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A REVIEW OF THE ECONOMIC BASIS FOR BROAD-BASED HORIZONTAL

MERGER POLICY

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and new interpretations are being given to classic works in industrial organization. While it is too soon to say what ultimate impact the debate over the market concentration doctrine might have, it is already clear that challenges to the traditional view have altered the consensus from what it was in 1968 and these challenges have important implications for future changes in public policy.

This paper is organized as follows: First, the basic concepts of oligopoly theory which form the foundation for the traditional view that market concentration can lead to poor economic performance are discussed very briefly. Next, much of the empirical evidence available on the welfare costs of monopoly and administered pricing is evaluated to obtain a rough estimate of the magnitude of the market power problem and to determine whether that literature provides a sound basis for broad-based horizontal merger guidelines.[1] Third, a large portion of the literature on the relationship between concentration and profitability is examined to answer two questions—what was the economic consensus in 1968 when the Justice Department merger guidelines were promulgated and what is the current state of our knowledge? The

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[1] We will not discuss some other possible bases for broad-based antitrust action. For instance, the relationships among market structure and technological progressiveness, political power, and worker alienation might be used to defend antitrust initiatives. In addition, case studies of actual mergers might be used to justify market intervention. We will not discuss this latter literature, however, since it tends to deal with conglomerate (as opposed to horizontal) mergers and because it does not lend itself to useful general conclusions. See Fisher and Lande (1981) and Scherer (1980) pp. 128-41.

INTRODUCTION

The eminently believable notion that the concentration of productive capacity in fewer and fewer hands in a given market will often lead to higher prices and lower output has been standard fare for students of economics for decades. This concern about market concentration is evidenced by the large amount of research devoted to the theoretical study of competition among few competitors and empirical studies of the effects of market concentration on welfare, prices, and profitability. During the last decade, however, the basis for this fear of concentration has been attacked by those who argue that increased concentration will most often lead to (or is the result of) efficient production, lower costs, and lower prices.

In this paper, we attempt to chronicle the debate over the effects of concentration on market performance. In the process we compare the state of our economic knowledge currently with that which existed in 1968. That year is chosen because at that point the Justice Department's Antitrust Division felt secure enough in its knowledge of the effects of market structure on market performance to issue market-share/concentration-ratio guidelines for horizontal mergers.[1] What we find in our review is an interesting, and as yet incomplete, metamorphosis in economic thought over the past few decades. Old ideas are slowly being modified

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[1] The Justice Department offered both horizontal and vertical merger guidelines. We will be concerned only with horizontal problems.

From these passages it is not possible to discern how Smith might have viewed current horizontal merger policy, but he clearly realized at least one of the tradeoffs that would have to be made in passing laws which limit the freedom of groups of firms.

With this early view as a springboard, we can begin to examine the more recent intellectual impetus for an antimerger policy. In the economics literature this impetus can be traced to the earliest ideas relating to oligopoly theory. Cournot's (1838) theoretical work indicated that an increase in the number of competitors in a market had a definite salubrious effect on performance. His model, which assumed rather naive behavior on the part of rivals, shows that the equilibrium achieved by non-colluding firms approaches the competitive equilibrium as the number of rivals increases. Although Cournot's model has been subject to a number of refinements in later years, it still serves as a basis for much economic research today.[1]

More recently a considerable literature on the characteristics of oligopoly markets has developed with at least two distinct models of pricing emerging. One, the dominant firm model, is really an exposition of monopoly pricing behavior by a large firm(s) given the existence of a fringe of competitors who

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[1] In fact, it is quite likely that this model is used more often in oligopoly research than is justified. See Alger (1981).

latter question is answered by an examination of a stylized debate between the "traditionalists" (who believe that market concentration often leads to monopoly power) and the "revisionists" (who argue that market concentration generally occurs due to efficiency considerations). Finally, assuming the traditional view to be correct, the literature on "critical" concentration or market share levels is examined to gain insight into possible revisions of horizontal merger policy. While our review of the literature does not lead to a statement of a consensus view, it does show the evolution of thought and evidence on the structure-performance paradigm through time.

OLIGOPOLY THEORY AND COLLUSION

As with almost every other idea in economics, one can argue that Adam Smith originated the thought that competitors would attempt to collude if given the opportunity when he wrote:

> People of the same trade seldom meet together,
> even for merriment and diversion, but the
> conversation ends in a conspiracy against the
> public, or in some contrivance to raise
> prices.

It is interesting that Smith immediately went on to note that:

> It is impossible to prevent such meetings by
> any law which could be executed, or would be
> consistent with liberty and justice. But
> though the law cannot hinder people of the
> same trade from sometimes assembling together,
> it ought do nothing to facilitate such
> assemblies; much less to render them
> necessary.[1]

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[1] Smith (1776), p. 128.

act as price takers.[1] The second model of oligopoly pricing

focuses on the interactions of the various rivals in a market.

Central to most of this literature is the idea that as the number

of firms in a market decreases, the probability of effective tacit

or explicit collusion increases, ceteris paribus. This result

stems from an increased awareness of significant mutual interde-

pendence among rivals as well as from the higher probability of

detection and punishment of "cheaters" in an oligopoly setting.[2]

There is an important reason for stressing the differences between

the dominant firm and collusion models other than the fact that

they imply different equilibria in many cases. The two models

also imply rather different treatment of mergers. For instance,

if one believes that the collusion model describes the world of

business rather well (and one also believes the collusive equili-

brium is relatively stable), then the traditional policy of inves-

tigating mergers of two fairly small competitors (say two

5-percenters) might be considered reasonable to reduce the likeli-

hood of collusion. On the other hand, if one believes that the

dominant firm model provides a better description of the world,

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[1] See Worcester (1957) and Gaskins (1971) for a discussion of the
dominant firm model and the long-run instability of the equili-
brium attained in that model.

[2] See, for instance, Markham (1951), Stigler (1964), and Osborne
(1976). Chamberlin (1933) can be credited as one of the first to
argue that as the number of rivals fell and mutual interdependence
rose, a "critical" level of concentration might be reached which
would allow concerted action to lead to noncompetitive price
levels.

one would be rather unconcerned about mergers among nonleading firms since the growth of larger third, fourth, and fifth firms would reduce the dominance problem.[1] Antimerger policy is clearly complicated if these theories each apply in many individual situations. If this occurs, then a broad-based policy toward mergers that applies to all situations may be wrong a substantial proportion of the time.

Obviously, current antimerger law is intended to have a beneficial effect regardless of the exact economic model assumed. Ideally it reduces both the occurrence of mutual interdependence (or collusion) and the probability of dominant firm pricing by maintaining the largest possible number of firms in a market, thereby maximizing the number of independent decisionmakers.[2] This result of antimerger policy would reduce the likelihood of

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[1] The different types of equilibria attained under the dominant firm and collusion models have implications for competition policy. In the first case mergers or growth of small firms is clearly to be encouraged to reduce resource misallocation. However, in the second case, growth of smaller firms (or elimination of small rivals) may simply lead to a more stable collusive outcome.

[2] The largest possible number of firms is (or should be) dependent upon the minimum size of firm necessary for efficient production. A reading of the 1968 Justice Department merger guidelines shows that scale economies or merger-specific economies were given little role as a defense; but efficiency considerations may well have been considered in setting the original guideline levels. For a discussion of the welfare tradeoffs between a higher probability of collusion and economies resulting from a specific merger, see Williamson (1968a, b; 1969), DePrano and Nugent (1969), Jackson (1970), Ross (1968), and Posner (1975).

successful oligopolistic interdependence and improve performance
if the traditional literature on oligopoly is correct.[1,2]

THE WELFARE COST OF MONOPOLY

An important economic rationale behind the enforcement of the
antitrust laws flows from the argument that market power can lead
to misallocation of resources and welfare losses. These losses
include those incurred from production of the wrong set of goods
and services, from technical inefficiency in production, and from
production of inappropriate quantities of output.[3] While all
economists recognize these potential costs of monopoly, there is
considerably less agreement on how substantial the costs really
are. Early work in estimating the allocative cost portion of the
welfare loss by Harberger (1954) and Schwartzman (1960) led to
the conclusion that the welfare costs of monopoly probably
amounted to a very small percentage (.1 percent) of gross national

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[1] Stigler (1955) argued the antimerger law was needed to prevent
situations which give rise to monopolistic (oligopolistic?)
practices. He offered a set of merger guidelines, in part, to
reduce the role of discretion by the antitrust authorities in
administering the Clayton Act.

[2] For a more complete review of oligopoly literature, see Scherer
(1980), pp. 151-168.

[3] See Kamerschen and Wallace (1972) for discussion of a longer
list of possible losses from monopoly power. Leibenstein (1966,
1978) has added X-inefficiency to the list in arguing that monopo-
listic firms may allow costs to rise since they are not subject to
stringent market constraints. This position is not, however,
without vocal critics. See Stigler (1976). For a review of
literature on the possible cost increasing aspects of monopoly
power due to suboptimal capacity, excess capacity, X-inefficiency,
and rent-seeking behavior, see Siegfreid and Wheeler (1981).

product. That the allocative effects were small was given further support in the work of Worcester (1973). These studies undoubtedly led to the belief held by many economists that the allocative costs associated with monopoly power were relatively small in the aggregate. However, other theoretical and empirical work by Kamerschen (1966), Bergson (1973), and Cowling and Mueller (1978) might lead one to question that view.

Kamerschen found that the aggregate loss could be as high as 6 percent of GNP when he used estimated industry demand elasticities rather than assuming (as Harberger did) that all elasticities were unitary.[1] Cowling and Mueller (CM) have obtained a range of welfare loss estimates for the United States that include values as low as 4 percent and as great as 13 percent of gross corporate production.[2] These estimates provide some support for the notion

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[1] Since higher elasticities lead to greater welfare loss estimates when using the Harberger methodology, Harberger's assumption of unitary elasticities for all industries insured that he would find low total losses. See Cowling and Mueller (1978), pp. 729-30, and Harberger (1954), pp. 81-2, note 2. However, this argument applies only to Harberger's particular method of calculating the welfare loss. As a general proposition, elasticity and welfare losses would be inversely related. See Landes and Posner (1981), pp. 991-96.

[2] CM compare the dollar values of the welfare loss to Gross Corporate Product. On the basis of average Gross National Product over the period the percentages are substantially lower, ranging from 0.6 percent to 2.2 percent of GNP. Of course, the appropriate comparison for policy purposes is not the absolute welfare loss versus GNP but rather the loss relative to the cost of reducing or eliminating the loss. Precisely what a well-constructed cost/benefit analysis of this tradeoff would show appears to be a matter of considerable debate.

that Harberger's estimates were too low. While the CM study corrects some of the shortcomings of Harberger's analysis, the upper range of the estimates depends crucially on the argument that a portion of advertising expenditures should be added to monopoly profits since, in their view, these expenditures can be regarded as excessive. They also add total advertising expenditures directly to the loss estimates due to the Tullock (1967)-Posner (1975) argument that such expenditures to attain monopoly power are a pure social loss. This procedure virtually assures that the welfare loss will be large and that the individual firms identified as the major producers of the loss are the large advertisers.[1] Those with a less extreme view of the evils of large scale advertising would be better served by placing greater reliance on the lower end of the range of welfare loss estimates.[2]

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[1] In addition to the research of CM, Siegfried and Tiemann (1974) have noted that estimates of the welfare cost can, in many instances, be attributed to specific industries. However, their attribution of a major portion of the welfare loss to the drug industry (due to its large R&D and advertising expenditures) has been challenged by Cocks (1975). Marvel (1980b) has also identified a group of highly concentrated industries that have a high probability of exhibiting monopolistic performance. His procedure is based, in part, on an examination of the stability of rates of return. Marvel's work is discussed in more detail in a later section.

[2] For a forceful defense of advertising and its lack of effect on welfare loss, see Worcester (1978).

Still, the lower end of the range indicates fairly substantial

absolute (if not percentage) losses due to monopoly.[1]

Given the lack of agreement in the welfare loss literature,

one cannot be too sanguine about basing policy prescriptions on it

alone. However, it does seem clear that some monopoly elements

exist and that an ability to pinpoint the sources of welfare loss

would allow greater precision in policies designed to reduce the

welfare loss imposed by monopoly. Whether the cost of pinpointing

the sources and pursuing a remedy is sufficiently low to make the

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[1] The losses estimated by CM are derived using a technique that
obviates the need for direct estimates of industry elasticities as
in Kamerschen's case. Using a COMPUSTAT tape sample of 734 firms
for 1963-1966, CM sum the "excess" profits (those above a competi-
tive return on capital) for the firms, including adjustments for
excessive advertising expense. They do not, however, include any
firms whose "excess" profits are negative. (That is, all firms
earning less than the mean rate are deleted. See CM (1978, p.
731)). This procedure might be thought to be preferable to making
arbitrary assumptions concerning demand elasticities in various
industries, but it is still subject to a number of nontrivial
criticisms. First, it is a partial rather than general equilib-
rium approach (see Bergson (1973)). Second, it seems to view all
monopoly profits as a net loss. If monopoly profits are largely
due to efficiency of large firms relative to smaller rivals, the
profits might be better viewed as rents that cannot be bid away.
(Much more will be said about this point in the section on the
revisionist view of the concentration/profitability relationship.)
Third, the treatment of advertising expense as a pure loss is
extreme, although CM argue that some other expenses incurred to
gain or maintain monopoly power are not included in the loss
estimate, making their calculations conservative. Recognition of
these problems is probably one reason that CM offer no policy
prescription even though they believe the welfare loss to be
substantial. (See CM, p. 746).

effort worthwhile is a broader problem that this literature does not address.[1]

THE ADMINISTERED PRICE HYPOTHESIS

The early oligopoly literature did not lend itself easily to empirical testing due to the relatively vague nature of the theories and the unavailability of inexpensive statistical and computational aids. However, one hypothesis did emerge: that being the thesis of administered pricing. Beginning with Means (1935) economists have sought to determine whether prices in concentrated industrial markets are less flexible than those in other environments. Until 1970, the accepted answer was yes. Work on a relatively small sample of Bureau of Labor Statistics data (BLS) had indicated that the price series of many industries were fairly insensitive to general economic trends. They tended to remain high in recessions and rise less slowly during periods of prosperity. Means did not originally argue that administered prices in an oligopoly setting would necessarily be higher than those in competition (though he might well have believed that), but only that prices would be more stable and less subject to

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[1] Pinpointing the sources of monopoly power is a tricky task at best given the inability of accounting profit data to reflect true economic profits due to monopoly. See Brozen (1969), Solomon (1970), and Stauffer (1971) for discussions of the accuracy of accounting data for these purposes.

change due to exogenous factors in industries using administered pricing.[1]

Since 1970 the bulk of the research in this area has questioned the validity of the administered pricing hypothesis.[2] The first major attack on the administered pricing hypothesis was the work of Stigler and Kindahl (1970). They argued that the BLS data on list prices used by Means was clearly deficient and that other more reliable data on transactions prices collected by the National Bureau of Economic Research (NB) led to a different answer than that obtained by Means. Stigler and Kindahl analyzed this alternative data and discovered that the NB and BLS price trends did not differ during periods of stable or rising prices, but the BLS price indices of list prices fell more slowly than the NB transactions price series during periods of generally falling

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[1] Means' administered price hypothesis should not be confused with the rather different argument that large firms in concentrated industries "cause" inflation. The two hypotheses can have precisely opposite empirical implications. For some evidence contradicting the concentration-inflation hypothesis see Wilder, Williams, and Singh (1977).

