

DO MARKETS DIFFER MUCH?

by

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ABSTRACT

The contributions of firm, industry, and market share differences to cross-section variability in business unit profitability are estimated through a descriptive analysis of FTC Line-of-Business data for 1975. Firm differences never approach statistical significance. Industry differences are significant and account for over 75% of the observed variance in industry average rates of return. Market share effects are statistically significant but account for less than 1% of the variance in business unit rates of return. Industry effects are estimated to be negatively correlated with market share in these data. Substantive and methodological implications are discussed.

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This essay reports the results of a cross-section study of differences in accounting profitability that sheds light on some basic controversies in industrial economics. Most previous cross-section studies in this field have been concerned with testing hypotheses about structural coefficients in models meant to apply to essentially all markets. As we have learned more about the difficulties of constructing such general models and of performing tests on their structural parameters properly, structural cross-section analysis has fallen out of fashion. In contrast to most of the cross-section literature, the analysis reported here is fundamentally descriptive; it does not attempt directly to estimate or to test hypotheses about structural parameters.

I hope to show by example that one can perform illuminating analysis of cross-section data without a host of controversial maintained hypotheses. Cross-section data can yield interesting stylized facts to guide both general theorizing and empirical analysis of specific industries, even if they cannot easily support full-blown structural estimation.¹ One can view the sort of search for stylized facts conducted here as either a replacement for or an input to inter-industry structural estimation, depending on one's feeling about the long-run potential of that research approach. This study also departs from much of the cross-section literature by being fundamentally concerned with the importance of various effects, not just with coefficient signs and t-statistics.

In particular, this essay provides estimates of the relative importance of firm, market, and market share differences in the determination of business unit (divisional) profitability in U.S. manufacturing. Using 1975 data from the Line of Business program of the U.S. Federal Trade Commission (FTC), we find support neither for the existence of firm effects nor for the importance of market share effects. Moreover, while industry effects apparently exist

and are important, they appear to be negatively correlated with seller concentration in these data.

Section I relates firm, market, and share effects to current issues and controversies in industrial economics and thus supplies the motivation for our empirical analysis. The remainder of the essay treats the data and statistical methods employed (Section II), the empirical results obtained (Section III), and the main implications of those results (Section IV).

I. Sources of Profitability Differences

In the classical tradition, following Joe Bain (1951, 1956), industrial economists treated the industry or market as the unit of study. Differences among firms were assumed transitory or unimportant unless based on scale economies, which were generally found to be insubstantial. Equilibrium industry profitability was generally assumed to be primarily determined by the ability of established firms to restrict rivalry among themselves and the protection afforded them by barriers to entry. A central hypothesis in virtually all the classical work was that increases in seller concentration tend to raise industry-wide profits by facilitating collusion. Most classical studies thus included concentration among the independent variables in regression analysis of industry average rates of return, and most published studies reported the coefficient of concentration to be positive and significant.²

An anti-classical, revisionist view of industrial economics has emerged in the last decade. In the simplest model consistent with this view, all markets are (at least approximately) competitive, and scale economies are absent (or negligible). The key assumption is that within at least some industries there are persistent efficiency differences among sellers.³ Because more efficient

enterprises tend both to grow at the expense of their rivals and to be more profitable, these differences tend to induce a positive intra-industry correlation between share and profitability even in the absence of scale economies. Moreover, the more important are efficiency differences in any industry, the less equal are market shares (and thus the higher is market concentration) and the higher are the profits of the leading firms (and thus the higher is industry average profitability). This model thus predicts a positive correlation between concentration and profitability in cross-section at the industry level even though, by assumption, concentration does not facilitate the exercise of market power.⁴

At the firm or (for multi-product firms) business unit level, the revisionist view implies that market share should appear as the primary determinant of profitability in cross section regressions, while market concentration should have no impact. David Ravenscraft (1983) checked these predictions with FTC Line of Business data.⁵ He found the impact of share on business unit profitability to be positive and highly significant, while the coefficient of concentration in the same regression was negative and significant. Ravenscraft interpreted his results as providing strong support for the revisionist argument that the significance of concentration in traditional industry-level cross-section regressions arises because concentration is correlated with share (and thus efficiency) differences, not because it facilitates collusion. Stephen Martin (1983) has recently obtained similar results in a simultaneous equations analysis of the FTC data. The strong relation between market share and profitability found by these and other authors is difficult to interpret within the classical tradition, given the apparent absence of important scale economies in most industries.⁶

A third tradition, which I will call managerial, has yet another set of implications for business unit profitability. Business schools and management

consultants exist because it is widely believed that some firms are better managed than others and that one can learn important management skills that are not industry-specific. In a widely-acclaimed best seller, Thomas Peters and Robert Waterman, Jr. (1982) stress the importance of firm-level efficiency differences based in large measure on differences in "organizational cultures." Dennis Mueller (1977, 1983) has recently reported econometric results implying the existence of substantial, long-lived differences in measured firm profitability. When profit rates in 1950 are taken into account, Mueller (1983) finds that concentration has a significant negative coefficient in an equation explaining projected firm profit rates in 1972, and industry effects in general are relatively unimportant.

