ad hoc, most notably the assumption that the relative change in efficiency between large and small firms depends on market growth. That assumption complicated the estimation procedure; as a result concentration entered the equation for changes in cost in complicated ways. Particularly, given the well-known imprecisions with these data, trying to detect complicated nonlinearities is a questionable strategy. Thus the estimation in this paper is a stripped-down version of Peltzman's model.

Table 3 reports regressions in which the dependent variable is the percentage change in price between 1972 and 1982.[51] In addition to structural variables, two independent variables are added to capture the effects of cost increases. If wages increase at a different rate than the price of other inputs, then labor intensity should be correlated with price increases. Thus one of the variables is labor expense at the beginning of the period divided by revenues. In table 3 no account is made of differential wage growth between sectors. The other variable

49. Bureau of National Affairs (1969).

50. This approach is also followed by Lustgarten (1984); and Gisser (1984).

51. None of the qualitative results reported here changes if the starting year is 1971 or the ending year is 1984. Thus the results are not biased by the 1972–74 price controls, and they are not sensitive to whether the ending year is one of recession or recovery.

Table 3. Relationship of Changes in Concentration and Changes in Prices, 1972–82[a]

	Constant	*ΔC4*	*|ΔC4|*	*Δ imports-sales ratio*	*Labor-sales ratio*	*Δ materials costs*	*R^2 and number*	*ΔC4 + |ΔC4|*	*ΔC4 − |ΔC4|*
1	.233	−.316	. . .	. . .	2.00	1.54	.21	. . .	. . .
	(1.82)	(−0.88)	. . .	. . .	(4.49)	(9.76)	366	. . .	. . .
2	.316	−1.89	−1.14	. . .	1.96	1.51	.22	−1.33	.950
	(2.37)	(−0.52)	(−2.10)	. . .	(4.41)	(9.51)	366	(−2.22)	(1.36)
3	.128	−1.00	−1.69	−.156	2.62	1.83	.32	−2.69	.689
	(0.72)	(−1.87)	(−2.18)	(−0.26)	(4.09)	(9.20)	215	(−2.67)	(0.79)

a. Dependent variable is percentage change in prices from 1972 to 1982. Values in parentheses are *t*-statistics. The ratio of labor to sales is defined as production worker wages divided by revenues.

is designed to capture changes in materials prices. Because the materials used vary across industries, it is important to capture differences in increases in materials prices, particularly in light of the change in oil prices between 1972 and 1982. Although the data set used in this study does not contain a materials-price index, it does contain real and nominal values for materials inventories. Under the arguably strong assumption that materials are inventoried in proportion to their use, a materials-price index can be calculated. This index is then multiplied by the ratio of materials costs to revenues at the beginning of the period.

Row 1 contains a regression of the change in price on just the change in concentration and the two cost variables. The coefficient on concentration is negative but insignificant. That result does not, however, fully address the issue of whether increases in concentration are associated with price increases or price decreases. Decreases in prices associated with decreases in concentration will tend to make the coefficient positive. However, Peltzman's hypothesis is that both large decreases and large increases in concentration are associated with cost-reducing structural changes. Thus the one nonlinearity from Peltzman's analysis that must be preserved is the separate treatment of increases and decreases in concentration. This is accomplished by including the absolute value of the change in concentration as well as the change in concentration as regressors.

The results are consistent with Peltzman's, even though they are for a different and shorter time period. The coefficient on the absolute value of the change in concentration is negative and significant and the coefficient on the change in concentration is negative but insignificant. They lend support to the idea that even though concentration is associated with the higher margins that may arise from collusion, the process by which markets become concentrated entails cost reductions that dominate any effects of collusion.

The changes in prices associated with changes in margins must be associated with changes in costs or margins or both. Table 4 presents the results of regressions of the change in margins on the change in concentration. Row 1 contains a regression of the changes in margins on changes only in domestic adjusted concentration. The coefficient on concentration is positive, significant, and roughly the same size as the cross-sectional estimates. The similarity of the coefficients when the concentration-margins equation is specified in levels and when it is

Table 4. Relationship of Changes in Margins and Changes in Concentration, 1972–82[a]

	Constant	*ΔC4*	*|ΔC4|*	*Δ imports-sales ratio*	*R^2 and number*	*ΔC4 + |ΔC4|*	*ΔC4 − |ΔC4|*
1	.013	.178	. . .	. . .	.03	. . .	. . .
	(3.41)	(3.49)	. . .	. . .	363	. . .	. . .
2	.006	.165	.118	. . .	.04	.283	.047
	(1.13)	(3.19)	(1.52)	. . .	363	(3.30)	(0.47)
3	−.008	.251	.147	.284	.09	.398	.103
	(−1.00)	(3.50)	(1.42)	(3.72)	213	(2.95)	(0.89)

a. Dependent variable is change in price-cost margins from 1972 to 1982. Concentration measure is adjusted domestic concentration in rows 1 and 2 and import adjusted concentration (*C4AI*) in row 3. Values in parentheses are *t*-statistics.

specified in first differences makes it plausible that the cross-sectional relationship observed at any time could have evolved from the dynamic effects captured by the results in table 4.