[2] This literature does not argue that pricing formulas and attempted coordination of pricing policies do not exist in oligopolies, but that, according to the available data, the existence of these practices and institutions does not have the net effect of reducing price flexibility.

prices.[1] More importantly, Stigler and Kindahl found no convinc-
ing evidence of administered pricing when a review of two contrac-
tions (July 1957 to April 1958, May 1960 to February 1961) and
two expansions (April 1958 to May 1960; February 1961 to November
1966) was undertaken. This was true even though the sample chosen
by the authors was designed specifically to include those
industries thought most likely to exhibit administered pricing
behavior.

Means (1972) offered a defense of the administered pricing
hypothesis using Stigler's and Kindahl's NB data. However, find-
ing evidence of such pricing behavior in the NB data required
arbitrary reclassification of several industries into a "market
dominated" pricing category and redefinition of the turning points
of the business cycle.[2] Regardless of whether one wholeheartedly
accepts the criticisms of Means' work, they do indicate that a
finding of administered pricing is very sensitive to the data used

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[1] Recent statistical time series research has indicated that at
least a subset of the BLS and NB price series examined by Stigler
and Kindahl do differ significantly, with the NB transaction price
series being more sensitive to market conditions than the BLS
series'. See Wecker (1981).

[2] See Stigler and Kindahl (1973). It is not entirely clear
whether Means applied more or less subjective criteria to choose
his sample than those employed by Stigler and Kindahl. However,
Stigler and Kindahl do make a fairly strong case that the
administered price thesis is so vague that it is virtually
unassailable (and it is therefore an empty theory.)

and the interpretation of the business cycle.[1] These facts alone
could lead one to question the validity of a test purporting to
demonstrate the existence of administered pricing.

With one notable exception, more recent research has found
little support for the administered pricing hypothesis and even in
the exception the evidence is equivocal. Recent work in which
little, if any, support was found includes Lus garten (1975),
Qualls (1977a, b), and Garber and Klepper (1980). Lustgarten
studied the period from 1958 through 1970 using BLS data on 225
4-digit manufacturing industries. He found that price changes
were unrelated to four-firm concentration and that prices in
concentrated industries were not less responsive to the recessions
of 1960-61 and 1969-70 than prices in less concentrated markets.
Qualls (1977a) examined BLS price index data for 85 4-digit manu-
facturing industries from 1967 through 1972 and found little
evidence of administered pricing, although he does note that dur-
ing one subperiod (1967 to 1969) a U-shaped relationship consis-
tent with the hypothesis existed between price changes and

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[1] See Kottke (1978) who discusses data and judgmental problems
inherent in any test of administered pricing. Kottke's paper
focuses on the most recent research regarding this subject, but
his criticisms are applicable to much of the research on
industrial pricing.

concentration.[1] Qualls' model does not perform particularly well in terms of overall explanatory power or in coefficient significance, but it does seem sufficient to support his conclusions. In a second related paper, Qualls (1977b) examined a 79-industry subset and found a positive relationship between the variability of price/cost margins and 4-firm concentration directly contradicting the predictions of his version of the administered price hypothesis.

In a final study which questions the administered price thesis, Garber and Klepper (1980) argue that prior studies by Weiss (1966), Dalton (1973), and Cagan (1975) were in error because they used an estimating technique that did not adjust for an errors-in-variables problem. To estimate the effect of concentration on pricing behavior, Weiss had regressed the percentage change in price on measures of average labor and materials cost changes and output changes as well as four-firm concentration. Garber and Klepper note that such a model is inappropriate because among other things it assumes fixed (rather than random) coefficients, because it employs average rather than marginal cost measures, and because it uses a cost-change weighting scheme that does not yield weights that sum to unity as the competitive model

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[1] Qualls (1977a), pp. 7-9, argued that competitive firms and very tight oligopolies might be expected to exhibit pricing flexibility while looser oligopoly structures (more subject to the collapse of pricing discipline) might exhibit less flexible prices. This reasoning led him to expect a U-shaped relationship between price changes and concentration during cyclical expansions.

would suggest. To remedy this situation Garber and Klepper estimate the model making specific assumptions regarding the nature of the measurement errors found in the Weiss model. Using data on 365 4-digit SIC manufacturing industries from 1958 to 1971, they find evidence that is generally consistent with the competitive pricing model. In the few time periods where the concentration coefficient is nonzero (a result inconsistent with the competitive model), the coefficients are such that the popular interpretation of the administered pricing hypothesis is also rejected. That is, the authors find "that relative to less concentrated industries, prices increased less in more concentrated industries during the expansionary period of 1967-1968 and the recessionary period of 1969-1970, and by a greater amount in more concentrated industries during the recovery of 1970-1971."[1]

The study of Garber and Klepper and the bulk of the other studies of administered pricing since 1970 seem to imply that the thesis is not a particularly robust challenger to the competitive model of price determination. However, one recent study does provide some weak support for the administered price thesis. Weiss (1977) examined the Means (1972) and Stigler-Kindahl (1970, 1973) data from the National Bureau of Economic Research arguing that the two data sets were not particularly different. Using Means' classification of prices as market dominated, intermediate, or administered, Weiss found that administered prices in the NB

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[1] Garber and Klepper (1980), p. 430.

series, used by Stigler, fell substantially less than market dominated prices during two recessions (July 1957 to April 1958; May 1960 to February 1961). However, he found that the prices of intermediate cases fell even more than market dominated prices, leading one to be suspicious of the classification scheme.[1] Weiss's evidence relating to recoveries was even weaker and gave little evidence for the administered price hypothesis (or any other pricing theory for that matter). Despite his relatively weak evidence, Weiss concludes that "the NB series do follow patterns that support the administered-price hypothesis."[2] A more conservative conclusion (and one in keeping with the empirical results and the level of data accuracy) would seem to be that the evidence is inconclusive.

Given the current state of the research on administered pricing, it is difficult to claim that such a theory provides a firm base of support for antitrust action. The evidence, based on rather poor aggregate price series data, is equivocal at best, and the most that an advocate for the hypothesis could currently claim is that the theory has not yet been fully rejected.

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[1] It is interesting that Weiss (1977, p. 617) apparently does not consider an examination of the relationship between price varia- bility and concentration over differing periods of the business cycle to be a true test of the administered price hypothesis. He cites Means for the proposition that prices may be "administered" even in relatively competitive industries with few participants. If Weiss's interpretation of Means is correct, then corroboration of the hypothesis is useless from a policy standpoint, since one should care little about administered prices per se if such pric- ing is generally consistent with a competitive outcome.

[2] Weiss (1977), p. 619.

THE CONCENTRATION-PERFORMANCE RELATIONSHIP

A. What Did We Think We Knew in 1968?

A basic economic element underlying horizontal antimerger policy is the notion that mergers reduce the number of independent competitors, increase concentration, and lead to a higher probability of tacit or explicit collusion. Since this idea served as the basis of economic and legal analysis prior to 1968, we will briefly review a representative portion of the literature produced on this issue to determine the state of economic knowledge at the time the 1968 Department of Justice merger guidelines were promulgated.

The empirical economics literature supporting the structure-conduct-performance paradigm was begun by Joe S. Bain (1951, 1956).[1] In his pioneering work, Bain focused on the role of concentration and its relationship to performance. He investigated empirically the relation between after-tax profit rates on equity and level of concentration for a select sample of 42 U.S. manufacturing industries for the time period 1936-40.[2] Using average profit rates for dominant firms as observations and dividing the

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[1] The following discussion of Bain's and Michael Mann's work is adapted from Qualls (1972).

[2] Since oligopoly theory predicts high prices (but not necessarily high profits), Bain's use of profitability as a performance measure can be questioned. As an alternative, one might use price/cost margins rather than profitability, but data on these margins were not available to Bain.

sample into two classes—high concentration (eight-firm concentration ratio equal to or greater than 70 percent) and moderate-to-low concentration (eight-firm concentration less than 70 percent)--Bain found a statistically significant difference between the average profit rates on equity of the two classes.

Since he found only a weak regression relationship between equity profit rates and concentration in the whole sample and within each of the two concentration classes, he tentatively concluded that there is, perhaps, a critical concentration threshold (presumably somewhere in the neighborhood of 70 percent 8-firm concentration) above which industry performance in terms of resource allocation leans toward monopoly and below which industry performance tends toward competition.

In his later study dealing with a restricted sample of 20 industries for the time periods 1936-40 and 1947-51, Bain (1956) investigated the impact of both concentration and barriers to

entry.[1] In regard to concentration, the sample was divided into
two groups, the demarcation again being an 8-firm concentration
ratio of 70 percent. In regard to barriers to entry, the sample
was divided into three groups designated as having "very high"
"substantial," and "moderate to low" barriers to entry. Industry
classification was accomplished on the basis of a judgmental
assessment of the general magnitude of entry barriers.

Bain once again found a distinct cleavage between concentra-
tion and class-average profit rates on equity, and a distinct
difference between the class-average rate for "very high" and that
for "substantial" barriers. A smaller difference between class
average profit rates for "substantial" and "moderate to low"
barriers was found in both time periods. In order to test for
separate impacts of seller concentration and barriers to entry, a

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[1] We should note at the outset that the term "barriers to entry"
has different meanings to various authors. Bain used the term to
denote situations in which an entrant would be at a cost disadvan-
tage for any reason. For instance, standard scale economies would
present a barrier to entry at less than minimum efficient size in
Bain's rubric. Stigler (1968a), on the other hand, defined
barriers as "a cost of producing (at some or every rate of output)
which must be borne by a firm which seeks to enter an industry but
is not borne by firms already in the industry." Thus, elements
that could present barriers in Bain's terminology may not present
barriers in Stigler's, but simply represent possible scale
economies. The current literature does not seem much clearer on
this point, with many researchers still equating scale economies
with barriers to entry. See Spence (1981) and von Weizsacker
(1980) for a comparison. Indeed, some (e.g., Weiss (1979), pp.
1119-1123 economists have argued that failure to recognize scale
economies as a barrier can make the term "barrier" useless in
evaluating market power.

cross-classification scheme was utilized. The high-concentration/ high-barriers class was found to have a distinctly higher mean profit rate on equity than the high-concentration/substantial-barriers class, but the difference was not subjected to statistical tests of significance.[1]

In a subsequent study Michael Mann (1966), following the approach of Bain's later study and using a 30-firm industrial sample covering 1950-1960, found a statistically significant difference between the class-average after-tax profit rate on equity for 21 industries of "high" concentration and that for 9 industries of "moderate to low" concentration.

Classifying industries on the basis of estimated heights of entry barriers, Mann found the "very high barriers" group to have a substantially higher average rate of return than the "substantial barriers" class. The average for the "substantial barriers" group was higher than that for the "moderate to low barriers" group; however, this difference was less than half that between the "very high" and "substantial" barrier groups.

The 21 industries of high concentration were then classified by Mann into the 3 entry-barrier categories, and the results paralleled those of the Bain study. As in Bain's case, a clear difference was found between the class-average rates of return on

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[1] Bain was very careful to indicate the shortcomings of his methodology and data and the tentative nature of his results. These qualifications were not reemphasized for at least 15 years. See Brozen (1971a).

equity for the high-concentration/high-barriers and high-concentration/substantial-barriers classes, but no such difference was observed between the two lower barriers-to-entry classes.

Mann agreed with Bain's conclusion that the failure to find distinct differences in class-average equity profit rates (where just the industries of high concentration are considered) between the "substantial barriers" and "moderate to low barriers" classes might be due to the condition of entry for several or most of the industries classified in the "moderate to low barriers" group. Since this group was characterized by "ineffectively impeded entry", established firms could charge high prices and earn relatively high rates of return for only a short time before entry would be induced.

These studies implied that both concentration and the height of entry barriers are important structural dimensions influencing market performance. However, Mann noted a need for further research because "rates of return may be inadequate indicators of price/cost margins . . . (and) the particular barrier-to-entry classification into which an industry was placed may be incorrect . . . ." (1966, p. 300).

To correct the problem involved in the use of return on equity as a profitability measure, Qualls (1972) investigated the results obtained when the return on sales above a competitive benchmark return (a proxy for the price/cost margin) was substituted for the return on equity as a performance measure in samples

close to those used by Bain and Mann. In addition, he obtained results using both Mann's entry barrier classifications and a refined classification. Using small samples of 20 and 30 industries, he found evidence that generally supports Bain and Mann and that strengthens the resulting relation between concentration and profits and that between barriers to entry and profitability. An overall conclusion to be drawn from these comparisons of average rates of return is that high barriers to entry matter and concentration may matter even if barriers are low, although the latter half of this conclusion is more tenuous.[1]

Prior to 1968 only one major study had appeared which indicated that the relationship between profitability and concentration was tenuous.[2] Using a larger sample than that available to Bain, Stigler (1963) found that during the 1938 to 1950 period

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[1] There is a considerably larger literature written prior to 1968 supporting the notion that concentration and profitability are positively related. Perhaps the best known of this literature is the work of Collins and Preston (1968). They used the largest and best available sample which consited of 243 four-digit SIC code industries. This data set indicated that the association between price/cost margins and concentration rose from 1958 to 1963 and that the relationship was stronger for consumer goods than producer goods industries. They also argued, based on concentration changes, that lower costs did not appear to explain the higher profitability of large firms. A complete review of the literature on profitability and concentration prior to 1968 may be found in Weiss (1974).