Both the revisionist and managerial alternatives to the classical tradition are based on plausible arguments and suggestive evidence. But I do not think that it has been shown that the classical attention to the industry was in any sense a mistake: case studies of real markets clearly reveal important differences. Why, then, do conventional market-level variables perform poorly or perversely when firm or share effects are included in cross-section regressions?

One probable reason comes readily to mind. It has long been recognized that we have very imperfect measures of the classic dimensions of market structure and basic conditions. Conditions of entry have proven particularly difficult to measure in a satisfactory fashion. Moreover, the link between the real, economic profitability dealt with in theoretical discussions and the accounting returns used in empirical work is weakened by inflation (Geoffrey Whittington, 1983), depreciation policy (Thomas Stauffer, 1971; Franklin Fisher and John McGowan, 1983), risk (Schmalensee, 1981), and both cyclical (Leonard Weiss, 1974) and secular (Ralph Bradburd and Richard Caves, 1982) disequilibria.⁷

Conventional, classical industry-level variables may thus perform poorly at least in part because they are poor, incomplete measures of the (classical and other) market effects present in available data. Since many of the usual classical industry-level variables are endogenous in the long run, and it is difficult to formulate enough non-controversial exclusion restrictions to identify all parameters of interest, it is not clear that problems of measurement and disequilibrium can be successfully attacked by structural modeling using available cross-section data.

II. Methods and Data

Instead of attempting structural analysis, this study employs a simple analysis of variance framework that allows us to focus directly on the existence and importance of firm, market, and market share effects without having to deal simultaneously with specific hypotheses and measurement issues related to their determinants. Specifically, we deal in all that follows with the following basic descriptive model:

$$(1) \quad r_{ij} = \mu + \alpha_i + \beta_j + \gamma S_{ij} + \epsilon_{ij},$$

where r_{ij} is the (accounting) rate of return of firm j 's operations in industry i , S_{ij} is its market share, the α 's are industry effects, the β 's are firm effects, μ and γ are constants, and the ϵ 's are disturbances. The assumptions that market share enters linearly in (1) and that γ is the same for all industries are made mainly for comparability to the literature, though both also simplify computation and interpretation. The 1975 FTC Line-of-Business data set, which we use, contains information on large multi-divisional firms. Such information is clearly required to separate firm and industry effects in (1).

While none of the coefficients in (1) can be given a defensible structural interpretation, analysis of that model as a whole can shed light on the relative merits of at least the extreme versions of the classical, revisionist, and managerial positions. An extreme classicist, for instance, would expect the β 's to differ substantially with $\alpha_i = \gamma = 0$ for all i . Estimates consistent with these expectations would of course not exclude the possibility that industry effects simply reflect industry-wide differences between accounting and economic rates of return or industry-level disequilibria, with variations in monopoly power of little or no importance. But such estimates would cast doubt on extreme managerial or revisionist positions.

Similarly, an extreme revisionist would expect a large γ with all α 's and β 's near zero, while an extreme managerial position might be that variations in the α_i should be much more important than those in the β_j or in γS_{ij} . There is in fact no basic conflict between revisionist and managerial positions. Firm-level efficiency differences could affect business unit profitability through the revisionist mechanism, so that firm and share effects would be hard to distinguish, or in some way that allows firm differences to have a discernable impact on profits conditional on market share.

