DIVISION OF THE HUMANITIES AND SOCIAL SCIENCES

CALIFORNIA INSTITUTE OF TECHNOLOGY

PASADENA, CALIFORNIA 91125

RESEARCH AND DEVELOPMENT EXPENDITURES AS A COMPETITIVE STRATEGY

David M. Grether

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David M. Grether
California Institute of Technology

I. INTRODUCTION

Making a better mousetrap has been one of the standard methods of achieving competitive advantages in American industries. Of course, making an equally good mousetrap with lower costs can be just as effective. To the extent that businesses compete with each other with product and process improvements, then one would expect investment in these activities (research and development [R&D]) to be a primary competitive tool. It is natural to ask which types of firms tend to engage heavily in R&D. A clearly related question one might ask is which market structures are conducive to R&D activity and which are not. In addition, it is important to remember that market structure itself may be affected by firms' R&D activities--raising the question of R&D's impact on market structure (see the related paper by Preston in this volume). The most obvious situation in which market structure is affected by R&D activity is, of course, that of a monopoly position achieved and maintained by patents. This latter question; that is, essentially asking if market structure is really exogenous, is often not directly addressed in the literature.

The purpose of this paper is to survey one portion of the so-called market structure literature, viz., the empirical literature dealing with the relation between market structure and the level of research and development activity. Weiss (1969) has surveyed the empirical literature in the entire field of industrial organization, and Kamien and Schwartz (1975) more recently surveyed the literature concerning innovative activity in general. In order to allow for an intensive examination of one body of literature, the scope of this paper has been kept narrow. Readers interested in other issues, for example, the rate of adoption or imitation and the diffusion of technological information, are referred to these other studies.

There are two aspects of the literature which to a considerable extent dominate the discussion that follows. The first has to do with questions of causality. Thus, for example, in some papers a variable, such as expenditures on research and development, is treated as the dependent variable with quantities such as firm size, profitability, and so forth, as independent variables. Other writers, however, would consider the relationship the other way around. That is, profitability or sales growth might be the dependent variable with R&D among the independent variables. In rare instances, a writer might support both positions. Thus the literature clearly suggests that there is a simultaneous equations problem. The second feature noted was the seriousness of the problems of measurement. In many if not all fields of applications, one can find complaints about the quality of available data. Problems of aggregation, excessively long time periods between observations, insufficient degrees of freedom, and so forth are found in most areas, and industrial organization is no exception (Grabowski and Mueller 1970). While those kinds of problems are serious, the

market structure innovation literature has in addition several more fundamental difficulties. For instance, are patent statistics a measure of inputs into the innovative process or a measure of the output of the process (Comanor and Scherer 1969)? Beyond questions as to the meaning of particular numbers, there are questions of how one could measure certain quantities, for example, the output of a firm's research department or the "size" of a technological breakthrough. Those who do research in the area are quite aware of the simultaneity problem as well as the difficulties of measurement. These problems are often discussed explicitly and even when not, they affect the research methodology adopted; for example, use of rank correlations as opposed to the usual correlation coefficients. Also, these problems are not all unique to the structure-innovation literature, but to a considerable extent are features of the entire field of industrial organization. Thus, prior to discussing the literature on research and development, it seems worthwhile to provide a brief overview of market structure analysis and the associated problems of measurement.

The plan of this paper is as follows: section II is introductory, containing a short description of the field as a whole. Research on some of the relevant measurement problems is discussed in Section III, and the survey is given in section IV.

II. MARKET STRUCTURE ANALYSIS

The basic idea in this literature is that there are relationships running from market structure to firm behavior and therefore to the performance of the industry. As a practical matter most of the literature is directed toward linking market structure and performance, performance being characterized by profit rates, price-cost margins, rate of technical progress, and so forth. One way of looking at the market structure literature is an attempt to determine a few (at least in principle) measurable variables which are sufficient for predicting the various aspects of industrial performance.

While the list of variables that describe market structure varies somewhat from one application to another, certain variables are generally considered important. These are: concentration, barriers to entry, product differentiation, price and income elasticity of demand, and the extent of economies of scale in production. For a discussion of the importance of these and other variables, see Bain (1959).

Concentration is generally measured by some feature of the size distribution of the firms in the industry in question. There is no general agreement on the appropriate definition of firm size; sales, assets, value added, and employment are all commonly used. However measured, the assumption is that if an industry is highly concentrated, i.e., is dominated by a small number of firms, price competition is less likely to prevail and nonprice competition and/or collusivelike behavior is likely to be more prevalent.

While a high level of concentration is itself thought to be significant, frequently one asks if this can be explained on purely technological grounds. One wants to know if policies designed to lower concentration and thus, hopefully, increase competition, will incur offsetting costs in terms of losses of productive efficiency.

Also, if economies of scale are such that only a few firms could be expected to survive, then this fact might be used as an argument for some sort of regulatory policy. Thus the relation between the size of the overall market (measured in terms of output or sales) and the minimum efficient firm size (the smallest output at which average costs are at a minimum) is of considerable interest. The latter is estimated from analyzing cost and output data for the firms in the industries being studied. Note that if a firm must produce a substantial proportion of industry output in order to avoid a cost disadvantage, this may well discourage entry. Quite apart from the absolute size of the required investment, it is argued that the large relative size of the entering firm would lead to hostile reactions from the existing firms.

A rapid rate of growth in the output of an industry is considered to be a force tending to lower concentration and to promote competitive behavior. The idea is that new firms may enter and small firms grow without provoking the retaliation that would occur if the sales of the new or growing firms led to an absolute reduction in sales by the larger firms in the industry. Also, with new buyers coming into the market, the results of secret price cutting may be harder to detect. Historical rates of increases in output and estimates of income elasticity of demand are thus relevant to the description of market structure. Similarly, estimates of the own-price elasticity can be useful as indicative of the incentive to engage in price competition.

Product differentiation is important both as a possible deterrent to entry (heavy initial advertising outlays might discourage some potential entrants) and as an indicator of nonprice competition. While it is not clear how to measure the "degree" of product differentiation, the ratio of expenditures on advertising to sales is frequently used.

Finally, the conditions of entry are, as noted above, related to most of the other elements of market structure. In addition, other factors, for example, patents or the control of raw materials, could be important. Though entry conditions are often referred to as "barriers to entry" (the heights of which could presumably be measured), they are not usually quantified in any single index. However, frequently for

efficient firm size and the overall size of the market determine concentration.

In some instances the direction of causation is generally agreed upon. It is widely assumed, for instance, that increasing concentration makes collusion easier and leads to higher profit rates. The theories that have been put forward to explain the level of concentration and much of the relevant work have been reviewed recently by Ornstein, Weston, Intriligator, and Shrieves (1973). They explicitly pointed out the interdependencies among many of the variables frequently used and suggested some alternatives. Possibly as a result of this interdependence, the interpretation placed upon the related empirical work seems to be largely descriptive. For the market, or four or fewer companies have accounted for 80 percent of sales" (p. 267). The primary remedy under their proposal was division or divestiture of assets thus directly lowering concentration and thereby improving the performance of the industry.

One of the major questions examined in the market structure literature reviewed below is the following: what is the relationship between inventive activity and concentration? In other words, are there relevant systematic differences between firms in highly concentrated industries and firms in unconcentrated ones? If it is the case that concentration affects the level of inventive activity, the policy importance of this question is self-evident. Will a vigorous antitrust policy--prevention of mergers among sizable firms and the breaking up of the giants such as General Motors--have a positive or negative effect on the rate of technological progress and, thus, on the rate of growth of the American economy? On the other hand some, for example, Phillips (1966), have argued that the nature of the underlying technology and the results of research and development are what determine concentration.