[2] Comanor and Wilson (1967) had found that concentration was an insignificant determinant of profitability when scale economies and capital requirements were simultaneously considered, but the insignificance of the coefficient of the concentration measure could have been due to collinearity with the scale economies measure. See Davies (1980).

there was not a significant relationship between concentration and after-tax return on assets for all IRS minor manufacturing industries. He also found that for a small subset of 16 industries that overlaps with Bain's (1956) sample a positive relationship did exist. Together these findings would tend to indicate that Bain's relatively small sample could have been biased. Stigler also investigated the relationship during the early to mid-1950's and found a significant positive correlation between concentration and return on assets consistent with the monopoly power explanation. Thus, Stigler's evidence was equivocal.[1] The following year Stigler (1964) presented more evidence from a very limited sample of highly concentrated industries that was consistent with his information-based theory of oligopoly and with at least one version of the traditional structure-performance paradigm. He found evidence that there is a relationship between concentration (as measured by a 4-firm concentration ratio or a Herfindahl (H) index) and rates of return but only when H exceeds .25 or when 4-firm concentration exceeds 80 percent.[2]

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[1] Weiss (1974), pp. 200, 203, has argued that Stigler's insignificant results for the 1940's are probably due to the existence of price controls during World War II. Weiss would not necessarily expect the profits/concentration relationship to be found for any such period. In addition, the markets delineated by IRS minor industries are very broad and poorly defined relative to four-digit SIC industries or line of business data which were used in later research.

[2] The Herfindahl index is defined as the sum of the squared market shares of the individual firms. This measure decreases as the number of firms increases and increases as the dispersion of firm market shares from the industry average increases.

Although Stigler's work did not provide strong support for the notion that market concentration was a general problem, his studies did indicate that increased concentration could have detrimental effects in some time periods or when concentration was relatively great initially. As such, they would not have done much to dissuade the Department of Justice from establishing horizontal merger guidelines.

As of 1968 it seems that the empirical evidence amassed by economists pointed toward a significant positive relationship between concentration and profits. This was particularly true when barriers to entry existed and when concentration was relatively high. In addition, the traditional explanation for the relationship--that it was caused by monopoly power--was well accepted and had yet to be seriously attacked. Finally, evidence on scale economies seemed to indicate that in most industries the cost savings related to plant and firm size were exhausted at relatively low market share levels implying that low costs were inposed by prohibiting further firm growth in these industries.[1] Given this understanding, the 1968 Department of Justice horizontal merger guidelines appeared to be on a rather firm footing.

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[1] For a review of this literature, see Scherer (1970), pp. 79-103. At the end of his review of plant and firm scale economies studies Scherer states what seemed to be a typical view in the late 1960's:

> "It is evident from studies of scale economies
> in particular industries and the observation
> of broad survival patterns that in many and
> perhaps most American industries high concen-
> tration is not a technological, marketing, or
> financial imperative."

The Neal Report on antitrust policy also indicated that several influential members of the economics and legal professions felt that broad-based antitrust policies were not inappropriate in 1968.[1] The majority of the members of the panel recommended an Act to deconcentrate individual oligopoly markets[2] and a second Act designed to prevent large conglomerate mergers.[3] In a separate statement attached to the Report, Paul MacAvoy noted the risks involved in deconcentrating individual oligopoly markets but concluded that they were worth the expected benefits. In a general statement of the economic basis for the oligopoly deconcentration recommendation, he noted that:

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[1] White House Task Force (1969), delivered to the President on July 5, 1968. The Report's panel included Phil C. Neal, Chairman, William Baxter, Robert Bork, Carl Fulda, William Jones, Dennis Lyons, Paul MacAvoy, James McKie, Lee Preston, James Rahl, George Reycraft, Richard Sherwood and S. Paul Posner.

[2] The Report contained specific target market shares for the reduction of concentration in oligopoly markets. It advocated a reduction of 4-firm concentration to below the 50 percent level and reduction of individual firm shares to 12 percent or less. See White House Task Force (1969), pp. A-2, A-13.

[3] The majority of the panel also proposed substantial revision of the Robinson-Patman Act, compulsory patent licensing, the accumulation of better information relevant to the formulation of antitrust policy, and that resale price maintenance be declared per se illegal.

> "Economic evidence, from a large number of
> research articles and monographs on the
> relation of concentration to industry
> performance, provides a sound basis for
> predicting general effects from reducing
> industry concentration. The lack of evidence
> indicating general loss of efficiencies from
> deconcentration furnishes further strong
> support for this policy. There is substantial
> basis on which to conclude that 'remedies to
> reduce concentration should be made available
> as part of a comprehensive antitrust policy.'
> Work remains to be done to establish that
> oligopolies of four or five firms can be
> expected to restrict output and raise price
> under most or all market conditions, but the
> evidence presently available is strong enough
> to provide rationale for this legislation."[1]

Although not everyone on the panel shared his view of the state of

our economic knowledge, MacAvoy's position probably reflected

generally accepted economic wisdom in 1968.[2]

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[1] The White House Task Force (1969), pp. 1-B to 2-B.

[2] In separate statements attached to the Report, Robert Bork and
Richard Sherwood indicated considerable skepticism about the
economic foundation for the market deconcentration proposal. Bork
noted that:

> "My objection to the proposed statute is
> that the studies relied upon are shaky and
> open to question and that the correlation
> [between industry concentration and
> profitability], if it were shown to exist,
> would prove nothing."

> "The dissolution of such firms would be a
> disservice to consumers and to national
> strength. When firms grow to sizes that
> create concentration or when such a structure
> is created by merger and persists for many
> years, there is a very strong prima facie case
> that the firms' sizes are related to
> efficiency." [pp. 1-A to 2-A]

B. Revisionist Views and Debate, 1970-?

1. The Persistence of Profits, Concentration and Barriers

To Entry. Since 1970 a reinterpretation of the concentration-profitability literature has been gaining adherents. The well-accepted monopoly power explanation for the concentration/profits correlation was first criticized in a series of writings by Yale Brozen (1970, et seq).[1] He argued that previous findings were largely the result of a market disequilibrium. He presented evidence that the high rates of return in the concentrated industries studied by Bain, Mann, and Stigler tend to disappear in later years. In addition, in the relatively unconcentrated industries studied by Bain (which had relatively low profits) the profit rate tended to rise. Thus, the high rates of return

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(footnote continued)

In another separate statement Sherwood argued that "in the present state of economic and legal knowledge the sweeping condemnation which the Task Force has accorded them [large firms, mergers, and single patent licensees] appears to be rooted in dogmas I do not share." (pp. 1-C to 2-C) Interestingly, in reference to the Justice Department's recently proposed merger guidelines of 1968, he went on to decry the trends away from "hard economic and legal analysis to the lotus-land of percentage tests." (p. 8-C)

[1] There are those who can reasonably claim precedence to Brozen. For example, Bork's dissent from the Neal Report's position and Demsetz's (1968) influential work on utility regulation should have had some influence on Brozen's work. In Demsetz's article, he argued that even a natural monopoly market need not necessarily lead to poor performance if potential service providers can bid for the right to serve the market. For a recent short critique of Demsetz's approach see Ekelund and Hebert (1980).

observed by all three researchers may have been indicative of disequilibria rather than collusion among firms in concentrated industries.[1]

In a reformulation of Bain's study, Brozen (1971a) noted that Bain's sample of 42 industries was biased[2] and that a larger sample of industries (using FTC data on 89 industries) does not produce the positive concentration/profitability relationship in two of the years studied by Bain.[3] Brozen also argued that Bain's decision (required by a paucity of data) to represent industry profitability by the profit rates of the large firms in the industry biased the results. This could be true if concentrated industries became concentrated due to the fact that larger firms were more efficient or innovative than smaller rivals. The relatively large firms would obtain high profits due to efficiency rather than monopoly. Ignoring the smaller (less efficient)

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[1] Precisely why the samples would have all shown disequilibria in the three time periods used is unclear.' Also, the fact that concentrated industries had profits above equilibrium and unconcentrated industries had profits below equilibrium in each case is certainly indicative of a nonrandom pattern. Thus, in his first study Brozen did not show that the relationship between concentration and profits was spurious, but only that it was not persistent in any individual industry.

[2] The argument that Bain's sample was not representative due to lack of a large sample of industries seems to be well accepted. See Weiss (1974), pp. 221-2.

[3] Brozen's FTC data related only to 1939 and 1940, not Bain's entire 1936 to 1940 period. On that basis, a claim of disequilibrium observation is at least as applicable to this portion of Brozen's study as it is to Bain's.

firms' profits would then bias the concentrated industry's rate of return upward.[1] Brozen found evidence for this view in the fact that, for the concentrated industries in Bain's sample, the largest firms tended to earn higher returns than the next largest set of firms. As Brozen expected, the converse was true in the unconcentrated industry subsample. This and other evidence presented by Brozen led him to conclude that "it seems that the less concentrated industries were less concentrated because that was the efficient pattern of organization just as the more concentrated became so because that was the efficient way to organize them. The market selected the appropriate structure for each industry."[2]

Brozen's analyses of these issues did not go unchallenged. Wenders (1971), and MacAvoy, McKie, and Preston (1971) took issue with the argument that a movement of rates of return toward the norm was occurring quickly enough in concentrated markets to vitiate the problem of industrial concentration. These authors reworked portions of the Stigler, Bain, and Mann studies to show that profits in industries with high and stable concentration did not fall to the sample averages within 10 years. They took this as evidence of persistently high rates of return and poor economic

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[1] The converse could be true in unconcentrated industries where relatively small firms are the presumably most efficient. Using large firm profitability as a proxy for industry profitability would bias unconcentrated returns downward.

[2] Brozen (1971a), p. 367.

performance.[1] However, Brozen (1971a, b) notes that as a theoretical matter, entry barriers are more relevant than concentration and he also shows that his critics relied on very small samples or on inappropriate or incomplete classifications of industries into the "high stable concentration" grouping. When these problems are corrected (to the extent possible), the already weak evidence of persistently high profits weakens further.[2] Although Brozen may have occasionally overstated his case,[3] he seemed to have won this round of debate.

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[1] Wenders also argued that the correct comparison would be whether profits in high-stable concentration industries fell to equilibrium as fast as they would were the industries unconcentrated.

[2] Only Wenders' analysis of Stigler's data avoids Brozen's axe and even there Brozen shows that a larger (but still small) sample of FTC data for 1939 to 1940 indicates no persistence of high profits.

[3] Brozen (1971a), p. 491, states:
> "We need not, however, be concerned that above-normal profits are more prevalent in concentrated industries. It appears that findings to this effect are the consequence of the use of small samples. Larger samples do not show any relationship between concentration and rates of return."

Subsequent research on larger samples and better data than that used by Brozen caused this conclusion to become questionable. During the early and mid-1970's the issue was more appropriately cast as whether high profits are persistent and whether antitrust policy is a useful means of approaching the problem of persistently high returns in concentrated markets. We may, however, find that Brozen was correct (albeit for the wrong reason) depending upon the ultimate findings from line-of-business data now being developed. (See section V.C.) This data should be welcomed by all sides in the debate due to its higher quality and lack of contamination by diversification. Of course, Brozen (1969), among
(footnote continues)

The issue of persistent profits was further examined by
Qualls (1974), Mueller (1977), and Marvel (1980b). Qualls used
two different samples updating the 30-industry sample of Mann
(1966) and the 220 large-firm sample of Shepherd (1972). Follow-
ing Bain (1956) and Brozen (1971d), he argued that concentration,
by itself, would not necessarily lead to higher prices and profits
unless it was supported by barriers which could keep potential
entrants out of the market. Qualls examined the differences
between average excess price/cost margins in industries classified
as having high, substantial, and moderate-to-low entry barriers.[1]
He also examined differences in these averages based on high (70
percent 8-firm) concentration versus low concentration industries.
For Mann's sample, Qualls found significant differences between
price/cost margins for industries with high versus low entry
barriers and between margins for industries with high versus low
concentration. Furthermore, he found that this relationship was
the same in 1961-65 as in 1950-1960, indicating that the margins

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(footnote continued)

others, has also attacked the use of accounting data as a guide to
economic rates of return and his analysis of those rates has
always been done in an in arguendo fashion. See Brozen (1971b),
pp. 512. Inflation may also have some rather important effects on
profitability comparisons in large samples. For some discussion
of the issues involved see Shoven and Bulow (1975, 1976), Holland
and Myers (1980), and Fraumeni and Jorgenson (1980).

[1] Excess margins were determined to be the calculated price/cost
margin less a 6 percent "normal" after tax rate of return for the
average unconcentrated industry. See Qualls (1972), p. 149, note
8.

did not fall to the average over time as one might expect if competition prevailed. Using Shepherd's sample, Qualls found that firms in highly concentrated industries tended to obtain higher price/cost margins than those in less concentrated industries and that barriers to entry generally had the anticipated impact on margins. These results appeared to be fairly consistent over time. That is, the high price/cost margins found in concentrated industries having entry barriers did not erode over the three periods (covering 1951 to 1968) studied. As a general matter, Qualls considers entry barriers and concentration to be inter-active and fully capable of producing persistently high economic profits.

Qualls' work was supported by another investigation of varia-tion in rates of return on gross assets over time by Mueller (1977). Mueller reasoned that if profits above and below the norm are a transitory phenomenon, the probability of a firm having a given rate of return at any point in time should be independent of its previous profit rate, for a period taken sufficiently far back.[1] Mueller divided his sample, covering 24 years (1949 to 1973) and 472 individual COMPUSTAT firms, into 8 groups of 59 firms each based on their 1949 before-tax rates of return on total

[1] One objection to this approach might be that efficient firms would be expected to retain high profits for an extended period due to their efficiency. However, if no barriers exist, one would also expect less successful firms to copy those that are the most successful, thereby driving down the high rates of return. See Mueller (1977), pp. 377-8.

assets. He tested to determine whether the probability that a firm initially in one of these groups had an equal probability of being in any other group in the later period. Mueller found that firms that began in the highest profitability categories tended to stay there and the same type of relationship existed in the lowest categories.[1] Firms in the middle of the distribution, however, were more likely to move out of their initial group than might be expected. Mueller takes all of this as evidence that indicates "strong rejection of the competitive environment hypothesis."[2] In a second test, Mueller examines the probability that the level of current normalized profits for a firm is independent of initial profits. He finds that this is not the case and that although there is a tendency for high profits to fall and low profits to rise, firms with high profits retain those profits longer and more frequently than one would expect if a high degree of competition

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[1] There was a movement of firms out of their initial categories but not as much as one would expect if the competitive model was a good approximation to reality. If full mobility of firms existed, one might expect that a firm in group 1 in 1949 would have a .125 probability of being in that group in subsequent years. However, Mueller found the probability to be .34. Similarly, firms in group 8 initially had a .19 (rather than a .125) probability of remaining in that lowest profit group.