Using firm and industry dummy variables, we first use ordinary least squares (fixed effects estimation) and the usual F-statistics to test for the existence of market effects (non-identical α 's), firm effects (non-identical β 's), and share effects (non-zero γ) in (1) and the natural special cases thereof. To analyze the importance of these effects, we treat the actual α 's, β 's, S 's and ϵ 's in any particular sample as (unobservable) realizations of random variables with some joint population distribution. Under the usual assumption that ϵ is distributed independently of the other

variables, the population variance of r can be decomposed as follows:

$$(2) \quad \sigma^2(r) = \sigma^2(\alpha) + \sigma^2(\beta) + \gamma^2\sigma^2(S) + \sigma^2(\epsilon) \\ + 2\rho(\alpha, \beta)\sigma(\alpha)\sigma(\beta) + 2\gamma\rho(\alpha, S)\sigma(\alpha)\sigma(S) \\ + 2\gamma\rho(\beta, S)\sigma(\beta)\sigma(S),$$

where the ρ 's are correlation coefficients and the σ 's are standard deviations. Depending on which effects are revealed to exist by the analysis of (1), we estimate either (2) or a special case thereof to provide information on the importance of the determinants of observed profitability. Estimates of (2) relate directly to the predictions of the alternative traditions discussed above. The particular (random effects) estimation techniques used in this phase of the analysis are presented in Section III.

In most of the statistical literature concerned with variance decomposition, orthogonality of effects is assumed, so that covariance terms like the last three on the right of (2) are set to zero.⁸ But that assumption is not plausible here. If an important attribute of efficient firms is their ability to pick profitable industries in which to operate, for instance, we would expect this feature of the data generation process on which we must condition our estimates to produce a positive $\rho(\alpha, \beta)$. Similarly, one expects efficient firms to have low costs and high shares, so that $\rho(\alpha, S)$ should be positive. Finally, if one knows that some particular S_{ij} is above average, one's conditional expectation must be that concentration in market j is above average. If one expects industry concentration to be positively related to industry profitability, it then follows that one expects $\rho(\beta, S)$ to be positive. On the other hand, since ϵ captures all profitability differences unrelated to firm, industry, or market share differences, the assumption that it is orthogonal to those effects seems natural and reasonable.

The strength of this descriptive approach is that our conclusions about the three relevant types of effects will not be conditioned by maintained hypotheses regarding the determinants of those effects. We can focus directly on the general implications of extreme classical, revisionist, and managerial positions without having to deal with issues of endogeneity or identification. In addition, if one doubts a priori that any of these extreme positions is tenable, one can look to quantitative evidence on the importance of firm, market, and market share effects and the correlations among them to suggest tenable compromise positions as well as questions and strategies for future research.

One important issue of research strategy can be very easily addressed within this framework: is it defensible to work with industry-level data? Given the central role of profits in industrial economics, the answer must depend critically on how important industry effects are in determining industry rates of return. Only if industry profitability mainly reflects industry-level effects can one hope that hypotheses about the (classical, accounting, disequilibrium, and other) determinants of those effects can be productively tested with industry-level data. If R_j is the (appropriately weighted) average rate of return of business units operating in industry j , equation (1) implies

$$(3) \quad R_j = \mu + \beta_j + \{\text{terms in } \alpha \text{'s, } S \text{'s, and } \epsilon \text{'s}\}.$$

Industry-level analysis would seem to be sensible if and only if (estimates of) $\sigma^2(\beta)$ are large relative to the cross-section variance of the R_j , so that industry-level differences are important determinants of industry average rates of return.

All empirical results reported below are based on a subset of the 1975 data on individual business units gathered and compiled by the FTC's Line of

Business program; see Ravenscraft (1983) and the sources he cites for a discussion of these data. In order to minimize the influence of newly-born and nearly-dead operations, only the 3,816 business units present in the FTC data in both 1975 and 1976 were considered. Sixteen industries that appeared to be primarily residual classifications were excluded because they seemed unlikely to correspond even approximately to meaningful markets.⁹ This removed 340 observations. In order to mitigate scale-related heteroscedascity problems and to focus on the revisionist mechanism (as distinguished from scale economies), the 1,070 remaining observations with market shares of less than 1.0% were excluded. (Note that none of these involve small firms; all are small divisions of the large firms sampled by the FTC.) Finally, one outlier (with operating losses exceeding sales and assets/sales several times larger than other business units in its industry) was excluded before analysis began. Our final data set contained 1,775 observations on business units operated by 456 firms in 242 industries.

r_{ij} in equation (1) was measured as the ratio of operating income to total assets, expressed as a percentage. This quantity provided an estimate of the total pre-tax rate of return (profits plus interest) on total capital employed; it seemed superior on theoretical grounds to the frequently-employed price-cost margin as a measure of profitability.¹⁰ Its mean was 13.66, and its variance, $s^2(r)$, was 348.97. For each industry in the sample, we also computed the asset-weighted average rate of return, R_j . The mean and variance of these 242 numbers were 13.08 and 86.91 ($\equiv s^2(R)$), respectively. For S_{ij} we used estimates computed and kindly supplied by David Ravenscraft.¹¹ The mean percentage market share in this sample was 6.14, with a variance of 59.23 ($\equiv s^2(S)$).

