COMMENT ON ‘INDUSTRY, CORPORATE AND BUSINESS-SEGMENT EFFECTS AND BUSINESS PERFORMANCE: A NON-PARAMETRIC APPROACH’ BY RUEFLI AND WIGGINS

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The Strategic Management Journal has published a series of articles on variation in business-segment performance. This comment addresses the insights and potential sources of confusion in the decomposition literature, with a particular focus on Ruefli and Wiggins (2003). We review the basic purpose of descriptive decomposition techniques and argue that they offer no information about the drivers of business performance or the mechanisms by which performance is generated. The techniques in the literature do not rely on ceteris paribus assumptions or a mapping between managerial influence and corporate effects. The estimated effects of industry, business segments, and corporate parents need not be independent to be modeled using variance decomposition. The comment also focuses on the distortions in estimates that can arise when the underlying sample of data does not represent the population accurately.

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by the decomposition literature on the variation in business performance. This comment addresses these potential sources of confusion not only to put Ruefli and Wiggins (2003) in perspective, but also to clarify our understanding of the entire line of literature.

At the root of many of the problems is a misunderstanding of the nature of variance decomposition itself. The technique has been used to describe industry, corporate-parent and business-specific influences on performance. It offers no information about the drivers of business performance or about the mechanisms by which performance is generated. In particular, the technique offers no information about how managerial action affects the influence of industry, corporate-parent, or business-specific effects on performance.
Overlooking this point leads to a series of unfortunate and fundamental problems of interpretation. We offer the following comments in response to specific points that have been made in the literature.

1. The mutatis mutandis vs. ceteris paribus assumptions are not relevant to the decomposition of variance.

A central argument in Ruefli and Wiggins (2003) is that the results of studies on the decomposition of variance were designed in a ceteris paribus (i.e., ‘all else equal’) context. Their principal point is that the research in this line does not account for responses to the emergence of particular effects. For example, they suggest that rivals may be able to elevate their own performance and hence the industry average by imitating the strategic action of a high-performing firm. This process of imitation induces a positive relationship between a firm-specific effect and an industry effect. Similarly, efforts to diminish commitment to a poorly performing industry could induce a negative relationship between a firm-specific effect and an industry effect.

Ruefli and Wiggins (2003) use mutatis mutandis nomenclature to motivate their analysis. Mutatis mutandis (i.e., ‘the necessary changes having been made’) evokes the notion of a system response to the variable in question. Ruefli and Wiggins (2003) suggest that their results account for managerial responses that influence the performance of rivals over time via improvements in performance ranking of firms or industries. They argue that changes in the performance rankings of industries, corporations, and business units may be influenced by managerial action, and that this action should be modeled explicitly because it carries the potential to dampen differences between firms. Variance decomposition is inferior, according to Ruefli and Wiggins, because it rests on the assumptions of ceteris paribus: ‘managerial activities . . . [represent] a possible deviation from an assumption of ceteris paribus, and thus a compromise of the conditions required for valid application and interpretation of variance decomposition techniques’ (Ruefli and Wiggins, 2003: 865).

This criticism by Ruefli and Wiggins (2003) is not valid because the ceteris paribus assumption is not evoked in the literature on the decomposition of variance. A ceteris paribus assumption allows the researcher to explore a theoretical relationship between an explanatory variable and performance outcomes without modeling the entirety of the system. In no article of which we are aware in this body of research is the ceteris paribus assumption evoked. The purpose of this literature is simply to describe the variance in performance without any claim about underlying causal relationships between the effects.

The variance-components analysis (VCA) method (discussed further below and called ‘components of variance’ and ‘COV’ in McGahan and Porter, 1997) relies on an assumption of ‘random effects,’ which has a specific technical meaning that is often misconstrued. The technical assumption is that the processes that relate various effects are not systematically tied to the levels of the effects. As we argue below, this assumption is not unreasonable even in context of the Ruefli and Wiggins critique since imitation and other competitive action may either raise or lower performance. The VCA method was adopted only because of computational constraints that were broken by the mid 1990s. As a result, the VCA method has not been used by most current researchers, who have relied instead on other methods that do not assume random effects (see McGahan and Porter, 2002). Furthermore, the assumption of random effects under the VCA method is so specialized that it should not be confounded with the broader agenda of the literature as a whole, which is simply to report on variation in performance among the population of businesses. This agenda requires no restrictive assumptions about the processes that give rise to variance in performance.