[2] Mueller (1977), p. 373.

prevailed. Finally, Mueller examines and generally rejects alternative explanations for his findings based on risk, sample bias, and superior talent of high profit firms.[1]

Finally, two interesting attempts to pinpoint those industries earning persistent monopoly profits are due to Marvel (1980b) and Lindenberg and Ross (1981). Using data on 267 low concentration (CR4 < 50 percent) 4-digit Census industries for 1967, Marvel obtained a "competitive benchmark" distribution of profit rates by regressing the price/cost margin on measures of capital and advertising intensity, geographic dispersion, industry growth, workers' hourly earnings, overhead payroll significance, a consumer goods dummy, and the significance of import competition.[2] Given the estimated coefficients and residuals from that benchmark equation, Marvel obtained estimates of the rates of return expected for 115 concentrated industries (CR4 > 50 percent) had they been competitive. He found that the mean of the distribution of residuals for the high concentration group was above that of

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[1] The missing element in Mueller's study is an explanation of how firms managed to maintain dominant status that leads to his results. It is also a bit surprising that the lowest profitability firms seem to remain in that category since one might expect them to exit the industry if their financial performance remained poor.

[2] While the use of a measure of import competition is not unknown, it is relatively uncommon in the concentration/profits literature. In this particular case, the coefficient of the import competition variable was insignificant. However, in another study Marvel (1980a) found a significant negative impact when an alternative (and more appropriate) estimation technique was used. The significance of import competition is also being emphasized in the recent antitrust law literature. See Landes and Posner (1981) and a comment by Schmalensee (1981).

the less concentrated group, indicating some monopoly contamination in the high concentration group. He also notes, however, that his estimates indicate that two-thirds of the high concentration group are competitive.[1]

Since the previously mentioned results are sensitive to his arbitrary classification scheme, Marvel tests his methodology by using 9 years (1963-71) of firm-level profit rate data for 778 firms from the COMPUSTAT file. In this analysis he found no significant difference in the rates of return on total assets achieved by the high-concentration monopoly group versus the other nonmonopoly groups,[2] but he did find that the monopoly group exhibited lower profit deviation within the group and a greater persistence of profit rates than other groups over time. While these results are not conclusive, they are consistent with the notion that monopoly power can cause significant problems in at least a not inconsequential subset of concentrated industries.

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[1] Over one-half of the industries with concentration above 80 percent were classified as exhibiting monopoly performance.

[2] Marvel argued that the most likely reasons for the equality of returns for the monopoly and nonmonopoly groups were measurement problems made worse by the fact that monopoly rents had probably been capitalized previously in the asset base. For a discussion of this now familiar argument see Tullock (1967) or Posner (1975).

Lindenberg and Ross (1981) use an approach that differs from virtually all previous research.[1] They argue that Tobin's "q," the ratio of a firm's market value to its replacement cost, should equal one for a competitive firm that does not earn monopoly or efficiency rents. Thus, actual "q" values can serve as an index of the upper bound of the monopoly rents obtained by firms. Using SEC data on replacement costs for 1976 and 1977 and a recursive estimating formula, the authors calculate the replacement cost of 246 large firms from 1960 through 1977. The market value of the firms is obtained from stock quotations and from calculations of the market value of each firm's debt taking the maturity of the various debt issues into account. The values of "q" for each year are then averaged over the 1960 to 1977 period to obtain an index of the upper bound on monopoly rents. Lindenberg and Ross find that values of "q" are above one for most firms[2] and that the dispersion of market power across firms and industries is quite wide. Firms that tend to have high "q" values are often those with unique, differentiated products and factors of production.

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[1] Thomadakis (1977) attempted to use a market value approach to calculating the relationship between market structure and profitability similar to that used by Lindenberg and Ross. He had concluded that profits due to efficiency or market power did not seem to erode over time and that firms seemed to be able to extend current market advantages into the future. Some of the advantages of Lindenberg and Ross's work over that of Thomadakis include better accounting for the replacement cost of the firm's capital and a larger sample.

[2] The overall average "q" after adjustment for economy-wide and firm effects was 1.5.

Conversely, low "q's" are often observed for highly regulated firms and dying or competitive industries.

Lindenberg and Ross also examined the relationship between "q" and two more traditional measures of monopoly power: the four-firm concentration ratio and the Lerner index.[1] They find that the Lerner index is significantly associated with "q" and that it tends to provide a lower bound for "q."[2] However, they find a positive but insignificant relationship between "q" and concentration.[3]

To date, the net results of the profit persistence debate seem to be that the profits of leading firms (with barriers to entry or in concentrated industries) do tend to erode over time, but only slowly and not as quickly as the existence of a highly competitive economy would suggest. To the extent that this profit persistence is due to efficiency considerations, we would not be too concerned. In cases where it is due to avoidable entry barriers it is still not obvious that the situation can be

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[1] The Lerner index is defined as the deviation of product price from marginal cost divided by the price. See Lerner (1934).

[2] This result is derived from a regression of "q" on the price-cost margin across 246 firms and 18 years. However, the authors argue tht the Lerner index is not a fully adequate measure of market power because some of the deviation of price from marginal cost occurs due to scale economy effects and does not contribute to market value in excess of replacement cost. See Lindenberg and Ross (1981), pp. 8-9, 28.

[3] This result was obtained using only 1972 data for 246 firms. The regression coefficient of concentration is insignificant when "q" is regressed on concentration alone or when the price/cost margin is also included in the model.

remedied at reasonable cost. However, some form of industrial policy aimed at slowing the growth of concentration or at reducing barriers to entry or firm mobility may be supportable based on the evidence from the debate. It seems that much of the debate boils down to whether barriers to entry and immobility among firms are really serious enough to be concerned with, and if they are, whether they can be reduced efficiently through antitrust measures. While Brozen was concerned with governmentally imposed barriers, Bain, Qualls, Mann, and Mueller seem to be concerned with a wider range of possible entry barriers including scale economies, advertising expense or intensity, patents, and the like.[1] As Qualls noted in 1974, at this stage of our knowledge, it seems likely that if we are going to be concerned with this problem we should "[p]erhaps . . . worry less about industries in which high concentration is unaccompanied by significant entry barriers. And perhaps attempts to reduce entry barriers should be regarded as important along with direct attacks on high seller concentration."[2]

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[1] The literature and debate concerning the existence and importance of these potential barriers (particularly advertising) is immense. We will not review it here. Discussion of the advertising barrier issues may be found in Ornstein (1977), Tuerck (1978), and Comanor and Wilson (1979), as well as Scherer (1980). For recent contributions to this literature many of which emphasize the entry enhancing features of advertising see Bloch (1980), Simon (1980), Spence (1980), Hirschey (1981), and Lynk (1981).

[2] Qualls (1974), p. 612. Similar thoughts have come from many quarters, including McEnally (1976).

2. <u>The Relative Profits of Large and Small Firms</u>. Following Brozen's lead, Demsetz (1973, 1974) began a debate on the relationship between concentration and the relative profitability of large versus small firms that would eventually span at least seven years. He opined that "in the absence of effective barriers to entry it would seem that the concentration of an industry's output in a few firms could only derive from their superiority in producing and marketing products or in the superiority of a structure of industry in which there are only a few firms."[1] He went on to argue that high profits resulting from the superiority of a particular size of firm need not be eliminated quickly by competition if the reason for the superior performance is firm-specific (such as superior management or specialized inputs).[2] Further, he characterized the attainment of monopoly as part of a competitive process to gain consumer acceptance. Monopoly power gained in this pursuit may not be attributable to the creation of entry

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[1] Demsetz (1973), p. 1. The simple logic is as follows: Firms became differentially profitable either because they charge high prices or have low unit costs or both. Given a lack of entry barriers, firms cannot long maintain prices above those of their competitors. Thus, low unit costs must be the explanation of high profits. These low unit costs could be the result of scale economies (as Brozen argued), or they could be due to innovations which reduce the unit cost structure of large firms at all output levels (as emphasized by Peltzman (1977)). Of course, to the extent that rivals can copy an innovation, or grow to efficient size, these factors will not lead to sustained high profitability.

[2] Demsetz seems to view competition as a slowly evolving process which, over time, weeds out inefficiency but which can yield high long-term rewards to the skillful entrepreneur.

barriers but to the "natural frictions and ignorance that charac-
terize any real economy."[1]

To support these views, Demsetz presents evidence that for 95
3-digit industry groups in 1963, the small firms tend to be less
profitable than large firms and that the differential profit-
ability of large firms does not fall as concentration (and the
probability of collusion) increases. Since an effective collusive
agreement would presumably include small as well as large firms,
the smaller firms should share in the gains from collusion.
Therefore, there should not be such a divergence of large and
small firm profitability if collusion is the correct explanation
for the positive relationship between concentration and profits.
However, such a divergence would be expected if large firms tend
to be the efficient form of organization in concentrated
industries.

Demsetz's conclusions were not above challenge, however.
Bond and Greenberg (1976) noted that using Demsetz's (1973) data
the coefficient of four-firm concentration is not significant in a
regression on profitability if industry advertising intensity is
also included as a regressor. They apparently interpret the
advertising intensity measure as an indicator of the ability of
firms to differentiate products or to erect entry barriers. As
such, their results could indicate that the entry barrier rather

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[1] Demsetz (1973), p. 3. Precisely how one distinguishes "natural
frictions" from the strategic use of market characteristics to
enhance profitability is not clear from Demsetz's discussion.

than concentration is the real cause of the observed positive concentration profitability relationship. Further, this result would cast some doubt on Demsetz's interpretation that the positive correlation between concentration and profit rates was due to the efficiency of large firms.[1] In a reply, Demsetz (1976) presented evidence that the correlation between return on assets and concentration across all size classes of firms was positive and generally significant but that no such relationship could be found within the individual size classes.[2] The results tend to indicate a stronger positive correlation within the larger size classes, but few of the individual size class correlations are significant. In addition, the correlations get weaker when the effects of advertising intensity on profitability are taken into account, indicating that concentration per se may not be a real problem in any event.[3]

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[1] Demsetz's (1976) reply to this particular criticism (p. 207) is not fully persuasive. He argues that our understanding of the effects of advertising on performance is so limited that he is unable to derive much information from a positive advertising intensity coefficient.

[2] This finding was based on data for 1958, 1963, 1966, 1967 and 1970 on approximately 100 industries (depending on the year examined).

[3] Round (1975) applied Demsetz's (1973) test of the differential profitability of large versus small firms to Australian industries. He found evidence consistent with Demsetz's hypothesis that efficiency explains the differing profit rates. Unfortunately, Round's data were so poor that rather little weight can be placed on the already weak empirical results he obtains.

Recently, Carter (1978) has attempted to test Demsetz's effi-
ciency explanation for the positive profits/concentration correla-
tion using a refined data set (of over 600 4-digit

Census industries for 1963, 1967, and 1972), an improved estimat-
ing equation, and relative (as opposed to absolute) firm size
classifications. Carter divides the firms in each industry into
two groups: leaders (the top four firms ranked by value of ship-
ments) and secondary firms (the fifth through eighth firms). In
his model he regresses the price/cost margin on advertising
intensity, capital intensity, and concentration with each explana-
tory variable stratified by the leader-secondary designation. He
finds that the concentration coefficients of, leading firms are
positive and significant but the secondary firm concentration
coefficient is considerably smaller and not statistically differ-
ent from zero. He interprets these results as being consistent
with the notion that leading firms are able to maintain prices
above costs but only to the extent of their cost advantage over

secondary firms.[1] The findings thus lend some support to
Demsetz's earlier work with smaller samples.[2]

While Carter's work supports Demsetz, other researchers have
been more critical. Porter (1979) has argued that mobility
barriers into and out of strategic groups are substantial and that
these barriers account for a major part of the differential prof-
itability of larger firms in concentrated industries.[3] He notes

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[1] See Carter (1978), pp. 438, 441. Carter seems to argue that
his results are consistent with a lack of competition among the
large firms as well as with the efficiency story. However, any
collusive gains are strictly limited by cost advantages. As a
sidelight, Carter also found that the advertising and capital
intensity variables obtained positive and significant coeffi-
cients. He eschews interpretation of the advertising intensity
result, leaving the reader to determine whether it is due to
advertising acting as an entry barrier or whether it reflects the
fact that profitable products are more heavily advertised.

[2] The works of both Demsetz and Carter are subject to the
criticism that umbrella pricing by the leading firm(s) may enable
the entry or continued viability of inefficient firms. If this is
true, one could weave a story of industry dynamics that leads to
the observation of differentially greater profits for the leading
firms due to monopoly pricing as opposed to the inherent relative
efficiency of large firms. See Weiss (1974), pp. 226-7.

[3] See Porter (1979), pp. 226-7. Porter also discusses some
shortcomings of Mancke's (1974, 1977) alternative random luck
theory of large firm profitability. Using a simulated Gibrat-type
growth model, Mancke demonstrated that a positive relationship
between profit and market share can arise without either monopoly
power or economies of scale, but by chance; the luckier firms are
more profitable and grow more rapidly. However, Caves, Gale, and
Porter (1977) have shown that the model Mancke assumes may not be
realistic. Their evidence suggests that the positive association
between profit and market share was not in general stronger, the
more uncertain the environment, as Mancke's model would predict.
For a simple example of how luck could lead to the observed
correlation between profitability and concentration, see Brozen
(1977), pp. 844-47.

that in the absence of these barriers the smaller or unsuccessful firms would imitate their more successful rivals, and one would not expect to observe the persistent profitability differentials that seem to exist. Porter has found evidence that the structural determinants of firm profitability differ between groups of firms depending on their status in their particular industry, and that in many industries the leading firms are not the most profitable.[1] He considers this evidence damaging to Demsetz's superior efficiency explanation of the relationship. However, Demsetz would probably concede that higher profitability can exist for firms in the smaller size class for some period of time, but he would expect that those industries would become less concentrated over time as the largest firms reduced their size to the optimal level.[2]

The debate over the correct interpretion of the concentration/profitability relationship was continued by Peltzman (1977), who took an eclectic view that concentration would likely act as a proxy for both monopoly and cost-reducing elements in a regression of concentration on profitability. Thus, he set out to establish the relative size of the two effects. His formal model allowed

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[1] Porter's results are based on a data set of only 38 IRS minor (3-digit) consumer goods industries. These industry definitions are probably too crude to provide much useful information.