III. Empirical Findings

Figure I summarizes the results of least squares estimation of equation (1) and restricted models excluding one or more of the three effects with which we are concerned. The values of the ordinary and adjusted R^2 statistics are shown,¹² along with the estimates of γ obtained from models with a share effect. Each arrow corresponds to the imposition of a restriction that one of the three effects discussed above is absent; the number next to each arrow is the probability level at which a standard F-test rejects that restriction. These numbers are referred to simply as P-levels in what follows.

All the high P-levels in Figure 1, which indicate failure to reject the null hypothesis at conventional levels, are generated by tests for firm effects (arrows pointing to the right in Figure 1). These data imply that firm effects simply do not exist. In the absence of industry effects, the null hypothesis that the realized α 's are identical can be rejected at the 29.2% level (no share effect) or the 27.3% level (share effect present). These results might lead a Bayesian analyst with a strongly managerial prior to accept the existence of firm effects. But both tests conducted in the presence of industry effects produce F-values less than unity that provide absolutely no support for the existence of firm effects. Firm effects seem to approach significance only when firm-specific dummy variables serve as proxies for industry effects. When industry effects are controlled for, firm effects fade into insignificance. The absence of a similar interaction between firm and share effects indicates that firm effects do not operate through the revisionist mechanism to any noticeable extent. Firm dummies do not serve as proxies for market share, and there is no difficulty disentangling firm and share effects.

In sharp contrast, all tests for the existence of industry or share effects produce significant results. All four tests of the null hypothesis of no share effects (arrows pointing to the left in Figure 1) signal rejection at P-levels below 4.5%, while the null hypothesis of no industry effects is always rejected at below the 0.01% level (vertical arrows in Figure 1).¹³

Let us now consider the importance of share and industry effects, postponing until Section IV a discussion of the implications of the absence of firm effects in these data. It is most instructive first to present an informal treatment based on information in Figure 1 and then to employ the relevant special case of (2) in a more systematic analysis.

Comparing adjusted R^2 's of models not involving firm effects, market effects seem to account for between 18.84% and 19.29% of the sample variance of r . Following the discussion of equation (3), above, note that these percentages correspond to 75.65% and 77.46% of $s^2(R)$, the sample variance of industry average rates of return. Industry effects thus seem to be quite important, apparently accounting for the bulk of inter-industry differences in accounting rates of return. The industry seems an easily defensible unit of analysis.

On the other hand, the adjusted R^2 's in Figure 1 indicate that market share effects add only between 0.17% and 0.62% to variance explained. Similarly, using $\gamma = 0.2304$ from Figure 1, $\gamma^2 s^2(S)$ amounts to only 0.90% of $s^2(r)$. It is interesting to note that in Ravenscraft's (1983) paper, which focuses on share effects, this ratio is even smaller; it is between 0.53% (GLS) and 0.82% (OLS).¹⁴ While Ravenscraft also uses 1975 Line-of-Business data, he uses the ratio of operating income to sales to measure profitability, does not delete "miscellaneous" industries or observations with small shares, uses classical variables like concentration in place of industry dummies, and attempts (in his GLS estimates) to correct for

a complex pattern of heteroscedascity. The statistical significance but quantitative unimportance of market share effects thus seems a robust feature of these data.

One final pattern in the statistics presented above deserves mention. Market share adds more to adjusted R^2 in the presence of industry effects (0.62% versus 0.17%), and industry effects add more in the presence of share effects (19.29% versus 18.84%). This sort of complementarity is suggestive of a negative correlation between market share and industry effects. Pointing in the same direction are the drops in the P-levels associated with share effects when industry effects are added and the corresponding changes (not visible in Figure 1) in the P-levels associated with industry effects. Finally, the fact that the estimate of $\gamma^2 s^2(S)$ discussed above exceeds the contribution of share effects to adjusted R^2 is also suggestive of a negative correlation between share and industry effects. (See equation (5), below.)