Thus, the distinction between the ceteris paribus and mutatis mutandis assumptions, while relevant to theoretical models and normative empirics, has nothing to do with the descriptive empirical analysis at hand, even when variance is estimated using VCA.

2. Many factors other than managerial influence can give rise to industry, corporate, or business-specific effects.

Ruefli and Wiggins correctly suggest on pages 864 and 865 of their article that managerial action may give rise to corporate, industry, or business-specific effects. With this suggestion they depart from prior authors, including Schmalensee (1985), who incorrectly associated only corporate effects
with the influence of managers. Ruefli and Wiggins (2003) are not entirely consistent, however. In their abstract and conclusion they associate managerial influence exclusively with corporate effects and claim that their findings on corporate effects ‘provide . . . evidence that managers can have a strategic influence on business performance’ (Ruefli and Wiggins, 2003: 861). While management certainly influences performance, the influence of management is not exclusively reflected in corporate effects.

Managerial action can influence any class of effects in a decomposition of variance. For example, Bill Gates has influenced the structure of the operating systems software industry and Jeffrey Katzenberg created a business-specific effect by leading Disney’s film division to new heights between 1984 and 1994. Of course, managerial action can also generate a corporate effect (Jack Welch was instrumental to GE’s performance while he was CEO). Conversely, corporate effects may arise from forces other than managerial influence: macroeconomic fluctuations, governmental decisions, competitor action, and many others. For example, a general increase in interest rates could lead to capital-budgeting restrictions within a corporation that affect all divisions (and thus generate a corporate-parent effect for the corporation).

Ruefli and Wiggins (2003) go a step further to suggest, incorrectly, that the findings of prior studies on corporate, industry, and business-specific effects should be re-evaluated because the low importance of a class of effects (such as corporate effects) may be related to successful managerial intervention. Their argument is that managers may be so effective at competing that they may perversely dampen variation within the class; in other words, the capabilities of managers to influence a class of effects may not be revealed in variation within the class.

The problem with this logic is that there are many other influences besides managerial intervention that can give rise to effects of each type. Just as managerial intervention may cause a particular effect (or variation in a class of effects) to be low, other forces such as governmental regulation, buyer behavior, natural catastrophes, macroeconomic events, and competitive dynamics also may cause an effect (or variation in a class) to be low.

Furthermore, managerial intervention (and these other forces) also may cause the effects or variation in a class of effects to be high. Bad weather, governmental regulation, and managerial action all contributed to a low industry effect for the property and casualty insurance industry in the wake of Hurricane Andrew. Creativity and a disciplined research program—both of which may be the direct result of management policy—can give rise to a high corporate effect as in the pharmaceutical industry of the 1990s. Thus, managerial intervention (as well as other forces) can give rise to either high or low effects.

In sum, managerial action may be associated with high effects, low effects, high variation in a class, or low variation in a class. Hence, no information can be gleaned from variation in performance—or from the estimated levels of effects—about the influence of management.

3. Assessing changes in performance cannot generate information about the causes of the effects.

On page 865 of their article, Ruefli and Wiggins note that ‘this area of strategic management research would be better served by stepping back, selecting fresh methodologies to employ, and then if necessary, attempting to revisit the relationships between levels of organization and performance.’ We agree entirely with this claim (and made a similar call for new research in McGahan and Porter, 2002). Our work on the persistence of profits (McGahan and Porter, 1997, 2003; McGahan, 1999a; Furman and McGahan, 2002) offers complementary insights into many of the conclusions in Ruefli and Wiggins (2003). The conceptual, technical, and methodological challenges are considerable and yet we agree that the study of persistence carries great potential for delivering important stylized facts about profitability patterns.