Thus it is not clear just how one should interpret a regression coefficient in a regression of, say, R&D expenditures on industry concentration ratios. To the extent that this is an equation from a simultaneous equations model such as that sketched above with regard to advertising, then ordinary least squares is not the appropriate method of estimation. Of course, without a more fully specified model, merely changing the estimation procedure cannot by itself be expected to lead to meaningful results.

III. MEASUREMENT

In order to test hypotheses about the relationships between inventive activity and market structure, one needs measures of both types of variables. For the former the deficiencies of the measures used as well as problems involved in developing acceptable measures have been widely discussed (in particular, see Sanders 1962, Kuznets 1962, Comanor and Scherer 1969, and Schmookler 1954a,b, 1957, 1962). Most researchers have used either a measure of inputs into the research and development process (viz., the rate of expenditure or the level of employment) or patents. The primary disadvantage of patent statistics is the obvious one: the substantial differences in the economic and technical importance of patented inventions. On the other hand, patent statistics are widely available, and a patent does signify a new and potentially useful device.

Much of the difficulty with determining the input to or output from inventive activity is associated with trying to quantify these variables for a specific device or process. Thus, there may have been several independent unsuccessful (and unreported) attempts to invent the same thing. Also, it may not be entirely clear how much previous effort was devoted to distinct but related projects. Similar considerations can make it difficult to determine the economic magnitude of an invention, that is, it may lead to numerous related inventions (or improvements), the existence of which may have been dependent upon the original invention. These types of problems are avoided in most of the papers surveyed since in these studies the relevant variable is the overall level of such activities in a firm or industry rather than the amount associated with particular projects.

As Kuznets (1962) has pointed out, properly measuring inputs in terms of man-hours or dollar expenditures is not easy. Part of the difficulty is that published data generally do not allow one to distinguish between basic research, applied research, and development work. In addition there is the usual problem of aggregating the efforts of individuals "endowed with different inventive capacities." Using wage payments instead of man-hours may not be a fully satisfactory solution to the latter problem: "Considering the difficulty of estimating the economic contribution of inventive activity . . . the assumption of marginal productivity would surely strain one's credulity" (p. 33). Still, despite all these difficulties, it seems that it would be important if one could, using the market structure approach, explain the amount of resources devoted to industrial research and development.

As discussed in the preceding section, market structure includes more than just concentration. In practice, however, some measure of concentration is frequently the only market structure variable employed. Most studies measure concentration by the standard four-firm concentration ratio defined as the percentage of the total sales of the industry accounted for by the largest four firms. Often employment or assets are used instead of sales, and frequently researchers will use more than one measure to insure that the results obtained are not simply an artifact of the measure chosen. In fact there are as many measures of concentration of this type as there are measures of firm size. As Shalit and Sankar (1977) note, no single measure of

firm size is completely appropriate.

However one chooses to calculate the level of concentration, there are a number of quite serious practical problems in using such variables. For example, the presence of some vertically integrated firm in an industry may make shipments the relevant variable rather than sales, and may make concentration levels computed using assets or employment be quite different from those computed using shipments as firm size. The SIC industries may not be entirely suitable so it may prove necessary to aggregate several concentration ratios (Kaysen and Turner 1959). Kilpatrick (1976) has found that in practice there is little difference between averaging concentration ratios for several industries and computing directly the concentration ratio for the larger set of firms. The latter of course is a more appealing procedure (see Boyle 1973). Also, concentration ratios are computed on the basis of national figures whereas in many industries the markets may be geographically segmented. For a discussion of these and other related problems see Bain (1959) and Adelman (1951).

Quite apart from questions of implementation there are the questions of what "concentration" is; that is, what is it that one wants to measure, and how at least in principle ought it to be measured? In fact it turns out that "concentration" means different things to different people and not surprisingly a number of ways have been suggested to measure it.

While concentration is expected to be related to the "level" of competition or the "degree" of competitiveness, it is not generally interpreted as an index of competition. Douglas (1969) provided a temporary exception to this rule. In his paper Douglas argued that geographical dispersion, restrictive trade practices, contervailing power, "market division" (related to cross elasticities of supply among the several products produced by an industry), and problems associated with the Census industry definitions all lead to differences between the measured or "apparent" levels of concentration and the "real" level by which he meant competitiveness. Douglas's work was attacked by Nightingale (1970 a,b) on a variety of grounds, but most importantly on the interpretation of concentration. In his reply Douglas stated that after thinking it over he had changed his definition. "Whereas previously intended as an index of competitiveness, 'real concentration' now refers simply to the degree of seller concentration at a more meaningful level of disaggregation. . . . From here it is plainly an additional step to an index of competitiveness, as the effect of restrictive practices, buyer concentration, further data problems, and possibly other factors as well, must be quantified and incorporated" (1970, p. 124). So concentration is a characteristic of the size distribution of the firms in an industry; however, it is not clear what characteristic is the appropriate one.

The four-firm concentration ratio is the measure most frequently used in practice; in part this is due to its availability. Concentration ratios based on the largest eight, twenty, and fifty firms are also available. Miller (1967) suggested using "marginal concentration ratios" to explain profit rate. The idea is that rather than looking only at the share of the top k firms, one also could use the share of the kth through mth largest firms. Given the various

published concentration ratios it is possible to compute some of the marginal concentration ratios, for example

$$MCR_{5,8} = CR_8 - CR_4$$

where CR_i is the share of the i largest firms and $MCR_{i,j}$ is the share of the ith to jth largest firms. Miller argued that rather than using CR_4 alone as is usually done, it might often be better to use as much information as is available on the entire size distribution. Further, he advocated use of the marginal concentration ratios as a way around the problem of multicollinearity (CR_4 and CR_8 are much more highly correlated than CR_4 and $MCR_{5,8}$). Suppose one has a model in which the profit rate is determined by CR_4 and CR_8:

$$\begin{aligned}\pi_i &= a + bCR_{4_i} + cCR_{8_i} + u_i \\ &= a + (b + c)CR_{4_i} + c(CR_{8_i} - CR_{4_i}) + u_i \\ &= a + (b + c)CR_{4_i} + cMCR_{5,8_i} + u_i.\end{aligned}$$

Now if CR_4 and CR_8 are nearly collinear, then estimates of b and c may not pass standard significance tests. If the regression is estimated using $MCR_{5,8}$ instead of CR_8, the estimated coefficients of $MCR_{5,8}$ and CR_8 will be identical as will their standard errors (apart from roundoff errors). It could happen that the coefficient of CR_4 will become significant, but by the standard properties of least squares regression this is simply the sum of the estimated coefficients of CR_4 and CR_8, i.e., $\hat{b} + \hat{c}$. Thus the two approaches are equivalent, and changing to MCR adds no new information.

Miller noted that in principle the entire size distribution is relevant and considered a model of the form

$$\pi_i = a + b_1 CR_{4_i} + b_2 MCR_{5,8i} + h_3 MCR_{9,20i} + b_4 MCR_{21,50i} + b_5 MCR_{51,\infty i} + u_i.$$

He correctly observed that there is an identification problem since the explanatory variables add to a constant (100). Eliminating the share of the smallest firms he estimated

$$\pi_i = a + 100b_5 + (b_1 - b_5)CR_{4_i} + (b_2 - b_5)MCR_{5,8i} + (b_3 - b_5)MCR_{9,20i} + (b_4 - b_5)MCR_{21,50i} + u_i.$$

Of course, it does not matter which variable is dropped, the results obtained will be equivalent. That is, if CR_4 is dropped, one will obtain estimates of $a + 100b_1$, $b_2 - b_1$, $b_3 - b_1$, $b_4 - b_1$, and $b_5 - b_1$, from which Miller's estimates could be deduced. Miller apparently did not recognize this and reported that equations "including $(100 - CR_{50})$ (but excluding each of the other marginal concentration ratios in turn) produced results without sensible economic interpretation" (1967, p. 267).