Let us now provide a more systematic analysis of the issues raised in the preceding three paragraphs. With no firm effects present, the relevant special cases of (1) and (2) are the following:

$$(4) \quad r_{ij} = \mu + \beta_j + \gamma S_{ij} + \epsilon_{ij},$$

$$(5) \quad \sigma^2(r) = \sigma^2(\beta) + \gamma^2 \sigma^2(S) + \sigma^2(\epsilon) \\ + 2\gamma\rho(\beta, S)\sigma(\beta)\sigma(S).$$

Readers uninterested in estimation technique and persuaded by the evidence presented above bearing on (5) may wish to glance briefly at Table 1, which summarizes the results developed below, then skip to Section IV.

Ordinary least squares estimation of (4), which appears in Figure 1 as the "Industry and Share Effects" model, yields a consistent and unbiased estimate of 281.05 for $\sigma^2(\epsilon)$. Following Searles's (1971, chs. 9-11) treatment of

variance components estimation in unbalanced models, we can compute consistent "analysis-of-variance" estimates of the remaining quantities on the right of (5).

Let the operator ESS mean "expected summ of squares about the sample mean," let N be the total number of observations, let N_j be the number of observations in industry j , and let M be the total number of industries. A bit of algebra yields

$$(6) \quad \text{ESS}(r_{ij} - \gamma S_{ij}) = (N-1)\sigma^2(\epsilon) + (N-G)\sigma^2(\beta), \text{ where}$$

$$(7) \quad G = \sum_{j=1}^M (N_j)^2 / N.$$

If all industries had only one firm, G would equal one. If there were only one industry, G would equal N , since industry effects would not contribute to overall variance. In these data, $G = 15.55$. Using $\gamma = .2304$ and $\sigma^2(\epsilon) = 281.05$ from above, setting the expectation on the left of (6) equal to its sample value, and solving yields an estimate of 68.47 for $\sigma^2(\beta)$. This is equal to 19.62% of the sample variance of the r_{ij} and 78.78% of the sample variance of the R_j . The quantitative importance of industry effects and the defensibility of industry-level analysis are again clear.¹⁵

In order to estimate the two remaining terms on the right of (5), it is necessary to be more specific about what is meant by a non-zero population correlation between market share and market effects. We imagine the data generation process first fixing the N_j , then drawing the β 's independently from their unconditional distribution, and finally drawing the S 's for each industry from the conditional distribution determined by the value of β previously drawn. We assume without loss of generality that the unconditional mean of the β 's is zero and of the S 's is μ_s . We use the following:

$$(8a) \quad E(S_{ij}S_{kj}) = \begin{cases} (\mu_s)^2 + \sigma^2(S), & i=k \\ (\mu_s)^2 + \sigma^2(S)\rho^2(\beta, S) & i \neq k \end{cases}$$

$$(8b) \quad E(\beta_j S_{ij}) = \rho(\beta, S)\sigma(\beta)\sigma(S).$$

The first part of (8a) and (8b) are not restrictive; the second part of 8(a) is consistent with but does not impose normality. These expectations are taken with respect to the unconditional population distribution, but they are conditional on the assignment of firms to markets. Similarly, for $h \neq j$, $E(\beta_h \beta_j) = E(\beta_h S_{ij}) = 0$, and $E(S_{ih} S_{kj}) = (\mu_s)^2$.

Let r_j be the unweighted mean of the rates of return of business units in industry j . Then if (4) is the true model, (8a) and some algebra yield

$$(9) \quad ESS(r_{ij} - r_j) = (N-M)\gamma^2\sigma^2(S)[1 - \rho^2(\beta, S)] + (N-M)\sigma^2(\epsilon).$$

The quantity on the left is the expected sum of squared residuals from a regression of the r_{ij} on M industry dummy variables. This regression appears as the "Industry Effects Only" model in Figure 1. Use of (8) and a bit more algebra yields

$$(10) \quad ESS(r_{ij})/(N-1) = E[\sigma^2(r)] = H\sigma^2(\beta) + \gamma^2\sigma^2(S)[1 - (1-H)\rho^2(\beta, S)] + \sigma^2(\epsilon) + 2H\gamma\rho(\beta, S)\sigma(\beta)\sigma(S), \text{ where}$$

$$(11) \quad H = (N-G)/(N-1).$$

Equation (10) provides a decomposition of the sample variance of business unit profitability corresponding to the decomposition of the population variance given by (5).

Setting expectations equal to sample values, solving (9) for $\gamma\sigma(S)$ and substituting into (10), we obtain an equation involving $\rho(\beta, S)$, sample

statistics, and estimates derived above. A search of the interval $(-1,+1)$ reveals a unique root; the estimated value of $\rho(\beta,S)$ is -0.089 . This confirms the negative correlation between industry and share effects. Equation (9) then yields an estimate of 2.182 for $\gamma^2 \sigma^2(S)$. As this is only about 3.2% of the estimated value of $\sigma^2(\beta)$, the unimportance of share effects is also confirmed. Table 1 reports the estimated population and sample decompositions, corresponding to equations (5) and (10), respectively, implied by our estimates.

