Strategic Reality Today:

Extraordinary Past Success, But Difficult Challenges Loom

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Dedications

This essay is dedicated to the founders and original members of the BPP Division in 1972, and to the organizers and attendees at the Pittsburgh Conference in 1977, from which sprung both SMJ and SMS. Strategic Management as a business school discipline exists because of their extraordinary efforts. It is also dedicated to Jeff Reuer and Michael Leiblein, Co-founders of *The Strategic Management Review* in the hope that SMR will thrive over the next several decades and add substantially and importantly to the research conversation in Strategic Management.

Abstract

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After quickly reviewing the early history and the subsequent extraordinary success of Strategic Management, we suggest that research in our field today shows signs of settling into a premature institutional equilibrium regarding some vital issues. This equilibrium is inconsistent with strategic reality today on important topical and methodological imperatives the field faces. We suggest that these "strategic realities" must be extensively and thoroughly addressed to ensure our continued success in the future. We also suggest addressing these realities is key to improving our research efforts and their managerial usefulness as we move forward in a rapidly changing world. In other words, they represent important research challenges the business and academic environments present us today. We assert that substantial progress on any subset of these strategic realities over the next 20 years could be an important step toward continued success of our field. We also note that such progress will likely be very difficult.

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"It was the best of times, it was the worst of times."
- Dickens, *A Tale of Two Cities*.

INTRODUCTION

In 1972 the BPP or Business Planning and Policy Division, of the Academy of Management was founded with Bill Guth[1] as the first president. This was an important first step toward formalizing our field as producing relevant and quality scholarship around the core concept of strategy. Without this first step, we might never have become an independent field of scholarship. The field of Strategic Management was then formalized at the extraordinary 1977 Pittsburgh Conference, over four decades ago. Over 100 scholars, executives and consultants met at the University of Pittsburg, where they planned to establish a new academic area in business schools, Strategic Management. Strategic planning was a relatively new and "hot" topic at the time in large firms. A capstone MBA "integrative" course often called "Business Policy" was being taught at a considerable number of schools and had been for several decades. Business Policy was taught by a variety of faculty from various departments but was not seen generally as a legitimate research area. Faculty at the Pittsburgh conference had been teaching this course with the addition of strategy as a way to think about the overall direction and competitiveness of the firm. These same faculty members were engaged in strategy research. Research faculty in

[1] Sadly, Bill Guth died in February of 2018, a few months before this paper was completed. His first teaching job was at HBS in 1959 where he had received his doctorate. He was still active as a teacher when he died and had attended both the AOM and SMS Meetings in 2017. Over a very long career, he made many service, research and teaching contributions to our field. He had a very fine mind and a quick wit. It was an honor to have known him.

various established academic areas at some schools were interested in capturing “strategic planning” as a component of their own departments. Strategic or long-range planning staffs had recently arisen in the home offices of some large industrial firms and in the BCG, McKinsey, and A.D. Little consulting firms.

The scholars, consultants and executives at the Pittsburgh conference were not merely expressing an interest in the realm of strategic management scholarship, they were plotting a revolution that would add a new department and discipline to business schools, change the core requirements for undergraduate and MBA students, and introduce strategic management Ph.D. programs into the curricula. John Grant of Pittsburg hosted the conference, while Dan Schendel and Charles Hofer edited and published the proceedings (Schendel and Hofer, 1979). In 1980 *The Strategic Management Journal* began publication, and shortly thereafter the Strategic Management Society was established. Many were involved, but the efforts of Dan and Mary Lou Schendel[2] were paramount in all of this. Some of the scholars at Pittsburgh paid a substantial price for their academic apostasy in terms of stalled or delayed promotions and/or minimal salary increases for years, but by the early 1990s Strategic Management had largely won, not just won, but had become wildly successful in firmly implanting the field of strategic management as a fundamental teaching and research discipline/department in business schools. Without the intense work a few dozen academic revolutionaries just discussed in the Business Policy and Planning Division of the AOM, the participants in the Pittsburgh Conference, and Dan and Mary

[2] Without the boundless foresight and energy of Mary Lou and Dan Schendel over many years, it is unclear if there would even be a Strategic Management research and teaching field in business schools today. Among many things, they founded and successfully ran both SMJ and SMS for many years.

Lou Schendel we likely would not have a successful and highly respected business school discipline.

The arrival of *The Strategic Management Review*, the most recent product of our success, represents a unique opportunity to reflect on some important and difficult research challenges that lie ahead for Strategic Management while striving to keep our research relevant to both academia and managers. At the same time, we need to keep in mind the enormous progress that has already been made over the last four decades. We have many reasons to be optimistic about the future of our field.