Row 2 includes the absolute value of changes in concentration as an independent variable. These results can be interpreted in two ways. The coefficient on $|C4|$ is not statistically significant. If one concludes that the coefficient should be considered 0, then the results suggest that decreases in concentration reduce margins by the same amount as increases in concentration increase them. Alternatively, the coefficient on $|C4|$ is nearly significant and of comparable magnitude to the coefficient on $C4$. In fact, the difference between the two is not statistically significant. That part of the result could be taken to indicate that decreases in concentration do not result in decreases in margins.[52] The

52. Peltzman (1977) argued that if all markets were perfectly competitive, then decreases in concentration should be associated with increases in margins just as much as increases in concentration are. His point was that any major structural change, be it concentration increasing or concentration decreasing, should be associated with higher margins. His result that decreases in concentration are not associated with increases in margins is his basis for adopting what he termed an eclectic view instead of accepting the hypothesis of perfect competition. The argument is not altogether persuasive, however. Suppose that markets are perfectly competitive and firms have diminishing returns. Furthermore, suppose the typical industry pattern is that some firms gain a large advantage, after which the other firms catch up. In the first stage, there is an increase in concentration and a reduction in prices. Provided that the increased margins for the innovators dominate the reduced margins for the laggards, industry margins will also increase. In the second stage, both concentration and prices decrease; and industry margins will decrease as well (assuming that they increased in stage 1). This process is consistent with the findings here and in Peltzman. Thus, the rejection of the Demsetz hypothesis still rests on the logical problem of reconciling large market shares, diminishing returns, and price-taking behavior rather than a statistical result.

Table 5. Relationship of Price-Cost Margins, Lagged Concentration, and Changes in Concentration, 1982[a]

	Constant	*1972 C4*	*1972–82 ΔC4*	*1972–82 |ΔC4|*	*Capital-sales ratio*	*R^2 and number*
1	.261	.135	. . .	. . .	−.070	.10
	(20.8)	(5.18)	. . .	. . .	(−4.70)	360
2	.259	.145	.121	−.051	−.070	.11
	(19.8)	(5.38)	(1.71)	(−0.48)	(−4.72)	360
3	.229	.248	.100	−.119	−.070	.19
	(12.5)	(6.36)	(1.04)	(−0.81)	(−3.70)	214

a. Dependent variable is 1982 price-cost margin. Values in parentheses are *t*-statistics. Concentration measure is adjusted domestic concentration (*C4AN*).

third row includes the change in import intensity as an explanatory variable (and as a result is based only on the sample for which such data are available). The coefficient on import intensity is positive and significant, perhaps because import intensity cannot be taken as exogenous.

It is of interest to examine how much of the cross-sectional relationship in, say, 1982 can be attributed to changes between 1972 and 1982. Doing so sheds light on two issues. First, one of the revisionist critiques, primarily associated with Yale Brozen, was that the concentration-profits relationship reflected short-term rents.[53] If so, then 1972 concentration should have little force in explaining 1982 margins. Second, even if the data do describe a process in which firms attain short-term monopoly power in return for successful innovation, it is of interest to know what is meant by short term. Table 5 presents regressions of 1982 price-cost margins on 1972 values of concentration as well as the changes and absolute values of changes in concentration from 1972 to 1982. The first row can be compared with the third regression on the 1982 row in table 2. The only difference between the two is the use of the 1972 instead of the 1982 concentration ratio. The coefficients are remarkably close to each other. When the change in concentration and absolute value of the change in concentration are added to the regressions, the coefficient on neither is significant, while the coefficient on 1972 concentration remains positive, significant, and of roughly the same magnitude. When import-adjusted data are used, the results are

53. Brozen (1970, 1971a, 1971b).

still stronger. In that case, the coefficient on 1972 concentration is 15 percent greater than the coefficient on 1982 concentration reported in table 2. Unless one takes a rather long view of what is properly meant by the short term, the data fail to support Brozen's hypothesis. And although the data are consistent with the hypothesis that market power is the reward for successful innovation, the market power that does result tends to be long-lived.

Even if increases in concentration are associated with price reductions, the level of concentration could be a concern if it were associated with increases in prices. One might argue that the theoretical effect of concentration is on the level of prices, not the rate of increase. However, there are plausible industry dynamics in which concentration would be associated with price increases. The results so far have suggested that markets become concentrated when one or a few firms develop competitive advantages that result in at least temporary monopoly power. The results of table 5 suggest that although this market power is not permanent, it tends to persist for long periods. A possible result of this monopoly power is that suppliers would gradually bid away some of the monopoly rents by raising input prices, which would induce the firm to raise the price of the final product.

Table 6 shows regressions similar to those in table 3, except that the concentration level at the beginning of the period is included as an explanatory variable. The coefficient on lagged concentration is positive and statistically significant. From table 1 it can be seen that the standard deviation of concentration levels is four times as great as the standard deviation of changes in concentration. Thus, the effect on price of the level of concentration is of roughly the same magnitude as the effect of changes in concentration.