IV. Conclusions and Implications

The analysis of Section III indicates that the 1975 FTC Line-of-Business data provide strong support for the following four empirical propositions:

1. Firm effects do not exist.
2. Industry effects exist and are important, accounting for at least 75% of the variance of industry rates of return on assets.
3. Market share effects exist but are of negligible quantitative importance.
4. Industry and market share effects are negatively correlated.

The apparent non-existence of firm effects is somewhat surprising. This finding is perfectly consistent with substantial intra-industry profitability differences, which Table 1 shows to be present in these data. The absence of firm effects in (1) merely means that knowing a firm's profitability in market A tells nothing about its likely profitability in randomly-selected market B. This is consistent with the conglomerate bust of the past decade and with a central prescriptive thrust of Peters and Waterman (1982, ch. 10): wise firms do not diversify beyond their demonstrated spheres of competence. The non-existence of firm effects suggests that Mueller's (1983) persistent

firm-level profitability differences are traceable to persistent differences at the business unit or industry level, combined with relatively stable patterns of activity at the firm level.¹⁶

The finding that industry effects are important supports the classical focus on industry-level analysis as against the revisionist tendency to downplay industry differences. But it is important to note that our analysis is generally silent on the merits of classical models and hypotheses. The empirical analysis here is basically descriptive, not structural. Our results cannot exclude the possibility that industry-level profitability differences in 1975 were dominated by the effects of the severe recession and energy price shocks that were buffeting the economy. This study cannot be interpreted as supporting an uncritical return to classical cross-section regressions. Finally, it is important to recognize that 80% of the variance in business unit profitability is unrelated to industry or share effects. While industry differences matter, they are not all that matters.

The statistical significance of market share in our fixed-effects regressions is consistent with previous studies that have reached revisionist conclusions. We depart from those studies by directly examining the importance of market share in explaining variations in business unit profitability. Our finding that share matters but doesn't matter much might seem to justify ignoring the revisionist mechanism in future research and policy-making. I think that would be a mistake.

First, the estimated coefficient of market share is quite large in equations with industry dummy variables. The "Industry and Share Effects" estimate of $\gamma = .2304$ reported in Figure 1 implies that an increase of market share from 10% to 50% is on average associated with an increase of 9.2% percentage points in r_{ij} . Average profitability differences of this magnitude cannot sensibly be ignored.

Second, even if the revisionist mechanism is unimportant on average in explaining profitability differences, it may be of central importance in some markets or classes of markets. The coefficient of share is constrained to be equal across industries in our regressions, even though a large number of authors have found substantial and (to some extent) systematic differences in the profitability/share relation across industries. Mueller (1983), for instance, finds the coefficient of market share in a profitability equation rises in cross-section with increases in industry advertising intensity, which he interprets as reflecting basic conditions that make possible product differentiation.¹⁷ The basic revisionist mechanism seems too plausible to dismiss entirely; we ought instead to investigate the industry-level factors that affect its nature and importance.

Finally, the negative correlation between market share and industry effects is surprising indeed. Since concentration and market share are positively correlated, this finding is perfectly consistent with the negative concentration coefficients obtained by Ravenscraft (1983), Martin (1983), and Mueller (1983) in cross-section profitability regressions. Moreover, our results imply that those coefficients cannot be made to change sign by obvious re-specification along classical lines.

One plausible explanation for negative concentration coefficients that also applies to negative values of $\rho(\beta, S)$ has been advanced by Martin (1983). Martin argues that capital-intensive, concentrated industries were hit hardest by recession and energy shocks in 1975 and that these same disequilibrium effects swamped any long-run effects of concentration on collusion. Note, however, that Ravenscraft (1983) finds that concentration has a positive sign in industry-level profitability regressions with these same data. At the very least, all this suggests the value of gathering and using panel data that would permit explicit analysis of cyclical and secular disequilibria.

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Footnotes

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1. Christopher Sims (1980) has expressed a similar methodological position in the context of macroeconomics.
2. Leonard W. Weiss (1974) provides a survey of cross-section studies in the classical tradition; see also Frederic M. Scherer (1980, ch. 9).
3. Efficiency should not be interpreted in narrow process terms here. A product innovation may simply make a firm more efficient in the production of the Lancasterian characteristics it supplies to an existing market. While product innovations that yield true differentiation (by creating something approaching a new market) cannot be formally modeled

3. (cont.) in this fashion, it seems appropriate to think of non-dramatic product innovations in efficiency terms for purposes of positive analysis of profitability.