The usefulness of marginal concentration ratios has been disputed by Collins and Preston (1969). They noted that by definition $MCR_{5,8}$ cannot exceed CR_4, and that if CR_4 exceeds 50, then $MCR_{5,8}$ must be less than or equal to $100 - CR_4$. In order to eliminate the range

restrictions, Weiss suggested using

$$V = \frac{MCR}{CR} \qquad CR \le 50$$

$$= \frac{MCR}{1 - CR/100} \qquad CR > 50.$$

Weiss reported regressions estimated using the data of Collins and Preston with V replacing $MCR_{5,8}$. It turned out that the estimated coefficient of V had a positive sign but was less than its standard error, leading Weiss to conclude that "MCR has little effect one way or another" (1969, p. 373).

Usually one interprets regression coefficients as being estimates of partial derivatives. That is, one is attempting to answer the question, "If x goes up by one, <u>other things being equal</u>, by how much will y change on average?" Now when the independent variables must sum to a constant, this question is meaningless; one must specify the changes in the other variables. As Collins and Preston in effect pointed out, omitting the share of the smallest firms from the estimated regression does not eliminate this problem. Neither, of course, can resorting to variables such as V. For example, Weiss regressed price-cost margins on CR_4, V, plus some other variables. Since the value of V is not constrained by the value of CR_4, one can ask about the effects of unit changes in CR_4 or V (<u>ceteris paribus</u>); however, the interpretation of these changes depends crucially on the value of CR_4. Notice that if CR_4 is larger than 50, V has a relatively straightforward interpretation, namely the level of concentration among the smaller firms in the industry.

There are a number of other measures of concentration in addition to marginal concentration ratios that use information about the entire size distribution of the firms in the industry. Three of these are related to entropy H.

$$H = -\sum_{i=1}^{n} s_i \log s_i$$

where n is the number of firms in the industry and s_i is the share of sales (employment, etc.) accounted for by the ith firm, and the logarithms are usually base 2. If s_i is taken as the probability that a dollar of sales goes to the ith firm, then H may be interpreted as the amount of information obtained in learning which firm made the sale (Theil 1967, Khinchin 1957). For a given n, H attains its maximum (log n) when all firms are of equal size suggesting the use of "relative entropy" defined as

$$G = \frac{H}{\log n}.$$

Another entropy related measure is F the "numbers equivalent" (Horowitz 1971) defined by

$$H = \log F.$$

Suppose that for some particular industry, entropy is calculated to be H, then F is the number of equal-sized firms which would result in an

entropy of H. Miller (1972) used G, F, and CR_4 to explain profit rates for twenty-five, four-digit industries. In all of his regressions, G was significant with a positive coefficient and F insignificant. CR_4 appeared to be significant in regressions which also contained G, and was insignificant otherwise. Miller concluded that F, G, and CR_4 "reflected different aspects of market structure," but unfortunately, was not able to provide any clear interpretation for G, the one measure which consistently performed well in his regressions. The difficulty concerned comparing G's computed for industries with different numbers of firms.

Exactly the same difficulty arises in using the Gini coefficient. This coefficient which is a measure of the inequality in a size distribution is defined as the area between the Lorenz curve and the 45 degree line corresponding to cases in which all firms are of equal size. It can be shown (Kendall 1943) that the Gini coefficient g is equal to:

$$g = \frac{1/2 \sum_i \sum_j |x_i - x_j|}{n^2 \bar{x}}$$

where x_i is the size of the ith firm and $\bar{x}$ is the average firm size. Hart and Prais (1956) suggested an interesting interpretation for the Gini coefficient. "Suppose the difference in the size of two firms provides a measure of the degree of 'dominance' that the one may exert over the other's price or output policy. . . . Then a measure of dominance for an industry as a whole may be found by taking the mean difference, irrespective of sign, between all possible pairs of firms. . . . The Gini coefficient can therefore be interpreted as a measure of average dominance in the group of firms considered" (1956, pp. 152-153). As Adelman (1959) has pointed out, the Gini coefficient by itself is not of much use for comparing industries with different numbers of firms as, for instance, it does not differentiate between an industry with two equal-sized firms and one with a thousand firms all of the same size.

There have been two rather similar attempts to obtain measures of concentration using an axiomatic approach (Hall and Tideman 1967 and Niehans 1958). Hall and Tideman posited six criteria that a measure of concentration should satisfy:

(1) It should be a single number.

(2) It should depend upon all the market shares.

(3) It should increase whenever there is a shift from a smaller to a larger firm.

(4) If each firm in an industry is divided into K equal ones, the measure should change by a factor of 1/K.

(5) It should be a decreasing function of N when an industry is divided into N equal firms.

(6) It should range between 0 and 1.

Of the six properties it seems likely that most would find numbers 2, 3, and 5 unobjectionable in principle. Adelman (1951), however, argued that as a practical matter using the full size distribution is risky since this makes the measures sensitive to the often arbitrary industrial classifications of numerous small firms. Properties 4 and 6 are clearly ad hoc and serve primarily to narrow

down the list of possible candidates. Hall and Tideman considered indices based upon

$$R = \sum_{i=1}^{n} is_i$$

where s_i is the share of the firm with rank i, so that if s_i is the probability that a random dollar of sales goes to the firm with rank i, R is the expected rank. The index they suggested is:

$$TH = \frac{1}{2\sum_{i=1}^{n} is_i - 1} .$$

Property 6 led them to consider functions of 1/R, the exact form being chosen to satisfy property 4. Hall and Tideman noted that a large number of indices could satisfy their criteria and that one other popular index, the Herfindahl index HH (which will be discussed presently), does so. They reported that empirically both measures as well as CR_4 turn out to give essentially the same rankings of concentration. They concluded that if "HH or TH is the correct measure of concentration, then the concentration ratio is certainly a good proxy" (1967, p. 168).

Niehans (1958) was concerned with developing an index of average firm size rather than relative concentration, so he did not adopt property 6. He set down four criteria:

(1) Same as 1 above.

(2) If all firms in an industry are the same size, then the index should equal that size.

(3) Same as 2 above.

(4) If a large or "medium-sized" firm grows, other firms staying the same size, then the index should rise, and if a small firm grows, ceteris paribus it should fall.

The index proposed by Niehans is

$$\gamma = \sum_{i=1}^{n} x_i s_i = \frac{\sigma^2}{\overline{x}} + \overline{x},$$

where x_i is the size of the ith firm and s_i its share of the industry's total sales or employment, $\overline{x}$ is the average firm size, and σ^2 is the variance of firm size (Hart 1961). Thus Niehans's index is larger than the average firm size unless all the firms are equal.

One other commonly used measure of concentration is the Herfindahl index:

$$HH = \sum_{i=1}^{n} s_i^2,$$

where s_i is the share of the ith firm. Again if s_i is interpreted as the probability of the ith firm obtaining a given dollar of sales, then HH is the expected value of that probability. Stigler (1964) has argued that HH is the appropriate measure of concentration in that it is related in a simple fashion to the ease with which firms can detect secret price cutting or cheating on a collusive agreement. In Stigler's theory firms use classical hypothesis-testing rules and decide that secret price cutting is going on whenever actual sales fall

below expected sales by a predetermined number of standard deviations. For example, suppose that in each period, n_n, new customers enter the market and "let the probability of attracting a customer be proportional to the firm's share of industry output, s. Then the variance of the firm's sales to new customers will be $n_n s(1 - s)$, and the aggregate for the industry will be

$$C = n_n \sum_{i-1}^{n} s(1 - s)$$

for n firms" (Stigler 1964, p. 55). Note that $C = n_n(1 - HH)$.