In the sample for this paper, the level of concentration is not associated with increases in margins. Thus the correlation between concentration and price increases must be the result of cost increases. In light of the literature on rent sharing, a cost increase that one might expect to be associated with concentration is wage increases. Table 7 represents regressions of wage increases on levels and changes in concentration and the absolute value of the change in concentration. Lagged wages are included to capture any regression toward the mean, and changes in the ratio of imports to sales are also included for the sample in which those data are available. In all the regressions, the coefficient

Table 6. Relationship of Level of Concentration, Changes in Concentration, and Changes in Price, 1972–82[a]

	Constant	*ΔC4*	*|ΔC4|*	*Δ imports-sales ratio*	*Labor-sales ratio*	*Δ materials costs*	*C4*	*R^2 and number*	*C4 + |C4|*	*C4 − |C4|*
1	.003	.138	−1.59	. . .	2.42	1.59	.541	.25	−1.45	1.73
	(0.02)	(0.38)	(−2.91)	. . .	(5.36)	(10.2)	(3.93)	366	(−2.46)	(2.41)
2	−.293	−.573	−2.20	.207	3.10	1.98	.750	.36	−2.77	1.63
	(−1.41)	(−1.07)	(−2.87)	(0.35)	(4.87)	(10.0)	(3.64)	215	(−2.83)	(1.84)

a. Dependent variable is percentage change in prices from 1972 to 1982. Concentration measure is adjusted domestic concentration (*C4AN*) in row 1 and import adjusted concentration (*C4AI*) in row 2. Values in parentheses are *t*-statistics.

Table 7. Relationship of Levels of Concentration and Changes in Wages, 1972-82[a]

	Constant	*C4*	*ΔC4*	*|ΔC4|*	*Wages*	*Δ imports-sales ratio*	*R^2 and number*	*C4 + |C4|*	*C4 − |C4|*
1	1.15	.420	.139	−.350	−0.73	. . .	.22	−.211	.489
	(30.2)	(7.63)	(0.95)	(−1.60)	(−4.85)	. . .	366	(−0.90)	(1.70)
2	1.15	.389	−.219	−.504	−.443	−.487	.19	−.723	.285
	(23.2)	(4.96)	(−1.06)	(−1.69)	(−2.13)	(−2.15)	215	(−1.90)	(0.83)

a. Dependent variable is percentage change in wages. Concentration measure is domestic adjusted concentration (*C4AN*) in row 1 and import adjusted concentration (*C4AI*) in row 2. Values in parentheses are *t*-statistics.

on the level of concentration is positive and highly significant. Moreover, the magnitude is of at least moderate economic significance. The standard deviation of the concentration level is about .2. Thus a difference in concentration of one standard deviation is associated with about an 8 percent greater increase in wages. In turn, that figure is about 40 percent of a standard deviation in wage increases.

These results beg the question of whether the relationship between concentration and wage increases explains the relationship between concentration and price increases. Table 8 presents regressions that are similar to those in table 6, except that measures of changes in labor costs are substituted for labor intensiveness. Two measures are used. The first is simply the percentage change in wages. The second, which is labeled the percentage change in labor costs, is the percentage change in wages multiplied by the ratio of production labor cost to revenues at the beginning of the period. The coefficient on the concentration variable is insignificant when just the percentage change in the wage is used to measure labor cost increases (row 1). With the alternative measure, which is arguably better on theoretical grounds, the fit of the equation is better and the coefficient on the level of concentration is positive and significant (row 2). When both measures are included, both are statistically significant and the coefficient on the level of concentration is insignificant. The point estimate is about 60 percent lower than the comparable coefficient in table 6, row 1. With the import-adjusted sample, the qualitative results are similar. With both measures of labor cost increases included (row 6), the coefficent on concentration is 35 percent lower than in table 6, row 2. In the import-adjusted sample, the coefficient on concentration remains statistically significant. Thus these results do not suggest that wage increases explain all the effect of concentration on price increases. The differences in the point estimates suggest, however, that they explain about half of them.[54]

54. One might be concerned that the equations estimated in tables 6 and 8 represent explosive dynamic systems. That is not the case, however, because concentration regresses toward the mean in this sample. Specifically, $C4\ (t) = .030 + .933\ C4\ (t-10)$. One might also wonder whether the price increases resulting from the higher levels of concentration ever dominate the initial price decrease resulting from an increase in concentration. Simulations based on the above concentration autoregression and equation 1 of table 6 make it clear that they can, but it takes a long time. For example, an increase in concentration of 20 percent from the steady-state level of 44 percent results in lower prices for 160 years.

Table 8. Relationship of Concentration, Changes in Labor Costs, and Changes in Prices[a]