Ruefli and Wiggins (2003) claim to make progress on this new research agenda through their investigation of relationships between changes in the relative ranking of business-segment profitability and the ranking in prior years of industry, corporate, and business-specific effects. They argue that changes in rankings motivate managers to act in ways that would subsequently influence the organization’s performance ranking. Hence they interpret consistency in the rankings as evidence that managers have successfully controlled their businesses and no longer seek to disrupt the status quo.

This claim and interpretation are problematic. It is impossible to infer the cause of persistence in
performance from the fact that persistence occurs. (Later, we will also argue that persistence is not properly modeled by rank in the first place.) Persistence may be due to fixed resources, consistent industry structure, financial anomalies, price controls, or many other influences that endure. In short, many factors other than managerial intervention can generate persistence in rankings.

Furthermore, managerial control does not necessarily cause persistence. Through creativity, entrepreneurial insight, process innovation, and many other actions managers can induce new effects, cause rankings to change, and enhance overall variation (this point is discussed further below).

In sum, reliable inferences about the causes of persistence cannot be generated from an analysis that only documents whether or not persistence occurred.

4. An increase in managerial influence does not necessarily dampen variation in performance, and may increase variation in performance.

Ruefli and Wiggins (2003) make the related argument that managerial control leads to (a) diminished levels of poor performance and/or (b) diminished variability in performance. They argue that a manager’s incentive is to enhance positive influences and dampen negative influences. Yet it is incorrect that the presence of this incentive diminishes poor performance or diminishes variability in performance. Poor management can exacerbate poor performance just as good management can enhance good performance. When investment is required to generate future return, even good managers may have an incentive to take actions that temporarily depress profitability, and thus good management can lead to poor performance.

Even when managers enhance high performance and neutralize low performance, it is ambiguous whether this success would dampen the difference between the best and worst performer. Furthermore, managerial success in improving the trajectory of performance could either increase or decrease variability around the trajectory. (Real options theory suggests that improvements in the trajectory are likely to be associated with increased variation.) As a result, success by managers at enhancing the positive contributions of an effect may increase the explanatory power of the effect rather than dampen it.

In sum, there is no connection between the extent of managerial influence, the degree of variance, and the rate of persistence in the effects. The ordinal methodology in Ruefli and Wiggins (2003) does not yield information about whether managerial or non-managerial explanations are valid for why the performance occurs and/or persists.

5. Variance is an attractive and appropriate method of measuring performance differences.

The decomposition of variance to identify industry, corporate, and business-specific effects is attractive because it is simple and statistically robust. The use of variance has been criticized, particularly by Brush and Bromiley (1997), because it gives weight to observations that are far from the mean. Brush and Bromiley (1997) are concerned that an analysis of variance on a sample from the population can lead to incorrect inferences about the population’s characteristics, particularly when the population’s characteristics are systemically determined.

In McGahan and Porter (1997, 1999, 2002, 2003) and McGahan (1999a, 1999b), we strive to represent the population of firms rather than a sample to mitigate this concern. In no sense are we simulating the population’s characteristics through repeated inferences from a sample as in the example from genetics cited in Brush and Bromiley (1997). When the decomposition of variance is run on the population, far-flung observations are actual data points that are of inherent interest. Our purpose is to describe variation in the population without imputing causality on why any of the particular effects arise.

Ruefli and Wiggins (2003: 865) go further to assert that variance analysis is inappropriate for describing the performance characteristics of the population of firms because of the importance of learning and feedback: ‘Recognizing the context in which industry and corporate-level factors affect performance as one in which learning and feedback loops are important renders discussion of the operational details of the use of techniques such as VCA analysis, as historically applied, moot.’ We agree that no inferences about managerial action can be drawn from a study of the variance of the effects. Ironically, Ruefli and Wiggins later make incorrect inferences about the influence of managerial action on changes in performance ranking.
We disagree that learning and feedback make variance decomposition (and its operational detail) moot. The purpose of variance decomposition is to describe the system, which may incorporate learning, feedback, inertia, managerial action, and many other influences, mechanisms and processes. While a study of variance decomposition yields no information about the processes of learning and of feedback within the system, it does generate descriptive information about the system as a whole that is valuable.