More recently Dansby and Willig (1979) and Hause (1977) have presented theoretical justification for HH. Dansby and Willig derive HH and other indices from welfare maximizing criteria (maximizing consumer surplus). Hause argues that HH should be a lower bound for "reasonable" indices of concentration that range from zero for competition to one for monopoly. Hause, like other writers, assumes the cardinality of concentration without questioning it. As Schmalensee (1977) points out, generally one cannot compute HH as its computation requires the full size distribution. Schmalensee reports tests of a number of computible alternatives to HH. Unfortunately, which surrogate works best seems to depend upon the level of concentration as measured by HH. It does appear that the standard four-firm concentration ratio is superior (as an approximation to HH) to using one based upon the eight largest firms.

As with the entropy measures, the Herfindahl index can also be converted to a numbers equivalent. Thus, if an industry consists of n equal size firms,

$$HH = \sum_{i=1}^{n} (1/n)^2 = 1/n;$$

so the reciprocal of HH has an interpretation similar to F discussed previously.

As mentioned earlier the concentration ratio CR_4 is the most commonly used measure. In part this is due to the fact that these figures are readily available, but an additional advantage is that it is more easily interpreted (at least understood) than the entropy measures or the other indices. In other words, to say that the top four firms in one industry produce x percent of the industry's output, whereas ten years earlier the top four produced y percent is a reasonably concrete statement whether or not its implications are entirely clear. Similar statements in terms of H, HH, TH, G, etc. are not so readily understood. These difficulties in interpretation provide much of the motivation behind the use of numbers equivalents (Horowitz 1971, Adelman 1959) as these measures are expressed in units that are on the surface at least more familiar.

In addition to their uses in comparing the levels of concentration among different industries, the various measures of concentration are also used in time series studies to determine if there are any discernible trends in concentration (Bain 1970). The study of changing concentration over time has led to another entirely different class of measures based upon mobility (see for example, Collins and Preston 1969, Boyle and Sorenson 1971). Suppose the firms

in an industry are ranked according to some measure of size. The idea behind the mobility measures is roughly as follows: if there is intense rivalry within an industry, this should be reflected in changes in the rank orders as different firms gain and lose relative to the competition. Conversely, if there is little competition, one expects the rankings to be relatively stable. These considerations lead to measures of mobility such as rank correlations. The important thing to note here is that concentration or "competitiveness" measures based upon mobility could lead to orderings of industries that are quite different from those obtained using the other types of measures. The measures described previously either ignore rankings of the firms or weight them by firm size.

Each of the proposed measures of concentration is based upon some feature of the size distribution of a particular group of firms. Now in general one cannot expect to be able to summarize arbitrary frequency distributions with a single index. In fact there have been a number of empirical and theoretical studies of firm size distributions (Adelman 1951, Hart 1957, Mansfield 1962, Simon and Bonini 1958; see also Kalecki 1945, and Champernowne 1953). These studies generally deal with distributions that would arise if Gibrat's Law or the law of proportionate effect held, the key assumption being that the probability of a given percentage change in a firm's size is independent of the size of the firm. It has been well known for some time that this assumption will yield a skewed size distribution (the precise distribution depends upon the assumptions made about exit and entry). Clearly if a parametric representation of firm size distributions (or the upper tail of these distributions) were known, it would have implications for measurement of concentration. Since concentration is simply a characteristic of the size distribution, it would be reasonable to use measures expressible in terms of the parameters of this distribution. Alternatively, representations of the parameters themselves might be used as explanatory variables. For example, Aitchison and Brown (1957) and Hart and Prais (1956) suggested using the standard deviation of the log normal distribution, and Simon and Bonini (1958) who derived a Yule distribution for firm size recommended the percentage of growth accounted for by new firms. For criticisms and defenses of these sorts of measures, see Adelman (1951, 1959), and Hart (1961).

In summary, there are a substantial number of ways that have been suggested to measure concentration (the preceding list is not a complete one). A few have some theoretical support, but most are simply empirically based. In addition to several quite different kinds of measures even within a given class of measures, there does not appear to be any consensus as to what form of the measure should be used, for example, entropy, relative entropy, or numbers equivalent (antilog of entropy). Studies comparing various measures do not seem to have produced a clear winner by any criteria. Fortunately, many of the measures tend to be highly correlated with each other so possibly the exact choice does not really matter a great deal. See for example, Kilpatrick (1967) or Bailey and Boyle (1971). Nevertheless, the various measures are far from identical so that the quantitative results obtained from a statistical analysis will depend to some degree upon the measure chosen.

In general it does not seem possible to make any very useful statement about the effects on estimated regression coefficients of the use of an "incorrect" index of concentration. For example, consider the following model:

$$y_i = \alpha + \beta x_i + \gamma z_i + u_i$$

where x_i and z_i are exogenous variables and u_i is the random error term. Suppose that data are available on y_i and x_i but z_i is unobserved. What is observed, however, is p_i -- a proxy for z_i. For instance, z_i might be the output of a research department, firm size, or market power and p_i could be the number of patents applied for or new products introduced; the total of sales, assets, or employment; or some measure of concentration. Clearly, using p instead of z will lead to estimates that do not have the usual least squares properties (unbiasedness, consistency, and so on).

McCallum (1972) and Wickens (1972) have considered the following question: when is the estimate of β obtained from a regression including p closer to β (in the sense of plim $|\hat{\beta} - \beta|$) than the estimate obtained using only data on y and x. What they showed is that if

$$p_i = z_i + v_i ,$$

where v_i is uncorrelated with the other variables in the model, then use of the proxy variable will always give a "better" estimate of β.

Since the result does not depend upon the relative variances of z and v, even a very poor proxy, that is, one with a low correlation with z, is better than none at all. To the extent that the discrepancy between p and z is due to sampling error, this result should be applicable. In the applications considered here, however, the differences between the desired variable and the various variables used to represent them can hardly be ascribed to sampling error. What happens, presumably, is that one picks a variable which is, hopefully, highly correlated with the correct one and introduces it into the regression equation in order to control for variations in the unobserved variable. Needless to say, this procedure could lead to either improved or worse estimates of β.

In the measurement error case, the correlations between the proxy variable and the other variables in the model are smaller in absolute value but have the same sign as the correlations between the correct variable and the other variables. Assuming without loss of generality that p and z are positively correlated, then if this same condition is satisfied, and the correlation between z and the proxy p is moderately large, that is, $\text{corr}(p,z) > \text{corr}(x,p) \cdot \text{corr}(x,z)$, where $\text{corr}(x,z) \neq 0$, then the estimate obtained using the proxy will be "closer" to β than that obtained without it. Thus, provided the proxy is sufficiently highly correlated with the true variable, behaves at least qualitatively like it, and does not make the multicollinearity problem worse, using the proxy will improve the estimate of β. While these kinds of assumptions may not seem unreasonable, they need not hold, as for instance, the relation, "is positively correlated with," is not generally transitive. Of course, even if these conditions are satisfied, the resulting estimates will not have the usual desirable properties. In summary, the substantial difficulties of measurement combined with the simultaneous equations problems mean that many results obtained by least squares regression methods should be interpreted with caution.

IV. MARKET STRUCTURE AND R&D

The empirical papers discussed in this section deal with the relationships between various features of firms, market structure (concentration), and innovative activity. All these studies could have important consequences for policy (in particular, antitrust policy) though the papers do not necessarily directly discuss these policies. The first group of papers covered contains information of the role of product diversification in research and thus is primarily of interest with respect to the question of conglomerate bigness. The second group of results to be surveyed deals with the relation between firm size and the level of research activity. Considerable work has been devoted to determining if research intensity (R&D activity per unit of size) is an increasing function of firm size. Finally, the relation between concentration and research activity measured at the industry level is examined. Thus, it is hoped that these studies should provide some insight into the following kinds of questions: Who innovates? That is, what are the characteristics of the most innovative firms? To what extent are there economies of scale to R&D? Is market structure analysis a useful tool in studying innovative activity? Does it appear, for example, that certain types of market structures are relatively more conducive to the creation or adoption of new technologies?