4. This new, revisionist view seems to have been articulated explicitly first by Harold Demetz (1973); see also Sam Peltzman (1977). Interesting formal models consistent with this view have recently been developed by Boyan Jovanovic (1982), S.A. Lippman and R.S. Rumelt (1982), and others. It is important to note that something like the classical notion of entry or mobility barriers (Richard Caves and Michael Porter, 1975) must be invoked to explain why imitation does not suffice to eliminate efficiency differences among firms in the revisionist model.

5. Scherer (1980, ch. 9) reviews earlier studies of the effects of market share. Most obtained results broadly consistent with those of Ravenscraft (1983) and Martin (1983) but used data sets apparently inferior to theirs.

6. See Scherer (1980), ch. 4) for an excellent survey of the available evidence on economies of scale.

7. An additional accounting problem arises with business unit data: the allocation of shared assets among individual lines of business is inevitably somewhat arbitrary. If firms follow similar rules of thumb for doing this, spurious industry effects can be added to business unit data.

8. See, for instance, S. R. Searle (1972, chs. 9-11).

9. The industries dropped were the following: 20.29, 22.12, 23.06, 23.07, 24.05, 25.06, 28.17, 29.03, 30.06, 32.18, 33.13, 34.21, 35.37, 36.28, 37.14, and 39.08

Footnotes (cont.)

10. Capital markets serve to equalize (risk-adjusted) rates of return on investment, not on sales. The case for using rate of return on sales as a measure of the Lerner index rests a belief that accounting average cost is a good proxy for marginal cost, which I doubt, and the undeniable proposition that sales are measured more accurately than assets.

11. This variable is 100 times the variable MS used by Ravenscraft (1983).

12. The adjusted R^2 is equal to $\{1 - [s^2(\epsilon)/s^2(r)]\}$, where s^2 is the usual unbiased estimator of the variance, so that changes in this quantity, rather than in R^2 itself, correspond to changes in an unbiased estimator of the fraction of variance "explained."

13. This is a very conservative statement of the strength of the evidence for the presence of industry effects. The F-statistics and corresponding restricted models are the following: $F(241, 1533) = 2.709$, null model; $F(241, 1532) = 2.762$, share effects only; $F(241, 1078) = 2.007$, firm effects only; $F(241, 1077) = 2.033$, firm and share effects. I calculate the probability of obtaining F's above any one of these values under the null hypothesis to be less than 10^{-13} .

14. The necessary statistics are in Tables 1 and A.1 of Ravenscraft (1983).

15. As a final check on the robustness of this conclusion, we computed MIVQUEQO estimates of orthogonal firm, market, and error variance components of r_{ij} and $(r_{ij}, \gamma S_{ij})$. (See H. O. Hartley, J. N. K. Rao, and Lynn LaMotte (1978).) We obtained estimates of $\sigma^2(\beta)$ of 62.03 and 64.88, respectively. This very different technique thus produced estimates very close to those in the text, further strengthening the case for the quantitative importance of industry effects in these data.

Footnotes (cont.)

16. Using firm and industry dummy variables to analyze Line-of-Business data, Scott (1984) finds significant firm effects on R&D intensity. This finding indicates that the absence of firm effects is not in any sense built into the FTC data. Since R&D spending reflects policy rather than performance, it is not surprising that firm effects show up there but not here. This reasoning also argues against the hypothesis that the r_{ij} primarily reflect accounting policy choices, which are presumably generally made at the firm level. But this is fairly cold comfort, since the same accounting system can show radically different biases under different conditions. (See, for instance, Fisher and McGowan, 1983.)

17. See also Richard Caves and Thomas Pugel (1980), William Comanor and Thomas Wilson (1974, ch. 10), Allan Daskin (1983), and Michael Porter (1979).