We also disagree that VCA is an inappropriate method for decomposing variance. This technique, which was originally motivated to lower computational requirements, was adapted by Schmalensee (1985) and incorporates an assumption that the observed effects are each randomly generated. This means that there is no relationship between (i) the sizes of the industry, corporate, and business-specific effects on an observation and (ii) the frequency with which effects are generated and their sizes. For example, a business segment with a high industry effect is equally likely to have a high or low corporate effect; and furthermore, any particular business segment is just as likely to have a high industry effect as any other business segment. Brush and Bromiley’s (1997) criticism, which is incorporated by reference in Ruefli and Wiggins (2003), is that the effects are not randomly conferred and that relationships among them may arise. This is indeed a valid concern that has been tested directly by McGahan and Porter (2002) and that has been discussed extensively by prior authors such as Bowman and Helfat (2001).

It is worthwhile to reflect on the fact that the original VCA (or ‘COV’) by Schmalensee (1985) did not depart from historical precedent and was entirely appropriate for this context. The technique of VCA identifies the first-order influence of the classes of effects and apportions variance between them. Without additional information, it is not unreasonable to make a first-order approximation of the decomposition of variance under the assumption that the relationships are randomly conferred. In particular, purposeful managerial action can lead to either improvements or deterioration in any of the estimated effects. There is nothing about the application of the VCA technique to the population of firms that departs from historical precedent or that makes variance decomposition moot because of learning or feedback.

6. The decomposition of variance does not involve any per se assumptions about the independence of the effects, and the non-parametric method does not involve fewer assumptions than variance decomposition.

Ruefli and Wiggins (2003) assert that their ‘non-parametric’ method for analyzing performance differences is superior to other methods that have been used in this literature because it contains fewer restrictive assumptions than a decomposition of variance. The ordinal technique and focus on persistence in their paper are designed to generate information on the relationships between the estimated effects. In particular, Ruefli and Wiggins (2003) explore how changes in the ranking of the performance of a business segment are related to its prior rank and the prior rank of its industry and corporate parent.

Ruefli and Wiggins (2003) criticize previous studies by arguing that they involve restrictive assumptions. They are most concerned with the assumption of random effects under VCA, which we have already discussed, but they fail to acknowledge the important contributions of Rumelt (1991) and McGahan and Porter (1997, 1999, 2002, 2003), which report on results obtained through ANOVA under the non-restrictive assumptions of ordinary-least squares. In particular, McGahan and Porter (2002) implements an ANOVA that does not assume, constrain, or rule out any form of relationship between the estimated effects.

Ruefli and Wiggins (2003) are incorrect in suggesting that their approach involves fewer assumptions than variance decomposition techniques. The ordinal approach in Ruefli and Wiggins (2003) does not incorporate the same assumption as VCA about random effects, but it does incorporate many assumptions about competitive processes. The following is a partial list of them:

- Relative ranking in the performance of organizations, industries, and corporations stimulates processes that affect the subsequent rank of organizations.
- The processes that influence rank do not arise randomly, but rather are systematically related to prior ranking.
- The processes that influence rank take effect quickly (i.e., within the time span of this study).
The processes are dominated by managerial influence.
Managerial control leads to a dampening of variability in performance ranking over time.
Managers do not intentionally take actions that could diminish short-run performance ranking.

Thus, Ruefli and Wiggins’ (2003) methodology does not incorporate fewer restrictive assumptions than the prior variance decomposition studies. The decomposition of variance does not impose any functional form on the relationships between the estimated effects, but simply describes the population statistically. Studying variance is analogous to calculating the mean or even counting the number of members of a population. There is no stipulation about the relationships between the members of the population in simply counting them, in calculating their mean characteristics, or in calculating the variance around the mean. In fact, there are no ‘restrictive assumptions’ at all associated with the calculation of variance.

7. There is no statistically valid reason to focus on three categories of performance instead of two, four, or any other number; the ‘non-parametric’ approach in Ruefli and Wiggins (2003) omits a significant amount of information and does not represent the persistence of effects accurately.