Diversification

The relationship to be considered here is that between research activity and diversity. In his survey Weiss (1969, pp. 393-94) said, "I judge that the issue is unsettled and a good candidate for more work, especially in view of its potential relevance to the conglomerate merger debate." There are three studies bearing on this issue: Grabowski (1968, diversification has a positive effect), Scherer (1965c, no systematic effect), and Comanor (1965, a negative effect). Weiss concluded that he "can reconcile Scherer with either Grabowski or Comanor but not with both."

Grabowski analyzed data on firms in the chemical, drug, and petroleum industry for the period 1959-1962. The basic model he estimated is the following:

$$\frac{R_{it}}{S_{it}} = a + bP_i + c\frac{I_{i,t-1}}{S_{it}} + dD_i + e_{it}$$

where

R_{it} = R&D expenditures in year t.

S_{it} = sales in year t.

P_i = number of patents received per scientist and engineer employed during the previous four years.

$I_{i,t-1}$ = after tax profits plus depreciation and depletion in year t - 1.

D_i = the number of five-digit SIC products produced.

The equation was estimated by least squares separately for each industry. The specification is somewhat similar to Hamberg's (1966) in that the role of internal finance is explicitly allowed for. Note the direction of causation assumed here -- research inputs depending upon the results of previous research and development activity. In all three regressions the estimates of d were positive and in two cases, chemicals (t-ratio 4.8) and drugs (t = 5.8) highly significant.

Scherer (1965a,b,c) used the same sort of index of diversity. The 447 SIC four-digit classification was reduced to 200 "technologically meaningful" industries, and diversification was measured by counting the number of these in which a firm operated. Scherer worked with data on 448 firms from the Fortune 500, and reported that the diversification variable was significant in regressions of R&D employment on sales, patents on R&D employment, and patents per billion dollars of sales on sales. However, when the sample was divided into two-digit SIC industries, "diversification played an uneven role." Generally its coefficient was significant and positive in regressions explaining patents or R&D employment only for the less technologically progressive industries. For the more research oriented industries the coefficients were generally insignificant (even negative in regressions explaining patents). Thus diversification does not appear to have a strong systematic effect on either R&D inputs or patents. Scherer explained these results as being at least to some extent an artifact of the measure of diversity chosen. Consider a firm operating primarily in a low research industry. The higher the measure of diversity the greater the chance that the firm will have a division in a more research oriented industry.

Comanor (1965) had data on 57 drug firms (accounting for 80 percent of industry sales) and fit a rather elaborate model:

$$\frac{y_i}{S_i} = a + b\frac{RD_i}{S_i} + c\frac{(RD_i)^2}{S_i} + dS_i + eRD_i \cdot S_i + fD_i + e_i$$

where

y_i = sales during the first two years of all new chemical entities introduced during the period 1955-1960.

S_i = average sales over the period 1955-1960.

RD_i = number of professional persons employed in research (average of 1955 and 1960 figures).

D_i = index of diversity defined as follows:

Let D_{1t} = number of markets in which a firm operates that account for at least 2 percent of its sales in year t.

Let D_{2t} = proportion of sales outside of its "primary" market.

Then $$D \equiv 1/6 \sum_{t=1955}^{1960} D_{1t}D_{2t}.$$

For the purpose of constructing these indices, forty submarkets were identified.

Comanor also had data on the number of nonprofessional research and development workers and the model was reestimated using total R&D employment. Further, both specifications were rerun with y redefined as sales from all new products as opposed to just new chemical entities. In all four regressions, the coefficient of diversity was negative and significantly so in three of them.

Notice that the direction of causation assumed by Comanor is the opposite of that assumed by Grabowski. Even if Comanor's specification is the correct one, there still remains a simultaneous equations problem here due to the dating of the variables in the regression. In particular, consider the index of diversity. It is quite possible that a successful research program could lead to a reduction in diversity as defined by Comanor, and the greater the success the greater the drop in diversity. This objection would not hold if diversity had been measured as of the beginning of the period rather than averaged over it.

Comanor also considered the possibility that the direction of causation may be opposite from what he had assumed. He argued that it could be that the successful introduction of new products generates funds used internally to expand research staffs, and thereby to diversify. He noted that if this latter specification is correct, then the timing can be reversed from that posited in his model. He did not formulate and estimate the alternative model, but instead he estimated his equations using alternative lag schemes. He found that regression equations of new product sales on future R&D inputs are inferior to equations using past R&D employment. In these latter regressions new product sales (1958-1960) are explained by diversity based on 1955-1957, and as before the coefficients are negative and significant. Thus for these regressions the objection about the dating does not hold. Note, however, that for some reason the same measure of firm size (average sales over the whole period) is also used in these regressions.

Size

There have been a number of studies which bear on the relationships between firm size and research activity. Generally, in these studies the level of R&D is measured by inputs, either employment or expenditures. Comanor's work mentioned above is an exception in that he did have an indicator of output. Also, both Schmookler (1954a,b) and Scherer (1965a,b,c) have worked with patent statistics. While one tends to think of patents as an output of the research process, Comanor and Scherer (1969) concluded that at least for the pharmaceutical industry, patents may be a better measure of input. They reported the results of correlating series on patent applications and patents issued with both of the dependent variables used in Comanor's study as well as with both Comanor's measures of R&D inputs (professional and total employment in research). Simple correlations were positive and statistically significant as were the partial correlations computed controlling for firm size. Generally the correlations were higher between patents and employment than between patents and new product sales.

Each of the three papers discussed above with respect to diversity contains statistical evidence on the firm size-research question. Grabowski (1968) reports the results of regressing R&D expenditures on sales and sales squared for his samples of firms in the drug and chemical industries. In both cases the coefficients of the linear and quadratic terms are significant with the coefficients of sales being positive. The quadratic term is positive for the chemical industry and negative for drugs. Thus research intensity (R&D expenditures per dollar of sales) increases with firm size in the chemical industry and rises at first and then falls as sales are increased for firms in the drug industry. Grabowski qualified his results noting that the measure of size is total sales rather than just sales in the product line associated with the research effort. This raises essentially the same point that Scherer made about the interpretation of his measure of diversity. Grabowski specifically pointed out that large drug firms do diversify into other areas. Indeed the correlations between sales and the diversity index are positive for both industries (0.8 for chemicals and 0.2 for drugs).

Scherer (1965c) estimated various relations between patents issued in 1959 and sales and employment in R&D in 1955 for his sample of 448 large manufacturing firms. For the entire sample he found a significant positive effect of firm size (sales) on patents. There appeared to be significant differences in the propensity to patent between industries (splitting the sample into 14 two- and three-digit industries roughly doubled the explanatory power). Though the estimated magnitudes of the coefficients and their significance levels varied a good deal across industries, all the results with the exception of textiles indicated a positive association between sales and patents.

Scherer also estimated regressions of patents issued on sales, sales squared, and sales cubed (the coefficient estimates were positive, negative, and positive, respectively). He found diminishing returns to sales up $5.5 billion (which included all but three firms in his sample). The sample was divided into four subsamples (electrical, chemical, moderately progressive, and unprogressive) and the same regression run for each group with substantially similar results. In order to give less weight to the giant firms in the sample, cubic equations using the logarithms of sales were estimated. Diminishing returns were again apparent for the chemical and electrical groups, but less so for the others. Each industry grouping was further subdivided into size classes and separate regressions estimated for each. The results indicated increasing returns up to sales of $500 million for chemicals and electricals, slight decreasing returns generally for the moderately progressive group, and no discernible pattern for the unprogressives.