[2] At a minimum, it seems that this is Brozen's position. See Brozen (1971a), p. 367. Since Porter's "mobility barriers" might work in to inhibit movements of firms to smaller sizes he would not have to agree that differential profitability would be only a very short term phenomenon.

for shifts in entire cost curves and not just economies of scale attained from moving along one curve. This change in emphasis (relative to McGee[1]) was due to the fact that the bulk of econometric evidence indicated that scale economies were not so substantial that most industries should be expectd to be oligopolies.[2] Using data on 165 4-digit SIC consumer and producer industries which experienced major unit cost changes during the 1947 to 1967 period, Peltzman found that both increased and decreased concentration were associated with substantially lower unit costs over the period, but that the cost reductions in industries with increasing concentration were substantially larger.[3] Thus he concluded that "long period changes in market structure are accompanied by increased efficiency. This efficiency gain is most pronounced where concentration is high and rising and where demand is growing."[4] This finding did not, of course, end the inquiry, since the question only reverts to whether prices increase enough due to the collusion effect of increased concentration that the efficiency gains are offset. To answer this, Peltzman undertakes two analyses: one, a regression

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[1] See McGee (1971, 1974).

[2] See Peltzman (1977), p. 231.

[3] Peltzman did not distinguish producer from consumer goods manufacturers in his empirical model. Given the differing results that have been obtained by other researchers for the two groups, one must consider this a weakness in Peltzman's approach. See Scherer (1979).

[4] Peltzman (1977), p. 251.

of price/cost margins on concentration and the asset/value of shipments, and two, a regression of the changes in concentration, unit costs, and total revenues on industry price indices. From these regressions, he finds that less than the entire cost reduction is passed on to customers (as one might expect in the long run in a purely competitive world), but that the net effect of changes in concentration is to reduce price substantially. He notes that profits rise not because prices rise but because they fall less than unit costs.[1] He thus concludes that broad-based deconcentration policies are likely to be detrimental.

In a comment on Peltzman's work, Scherer (1979) argued that the failure to distinguish process from product innovations led Peltzman to miss the true engine of economic change over the period he studied. Scherer notes that since 1947 concentration tended to fall in producer goods industries where process innovations predominate but that it tended to rise in consumer goods industries characterized by product innovation, indicating that more consideration should be given to the type of buyer. After a detailed examination of the histories of many of the more concentrated industries in Peltzman's sample, Scherer concludes that cost-reducing innovations were directly not responsible for the

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[1] Peltzman, pp. 259-60, credits the erroneous popular view that concentration is bad to the quick incidence of the collusion effect and the lagged occurrence of the cost-reducing effect. This explanation leads one to wonder how the net effect calculation would come out if appropriate discount rates were applied.

increases in concentration observed in many real markets but rather that unit cost reductions may have been caused by product innovations which led to rapid growth and resulting scale economies.[1] Finally, Scherer observes that antitrust works with a long lag and any gains from efficient operations will have been achieved by the time the antitrust authorities act to alter an industry's structure. While Scherer's point applies to a case-by-case approach to monopoly power, this may not be true of an anti-merger policy since such a policy can prevent the attainment of possible economies at the outset.[3] Of course, antimerger policy would allow internal expansion to obtain such economies (unlike some across-the-board deconcentration recommendations).

The debate over the meaning of a positive relationship between profitability and concentration has still not ended. In fact, recent research by Ravenscraft (1981a) suggests that we may have only begun to disentangle the efficiency story from the collusion story. He takes a novel approach by applying Monte

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[1] Scherer also criticizes Peltzman's use of indices to measure cost reduction, but the errors in the indices may work to understate the efficiency gains from concentration estimated by Peltzman. See Peltzman (1979), p. 210.

[2] See Scherer (1979), pp. 206-7.

[3] In his reply, Peltzman (1979) seems to be bothered little by Scherer's critique mainly because he feels that a distinction between process and product innovations is unnecessary. Both types can reduce unit costs in his model. Peltzman simply reemphasizes his skepticism of deconcentration policies. Since the deconcentration movement is dying (if not dead), the debate over the policy implications of Peltzman's work is probably of less importance today than it was a few years ago.

Carlo simulation analysis to two regression models relating prof-
its at both the industry and firm level to four-firm concentra-
tion. The competing models are the 'traditional' model and the
superiority (or efficiency) model. The only difference between
these models is that in the competitive equilibrium of the the
traditional model, expected industry price should equal unit cost
at minimum optimal scale whereas the superiority model allows the
possibility that price will not be driven to unit cost even in the
absence of collusion as the more efficient firms obtain rents.[1]
Using simulated data, Ravenscraft examines the bias that exists in
the coefficient of the concentration measure. For the superiority
model, the coefficient in the industry profit equation is biased
upward if variables measuring the cost-reducing effect of relative
size are omitted.[2] This is because the concentration coefficient
proxies both the efficiency and monopoly elements in the less-
than-fully-specified model. This bias does not seem to exist if
the superiority model is used to estimate a firm-level (rather
than industry-level) profit function, because a market share

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[1] See Ravenscraft (1981a), p. 9.

[2] Ravenscraft discusses two such variables: minimum optimal scale
(MOS) and a cost disadvantage ratio. See Caves, Khalizadeh-
Shirazi, and Porter (1975) for a discussion of these measures.
Inclusion of both measures substantially reduces (but does not
eliminate) the upward bias in the coefficient of the concentration
variable. Much of the empirical research since 1975 has included
cost disadvantage and MOS measures to reflect the view that
efficiency due to scale economies may be a major reason for prices
in excess of cost for large firms.

measure can be directly used in firm-level regressions to pick up any cost-reducing effect of relative size.

For the traditional model, where all firms produce at minimum optimal scale or above in equilibrium, the coefficient of the concentration measure is biased downward and underestimates the collusion effect in both the industry- and firm-level equations.[1] At the firm level, this bias occurs because market share is an endogenous variable in the traditional model. At the industry level, the bias occurs from the endogeneity problem as well as from an omitted variable bias due to the lack of a cost-reduction proxy variable. These results from simulated data indicate that one must be very careful in interpreting the regression coefficient of a concentration measure. The biases included in the coefficient can lead one either to infer monopoly power when it does not exist or to infer a lack of such power when it is, in fact, present. Fortunately, the biases can be partially handled through the use of firm-level profit equations which seem to be less subject to bias.[2]

3. <u>The Relationship Between Prices and Concentration</u>. The revisionists have rather effectively raised nagging doubts about

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[1] As in the superiority model, the firm's profit is a function of both concentration and market share. The coefficient of market share is also biased downward in the traditional model of firm profitability.

[2] This conclusion rests only on the simulation evidence reported in Ravenscraft's paper. Other simulation (using various parameter values) may lead to different conclusions.

the correct interpretation of the results obtained from profit/ concentration studies. Since profits can be high due to high prices or low unit costs, we cannot unambiguously interpret many of the empirical results. However, the literature linking prices and concentration is not subject to this ambiguity. Studies of the price/concentration relationship are not plentiful due to a paucity of useful data on prices and (as with all the other issues reviewed so far) the evidence is not one-sided, but that which does exist tends to weaken the revisionist position that increased concentration generally leads to a net reduction in product price.

A number of price/concentration studies have been done in the area of banking services. Bell and Murphy (1969), Aspinwall (1970), Heggestad and Mingo (1976), and Hester (1977) have all examined some aspect of banking and found that an increase in the number of competitors or decreased concentration is associated with better performance.[1] Bell and Murphy (1969) studied the relationship between the annual service charge per regular check-ing account, the marginal cost of that service, and the three-firm concentration ratio. Their data covering 14 market areas indicate that 3-firm concentration was above 70 percent in each of the

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[1] We should note that banking may be a special case due to the regulation of entry into the industry. In fact, studies of the banking industry may actually be consistent with the revisionist position that governmentally-imposed barriers to entry are the most formidable and cause the greatest distortions. For a longer review of three of the banking studies listed here see Weiss (1979), pp. 1107-15.

markets regardless of whether the concentration calculation included only small, only large, or all checking accounts. Using this relatively concentrated sample, the authors found a consistent positive and significant relationship between the natural logarithm of the price of checking services and the logarithm of market concentration. However, the authors note that since some evidence of scale economies in banking exists, this evidence of a price-raising effect of concentration cannot be used by itself to deny bank mergers.[1]

Aspinwall (1970) examined the effect of 3-firm time deposit concentration and the number of mortgage lending institutions on contractual mortgage rates in 31 Standard Metropolitan Statistical Areas for 1965. His model adjusted for other influences on the rates such as the demand for credit (the percentage change in the number of households), per capita income, loan risk (loan price ratio), and the size of the lender (logarithm of the number of deposit accounts). Using a sample with average 3-firm concentration of 38 percent, Aspinwall found that the concentration variable obtained a positive significant coefficient.[2]

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[1] Bell and Murphy did not include any measure of bank size or minimum optimal scale in their model. Thus, one cannot make an educated guess about the net effect of a merger in one of these markets.

[2] The results on concentration are subject to change depending on the method of calculating three-firm concentration. If total deposits (rather than time deposits) are used, the concentration coefficient, is positive but insignificant. Aspinwall dismisses this result, however, due to the lower R^2 obtained in the equation using total deposits (.45 versus .56).

Similarly, a significant negative coefficient was obtained for the logarithm of the number of mortgage lenders. He concludes from his study that considerable support exists for the premise that market structures affect interest rates (prices) and that the relative differences among areas in the number of lenders is a more important determinant of mortgage rates than the absolute differences.

Heggestad and Mingo (1976) examined the relationship between Herfindahl index (based on total deposits), several interest rates, and measures of nonprice competition among 332 banks in 69 SMSA's.[1] The authors regressed (in turn) their various measures of interest rates, service charges, hours open, and the provision of special services on independent variables denoting the bank's location, total deposit size, area personal income growth, per capita income, the bank's market share, and the demand deposit/total deposit ratio as well as the Herfindahl (H) index. They found a positive nonlinear (concave from below) relationship between the H-index and loan rates which indicated a larger effect of increases in concentraton in those markets which were initially unconcentrated. In addition, they found that the service charges on demand deposits were higher in more concentrated

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[1] The Herfindahl index values in the sample ranged from .0465 to .4471. The mean value is .18.

markets supporting the prior findings of Bell and Murphy (1969).[1]
Interestingly, the bank's market share is insignificant in
explaining interest rates or service charges. This finding (that
market share is insignificant while a concentration measure is
significant) differs considerably from some other research that is
occurring in line-of-business studies of manufacturing.[2]

A final banking study by Hester (1979) examines loan terms
(interest rates, maturity, amount, and collateral required) and
their relationship to the Herfindahl index. Using a sample of 62
banks in 44 markets for 1972, he regresses the loan terms on
measures of bank characteristics[3] and one summary measure of
borrower characteristics, the logarithm of borrower's total
assets. Hester concludes that:

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- The authors do not discuss at length the results obtained for
many of the nonprice dependent variables. Since four of their
nonprice measures were dummy variables taking on only zero or one
values, the OLS estimating technique used by Heggestad and Mingo
was probably in error in any event. The authors do note the
interpretation problem, however. See Heggestad and Mingo (1976),
p. 111, n. 13. This problem did not deter them from concluding
that studies using just one price measure and eschewing nonprice
variables may significantly underestimate the deleterious effects
of market concentration on price.

[2] For a discussion of the line of business results see section
V.C. below. A comparison of the effects of market shares in
banking versus line of business manufacturing studies is made
difficult by the fact that market share may not proxy scale
effects very well in banking. If banking markets are localized,
scale may be better captured by asset size, since small banks
could have large shares of small markets

[3] Bank characteristics include loan/asset ratios, demand deposit
and time deposit ratios, net worth growth in demand deposit and
time deposit ratios, and bank size (as measured by the logarithm
of total bank assets).

> [T]o an astonishing extent hypotheses involving
> the Herfindahl index are accepted. All three
> coefficients differ significantly from zero at
> the 0.05 level with the expected signs. The
> estimates imply large differences in loan terms
> when extreme market structures are compared.
> For example, relative to a nearly competitive
> market a monopolist would charge a 27 percent
> higher interest rate and would offer a 74
> percent smaller loan to a customer of a given
> size. Similarly, collateral is much more
> likely to be required in a monopolistic
> market.[1]

Taken together these four studies of the industrial organization of banking lend support to the belief that the net effect of increased concentration is to raise prices. They seem to represent the best available evidence of one particular kind of market. Studies of the price/concentration relationship in various other markets also exist and we will briefly review a few of these.

Possibly the best known study of the relationship between price and the number of competitors is that of Kessel (1971). He examined the tax-exempt bond market during the 1960's and found that as the number of bids received from underwriters increases, the "spread" between the buying and selling prices of underwriters decreases and the price received by the issuer rises.[2] He also

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[1] Hester (1979), p. 357.

[2] Again, as in the case of the banking studies, government regulation which restricted entry into the bidding for tax-exempt bonds by commercial banks may have been a significant reason for the market distortion found by Kessel. He chose to study this market, in part, due to the existence of the entry barrier.

found that the effect of an increased number of bidders is not linear. That is, beyond the point where seven to ten bids are received an increase in the number of bids does not appear to reduce the spread or the price received.[1]

Both the banking and bond market studies focused on markets where entry was closely regulated by the government. As such, these markets do not provide a very clear "test" of revisionist views. However, price/concentration relationships have also been examined in unregulated food industries by Marion, Mueller, Cotterill, Geithman, and Smelzer (1979) and by Lamm (1981).[2] Both studies find a positive effect of market structure measures on prices. Marion et al. find a positive relationship between four-firm concentration and prices as well as between relative market share and prices in food retailing. Lamm finds similar results in that individual market shares and concentration measures both obtain significant coefficients and the market share measures explained more of the variation in prices than the more aggregated concentration measures. Lamm uses Bureau of Labor Statistics data on a food market basket for a family of 4 in 18 SMSA's over the years 1974-1977. He regresses price on firm market shares, prices of finished consumer foods to retailers, a wage rate index,

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[1] Kessel (1971), pp. 718-727.