	Constant	*C4*	*|C4|*	*Imports-sales ratio*	*Materials costs*	*Percent Δ wages*	*Percent Δ labor costs*	*C4*	*R^2 and number*	*C4 + |C4|*	*C4 − |C4|*
1	.091	−.153	−1.32	. . .	1.00	.644	. . .	.041	.25	−1.48	1.17
	(0.62)	(−0.42)	(−2.41)	. . .	(7.52)	(4.80)	. . .	(0.28)	366	(−2.49)	(1.62)
2	−.128	.175	−1.48	. . .	1.69	. . .	2.84	.395	.32	−1.30	1.65
	(−0.99)	(0.50)	(−2.84)	. . .	(11.8)	. . .	(8.06)	(3.10)	366	(−2.31)	(2.42)
3	−.378	.080	−1.36	. . .	1.56	.374	2.51	.211	.33	−1.28	1.44
	(−2.45)	(0.23)	(−2.62)	. . .	(10.4)	(2.92)	(6.85)	(1.50)	366	(−2.29)	(2.12)
4	−.086	−.643	−2.05	.949	1.33	.610	. . .	.274	.32	−2.69	1.41
	(−0.38)	(−1.17)	(−2.58)	(1.59)	(7.68)	(3.31)	. . .	(1.23)	215	(−2.65)	(1.54)
5	−.281	−.375	−2.01	.487	1.97	. . .	2.84	.595	.39	−2.39	1.64
	(−1.55)	(−0.72)	(−2.01)	(0.87)	(10.8)	. . .	(6.01)	(3.02)	215	(−2.49)	(1.90)
6	−.435	−.365	−.194	.607	1.88	.228	2.60	.490	.40	−2.30	1.57
	(−1.96)	(−0.70)	(−2.58)	(1.07)	(9.55)	(1.20)	(5.04)	(2.28)	215	(−2.39)	(1.82)

a. Dependent variable is percentage change in prices. Concentration measure is adjusted domestic concentration (*C4AN*) in Rows 1 to 3 and import adjusted concentration (*C4AI*) in rows 4 to 6. Values in parentheses are *t*-statistics.

Conclusions and Implications for Antitrust Policy

The traditionalist-revisionist debate generally concerns whether the relationship of concentration and profits or concentration and margins best reflects competition or market power. The most plausible interpretation of the data is that the relationship reflects both. Dynamic competition gives rise to short-term market power. Such a conclusion is not particularly profound. Edward Chamberlin and Joseph A. Schumpeter articulated the point earlier and better than I have and many will undoubtedly consider it self-evident.[55] Nevertheless, the literature has focused on whether markets are perfectly or imperfectly competitive, and the current conventional wisdom is that the data are more consistent with perfect competition.

The traditionalist-revisionist debate developed amid the controversy over deconcentration policies. Merger policy has, however, supplanted deconcentration policy as the focus in antitrust matters. The implications of these conclusions for merger policy turn on how mergers fit into the process that the data suggest. For example, one might suspect that mergers are an integral part of the process by which markets become more concentrated and prices are reduced. The evidence from Peltzman and this paper concerns the years 1947 to 1982. It is extremely unlikely that any substantial increase in concentration was accomplished by merger in this period: the antitrust authorities and the courts would not have permitted it. Thus these results cannot be taken as evidence that horizontal mergers create efficiencies.

Still, a prominent theory of takeovers is that well-run companies acquire poorly run companies and improve their performance. Some point to large takeover premiums as evidence for this hypothesis.[56] Whether the combined value of acquirers and targets increases at the time of merger announcements (as opposed to just the value of the target's shares) is not, however, clear.[57] Moreover, there is considerable evidence that the acquiring firms experience large and statistically significant negative abnormal returns in the two years after mergers.[58]

55. Although based on a completely different set of results, this conclusion is similar to the one in Hall (1988).

56. See Jensen (1988); and Jensen and Ruback (1983).

57. Roll (1986).

58. Asquith (1983); Langetieg (1978); Magenheim and Mueller (1988); Malatesta (1983); and Salinger (1989).

Ravenscraft and Scherer fail to find any evidence of increases in profitability from mergers.[59] Thus neither the stock market nor the accounting evidence provides support for efficiencies resulting from mergers.

In the most convincing evidence to date of such efficiencies Frank Lichtenberg and Donald Siegel show that plants changing ownership have, on average, lower productivity than plants in the same industry that do not change.[60] But seven years after the ownership change, the productivity of those plants is nearly the same as productivity in plants that did not change owners. Although the authors obtain significant *t*-statistics in regressions of productivity changes on ownership change dummies, the effect is economically small.[61] At most, the finding suggests that a policy to discourage ownership changes in general would be ill-advised. To justify a tolerance of horizontal mergers, competitors would have to be better able to bring about productivity increases than noncompetitors. One might suspect that to be the case, but I know of no systematic evidence to support it.

Although increases in concentration resulting from mergers cannot be assumed to have the same cost-reducing benefits as endogenous increases in concentration, they could have the same effect on margins. The empirical results presented here suggest that markets should be viewed as oligopolistic. In virtually any oligopoly model, a merger of two firms makes the market less competitive.

In addition to this concern, the results suggest that increases in concentration may eventually result in increased costs and prices. This possibility is more speculative. The relationship between the level of concentration and cost and price changes may simply be a reflection of the economy in the 1970s rather than a standard pattern for industry dynamics.[62] Nonetheless, the possibility that it is a standard pattern should not be dismissed out of hand.

Optimal merger policy must take into account imperfection in the judgment of the antitrust authorities and the courts. Thus if there were

59. Ravenscraft (1987).

60. Lichtenberg (1987).

61. The results are based on some 18,000 observations. With such a large sample the coefficients can be estimated precisely enough to reject the hypothesis that even very small effects are significantly different from 0.