Comanor (1965) also included a measure of firm size in his regressions, though given the complexity of the estimated equation, its interpretation is difficult without access to the data. The coefficient of size is given by

$$\underset{(+)}{a} + \underset{(+)}{dS_i} + \underset{(-)}{eRD} \cdot S + \underset{(-)}{fD_i},$$

(The signs of the estimated coefficients are shown in parentheses.)

and the coefficient of R&D input by

$$\underset{(-)}{b} + \underset{(+)}{cRD} + \underset{(-)}{eS}.$$

Comanor estimated the elasticity of new product sales with respect to R&D input for firms with sales of 1, 10, and 50 million dollars, the elasticity estimates being 1.4, 0.6, and 0.5, respectively.

Worley (1961) and Hamberg (1964, and 1966 Chapter 4) report in rather similar studies on the relation between firm size and R&D. Worley regressed the logarithm of the number of R&D personnel on the logarithm of firm size measured by total employment for a sample of 198 large firms in eight, two-digit industries. The data referred to 1955. In six out of eight cases (chemicals and allied products, petroleum and petroleum products, stone, clay, and glass products, nonelectrical machinery, electrical machinery, and transportation equipment) the estimated coefficient exceeded unity, being less than one only for food and kindred products and primary metals. The average of the estimates was slightly greater than one (1.08). The estimate was significantly greater than one at the 5 percent level only for petroleum and at the 10 percent level for electrical machinery, and none of the remaining estimates was significantly larger than unity at conventional levels. Of the two point estimates which were less than one, neither was significantly so. Worley came out against the hypothesis that size is a stimulus to research though he was appropriately cautious: "One cannot assert that the evidence offered here . . . nullifies the case for bigness" (Worley 1961, p. 186).

Hamberg computed correlation coefficients and rank correlations between R&D employment and firm size measured alternatively by total employment and book value of assets for seventeen manufacturing industries in 1960. Of the sixty-eight estimates, all but one were positive and nearly all the rest significantly so at a 5 percent level. To test the hypothesis that an increase in firm size is associated with a more than proportionate increase in R&D, Hamberg computed rank correlations between R&D intensity (percentage of employees in R&D) and both measures of firm size. The results of this experiment were more mixed (twenty-three of thirty-four estimates were positive but most not significantly so). Hamberg also ran double logarithmic regressions between R&D intensity and firm size. Using employment as size, twelve of the seventeen coefficients were greater than one with three passing at the .05 level (one estimate is significantly less than one). When firm size is measured by total assets, only ten are larger than one, though four others are larger than 0.98. In this case, while one coefficient (primary metals) is again less than one (.05) only two coefficients appear to be significantly greater. Hamberg concludes that "the case for bigness and fewness as a stimulus to industry appears, on the basis of fairly extensive evidence, to be quite weak" (1966, p. 68).

Scherer (1965b) commented on the work of Hamberg and Worley. He noted that if their elasticity estimates were mutually independent, then the evidence they presented, if anything, would support the case for corporate bigness. He also pointed out that many firms (especially small ones) do no R&D at all, but that Hamberg's sample did not include any such firms. Further, since both Hamberg and Worley used the

logarithm of R&D input as the dependent variable in their regression equations, how to handle the zero observations is no small problem. He went on to demonstrate empirically that the estimates can be made greater or less than one by adopting various conventions for handling the zeros. For his sample (the same as used in Scherer 1965a) ignoring the zero observations gave estimates less than one, while replacing the zeros with a small positive number gave estimates greater than one. Regardless of the conventions adopted for handling the zeros, the measure of firm size used also affected the results. Measuring firm size by employment gave estimates that were the most favorable to bigness, while estimates based upon sales as the definition of firm size were less favorable, and those using total assets were the least favorable. Thus, Scherer showed that the statistical results obtained by Hamberg and Worley were quite sensitive to both the definitions of firm size and the treatment of firms which did not engage in R&D.

For his sample Scherer estimated regressions with R&D employment explained by cubic equations in sales and the logarithm of sales The results obtained were similar to those he obtained for patents. With the exception of the chemical industry and a small number of giant firms, his estimates indicated that R&D intensity was a decreasing function of firm size for firms with sales in excess of $500 million.

Hamberg (1966, Chapter 6) studied the effects of various characteristics of firms on the number of R&D personnel employed. His sample consisted of 405 large firms for the year 1960.

Hamberg argued that the size of the R&D effort should be related to the total sales of the firm. He felt that R&D should be positively related to sales, but that a negative relationship was possible as "a shrinkage of sales, or perhaps a retardation in their growth, could be a signal to increase R&D spending" (p. 117). Profits were also considered as a possibly important variable, and as with sales it was argued that the sign of the regression coefficient could plausibly be either positive or negative. To the extent that internal finance is important for R&D, Hamberg felt that depreciation expenses could be significant. He felt that the sign of this variable should be positive, but that a negative coefficient would not be unacceptable. The level of government R&D contracts held by the firm was also included as an explanatory variable. Hamberg argued that in general one would expect the sign of the regression coefficient for this variable to be positive. The coefficient could be negative he argued, as a shortage of scientific personnel could lead a firm to reduce its own R&D effort as a result of increased government contracts. The level of gross investment in plant and equipment was included in the equation with the expectation of a negative sign (R&D expenditures compete with other investment projects for funds). Hamberg qualified this argument, however, noting that for tax purposes R&D expenditures could be treated as current expenses and might not be determined by the same budgetary processes as fixed investments. One other variable was included in the equation, namely the level of R&D employment lagged one year. Hamberg expected the sign of its coefficient to be positive.

According to econometric folklore, if one can argue for both positive and negative signs for a single regression coefficient, then one should worry about a possible simultaneous equations problem.

Hamberg's study seems to be a case in point. Consider, for example, the case of profits. If profits provide a source of funds for R&D expenditures, then this suggests a positive coefficient and also is consistent with the direction of causation implicit in Hamberg's model. If falling profits are a signal to engage in more R&D, however, thus giving Hamberg's argument for a negative coefficient, then this position is consistent with profits being at least in part determined by R&D, though possibly with some time lag. In short, it seems that Hamberg is really arguing that a simultaneous equation model is appropriate, thereby casting considerable doubt on his results which were obtained by ordinary least squares.

Hamberg was concerned with heteroscedasticity and attempted to correct for this by deflating by firm size. All variables which are measured in money units were deflated by total assets lagged one year. R&D employment, however, was deflated by total employment unlagged. Hamberg remarked that using the same deflator for all variables would have been desirable, but that his procedure was more meaningful because of the different units of measurement. Note that the method of deflation used does affect the interpretation of the estimated regression coefficients.

The estimating equations were of the form (the firm subscripts have been omitted):

$$\frac{R_t}{E_t} = a + b\frac{P_t}{A_{t-1}} + c\frac{D_t}{A_{t-1}} + d\frac{G_t}{A_{t-1}} + e\frac{I_t}{A_{t-1}} + f\frac{R_{t-1}}{E_{t-1}} + u_t$$

where

R_t = employment in R&D

E_t = total employment.

P_t = after-tax profits.

D_t = depreciation expenses.

G_t = government research contracts.

I_t = gross investment.

A_t = total assets.

t = time.

Hamberg also estimated a number of variations on this model, varying the lag structure, using sales instead of profits, and deleting the government contracts variables.