[2] Lamm (1981) has argued that substantial nongovernmental barriers to entry exist in food retailing. He lists capital requirements, site availability, economies in purchasing inputs, promotion expenses and advertising advantages as some of these barriers.

average store size (to proxy plant scale economies), and three regional dummy variables. His results indicate that larger shares by the leading three firms tend to raise prices of food while a larger share for the fourth firm tends to lower price.[1] When the standard four-firm concentration ratio is used in the regression the coefficient is also positive and significant.

In a study of the retail gasoline market, Marvel (1978) has examined the relationship between prices and market concentration as measured by the Herfindahl index. Using BLS price data for 1964-1971 covering 22 cities, he regressed the mean price on gasoline transport costs, taxes, Herfindahl index, and city population. He finds that the coefficient of the Herfindahl index is positive and significant for the entire 1964-1971 period particularly in the low-priced end of the gasoline retailing samples.[2] When the equation is estimated on a year-by-year basis, the H-index obtains a positive sign in 1964 and 1968 through 1971 in the high-price segment of the industry but the coefficient is insignificant and negative in 1965 through 1967. Marvel takes this as evidence that collusion began in the gasoline retailing market in 1965 and broke down in the late 1960's and early

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[1] In section V.D. on critical concentration ratios we will discuss the varying effects of different market shares in somewhat more detail.

[2] Marvel segmented his sample into high-priced and low-priced subgroups. The coefficient on the H-index is much larger for the low-priced subgroup.

1970's.[1] This pattern is considered to be consistent with
Stigler's (1964) information-based theory of oligopoly and it
tends to support the notion that collusion can have significant
short-run price effects.[2]

Finally, a price/concentration study in the newspaper indus-
try was conducted by Landon (1971). Using a relatively crude
model, he regressed newspaper advertising rates (or the spread of
advertising rates)[3] on the largest firm's share and circulation.
He consistently found a deleterious effect of larger first firm
shares on advertising rates in his sample of 120 newspapers. How-
ever, he also found a significant price reduction effect of large
size up to a circulation of 100-200,000 copies. He concluded
that the net effect of increases in size due to horizontal merger
of two morning or evening newspapers appeared to be a decrease in
advertising rates across the board. Although a price-raising

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[1] Marvel [1978] pp. 255-56) interprets the coefficients of the
Herfindahl index in a unique manner. He argues that in a regres-
sion of prices on the Herfindahl index, a low H-index coefficient
indicates collusion since it can imply that price dispersion is
absent. An increase in the coefficient after 1965 indicates a
collapse of the collusion as monitoring of cheating in both the
low- and high-priced subgroups failed. Care must be taken in
applying this type of interpretation to other situations since a
low H-index coefficient might be used to support a finding of
collusion when such a finding could also be consistent with
competition.

[2] This data on retail gasoline markets has recently been used by
Geithman, Marvel, and Weiss (1981) to examine the issue of
"critical" concentration levels. Geithman et al. also examined
data from Kessel's (1971) bond market study and Marion et al.'s
(1979) supermarket pricing survey. See p. 81 note 2 below.

[3] Landon (1971), p. 99.

effect of concentration was often found by Landon, this study would seem to lend some support for the revisionist view that the net effect of increasing size will likely be an improvement in economic performance.

Such a conclusion also receives some weak support from a multi-industry study of pricing by Bodoff (1975). She updated and expanded Schwartzman's (1959) comparison of price/cost margins (ratio of value of shipments to direct costs) in unconcentrated U.S. and concentrated Canadian industries. Following Brozen's approach discussed earlier, Bodoff expanded the Schwartzman sample from 19 to 34 industry pairs and examined the disequilibrium nature of high rates of return.[1] Bodoff found that Canadian concentrated industries had insignificantly higher price/cost ratios than their unconcentrated U.S. counterparts. In addition, she found that margins tended to fall slightly over time in those industries facing significant import competition. Bodoff's results are not particularly striking evidence for either the traditionalist or revisionist views. International comparisons are difficult at best and the use of price/cost margins rather than simple price data leads to ambiguous findings. In addition, the strength of the results is such that they could be used for virtually any position. However, Bodoff's effort is the only

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[1] As a basis for comparison Bodoff used 65 industry pairs in which the industry was unconcentrated in both the U.S. and Canada. A 59 percent 4-firm concentration level was chosen as the arbitrary dividing line between concentrated and unconcentrated industries.

available study which attempts to match industries in a meaningful way to obtain a large sample of industry data on the relationship between margins and concentration.

C. What Do We Think We Know Now?

The question remains as to whether the economic literature concerning the concentration/profitability and concentration/price relationships can serve as a useful basis for broad-based horizontal merger policy. It is clear that the literature is in a state of flux. The traditional view has been losing ground to the revisionists for some time. However, neither side has proven its case beyond a reasonable doubt. The traditionalists must contend with the efficiency argument as well as with rather formidable econometric and data problems.[1,2] The revisionists, on the other hand, have failed to rebut evidence that decreases in the number of competitors seem to raise prices (and not just profits) nor have they fully explained the persistence of profits in many industries. In addition, evidence on scale economies does not

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[1] The econometric problems highlighted by Phillips (1976) are slowly being examined. Systems estimation (rather than single equation estimation) is becoming more common in an attempt to reduce endogeneity problems. See Stickland and Weiss (1976) and Martin (1979a, b). However, these studies do not find a particularly strong direct relationship between concentration and profitability nor can the effects of minimum efficient scale be easily disentangled from the collusive effects of higher concentration.

[2] The data problems inherent in the use of two- through four-digit SIC or census industries may be remedied through use of the FTC line-of-business or PIMS data sets. More will be said about studies using this less contaminated data shortly.

indicate substantially greater economies than were previously thought to exist,[1] although the theory of multiplant scale economies tends to point in that direction. Finally, studies of individual mergers have not consistently indicated that economies occur due to merger activity as one might expect if the revisionist position is correct.[2] On net, it does not appear that we now know whether the traditional view should be given more or less weight than the revisionist's position. However, there is hope that two sets of recently developed data will yield at least enough definitive answers to reduce the scope of the debate.

1. <u>Business Unit Data Research</u>. During the last few years, improvements have been made in the data available to economists to test the structure-conduct-performance paradigm and the revisionist positions. Two data sets on business unit profits and shares are currently being mined to determine whether the positive correlation between profitability and concentration exists in this

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[1] Although some studies (McGee 1971) seem to imply that scale economies are virtually limitless in many industries, the majority of studies would lead one to believe that gains from scale increases vary quite widely among industries and across firms within an industry. Of course, as Peltzman (1977, 1979) noted, scale economies per se are not essential for the efficiency story to be correct. For reviews of much of the scale economies literature see Scherer (1980), pp. 81-118, Gold (1981), and a review by Siegfried and Wheeler (1981) on the inverse relationship between suboptimal capacity and concentration.

[2] The studies of mergers and resulting efficiencies have not been reviewed here. They tend to be of rather mixed quality and present ambiguous results. In part, this may be due to the difficulty of quantifying the gains from merger. For a review of this literature in its various forms see Fisher and Lande (1981).

less contaminated data and to determine the cause of the relation-
ship if it exists.[1]

Beginning in the mid-1970's the Strategic Planning Institute
began to develop a business-unit-level data set containing infor-
mation on market share; return on investment; R&D, advertising,
and marketing expenses; capacity utilization; capital structure;
and other variables. The project focused on the profit impact of
marketing strategies (PIMS). While several studies using this
data have been undertaken, we will focus on only one of these.[2,3]

In a preliminary paper, Gale and Branch (1979a) have used the
PIMS data on 1,080 lines of business to determine whether market
concentration is a cause of high profits. They present results

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[1] While researchers have found a positive effect of both concen-
tration and market share on profitability using firm-level data
(see Dalton and Penn, (1971), and Dalton and Levine (1977), for
such evidence for food manufacturing) until very recently they
have not had access to data at the business unit level.

[2] A description of the PIMS data can be found in Buzzel, Gale,
and Sultan (1975). The data were obtained from large firms who
voluntarily contributed information on their various lines of
business. Therefore, the PIMS data does not represent a random
sample of lines of business or industries. In addition, the firms
were free to define the markets within which they operated which
could cause inconsistencies in reporting patterns by firms as a
group.

[3] Recent studies of scale economies and market share measurement
are also available. (Gale and Branch 1979b, Gale 1979). Since
the conclusions of these other studies are consistent with those
found in Gale and Branch (1979a) we will discuss only the latter
paper. The PIMS data have also been used to study the relation-
ship between market share stability and concentration. See Caves
and Porter (1978).

that indicate that concentration is not significant in rate-of-return regressions when firm market share is also included. While part of this result may be due to collinearity between share and concentration and to less than complete model specification, it is not clear that these problems account entirely for the insignificance of the concentration coefficient.[1] Gale and Branch conclude that market share is much more important than concentration in explaining profitability, and they then examine the reasons for the relationship between market share and profits. Again using rather incomplete models, they find that more of the variation in relative prices in nonhomogeneous product categories is explained by perceived product quality than by market share, but market share is still significant and positive in a regression of relative price on quality and market share.[2] In addition, they find that relative direct per unit costs tended to be lower for businesses with large relative market shares. Together, these results

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[1] Gale and Branch note that the correlation between share and concentration is .27 in their sample. Since the model they used contains only concentration and share as independent variables, it is probably underspecified. Gale and Branch indirectly note this point themselves at p. 17. In more fully specified models other researchers have also found an insignificant or even negative coefficient on concentration measures in regressions of firm or line of business price/cost margins on concentration. But some of these results may be rather sensitive to the specification of the models. See Shepherd (1972), Weiss (1980), and Grabowski and Mueller (1978).

[2] Product quality may have been judged by the businessmen themselves, possibly presenting a sample bias problem. See Buzzel, Gale, and Sultan (1975), p. 101, and compare Gale and Branch (1979a), p. 16.

imply that high-share businesses tend to charge somewhat higher prices than their rivals partially due to high quality (and possibly some market power), but much of their higher profitability is likely to be due to lower per unit costs.

In the same paper the authors also undertake an analysis of a richer model including virtually all imaginable share index interactions. Two particularly interesting results are uncovered. They find that market share instability is associated with significantly lower profitability. This result is consistent with earlier work by Caves and Porter (1978).[1] In addition, they find that when product quality and market shares are combined in an interaction term, the interaction is negatively related to rates of return. They interpret this as evidence that "a strong market

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[1] Caves and Porter (1978) argued that market share instability could be used as a measureable indicator of rival firm's behavior. However, share instability has ambiguous welfare implications to Caves and Porter since it could be due to the existence of a high collusive price and excessive nonprice competition. The result that market share instability is associated with lower profitability may be somewhat at odds with Lustgarten and Thomadakis' (1980) recent work on the positive serial correlation of rates of return in concentrated industries. In that paper they argue that capital immobility can be a cause of high risk and high profits in many concentrated markets since entrepreneurs must be compensated for the high risk involved in production in industries characterized by high barriers to entry and exit.

position can help offset the disadvantages of lower product quality."[1]

This research using disaggregated data is interesting, but it still seems to be in a fledgling stage. In future years, better models may be applied to the data to allow us to draw more definitive conclusions. At this relatively early stage, we can say that the data do not appear to support the notion that concentration leads to substantially higher prices or profits. Whether the evidence of a positive effect of market share on return on investment is an indication of efficiency or some manifestation of market power, such as dominant firm pricing, is not obvious.

A second business unit data set containing information on 3007 businesses in 257 4-digit industries has been compiled in the Federal Trade Commission's Line of Business (LB) Program. This data has recently been used by Ravenscraft (1981b) to study the effect of market share on return on sales and price/cost margins at the business unit and industry levels, respectively.[2]

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[1] Gale and Branch (1979a), p. 28. This result may be largely due to collinearity of the quality/share interaction and another interaction term (price-difference/share) included in the model. Gale and Branch argue that the price-difference/share interaction picks up the effects of higher quality offset by higher prices. The only remaining unexplained variation to be captured by the quality/share interaction term occurs where (for some unexplained reason) high quality (cost) products are sold at noncompensatory prices. Thus, they argue that the negative relationship between return on investment and the quality/share interaction is not unexpected.

[2] Another recent study using this data has been done by Weiss and Pascoe (1981). Ravenscraft's work is an extension and modification of their model.

Ravenscraft followed previous theoretical and empirical work to
develop a rather complete estimating equation.[1] Using generalized
least squares estimation (to correct for heteroskedasticity), he
found that in the business unit regression, four-firm concentra-
tion was _negatively_ correlated with return on sales while market
share obtained a significant positive coefficient.[2,3] Very
different results were obtained in the industry price/cost margin
regression where the coefficient of concentration was positive and

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[1] The large number of variables employed precludes listing them
here. A two-and-one-half-page summary table may be found in
Ravenscraft (1981b), pp. 10-12. The richer model specification is
one major advantage of this study over that of Gale and Branch
(1979a).

[2] This pattern may imply that there are advantages to higher mar-
ket share, but these advantages diminish if rival firms are also
large. This interpretation is consistent with Kwoka's (1979)
results.

[3] Other line-of-business studies by Martin (1981) and Scott
(1981) have revealed similar results for concentration measures.
Martin found that the Herfindahl index was negatively related to
price/cost margins. Martin's study emphasized the effects of
interfirm (as opposed to interindustry) organizational differences
on price/cost margins. His simultaneous equations model included
42 variables measuring profitability, sales and concentration,
sales efforts, scale economies, demand conditions, countervailing
power, distribution expense, transportation, R&D, size, and
capital intensity. While he found that many relationships
observed at the industry level are also found at the line-of-busi-
ness level, differences arise in the impact of concentration on
price/cost margins, capital intensity on margins, and profit-
ability on sales efforts. In addition, Martin finds some weak
evidence consistent with countervailing power arguments. Scott
(1981) also observed a negative relationship between concentration
and price/cost margins and a positive relationship between market
share and margins in his study of multimarket firm contact. How-
ever, Scott also noted that if firms tend to meet each other in
many markets and those markets are concentrated, then price/cost
margins are significantly higher than if only one or none of those
conditions holds.

significant and a cost disadvantage measure was insignificant.[1,2]
The shift in sign for the concentration coefficient from the busi-
ness to industry level "supports the hypothesis that concentration
acts as a proxy for market share in industry regressions. Hence,
a positive coefficient on concentration in the industry level
regression cannot be taken as an unambiguous representation of
market power. It most likely reflects the price-raising or cost-
reducing effect of market share."[3] Several other coefficients
differed between the industry and LB level, leading one to wonder
whether previous research using smaller samples at the industry
level really has given us much reliable information.[4]

   A more complete model using market share interaction terms
that include the intensities of advertising, assets, R&D, and con-
centration was also estimated. In this specification, the share

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[1] Industry data were obtained by aggregating up from the LB
level. A weighting scheme based on market share was used to
accomplish the aggregation.