62. I am indebted to Sam Peltzman for pointing out to me that the level of concentration was associated with price decreases in earlier periods. It is, of course, possible that unions became better at bargaining away rents in the 1970s than they were previously.

strong evidence of efficiencies resulting from mergers and little evidence of collusive effects from concentration, then there should be a presumption that horizontal mergers should not be blocked. But there is little if any general evidence supporting efficiencies from mergers, and there is some possibility that concentration results in higher prices. Thus a stricter policy toward horizontal mergers than the one that prevailed during the Reagan administration would be appropriate. This is not to suggest that efficiencies never arise or that an efficiency defense should not be allowed. However, there should be a strong presumption that mergers violating the concentration standards in the merger guidelines are illegal, and merging parties should bear a strong burden of proof that efficiencies justify overturning that presumption.

Appendix: Description of Variables and Data Sources

MOST OF THE data for this project come from a data set provided by David Lebow. It contains a panel of Census of Manufacturers data. The variables used in the study were total compensation of employees (*PR*), hours of production workers (*H*), compensation of production workers (*PPR*), materials inputs (*M*), shipments (*S*), and inventories (*INV*). Following Ian Domowitz, R. Glenn Hubbard, and Bruce Petersen, price-cost margins (*PCM*) are measured with an inventory adjustment as follows:

$$PCM = \frac{S - PR - M + \Delta INV}{S + \Delta INV}. \tag{1}$$

Wages (*W*) are measured as the ratio of production worker compensation to production worker hours (*PPR*/*H*). In addition to nominal values of the above variables, the data set contains real values of some of them, including shipments (*RS*). For each year, a price index is inferred as the ratio of nominal to real shipments (*S*/*RS*). The percentage change in prices over time is then based on the ratio of the price indices.

In addition to the census data, the Lebow data set contains a capital stock series. It also has both nominal and real values (*K* and *RK*, respectively). Net investment (*I*) in year *t* is measured as the ratio of end-of-year (that is beginning of the following year) to the beginning

of year *RK*. The nominal capital stock series is used in estimating the capital to sales ratio.

Four-firm concentration ratios (*C*4) were collected from the Census of Manufacturers for 1972, 1977, and 1982. The Weiss and Pascoe adjusted concentration ratios are for 1972 and 1977. That data set consists of two series, one adjusted for imports and one not adjusted for imports. The one not adjusted for imports (*C*4*AN*) is used in this study. For 1982, *C*4*N* is estimated as the 1982 *C*4, the 1977 difference *C*4*AN*, and *C*4. Concentration ratios in non-census years are estimated as weighted averages of the concentration ratios in the immediately preceding and succeeding censuses, with the weights proportional to the time differences. That is, the 1973 concentration ratio is 4/5 times the 1972 concentration ratio plus 1/5 times the 1977 concentration ratio.

Although the Weiss and Pascoe data do contain import adjustments, they are only for two years (1972 and 1977). Because import changes are a potentially important source of concentration changes, annual import to sales ratios (*IMP*/*S*) were collected from *U.S. Commodity Exports and Imports*. The import-adjusted concentration ratio (*C*4*AI*) was then calculated as:

$$C4AI = C4AN\,(1 - IMP/S). \tag{2}$$

The industrial classification scheme used to record imports and exports is somewhat different from SIC codes. *IMP*/*S* was recorded for an SIC code whenever it was completely covered by an import code. In many cases, the value for *IMP*/*S* is for a broader classification than the 4-digit SIC code. In some cases, an import number is reported but a value for *IMP*/*S* is not, and a footnote indicates that imports and domestic production are not comparable. In those cases, *IMP*/*S* was recorded as missing. Because of the differences in the industrial classification scheme, import-adjusted concentration ratios cannot be calculated for all industries. Accordingly, the estimation in which import-adjusted concentration ratios are used is based on a smaller sample.

Comments and Discussion

Comment by Richard E. Caves: Salinger's paper revisits most of the issues in industrial organization that have been debated in the past two decades. I shall concentrate on a few main issues.

One major topic addressed by the paper is the relationship between changes in concentration and changes in prices, with Salinger replicating Sam Peltzman's result that productivity (output price with changes in input price controlled) declines in industries experiencing increasing producer concentration. The important question is which of many possible mechanisms underlies any relation between changes in concentration and the revenue productivity of an industry's resource usage?

The trouble, which also plagues analyses of the bivariate relationship of profits and concentration, is that empirically concentration is a decidedly endogenous variable in two senses. In the long run it is determined by the various possible forms of economies of scale, first-mover advantage, and input-market imperfection identified by the theory of entry barriers. The normative significance of these entry barriers is controversial and varies from one to another. Nonetheless, entry barriers account for much of the variance of concentration among industries. Although changes in concentration cannot be explained well (concentration usually does not change much, as Salinger notes), it would be surprising if changes in structural determinants did not count. In the short run, concentration depends on the price and output decisions of the firms in the market, because these decisions affect their market shares. Shares could change to reflect changes in firms' variable costs but also breakdowns in collusive behavior and various other sorts of

strategic interaction. Until the mechanism responsible for a change in concentration, either typically or in a specific case, is known, normative conclusions about any relation between productivity and changes in concentration are difficult to draw.