Note that the use of different deflating variables makes the equation for R&D employment be the following:

$$R_t = aE_t + b\frac{E_t}{A_{t-1}}P_t + c\frac{E_t}{A_{t-1}}D_t + d\frac{E_t}{A_{t-1}}G_t + e\frac{E_t}{A_{t-1}}I_t + f\frac{E_t}{E_{t-1}}R_{t-1} .$$

If the ratios E_t/A_{t-1} and E_t/E_{t-1} are not constant across the firms in the sample, then the regression coefficients are made somewhat difficult to interpret. Even if these ratios are constant, the relationship between current and past levels of R&D needs to be reinterpreted and the constant terms in the regressions (which were not reported) become quite important. Suppose, for example, that E_t/E_{t-1} is equal to λ for each firm in the sample. Then

$$R_t = a\lambda E_{t-1} + \ldots + f\lambda R_{t-1} + u_t = \lambda(a + f)R_{t-1} + \ldots + a\lambda\theta_{t-1} + u_t$$

where $E_t = R_t + \theta_t$. Thus the coefficient should be the sum of the unreported constant term plus the estimated coefficient. For all these reasons Hamberg's results are difficult to interpret.

Hamberg estimated regressions using 1960 data for samples of firms in eight different industries. The results were mixed. Three variables generally entered with negative coefficients: profits (four of five), depreciation (six of eight), and gross investment (five of eight) with five coefficients being significant at .05 or higher levels of significance. The estimated coefficients for sales, government contracts, and lagged R&D personnel were generally positive with thirteen of the nineteen estimates being significant. The model was estimated without the government contract variable for twenty-one industries with similar results. Hamberg concluded that liquidity had little influence on R&D, and if gross investment has any effect, it is negative (of the thirteen significant coefficients, twelve are negative).

Mansfield (1963b; 1968, Chapter 2) estimated logarithmic regressions between R&D expenditures and firm size (sales) for samples of firms in the chemical, petroleum, drug, steel, and glass industries. The equation estimated was:

$$\ln R_{ti} = \text{year dummies} + (a + bt)\ln S_{ti} + e_{ti}).$$

In all cases the estimate of a was positive and significant. With the exception of the steel industry, all the estimates were significantly different from one, the estimate being greater than one for chemicals and less than one in the other industries. The estimates of b were not significantly different from zero, indicating that the elasticity of R&D expenditures with respect to firm size was constant over the period (1945-1959).

Loeb and Lin (1977) reestimated the equations suggested by Scherer (1965b,c) Grabowski (1968) and Hamberg (1964). They used data on six unnamed but "representative" pharmaceutical firms. Their product is differentiated methodologically by their use of specification error tests developed by Ramsey (1969, 1970, 1974). Their general conclusion was that research intensity tends to diminish as size increases for that industry. Howe and McFetridge (1976) studied a sample of eighty-one Canadian firms in the electrical, chemical, and machinery industries over the period 1969-1971. The dependent variable in their study was expenditures on R&D, and the main independent variables were firm size (sales), profits, depreciation, the Herfindahl index of the three-digit industry, and government incentive grants. The equations were tested for homogeneity over time and heteroscedasticity was allowed for. Quite sensibly it was assumed that the scale of the disturbance variances varied with firm size. The effect of firm size was not generally significant, the estimated coefficients being a bit erratic. Also the Herfindahl index was statistically insignificant. In the chemical and machinery industries the coefficient of government grants was not significantly different from zero. Thus for these industries, one could not reject the hypothesis that the receipt of government incentive grants simply replaced R&D expenditures that the firms would have made. It should be pointed out that a

condition for receiving the grants is that matching private funds are spent on the R&D projects being subsidized. Nevertheless, only for the electrical industry is there evidence that the grants actually caused increased private R&D expenditures.

Shrieves (1978) studied a sample of 411 firms. The criteria for being chosen was that they could be assigned to a three-digit industry and were included in COMPUSTAT and the 1965 edition of <u>Industrial Research Laboratories of the United States</u>. The dependent variable was R&D employment and the main independent variables were the percentage of 1965 R&D financed by the federal government, firm size (again sales), and the four-firm concentration ratio. The coefficient of the governmental subsidy variable was negative and significant which is consistent with the Canadian results discussed in the preceding paragraph. The results on size and concentration tend to indicate that, <u>ceteris paribus</u>, small firms in concentrated industries do more R&D than others. The results are flawed by the inclusion of a number of more or less uninterpretable variables based on applying factor analysis to sets of characteristics of the product market and the technology. Factor analysis was used to reduce the dimensions of the explanatory variables. In this sense it was successful, but not surprisingly the results are hard to interpret. It is unclear why this is a preferable state to "confounding," that is, multicollinearity among known variables.

Shrieves' findings on size and concentration were also obtained by Rosenberg (1976) using a sample of 100 of the Fortune 500. A firm was entered in the sample only if its primary area of production was classified as high or low technology. For this study R&D was measured by the percentage of employment and firm size by market share.

Fisher and Temin (1973) have argued that most studies of the relation between firm size and the level of R&D activity are inappropriate. They argued that these studies are attempting to test the so-called Schumpeterian hypothesis (Schumpeter 1942) that "there are increasing returns in R&D both to the size of the R&D establishment and to firm size," as follows:

Let R = number of workers in R&D.

S = total number of workers in a firm.

$N = S - R$.

$F(R,N)$ = the average labor productivity of R&D.

<u>Hypothesis</u>:

$$\frac{\partial F}{\partial R} > 0$$

$$\frac{\partial F}{\partial N} > 0.$$

The empirical literature has mainly been concerned with whether

$$\eta = \frac{S}{R}\frac{dR}{dS} > 1.$$

Yet the question of the impact of antitrust policy really involves the relation between the <u>output</u> of R&D and firm size. That is, whether

$$\varepsilon = \frac{S}{R \cdot F}\frac{d(R \cdot F)}{dS} = \eta + \frac{S}{F}\frac{dF}{dS} > 1.$$

Fisher and Temin argue that η being greater than one is neither necessary nor sufficient for ε greater than 1 or for the truth of Schumpeter's hypothesis. Thus they conclude that the empirical work is "of very little interest."

The general point made by Fisher and Temin seems sensible, namely, that many of the interesting questions involve the behavior or the output of the innovative process while, in general, all that is measured are the inputs. Further, though the studying of the relationship between firm size and R&D effort may be interesting, it does not bear directly on the question of increasing returns, either to firm size or research effort.

In summary, the evidence suggests that firm size is positively related to R&D effort. Research intensity in general does not seem to be an increasing function of firm size at least when attention is restricted to the largest firms. The major exception seems to be the chemical industry; in most others there is evidence that research intensity decreases with firm size. As Scherer noted, many small firms do no R&D and thus studies that deal only with large firms may bias the results against bigness. In particular, if there is some threshold size required for a firm to have a research program, these studies could not detect it. Weiss has stated that this latter problem is most likely of little concern from the policy point of view as antitrust policy generally involves only the largest firms in an industry.

Concentration

Hamberg (1966, Chapter 4) correlated total industry, company-financed R&D expenditures with an "average concentration indicator" using 1958 data on two-digit SIC industries. The measure of concentration used was a weighted average of the concentration ratios for the relevant five-digit industries. The correlation was positive (0.50) and significant. A similar result was obtained when industry R&D intensity was correlated with concentration (r = 0.54). Hamberg also calculated rank correlations of 0.46 (significant) and 0.36 (insignificant), respectively. Hamberg stated that he considers the rank correlations more accurate. He concluded that there is "too much variance in industry R&D spending left unexplained by industrial concentration to attach much importance to the latter variable as a determinant of R&D" (1966, p. 64).

Horowitz (1962) computed rank correlations between several measures of research and four-firm concentration ratios, average employment, and average value added for two sets of data relating to two- and three-digit industries. He used the Niehans index in calculating average firm sizes. The measures of research used were: the percentage of firms that responded to the survey and had research organizations, research intensity, the percent of research laboratories in the largest 20 percent of the firms, and the percent of firms with R&D expenditures but without their own research laboratories. Concentration was correlated significantly with all four variables, the signs being plus, plus, minus, minus, respectively. Correlations calculated using the firm size variables were quite similar. Horowitz

noted that firms in highly concentrated industries are more likely to engage in research and tend to spend a higher percentage of their sales than firms in less concentrated industries. He concluded that the evidence is consistent with the hypothesis that bigness is conducive to research effort. But he also stated that "it is not clear, however, whether the large firm in the concentrated industry has created the research laboratory, or whether the research laboratory has brought about the emergence of the large firm and increased concentration" (p. 300).