[2] A cost disadvantage measure replaced the share variable in the
industry regressions since there is no useful industry level
equivalent to market share.

[3] Ravenscraft (1981b), p. 19.

[4] The coefficients of variables measuring advertising and R&D
intensity, the assets-to-sales ratio, the degree of vertical inte-
gration, and the extent of diversification differed substantially
depending upon the aggregation level of the regression. One
explanation for the differing LB and industry results is that
there are different theoretical explanations for the use of LB and
industry variables. The industry results must therefore be
interpreted with caution since the industry level specification
cannot include relevant LB variables.

variable itself had an insignificant positive coefficient indicating that the interactions capture most of the positive profit/ market-share relationship. All of the interactions had the expected signs but only advertising and assets were significant. Ravenscraft takes this as evidence supporting both the cost-reduction and price-raising explanations of the share/profit relationship; but the larger and more significant coefficient on the advertising interaction suggests that the price-raising effect may be dominant. Finally, the negative (but insignificant) coefficient on concentration and the concentration/share interaction implies that a collusion model may be inappropriate.[1,2]

To determine the cause of the usually observed positive market-share/profit relationship, Ravenscraft regresses the coefficients of the market share variable from 241 4-digit line-of-business industry equations on a number of industry-specific

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[1] Ravenscraft (1981b), pp. 20-21.

[2] Ravenscraft also examined subsamples of the data, making consumer/producer goods and convenience versus nonconvenience goods distinctions. He found that concentration was not a significant determinant of profitability in any of these particular subgroups while market share was significant and positive in each case. In addition, Ravenscraft examined some 2-digit industry subsamples and found some statistically weak but positive concentration/profit relationships even when market shares were held constant. This latter result tends to indicate that a collusion effect of concentration may exist in some (but certainly not all) industries. Finally, a small number of industries exhibited a negative relationship between market share and profitability in contrast to the general pattern of results. These negative coefficients may simply be anomalies (as yet unexplained), or they may indicate those industries in which some firms have grown beyond optimal size.

variables. While most of the variables obtain insignificant coefficients, supplier dispersion and four-firm concentration obtain negative significant coefficients and the industry asset/ sales ratio obtains a positive coefficient. From these results he concludes (tentatively) that market share results in some discretionary monopoly power that is enhanced by barriers to entry due to assets. If other firms in the industry are large or supplies come from only a few industries this power is lessened.

Both the data sets composed of business unit data, PIMS and LB, indicate that market share is a more significant determinant of profitability than concentration.[1] Beyond this general point of agreement, the researchers have failed to come to unanimous conclusions. The reasons for the significance of market share are still in dispute and will likely remain so for the foreseeable future. The one thing that does seem clear from all the business unit data studies is that the heavy emphasis economists have placed on four- and eight-firm concentration in the past is probably misplaced. Future research should more fully address the effects of differential market shares and market dominance rather than emphasizing concentration per se.[2]

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[1] In many traditional profit/concentration studies the relationship between profits and concentration is weakest during periods of unstable prices. To the extent this is true, the 1970's (for which the PIMS and LB data are available) may be less than ideal for testing this relationship.

[2] Much of the evidence supporting the switch of emphasis from industry-wide concentration measures to market shares or two-firm concentration is discussed in section V.D. below.

2. <u>The State of the Debate</u>. A vast amount of ink has been spilled in the traditionalist/revisionist debate. Discussions of the appropriate data and model specification abound. Unfortunately, the debate has yet to lead to any generally accepted conclusions. It seems that much of the difference between the traditional and revisionist views boils down to a difference of opinion on two crucial issues: the extent of barriers to entry, and the presumption of efficiency. If barriers are generally low, then high profitability should be a transitory phenomenon or due to nonreproducible advantages of large firms.[1] Where barriers are substantial, both sides would agree that high profitability can be maintained. The difference is that the revisionists see competition eroding any barriers rather quickly (with the exception of governmentally imposed barriers), whereas traditionalists see a broader menu of more durable barriers to entry including advertising intensity, scale economies, absolute capital requirements, natural resource monopoly, patent protection, strategic predation,

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[1] An example of a nonreproducible asset might be superior entrepreneurship. Many traditionalists would argue that such assets are precisely the type of barriers to entry they are discussing as a means to monopoly power. The difference in views then hinges on the definition of a barrier to entry and the welfare implications of the barrier.

etc.[1] While these barriers need not be "unnatural," they may
still have the effect of reducing competition and maintaining
differentially high profitability.

The revisionists also pass the burden of proof for efficien-
cies to their traditionalist opponents. Based on a dynamic view
of the competitive process and profit maximizing behavior, the
revisionists conclude that the concentration levels that exist
must be due to market forces and that they represent efficient
resource allocations. Thus, the burden of proving otherwise is
squarely placed on those who would intervene in the market. The
best that the traditionalists have been able to show is that scale
economies are not pervasive and that studies of specific mergers
do not tend to indicate many resource savings or efficiencies.[2].
They have failed, however, to muster enough evidence to rebut the

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[1] We have not discussed in detail many aspects of the barriers-
to-entry problem. The traditional static discussions of barriers
began with Bain (1956) and continued with the work of Mann (1965),
Qualls (1972), Orr (1974), and Webbink (1979). These efforts were
complemented by work on limit-entry pricing models by Gaskins
(1971) and Pashigian (1968), among others. More recently, Caves
and Porter (1977) and Porter (1979) have popularized the idea of
mobility barriers among firms within an industry, and several
researchers have extended the theoretical discussion of entry
barriers to include strategic elements such as preemptive capacity
expansion, or patent accumulation, product proliferation, first
mover advantages, and learning curve effects to mention a few.
See, for example, Schmalensee (1978), Rao and Rutenberg (1979),
Spence (1979, 1981), Dixit (1979), Salop (1979), Gilbert (1981),
and Porter (1981). For an interesting argument that entry
barriers might improve welfare, see von Weisacker (1980). For a
very recent and nontechnical overview of many of the issues
involved in the barriers-to-entry problem, see Waterson (1981).

[2] See Meehan (1978), pp. 788-91.

assertion (based on a broad view of competition as a process) that current concentration levels are generally consistent with economic efficiency.[1]

While attacks on the usefulness of case-by-case antitrust action to remedy the most egregious problems are probably over-stated,[2] even some of the most noted proponents of antitrust have weakened their positions over the past decade. Scherer (1981), for instance, has noted that "simplistic interpretations of positive concentration profit correlations as evidence of 'market power' are no longer supportable."[3] While one can hardly take such a statement as a recantation of the market concentration doctrine, it is a step in that direction. In addition, Weiss (1979) cautiously stated that recent empirical work on the concentration/profits relationship tends to indicate "that we should not contest horizontal mergers that cannot increase the 2-firm concentration ratio above 35 or the 4-firm ratio above 50 and we should not contest horizontal mergers unless they affect firms that rank first or second or would rank first or second

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[1] Perhaps we should note that whichever side of the traditionalist/revisionist debate bears the burden of proof will probably lose. Revisionists and many former traditionalists have been adept at showing that concentration does not usually lead to poor performance. However, there does not yet exist much (if any) direct empirical evidence that market dominance is generally benign.

[2] For two differing views of the role of antitrust activity, see Eckhard (1980) and a response by Hilke and Nelson (1981).

[3] Scherer (1981), pp. 788-91.

after the merger."[1] If Professors Scherer and Weiss have come

this far toward the revisionist view, one might conclude that

economists in general find the economic rationale for the

stringent standard of present broad-based antimerger policies to

be less than compelling.

Despite this conclusion, the evidence that concentration (or

market share) and profits are positively related may still be dis-

turbing enough to warrant broad-based horizontal merger enforce-

ment through guidelines if one feels that the cost of erring on

the side of too lenient a policy is high.[2] This would be likely

to be the case if one finds the essence of the revisionist's argu-

ments unconvincing.

If we assume that a benefit/cost calculation of a broad-based

merger policy would lead to continued use of antimerger guide-

lines, then it is useful to determine the extent to which the

economic literature sheds light on the appropriate structural

rules of thumb. In the next section, the literature on "critical"

concentration ratios will be examined to accomplish this task.

Throughout the review we will <u>assume</u> that the effect being

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[1] Weiss (1978), p. 1119. While he felt these results, largely
based on Kwoka's (1979) work, are too tentative to serve as a
basis for precise recommendations, Weiss did note that these new
standards would significantly change merger policy.

[2] Since we now think that economists were too unconcerned with
efficiency in the 1950's and 60's, there is room for healthy
skepticism about the profession's current emphasis on those
economies and diminished concern that monopoly or oligopoly poses
a problem.

measured is, in fact, the monopoly power effect of concentration (or shares) rather than an efficiency effect. Thus, the approach presents the view that horizontal mergers may produce monopoly power in the kindest light--a light much kinder than the current state of economic understanding allows

D. Critical Concentration Levels

A few researchers have attempted to go beyond the general linear concentration/profits relationship and determine whether the empirical evidence indicates that a critical level of concentration exists. Identification of a clear break in the concentration/profits relationship could have important implications for antitrust policy. We have already discussed the earliest work on this subject by Bain (1951). He noted a break at the 8-firm 70 percent concentration level. However, he also observed a second discontinuity at 30 percent—that is, profits were higher in both the least concentrated and the most concentrated portions of his sample while middle range concentration was associated with relatively low profitability. Several researchers after Bain used this 70 percent 8-firm cutoff as a convenient place to break their samples, and additional efforts to delineate critical concentration levels languished.[1] However, in the early

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[1] In part, this lack of interest in the critical level of concentration was due to the fact that many researchers found that a continuous relationship seemed as appropriate as a discontinuous one. However, the studies reaching this conclusion were all based on small samples or failed to test for a discontinuous relationship. See, for instance, Collins and Preston (1968) and Kamerschen (1969).

1970's, Rhoades and Cleaver (1973) attempted to define such a
level using 1967 Census data for 352 4-digit manufacturing
industries. They regressed price/cost margins on four-firm con-
centration, industry growth, the capital/output ratio, a producer/
consumer goods dummy variable, and a geographic market index.
Using this model they found four-firm concentration to be posi-
tively and significantly associated with the price/cost margin
when a continuous specification was used. However, they also
examined the possibility that a discontinuous relationship might
exist by searching (over 24 possible cutoff levels) for a break-
point in the concentration/margin relationship.[1] They found that
the model was not particularly sensitive to the breakpoint
chosen, but they concluded that a 51 percent 4-firm concentra-
tion level provided the most distinct break. They also found a
significant difference in the slope of the concentration/margin
relationship above and below the 51 percent cutoff when dummy var-
iables and pooled data were used.[2] In addition, they found that a
significant positive relationship existed for the 104 high concen-
tration industries but that no such relationship existed for the
248 low concentration industries when separate group regressions

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[1] The search procedure used to obtain critical values for
concentration can cause considerable problems in interpreting the
final results. See p. 81 note 2 below.

[2] From the graph presented by Rhoades and Cleaver plotting mar-
gins against concentration, the 51 percent cutoff is difficult to
discern and the significance levels of the concentration slope and
intercept dummy variables are not particularly high.

were estimated. Furthermore, the authors tested the two subsamples separately using the same model on each. They noted that important structural differences existed between the high and low concentration groups.[1] The finding led them to conclude that both the continuous and discontinuous methods of estimating the concentration/margin relationship in a single equation are inappropriate since those pooled data methods constrain the coefficients of the variables in the equation to be equal for the two subsamples.[2]

The robustness of Rhoades and Cleaver's results was challenged by Dalton and Penn (1967) who used a sample of 97 food manufacturing firms to test for a critical concentration ratio.[3] These authors regressed the after-tax return on equity on four-firm and eight-firm concentration levels and critical level

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[1] The slope coefficient of concentration was positive and significant at the .05 level in the high concentration sample. In addition, the coefficients on the capital/output ratio and the producer/consumer goods dummy (consumer = 1) appeared to be substantially higher in the high concentration subsample.

[2] Rhoades and Cleaver go on to draw the policy implication that merger enforcement and divestiture actions should be directed at industries at or approaching 50 percent in 4-firm concentration. Below that level no systematic evidence exists linking concentration to higher margins.

[3] Dalton and Penn (1976), p. 135, criticize the Rhoades and Cleaver study on a number of grounds, most notably because their price/cost margin fails to net out advertising expense and the model does not include an advertising intensity measure. The Dalton and Penn study also reviews work by Meehan and Duchesneau (1973) on the critical concentration issue. They had concluded (from rather weak evidence) that critical levels exist at 4-firm concentration of 55 percent and at 8-firm concentration of 70 percent.

interaction terms,[1] firm market share, the advertising/sales ratio, the inverse of the logarithm of assets (a firm size measure), and industry growth. They found that critical levels exist for 4-firm concentration at 45 percent and for 8-firm concentration at 60 percent.[2] They also found a significant difference in the slope of the relationship between high and low concentration subgroups when the data were pooled, but when the subgroups were examined in separate regressions, no significant relationship was found between concentration and profitability.[3] Dalton and Penn conclude from this evidence that the relationship between concentration (at both the four- and eight-firm level) and

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[1] The authors search over 5 percent increments in concentration for the "best" breakpoint. The authors do not present evidence in the paper regarding the results of the search for this break, but they note that such evidence is available.

[2] The coefficients of dummy variables reflecting those levels were significant at the .05 level in the regressions using the full sample and a heteroskedasticity adjustment.