A substantial new finding by Salinger is that the effect of concentration on price-cost margins is largely independent of any link between productivity growth and changes in concentration. Salinger's finding that 1982 margins relate as well to 1972 as to 1982 concentration agrees with a number of previous studies that the level of concentration in a given year retains its significant positive influence on the price-cost margin for a long time. This persistence is consistent with the fact that concentration simply does not change very rapidly in most markets. It is inconsistent with contestability or a strong dependence of concentration on the short-term interaction of competitors. Indeed, the only statistical interplay that Salinger finds between Sam Peltzman's mechanism and the relationship of concentration and prices arises because of the association of changes in labor costs and concentration that was evident from 1972 to 1982.

A useful service of Salinger's paper is to extend the cross-sectional results of Ian Domowitz, R. Glenn Hubbard, and Bruce Petersen up to 1984.[1] The collapse of the relationship between margins and concentration that they reported appears to be an artifact of abnormal events concentrated in the 1970s. One of these was the rapid import penetration of a number of important U.S. markets. Salinger's curious positive effect of changes in import penetration on changes in margins may be partly explained by technical efficiency: for 1977–86, changes in the ratio of imports to total supply had a significant effect of squeezing out technical efficiency.[2] The other explanation for the lost relationship between margins and concentration is the positive association between concentration and wage increases in the 1970s. Because this correlation cannot be laid to structural changes in the labor market or in collective-bargaining institutions, it seems best attributed to responses to the inflation and decreases in real wages brought about by the 1973 energy crisis. The increase in import penetration seems permanent, but the invasion of concentrated industries' profits by wage claims was appar-

1. Domowitz, Hubbard, and Petersen (1986a, 1986b).
2. Caves and Barton (1990, chap. 8).

ently temporary. Some may take comfort in the revival of the relationship between margins and concentration in President Reagan's era of good feeling.

If normality has returned to this relationship, it seems to have departed from that between margins and the ratio of capital to sales. The ratio appears in the model only to control for interindustry differences in capital intensity, not for any hypothesis about how capital intensity affects competitive processes. Some evidence exists that profit rates are low in capital-intensive industries, perhaps because of commitment races, perhaps simply because of the duration of mistakes when capital is sunk. The latter (disequilibrium) explanation seems applicable. Variable costs in the capital-intensive industries were probably enlarged disproportionally by the increase in energy prices and wages (if not necessarily unit labor costs) in the 1970s. These disturbances should have led to rising relative prices and excess capacity. The persistence of such a disequilibrium from about 1977 through 1984 may account for this anomaly.

As I consider Salinger's policy conclusions, I return to my earlier emphasis: the importance of the endogeneity of concentration. A sufficient reason for not rallying around the White House to demand the blanket deconcentration of concentrated markets is the low statistical explanatory power coupled with normative diversity of the forces that seem to generate and sustain concentration. Although economists will continue to disagree on what is objectionable in the forces that sustain concentration, there should be a measure of agreement on two propositions:

—A deconcentration policy is pointless if existing concentration represents an equilibrium governed by more fundamental determinants. We should simply expect equilibrium to be restored.

—The only effective deconcentration policy is one that focuses primarily (perhaps entirely) on the underlying determinants rather than on concentration itself. The antitrust literature contains several examples of arguably successful policies that took this route.[3]

Comment by Sam Peltzman: The relationship of concentration and profit is a staple of the literature, and whenever one thinks the last word

3. Baldwin (1969); and Bresnahan (1985).

on it has been heard, a paper such as Salinger's tells us differently. It is one of the very few attempts I know of that tries to bring together the two main interpretations of this literature: the collusive story and the efficiency story.

I want to concentrate on just a few main results and on Salinger's interpretation of them. The first part of the paper, summarized by table 2, extends the literature on the relationship of concentration and margins into the 1980s. His main conclusion is that the previously heralded decline in the strength of this relationship is not true if concentration is correctly measured. The main reason for this is the growth of world trade, which has introduced increasing measurement error into domestic concentration as the right proxy for the relevant market structure. If this growth is taken into account, the deterioration goes away.

I suspect this conclusion is right, on balance, but Salinger's procedure is not entirely convincing because the adjustment for imports introduces its own error. His "adjusted concentration ratio" is some standard concentration ratio times one minus the ratio of imports to sales. This obviously goes in the right direction in the sense that if there are high imports, there are generally more sellers and less concentration. But this cannot be right for every case. For example, in consumer electronics the ratio of imports to sales must be close to one. This would make the adjusted concentration ratio close to zero, but the correct ratio is almost surely larger.

The problem, of course, is that the ranks of the large firms selling in the market now contain some foreign firms. There is no obvious way that I know of around this problem, but it calls for some caution in concluding that nothing has changed in the relationship between concentration and profitability.

In table 2 all of the effect of imports is forced to work through the concentration ratio. Suppose the truth is that imports constrain margins independent of concentration. Then the coefficient of adjusted concentration is a compound of two effects. One is a concentration effect and the other an effect operating through imports more generally. It is therefore unclear whether only the concentration effect is producing the pattern, or more precisely the lack of any pattern, in the time series of those coefficients.

It would have been, I think, better on the whole to have run the regressions with those two variables separated. Then one could ask, if

the import-sales ratio is held fixed, is there still about the same relationship between margins and concentration?