However, we assert that the current distribution of scholarship does not correspond to various pressing realities that strategic management faces today and will have to confront consciously sooner or later to remain robust and relevant. Hopefully sooner! The business world has changed in many ways since the early 1990s. By the middle of the 1990s strategic planning had ceased to be viewed as an essential and large staff headquarters function for medium and large firms. Other changes such as globalization, digitalization, outsourcing, alliances, joint ventures, the rapid invention/diffusion of new technologies, and increasing emergence of positive feedback industries became obvious in the early 1990s[3] and have since been topics of considerable strategic management research. However, we also maintain that effective research in strategic management today is being impeded by our failure to address pressing research and managerial realities that are receiving only limited scholarly effort. Some research areas and scholars in strategic management may be stuck in an aging research

[3] Bettis and Hitt (1995) document and discuss the changes that were becoming apparent in the early 1990s.

equilibrium that has become institutionalized and does not correspond to some realities we face today.

In this essay we discuss four “strategic realities” that we assert are important today. By “realities” we refer to important scholarly and managerial issues today that are currently receiving relatively little scholarship but are likely keys to improving our research efforts and their managerial usefulness as we move forward. We assert that some outdated concepts, assumptions and methods have become so common that scholars unconsciously accept them as defining characteristics of Strategic Management. We also note this is likely occurring to a greater or lesser degree in other business school disciplines. At the same time it should also be noted that some theories such as the Resource-Based View (RBV, e.g., Barney, 1991) have shown considerable robustness to the changes that have occurred and are still intellectually vibrant. What we hope to do in this essay is to metaphorically peel back the next layer or two of the strategy research onion. We hope that scholarly consideration by at least some readers of this essay will cause them to undertake research that can move us forward toward more strategic reality, thereby making our scholarship more meaningful to the accumulation of knowledge and providing better guidance for mangers. What we offer are opportunities to advance the field. *What we cannot offer are the solutions to these opportunities.* However, we are currently working on aspects of three of these strategic realities and know of others engaged in various aspects of all four, but much greater scholarly effort will be necessary to address all four satisfactorily over the next two decades.

We also willingly admit that there are likely strategic realities we have missed or omitted. “Strategic Reality” as we use it today, may imply a very different set of topics within two or three more decades,[4] perhaps even less, requiring peeling the strategy research onion even further. This paper is not intended as a fully comprehensive future research agenda for the field. That would be the height of presumption! It is an elaboration of what two active North American scholars from two different cohorts suggest to be *some pressing research realities* today that the mainstream of the field will hopefully address more extensively thereby producing scholarship that is both intellectually robust *and* relevant to managers in the near future. Unfortunately none seem to us easy to address effectively. Some will likely require new or enhanced methods and/or theories. Some and perhaps all will meet substantial institutional resistance. We maintain that even partial resolution of any subset of these issues would be an important step forward intellectually for our field. We hope that some aspects of these strategic realities may have application in other business disciplines and perhaps, even in some of the traditional social sciences. We also suspect that some scholars will strongly disagree with some subset or perhaps all of what we summarize as "strategic realities." This is fine with us. They could be right and we are glad they are passionate about their own perspectives. We are trying to stimulate new ways of thinking about problems Strategic Management must address going forward. Our hope is that a modest portion or more of readers, will soon add some of these "realities" to their conscious thoughts and/or research agendas. For all readers we hope they will find something in the essay that is intellectually stimulating.

[4] We hope Strategic Management will still be successful then, but note that path dependencies including professional research methodological and topical norms established in the next two decades will likely be key to our success or failure later in the future. Failure to make appropriate changes in the next two decades could lock us into an inappropriate research paradigm as the rate of change continues to accelerate.

STRATEGIC REALITY

"People always find it easier to be a result of the past rather than a cause of the future."

-Unknown Author

Strategic Reality One: *Strategic reality is complexity and feedback not linearity and additivity.* "Complexity" is a frequently used term in many fields (e.g., Computational Complexity, Algorithmic Complexity, and Kolmogorov Complexity) and can have very different meanings across types of disciplines. Usage in strategic management typically involves interdependence. The foundational paper on complexity for strategic management and other fields is Simon (1962). It was updated in Simon (1996). For a broad overview written for a general audience of "complexity" at a nontechnical level see Mitchell (2009). We will take our use of the term "complexity" to mean *substantial interdependency among components or agents,* in a *dynamic system of some sort*, sometimes called a *complex adaptive system.* See Gell-Mann (1994) for a fascinating readable yet deep introduction to complex adaptive systems by a Nobel Prize winner in Physics.