Ruefli and Wiggins (2003) assert that prior studies on persistence such as McGahan and Porter (1999, 2003) and Mueller (1986) arbitrarily impose a distinction between high and low performers by categorizing observations into just two groups, which is inferior to their choice of three categories. Our view is that either approach is justifiable. However, it is misleading to suggest that three categories are somehow better than two categories. The choice to stratify the population by category can generate descriptive statistics that yield important insights about how competition operates. For example, McGahan (1999b) uses four categories of performance to show how ex ante differences in the levels of industry, corporate, and business-specific effects are related to ex post performance. Furman and McGahan (2002) break the population into 10 categories and show that businesses that rank in the bottom 10 percent carry greater investor valuations than businesses with better performance, but are less likely to improve their performance than businesses with better ex ante performance.

Ruefli and Wiggins (2003) emphasize that there are significant differences in the mean performance of the firms in their categories. There are also significant differences in the mean performance of organizations classified using other categorization schemes. Thus, there is nothing about the Ruefli and Wiggins (2003) approach that leads to a unique, superior classification.

Ruefli and Wiggins (2003) criticize prior classification schemes for failing to assure that statistical differences arise between the performance characteristics of firms that are near the borderline between the categories. In other words, they argue that similar organizations are arbitrarily classified into the top half or bottom half of performers based on only small differences between their characteristics and the median. Yet the Ruefli and Wiggins (2003) schema raises the same issue except when ‘modal’ firms are excluded from the analysis. The cost of this exclusion is high because excluding these firms from the analysis makes the results unintuitive and means that the sample does not represent the population.

The ordinal ranking of data based on performance is necessary in the Ruefli and Wiggins (2003) analysis to categorize observations as ‘superior,’ ‘modal,’ or ‘inferior.’ Ruefli and Wiggins (2003) then examine whether particular business segments are classified in the same category over time to support their conclusions about the persistence of industry and corporate effects. This approach is fundamentally inaccurate because an observation with exactly the same level of profitability year after year may be categorized differently in each year. For example, an industry with exactly the same performance level in 2 years can be classified in different categories just because of differences in the other industries. Thus, the approach can lead to the inaccurate conclusion that performance is not persistent when in truth it stays exactly the same.

In sum, we endorse the use of multiple categories of performance to investigate intertemporal relationships. Yet Ruefli and Wiggins (2003) mislead the reader by reporting that their own categorization is more robust than others. The approach in the prior literature on the persistence of effects
(i.e., by Mueller, 1986, Waring, 1996, and McGahan and Porter, 1999, 2003) uses detailed information on actual levels of performance rather than only rankings, and therefore has advantages over the ordinal ranking. By examining the rate by which performance persists (regardless of whether ordinal ranking changes), this prior literature yields intuitive information about substantive changes over time.

8. The more aggregated the industry definitions (i.e., 3- or 4-digit industry effects), the more likely that industry effects are obscured.

Ruefli and Wiggins aggregate their data to the 3-digit level despite the availability of Compustat data at the 4-digit level ‘...to yield a sample size comparable to prior studies...’ (Ruefli and Wiggins, 2003: 867). Aggregation of this type obscures the true industry effects in the population by systematically enhancing intra-industry variation and thus driving estimated industry effects toward the sample mean. As a result, the variance of estimated industry effects is artificially low, and the contribution of industry effects to total variation is underestimated. Furthermore, this distortion of the estimated industry effects also may inflate the estimated rate of industry persistence because of artificially dampened intertemporal variation in the estimated industry effects.

9. Using only diversified firms distorts industry effects significantly and inflates corporate effects.

Our final comment is not specifically applicable to Ruefli and Wiggins (2003), although it is fundamentally important to a correct interpretation of the results in this literature. Roquebert et al. (1996) introduced a practice into the literature that has been endorsed by Bowman and Helfat (2002): they reported results on a sample of diversified firms rather than on the entire population of all publicly traded firms. This restriction significantly distorts the influence of each class of effects. To see this, it is essential to understand that firms with performance far from the mean tend to be single-segment businesses, and that the industries that host single-segment businesses also tend to host diversified firms. By excluding the single-business firms, total variance is diminished significantly and estimated corporate effects are artificially inflated because diversified firms tend to perform close to the overall mean. Including only diversified firms lowers total variation and makes it appear that the diversified firms account for a larger portion.

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