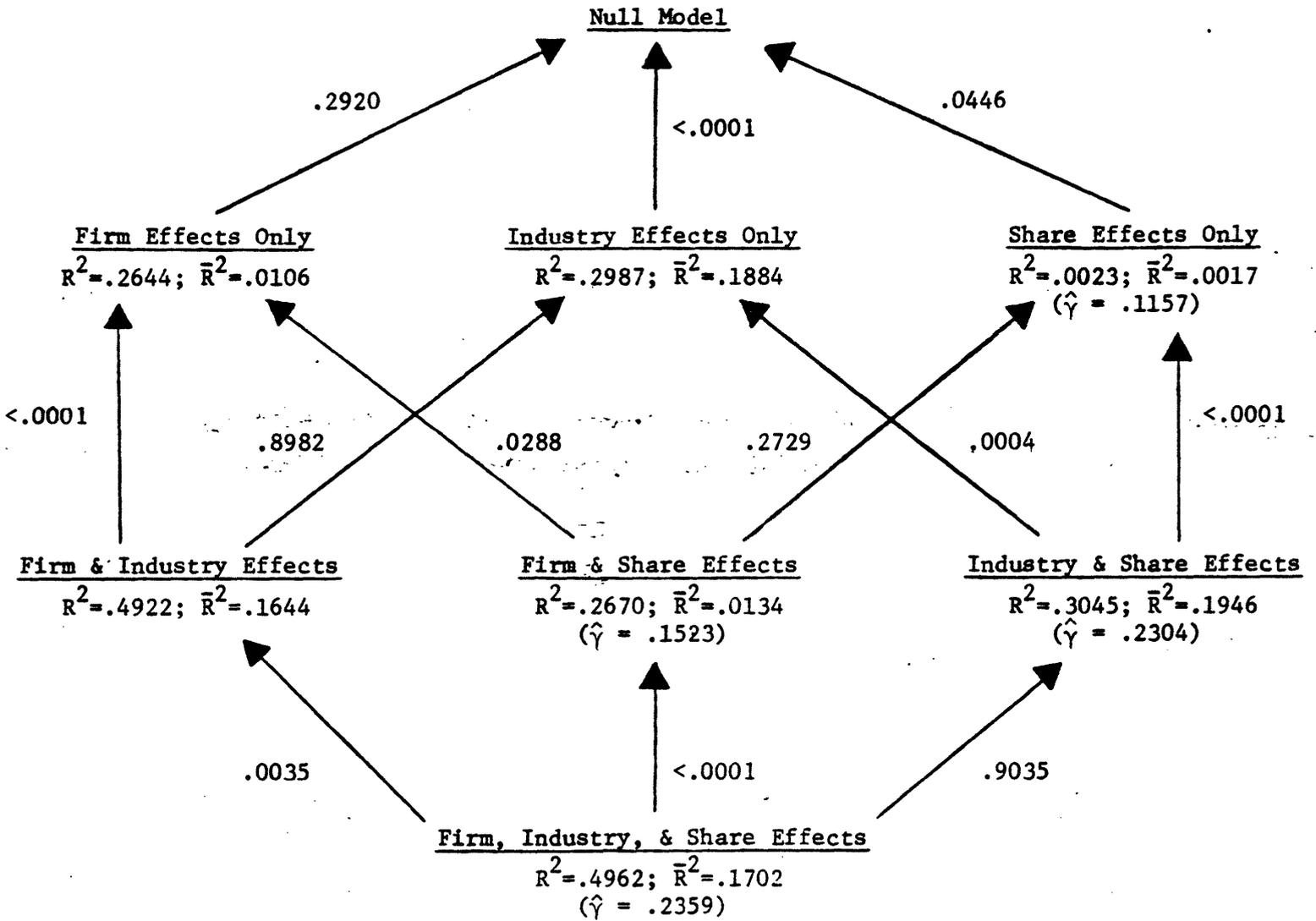


Figure 1 Summary Statistics from Fixed Effects Regressions

Table 1 - Estimated Variance Decompositions

Name	Population		Sample			
	Component	Estimate	Percentage	Component	Estimate	Percentage
Market:	$\sigma^2(\beta)$	68.466	19.59%	$\sigma^2(\beta)H$	67.905	19.46%
Share:	$\gamma^2\sigma^2(s)$	2.182	0.62	$\gamma^2\sigma^2(s)\{1-(1-H)\rho^2\}$	2.182	0.63
Covariance:	$2\gamma\rho\sigma(\beta)\sigma(s)$	-2.177	-0.62	$2H\gamma\rho\sigma(\beta)\sigma(s)$	-2.159	-0.62
Error:	$\sigma^2(\epsilon)$	281.049	80.41	$\sigma^2(\epsilon)$	281.049	80.54
Total:	$\sigma^2(r)$	349.520	100.00%	$s^2(r)$	348.977	100.00%

See text for sources and definitions. Totals may not add because of rounding.