[3] The authors also find some substantial differences in the coefficients other than that of concentration across the two groups of firms. The high concentration subsample tends to have lower market share coefficients, higher advertising/sales ratio coefficients, and larger coefficients on the firm size measure. Although the high concentration subsample has smaller coefficients on the market share measure, those coefficients are significant. Together, these results seem to indicate that pooling the two subsamples may be inappropriate, although Dalton and Penn appear to ignore this implication.

profits for food manufacturing firms is discontinuous, and a dichotomous variable captures this relationship appropriately.[1]

More recent research on the critical level of concentration has taken a slightly different approach. Kwoka (1977) has questioned the usefulness of four- or eight-firm concentration as a measure of monopoly power.[2] He argues that the asymmetry of firm share is masked by these measures and that this causes one to miss the real source of market power--firm dominance.[3] Using a sample of 322 Census 4-digit manufacturing industries, he provides evidence that a measure of market dominance performs better than 4-firm concentration in regressions using price/cost margins as

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[1] The general results differ from Rhoades and Cleaver (1973) only in that Dalton and Penn observe no differences in slopes within groups. Thus, a subsample approach may not be necessary on that basis. Dalton and Penn are unwilling to put forth any policy implications, noting that the positive concentration (or market share) and profits relationship could be due to efficiency.

[2] Although we will not spend much time on the issue of which particular market structure measure(s) is (are) implied by particular assumptions about the behavior of firms, the issue is of some importance. The behavioral model assumed (i.e., Cournot behavior, dominant firm, large-firm collusion, etc.) can have major impact on the type of index one might want to use to measure market structure. For a discussion of this point, see Dansby and Willig (1979), Encaona and Jacquemin (1980), Hause (1977), and Kelly (1981), Kwoka (1981), and Saving (1970).

[3] The idea that dominance is more important than concentration flows in part from Mann's (1970) earlier work on asymmetry of market shares. We should note that the effects of share asymmetry are not theoretically unambiguous. Unequal shares may lead to market dominance as Kwoka suggests, but they are also a factor which complicates the attainment and maintenance of a tacit or explicit collusive agreement.

the dependent variable.[1,2] Specifically, he found that the coefficient of the dominance measure was positive and highly significant and more robust with respect to model changes than the traditional concentration measure.

In later work, Kwoka (1979) refined this approach further by emphasizing the effect of individual firm market shares on industry price/cost margins. Using data on 314 4-digit Census industries for 1972, he regressed price/cost margins on various measures of the firm size distribution,[3] the capital/output ratio, a geographical dispersion index, industry growth, a consumer/

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[1] Kwoka's measure of dominance is the sum of the squared differences between shares of the firms, once they are ranked from highest to lowest. In addition to the dominance index examined by Kwoka, other alternatives to the concentration measure of market structure have been proposed. These include an entropy index (Stigler 1968b; Miller 1972), a welfare gradient index (Dansby and Willig 1979), a generalized Herfindahl measure (Kelly 1981), and several alternatives to the Herfindahl index (Schmalensee 1977).

[2] The model uses the dominance index, concentration, the number of companies, the capital/output ratio, a geographical market index, industry growth and a producer/consumer goods dummy (consumer = 1) as independent variables.

[3] The size distribution variables included the four-firm concentration ratio, the Herfindahl index (the sum of the squared market shares in an industry), two-firm concentration, individual shares, and dummy variables indicating critical values for the individual firm market shares.

producer goods dummy, and measures of minimum efficient scale.[1] A

series of regressions using individual shares of the top one

through four firms in the industry indicated that use of a four-

firm concentration ratio would hide a considerable amount of

information.[2] The shares of the top two firms have coefficients

that are consistently positive and significant, while the coeffi-

cients of the third and fourth firm's shares are negative and

often insignificant. In addition, a comparison of equations with

four-firm concentration as the firm size distribution measure

versus those using the individual shares of the top four firms

indicates that a significant increase in explanatory power is

gained from the latter (less restrictive) specification.

Taking this analysis one step further, Kwoka searched over

various combinations of critical share values for the first

three firms in an industry. He found that his model fit best for

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[1] Industry scale economies were measured in two alternative ways:
as the share of an industry's midpoint plant size and as an
interaction of this share with a cost disadvantage ratio of small
firms. See Kwoka (1979), p. 102. Apart from the more detailed
analysis of size distribution, the addition of the scale measures
is the substantive improvement over Kwoka (1977). We will not
dwell on the results of the independent variables other than
market share, except to note that they perform consistently and as
expected in the reported regressions with positive and significant
coefficients on all the variables other than the geographical
dispersion index which obtains the expected negative sign.

[3] Kwoka also examined the relative merits of using a Herfindahl
index rather than the four-firm concentration measure in his
model. He concluded (based on goodness of fit and coefficient
significance) that the results ". . . provide little support for
the alleged superiority of the [Herfindahl] index." Kwoka (1979),
p. 103.

critical shares of 26, 15, and 16 percent, respectively, and dummy

variables based on these critical values were highly significant

in the price/cost margin regressions.[1] Although the statistical

significance of these findings is difficult to evaluate, due to

the search procedure used to obtain the critical values, the

implications are quite important.[2] In brief, the results indicate

that if the first firm's share (S_1) exceeds 26 percent, margins

rise by 3.5 to 5 percent, and if the second firm's share (S_2)

exceeds 15 percent, margins are 4 to 5 percent higher than other-

wise. If both these conditions occur ($S_1 > 26$ and $S_2 > 15$), then

price/cost margins may rise by 8 to 9 percentage points. Finally,

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[1] Dummy variables based on the first two critical values obtain
positive and significant signs in the regression model. The dummy
variable based on the third-firm critical share obtains a signifi-
cant negative sign, but this result is based on only five observa-
tions and must be weighted accordingly. See Kwoka (1979), p. 107.
Kwoka also searched over various two-firm concentration levels to
obtain the best fitting relationship. He found a critical two-
firm level of 35 percent. However, he argued that even this
"best" two-firm level is not preferable to an individual share
approach since better fits to the data are obtained using
individual shares.

[2] Kwoka determined the critical share levels through successive
market share dummy variables and continuous variable interactions
using the entire model. This procedure causes the standard errors
of the share coefficients to be biased downward in the final
estimation and it spuriously increases the significance of the
estimated critical values. Recently, Sant (1979) has tried to
avoid this problem by using a procedure which estimates the cri-
tical level simultaneously with the remaining coefficients of the
model. His technique produces standard errors that are not sub-
ject to bias. When using Kwoka's (1977) data, Sant did not find
clear evidence of a critical level of share or concentration due
to the imprecision of the estimates. This result must lead one
to be somewhat skeptical of Kwoka's critical levels. This same
criticism also applies to the earlier critical-level literature.

.footnote continues)

the effect of a large third firm is salubrious. If the third
firm's share exceeds 16 percent, price/cost margins tend to fall
by 13 to 14 percent.[1] These findings indicate that concentration,
by itself, may not be detrimental, but that dominance may be a

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(footnote continued)

In a very recent effort to calculate critical concentration
levels in the markets for bonds, gasoline, and food retailing,
Geithman, Marvel, and Weiss (1981 have tried to avoid this
problem by using multiple concentration dummy variables in one
regression rather than searching for critical levels one at a
time. Using this procedure, Geithman et al. find support for
Kwoka's two-firm critical share level of 35 percent in their
examination of pricing in the retail gasoline market. They fail
to find a critical concentration level in their data on the prices
of supermarket chains. although they do find a positive linear
relationship between prices and concentration. In the market for
bonds, the authors find that critical 4-firm concentration is no
lower than 50 percent if such a critical level exists at all. In
conclusion. Geithman et al. argue that use of a single critical
concentration level based on studies for all manufacturing is
likely to result in errors in many specific cases. Thus, one
would want to be rather careful in applying the results of
broad-based studies of critical concentration.

[1] If all three firms exceed the critical level, then the measured
effect does not differ from zero. Thus, the existence of three
relatively large firms (no one of which is clearly dominant) can
lead to competitive performance. Again, this result is based on a
small sample. Recent research by Lamm (1981) on market baskets of
food products in 18 cities over 4 years provides some support for
Kwoka's critical share levels. However, Lamm found a large fourth
firm tended to increase competition, while a large third firm did
not. Both authors agree that the four-firm concentration ratio
obscures too much information in studies of market structure and
performance.

a more important problem.[1] They also indicate the substantial

amount of information hidden by the use of aggregated measures of

firm size distribution such as the Herfindahl index or four-firm

concentration.[2]

Although the study was not designed to examine the critical

concentration issue, work by Fraas and Greer (1977) sheds some

light on the number of firms required to foster competitive behav-

ior. These authors reviewed 606 cases of formal collusion cover-

ing 1910 to 1972 found in tne files of the Department of Justice.

Their analyeis is built on the premise that if fewness of firms is

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[1] The usef lnass of Kwoka's work has been questioned by Gale and
Branch (1979a), pp. 19-21. Using the PIMS line-of-business data
for 1080 firms, they stratify a sampl2 into two groups--one con-
sisting of the leading two firms in each industry, tne other con-
sisting of the remaining firms. They regress returr on investment
against relative market share and four-firm concentration and find
that the coefficient of concentration obtains a positive but
insignificant sign, while the coefficient on tne shar2 index is
positive and highly significant for ooth groups. They argue that
if Kwoka was correct, share would obtain an insignificant sign in
the regression for the second grou:. There are a number of
problems with this test, including general underspecification of
the estimating equation, very low explanatory power of the model,
and the use of four-firm concentration in the mode: even though
Kwoka's major point was that sucn a measur2 was inappropriate.
Still, the results indicate that using disaggregated data may make
a difference in the relationship found between returns and market
share.

[2] As with all of the critical concentration literature, one must
remain cautious in applying Kwoka's estimates, since the
industries typically used do not conform very well tc industries
as they would be defined in a case-by-case context For example,
it is interesting to compare the average four-firm concentration
levels found in Buzzel's (1981) study using the PIMS line-of-
business data with Kwoka's (1981) work with four-digit SIC cate-
gories. Buzzel notes average large-firm line-of-business four-
firm concentration is approximately 70 percent, whereas Kwoka's
sample implies an average CR4 of only 40 percent.

conducive to collusion. then a frequency distribution of all
conspiracy cases with respect to the number of firms involved
should have lower measures of central tendency than a comparable
frequency distribution of the number of firms in industry general-
ly. Their review leads them to believe that the number of firms
involved in a conspiracy among manufacturers tends to be low
(median of 7) while the number of conspiring firms is high in
the construction and distribution industry groups (11 and 15,
respectively). They also note that several institutions or prac-
tices facilitate collusion among large numbers of firms or were
originated to avoid coordination problems caused by demand and
cost complexities. These include trade associations, patents,
bid-rigging, product standardization, sales agencies, exclusionary
practices, and market allocations. The statistical results are
not strong, but they tend to indicate that "as the number of
parties increases and/or as the structural conditions become
increasingly complex, conspirators must increasingly resort to
arrangements of more elaborate design or greater efficiency if
they are to achieve joint profit maximizing objectives."[1] The
evidence leads the authors to conclude that an industry is likely
to be workably competitive on average if 12 or more firms exist

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[1] Fraas and Greer (1977), pp. 42-43.

which are not too unequal in size and if there exist no activities that facilitate tacit or formal collusion.[1,2]

It is not obvious how this evidence on optimal firm numbers might tie in with the concentration/profit rate literature. Nevertheless, some experimental economics research presently in progress tends to indicate that an even lower number of firms than found by Fraas and Greer can lead to competitive performance in the absence of collusion-facilitating practices.[3]

CONCLUSION

We have examined the theories and evidence relevant to many of the economic bases for horizontal antitrust activity. As is the case in most surveys, generalizations about the current state of knowledge in an evolving field can be made only with more than a modicum of trepidation. Nevertheless (and again, as in most surveys), we will proffer some disputable impressions of the current morass.

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[1] Given the results for manufacturing alone, a lower critical number of eight firms might be appropriate.

[2] One potential problem with the Fraas and Greer methodology is that their sample may not contain only cases of welfare-reducing collusion. Asch and Seneca (1976) have argued that collusion is often found in industries where producer profits are very low or negative and where consumers are not paying monopoly prices. If this is true, the sample of collusion cases used by Fraas and Greer may not unambiguously indicate substantially diminished welfare. Still, their sample of formal collusion cases is almost certain to contain at least as many cases of welfare reduction as an equal-sized sample of high profit or high price/cost margin firms.

[3] For instance, see Plott (1981) and Grether and Plott (forthcoming).

The debate concerning the correct interpretation of profit/ concentration studies does not quite seem to have ended, but the recent traditional literature has taken a change in direction. Prior to 1970, the collusion model of market power implying a positive concentration/profitability relationship held sway. Since at least 1977, the emphasis has shifted toward the market dominance problem, and the large market shares held by leading firms have become the focus of attention. This shift can in large part be attributed to the revisionist element of the economics profession, which has emphasized the difficulties inherent in achieving and maintaining collusive outcomes and the gains from large-scale production. Another reason for the shift from the more traditional collusion model has been the very recent results obtained from line-of-business data, which indicate that market shares, rather than concentration, are related to high profitability.

Despite the shift in empirical and theoretical research, a major issue still separates the traditionalists and the revisionists. This issue involves the existence and importance of entry barriers. Without such barriers, the traditional explanation for market power due to either collusion or market dominanc becomes unappealing. In addition, the revisionist description c an industrial economy makes use of the "natural frictions and ignorance that characterize any real economy."[1]

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[1] Demsetz (1973), p. 3.

Whether such "natural frictions" can reasonably be characterized
as barriers to entry may be debatable, but these frictions do pro-
vide the revisionist's rationale for the existence and persistence
of economic rents obtained by large firms.

What does our evidence imply for horizontal antitrust policy?
First, it would seem that a broad-based antimerger policy must
recognize the importance of the presence or absence of barriers to
entry in specific cases. Second, even the traditional literature
now indicates that poor performance due to market concentration
is not a serious problem if disparity in the size of leading firms
is not too large. This would seem to imply that mergers which
increase the equality of firm sizes may be beneficial to
competition as long as the absolute number of firms in a market is
not too small. Beyond these two general statements, rather little
can be said. The empirical evidence is too inexact to allow us to
determine the correct market share standards that should trigger
enforcement concerns and our theories give us little if any
guidance in choosing specific market share or concentration
levels that are likely to lead to poor economic performance.
Despite the lack of economic consensus on these important issues,
it seems clear from a review of the economic literature that
informed antimerger policy would call for a readjustment of the
Guidelines proposed by the Justice Department in 1968.

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