Apart from this interpretive problem, there is a puzzle in table 2. The coefficient of the capital-sales ratio has declined during the 1970s and 1980s, and in fact has become a negative. It is a puzzle because capital does not seem to be paying for the privilege of serving us. What the ratio actually implies is that highly capital-intensive industries have been doing badly.

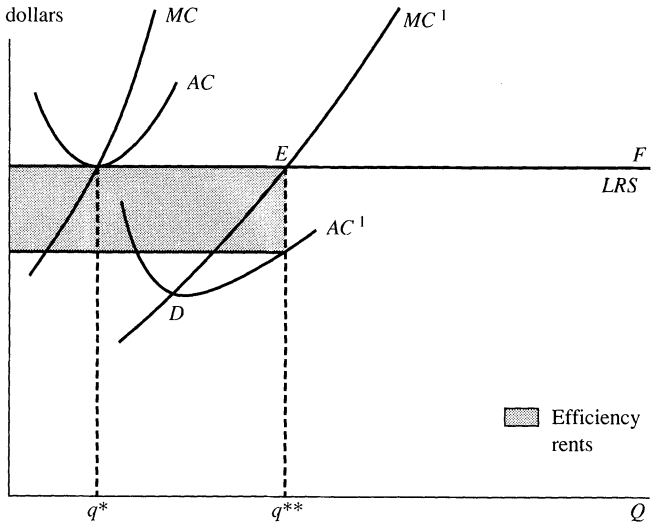
There is a related mystery in the results. The year-to-year changes in the coefficients of concentration in the capital-sales ratio, the second-to-last column of table 2, show a substantial negative correlation between the changes in these coefficients (the simple correlation is $-.7$). Thus high concentration apparently leads to especially high margins in those years when capital-intensive industries are doing especially badly. But this conclusion is the opposite of the true trend, which is that both concentration and capital-sales ratios are having a weaker effect over time. In the short run, the worse the capital-intensive industries are doing, the more powerful the relation between concentrations and margins.

I do not know what to make of this. It is just a fact. But it deserves some attention. There is something going on in terms of an interaction between margins, concentration, and capital intensity that deserves exploration. Salinger tells us that concentration and capital intensity are "not that highly correlated." So a simple explanation based on errors in variables may be inadequate. One bit of exploratory work suggested by this interaction would be to separate the sample into highly capital-intensive and less capital-intensive industries and then see what happens to the time pattern of these coefficients in the subsamples.

The last part of the paper focuses on what Salinger calls the dynamic effects of concentration. He shows in table 3 that increases in concentration in the 1980s have reduced prices. This corroborates my finding for an earlier period. My problem here is not with the result, but with his interpretation of it. Somehow this dynamic story must be different and distinct from what the levels regressions are showing. His argument is that the cost saving that is related to increasing concentration cannot possibly be driving the positive coefficient of concentration in the levels.

He gives two reasons for this, and I cannot understand either one. There is a theoretical reason, the conclusion of which is that the dif-

Figure 1. Kinked Supply under Competition with Cost Differentials



ferential-efficiency interpretation of the levels regression can be correct only if, "at some price, the market supply curve changes from increasing to flat. Such an assumption is difficult to refute, but it is hardly standard." So Salinger wants to rule out the revisionist view as an exotic special case.

It seems to me that the revisionist view is precisely what I have just quoted and is a plausible description of supply conditions in enough industries to drive the results shown in table 2. Figure 1 illustrates this point. We like to think that constant costs are a fairly good approximation to a lot of manufacturing industry supply conditions. I take a garden-variety, constant-cost competitive industry as my starting point. Every firm has the pair of cost curves *MC* and *AC*, and *LRS* is the long-run supply function. Now let one firm discover a technique such that marginal and average cost is lower at every output (MC^1 and AC^1). This is the simplest representation of differential efficiency. This leads first of all to the efficient firm's increasing its size relative to the average. That is its output increases from q^* to q^{**}. That shows up as an increase

in concentration. This firm also earns efficiency rents (the shaded area), which show up as an increase in industry margins. And, crucially, the long-run supply function has exactly the kind of kink in it that Salinger is describing (his *DEF* over the relevant range).

It is premature to say that this is an exotic case. One cannot rule out a priori the case that may be dominating the levels of regression. Salinger also has an empirical argument, which is that margins in 1982 are just as well explained by concentration in 1972 as in 1982. That is in table 5. One is supposed to draw the inference that cost and price changes caused by the 1972–82 changes in concentration have to be pretty small potatoes. I think more needs to be said. Look at the facts as they are laid out in this paper. Table 4 shows that an increase in concentration during these ten years led to an increase in margins with the coefficient roughly comparable to those found in the levels regressions of table 2.

Table 3 shows that increases in concentration result in lower prices. The only way table 4 and table 3 can be reconciled, obviously, is that costs had to go down more than prices went down to generate an increase in margins in those industries that have become more concentrated.

If cost-reducing increases in concentration are driving the relationship between changes in margins and changes in concentration and if that relationship is roughly the same as in the levels, I fail to understand why the levels regressions cannot be reflecting the cumulation of similar such effects over many decades.

There may be reasons to doubt that. For example, there ought to be adaptations by other firms to some of these cost innovations. But that is a separate issue not at all addressed in this paper. I think the only reasonable interpretation of the whole body of results here is the eclectic one that, on balance, I share with Salinger. The overall profit-margins relationship, whether it is in the levels of regression or the changes—I cannot make the distinctions he makes—is driven by a mix of efficiency rents and imperfect competition.