Scherer (1967) analyzed data on the number of scientists and engineers employed in fifty-six industries in 1960. He regressed the logarithm of the number of technical personnel on the logarithms of concentration (CR_4) and total employment in the industry. The concentration variable entered with a positive sign and was "highly significant." The coefficient was still positive and significant (nearly four times its standard error) when dummies for certain industry groups (electrical, chemical, traditional, regional, durable goods, and consumer goods) were added. When the ratio of technical personnel to total employment was regressed on concentration, the results were similar though somewhat less favorable to the Schumpeterian hypothesis. In this linear form of the model, however, the addition of the industry dummies resulted in substantial drop in the significance of concentration. While in all cases the results suggested that increasing concentration was associated with larger research effort (controlling for industry size), whether or not the result was statistically significant depended on the choice of functional form.

Comanor (1967) studied the relationship between R&D personnel (both total and professional) and concentration, barriers to entry, and firm size. He gives the results of log-log regressions of R&D input on firm size (employment) for twenty-one industries, and in the majority of cases the coefficients were less than one. These estimates were then regressed against average firm size and CR_8 with the results that both variables were positively related to the estimates, but only firm size was significant. These regressions taken together suggest that Comanor was working with the following random coefficient model:

$$\ln R_{ij} = a_j + b_j \ln S_{ij} + \varepsilon_{ij}$$

$$b_j = c + dC_j + e\overline{S}_j + u_j,$$

where

R_{ij} = R&D inputs for the ith firm in the jth industry.

S_{ij} = size of the ith firm in the jth industry.

C_j = eight-firm concentration ratio for the jth industry.

$\overline{S}_j$ = average firm size for industry j.

Combining these relations gives

$$\ln R_{ij} = a_j + c \ln S_{ij} + dC_j \ln S_{ij} + e\overline{S}_j \ln S_{ij} + u_j \ln S_{ij} + \varepsilon_{ij}.$$

Comanor's two-step procedure while inefficient does yield unbiased estimates of the parameters a_j, c, d, and e. However, there

is a heteroscedasticity problem at the second step so that statements as to which variables are statistically significant are suspect.

Comanor also estimated the following regression:

$$R = a + b \cdot C + cD + dC \cdot D + eE_1 + fE_2 \text{ and } u,$$

where

R = average number of research personnel (adjusted for firm size).

C = 1 if CR_8 exceeds 70.

D = 1 for consumer nondurables and material inputs.

E_1 = 1 if technical entry barriers are "moderate."

E_2 = 1 if technical entry barriers are "high."

Comanor found that research levels tended to be higher in the industries classified as having moderate entry barriers and, *ceteris paribus,* in consumer durables and investment goods. He interpreted this as the effects of higher degrees of product differentiation. It appeared that research staffs were larger in the highly concentrated industries though the effects were small for consumer durables and investment goods. The dependent variable for this regression was obtained from the double logarithmic regressions mentioned above, evaluated at "typical" firm sizes.

Williamson (1965) used data from Mansfield (1963b; 1968, Chapter 5) to examine the relationship between the proportion of important innovations introduced by the four largest firms and concentration in the steel, petroleum, and coal industries. He fit logarithmic and linear regressions between the P/C and C where P is the percent of innovations introduced by the four largest firms, and C is the percent of industry capacity held by the largest four firms. The results indicated that the largest four firms would contribute more than proportionately to innovation only at relatively low levels of concentration. Williamson split the data into two subperiods (1919-1938 and 1939-1958) to test the stability of the regression equations and concluded that the relations had not shifted over time. The entire sample consisted of six observations (thus the last mentioned regressions had one degree of freedom each), and Williamson was appropriately cautious in his interpretation.

Leonard (1971) argued the case that R&D expenditures influence the subsequent growth of an industry. He reported correlations between R&D intensity (1957-1959) and subsequent rates of growth of sales for sixteen industry groups and found high positive correlations between R&D and growth during the following five years. He also found quite low correlations between company financed R&D as a proportion of sales and previous sales growth. Using the proportion of company financed research personnel in total employment as a measure gave similar results. Leonard presented a good deal of other evidence to support his hypothesis, but collinearity generally made it difficult to distinguish between the variables. For example, R&D intensity was highly correlated with growth of capital stock.

Tilton (1973) claimed that the causation went the other way. To support his claim, Tilton correlated earlier sales growth with Leonard's measures of R&D input using total inputs rather than just

company financed R&D. In reply Leonard claimed that Tilton's data were suspect, and redid the calculations using data that he considered to be better. The results are correlations intermediate between Tilton's and Leonard's original calculations (though significant at the .05 level).

Wilson (1977) studied a sample of 350 manufacturing firms which reported both royalty payments and R&D spending in 1971. Wilson finds both R&D intensity and royalties as a fraction of sales decrease with concentration (four-firm concentration ratio) and increase with profits as a fraction of sales. Wilson's results are suspect, however, as included among the explanatory variables is a possibly ordinal variable constructed by the author to measure the "'multidimensionality' of the industries' product." For a discussion of the difficulties that use of non interval-level data in simple regressions present, see Grether (1974, 1976).

Overall it appears that concentration is positively related to R&D, though the evidence on this is not overwhelming. Further, there does not seem to be general agreement as to the proper interpretation of any such relationship. In other words, even if one believes that the association between concentration and research intensity is established, one cannot conclude that concentration caused the increase in R&D. Minasian (1962) concluded that R&D expenditures explain productivity increases as well as the level and rate of growth of profitability. He considered the alternative hypothesis that profitability determines R&D, but for his data (a sample of nineteen chemical firms) he rejected this hypothesis. Also, Comanor (1965), Hamberg (1966), and

Leonard (1973) assume that R&D influences growth of firms and industries while Grabowski (1968), Hamberg (1966), and Tilton (1973) assume that growth and profitability at least partially determine R&D. In fact, both Tilton and Leonard seem to agree that there could be mutual interdependence.

In conclusion it seems that there are good reasons for arguing that simultaneous equations models are more appropriate than the single equation approach that has generally been used. It should be apparent from the preceding discussion, however, that research workers are well aware of the problems of interdependence. In other words, the problem is not simply one of choosing the correct statistical procedure. One needs both more fully developed theoretical models (Montgomery and Quirk 1974) and better data (Grabowski and Mueller 1970). One hopes that much future research in this area will be based upon explicit models of firm behavior. At the least this should allow for a clear distinction between endogenous and exogenous or predetermined variables. Thus hopefully one can avoid studies of the "effects" of diversification, debt-equity structures, or other endogenous variables on the "productivity" of R&D expenditures.

It should be noted, however, that some of the best and most interesting work in this area has been largely descriptive, for example, Scherer's work on patent statistics, and there is still room for a good deal of exploratory empirical work. We may not need more studies of, say, R&D expenditures of two-digit industry groups, but relatively little is known about the breakdown of these efforts between development and various types of research activities at any level of aggregation.

Also, even at the firm level, aggregation problems may obscure the interpretations of seemingly straightforward relations. For instance, as Scherer and Grabowski have pointed out, the tendency for large firms to be diversified makes it difficult to interpret the correlation (or lack of it) between firm size and R&D. In addition, if hypotheses concerning more detailed aspects of market structure or organizational structure of firms are to be examined, studies of specific firms, industries or innovations could be important: for instance, comparisons of two industries with similar market structures (reputations for technical progress), but different records of productivity gains (market structure).

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