This leads me to policy conclusions that are also eclectic, more so than Salinger's. He wants policy to tilt toward restricting mergers, even though increasing concentration is generally associated with efficiency increases. He gives two reasons. One is that, after all, most increases in concentration occur without mergers. That is, of course, right. The inference we are supposed to draw is that efficient firms can increase

market share without mergers. His second reason is that direct evidence for the efficiency effects of mergers is spotty. The inference here is that other ways of increasing concentration are, perhaps, more important than mergers in producing the associated efficiency gains.

I have trouble with both arguments. First, I think he reads the evidence wrongly. Most of the mergers in the studies he cites are not horizontal mergers. As he points out, until the 1980s these were very restricted. Second, and more important, if increasing concentration is generally promoting efficiency, mergers are surely going to be the low-cost method of increasing concentration in some cases. For example, consider a declining industry that has to have some reduction in the number of firms and some increase in concentration. It may be cheaper to shift legal titles around than to force some more indirect transfer of resources.

This does not mean that all mergers should be allowed or that the current margin of policy is right. It does mean that, instead of presuming that efficiencies do not exist unless otherwise shown, which is how Salinger would like policy to proceed, the presumption ought to be the opposite. That is, policy ought to be tolerant of mergers unless there are fairly compelling reasons to fear an anticompetitive outcome.

Finally, there are some interesting puzzles resulting from his data that need some exploration. The first is in table 3, which shows that labor-intensive industries have had increasing relative prices in the 1970s and 1980s. Salinger put a labor-intensity variable in this change regression on the possibility that there could have been a change in the relative price of labor. But the positive labor coefficient makes sense only if the relative price of labor has increased. Table 1—his summary data—shows that this is not so. In fact, given the growth in productivity, unit labor costs have fallen in these two decades. The obverse of this result is that prices have gone down in capital-intensive industries. This is another indication of how the terms of trade have turned against capital-intensive industries.

The second fact is that industries with high levels of concentration in 1972 had increasing relative prices between 1972 and 1982. Salinger alluded to this result as part of the justification for a tough stance on mergers. This result is the opposite of the relationship that had been observed in the 20 years up to 1972. Up to that time most of the evidence showed that high initial concentration is associated with decreasing prices.

This has implications for the productivity puzzle of the 1970s and 1980s. First, it is clear that what Salinger finds is not a long-term relationship; relative prices in highly concentrated industries have not been increasing without bounds. What his result does say is that those manufacturing industries that were leading productivity growth before 1970—the highly concentrated industries—are lagging now.

General Discussion: A number of participants questioned the quality of the data used in the paper. Franklin Fisher noted that the substantial measurement errors associated with determining the value of capital call into question the accuracy of some of the variables used. Robert Hall said that the headquarters costs, notably advertising, omitted by the establishment-based Census of Manufacturers significantly distort the data that come from that census. Studies of census margins are effectively studies of advertising in some industries.

Steven Salop said that because Reagan administration antitrust policy permitted horizontal mergers, a rich new data set is emerging that should allow investigation into whether increased concentration leads to higher margins, higher prices, and lower costs. Frank Lichtenberg said that he and Moshe Kim had recently conducted a study on such data when they looked at the airline industry from 1970 to 1984. The conclusions were that in the airline industry, mergers that increase concentration lead to cost and price reductions and to only very slight increases in profit margins. He attributed part of the cost reductions to increased capacity utilization—that is, increased load factors.

Dennis Mueller said that monopoly profits are not such a problem in oligopostic industries or in industries where there is collusion. In many markets—for example, breakfast cereals—product differentiation created quasi monopolies, and it is in these industries that high profits are found.

Lawrence White said the positive correlation between 1972 concentration in an industry and the change in prices from 1972 to 1982 might be caused by the wage-price controls of the early 1970s. The wage-price controls might have affected concentrated industries more than nonconcentrated ones "because it is easier to control General Motors' prices than Joe's Garage's," so the change in prices might include a return to the long-run equilibrium of prices and wages disrupted by the price controls.

Richard Nelson offered an alternative dynamic theory for the relationship between prices and concentration. He said that when a new technology comes into being, it can take many forms and can result in many different products. There will be many firms, mostly small, and they will try to develop the technology in a number of different directions. Research will be undirected, and most firms will not have an advantage over other firms at first. As information about the technology begins to accumulate, certain forms of the technology will begin to look good relative to other forms. The research will become more directed and systematic, and this will show up as increases in productivity and decreases in cost and price. In addition, some firms will be successful and others will fail. Economies of scale in production will result. Nelson said this means that the period of rapid growth of technology will include productivity growth, cost reduction, price reduction, and increasing industry concentration. As the technology stabilizes, productivity growth slows and concentration growth tends to stop.

Dennis Carlton was interested in the policy implications that resulted from the paper. Current merger guidelines say that there should be concern if a merger will likely lead to a 5 to 10 percent increase in prices. Given some of the coefficients estimated in the paper, even an increase in the concentration ratio from 40 percent to 90 percent would lead to only a 5 percent increase in price, according to Carlton.

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