It is important to understand that individual firms together with their environment constitute a complex adaptive system. The same could be said of individual industries and their environment.[5] Also, witness how rapid technological evolution has changed the dynamic state of competition for many industries and continues to do so today with machine learning. It is also

[5] Notice the pervasive endogeneity encountered at different levels of analysis that is frequently ignored with firms and industries considered to be separate from their environments.

standard to note that such complex systems can result in "emergent behavior" that cannot be predicted by *ex-ante* analysis (e.g., Holland, 2000). It is interesting that one of the early pioneers of Strategic Management, Henry Mintzberg, developed and discussed the concept of "emergent strategy" for organizations in Mintzberg (1978). This was before the term "emergence" had gained currency in the literature on complex adaptive systems.[6]

"Adaptive" in "complex adaptive system" implies roughly that such systems can adapt to varying degrees as *circumstances* change[7] (e.g., industry-environment systems undergo endogenous changes). Feedback loops are an inherent feature of complex adaptive systems and can also alone constitute complexity. Without feedback loops there can be no possibility of conscious adaptation by managers or organizations, except as a random occurrence.[8] Figure #1, *Managers Face a Reality that is Complex,* illustrates a very simple conceptual example of a complex system and its causal dynamics. It shows eight "variables" with various causal relationships indicated by connecting arrows. Casual examination will illuminate various feedback loops in this conceptual example. Furthermore, complexity necessarily engenders nonlinear and often highly nonlinear behavior that can include chaotic behavior.

Critically, we also know that some key concepts, models and theories in strategy involve either complexity or are complex adaptive systems. Take, for example, the internal fit of the components of a firm (e.g., Porter, 1996; Siggelkow, 2001), the fit and fitness between the firm

[6] This interesting fact about Mintzberg's 1978 paper was pointed out to us by the referee.
[7] Discussion of adaptation in organizations goes back at least to Barnard (1938) a businessman who made many astute observations of how organizations actually operate. His book was an early influence on Simon, Cyert, March, and others.
[8] We do not address the case of generalized Darwinism (variation, selection and retention) since in raw form it leaves very little room for conscious managerial action. However, we strongly assert that firms, industries and industrial ecosystems are subject to very strong environmental selection pressures from time to time.

and its environment (e.g., Levinthal, 1996), the fit and fitness of firms within industries or business ecosystems (e.g., Isanti and Levien, 2004). Other examples of "fit" as complexity abound. The concept of "fit" is a key concept in strategy and it is inherently complex, and thus not a sum of linear additive effects. This leads to some crucial points.

By contrast to complexity and complex adaptive systems, both of which imply *interdependence and severe nonlinearity*, strategic management seems to have become increasingly dominated by linear statistical models in a variety of forms. Such linear models for examining relationships are built largely on assumed Gaussian distributions and *can be very useful* so long as the reality underlying the *data generating process is linear, can be transformed to linear, or is to good approximation linear and in the form of additive sums of linear terms.*[9] Furthermore, the residuals should be Gaussian to a close approximation. For some assumptions of regression, "asymptotic corrections" that can be applied that become increasingly correct in the limit as the sample size goes to infinity.[10] We note, of course, that linear statistical models can incorporate a variety of nonlinear functional forms via variable transformations that linearize each individual additive term. However, linear models by definition cannot *directly represent* some forms of nonlinearity, and virtually all forms of complexity and feedback. Furthermore, complexity and feedback are common in organizations and organization/environment adaptive systems.

[9] This is the fundamental basis or assumption of regression modeling. It is ignored in some and perhaps many regression studies. There is a general lack of awareness among Strategic Management scholars of the vital importance of the underlying stochastic data generating process in determining whether or not a regression model is likely to be appropriate.
[10] It is generally not possible to know exactly how well a particular asymptotic performs for an individual sample size and configuration. Large is a relative measure and hard to define in particular cases.

We strongly agree that linear statistical models*, if used appropriately and with great care*, have important applications in strategic management and can be a powerful and valuable empirical tool. We currently use such models and intend to continue when we believe their use is appropriate. However, this class of models also has some rather severe limits that are often ignored by scholars. As interpreted by many scholars, the regression model is seen and sometimes actually declared implicitly or explicitly, to be a *universal dataset analysis tool*. In this view, if you have a dataset you can use some variety or variation of a linear statistical model to appropriately and correctly "analyze" these data including the usual fit statistics, confidence intervals, and significance tests for coefficients, *regardless of the data generating mechanism*.

Figure 2 is a conceptual diagram of what a linear statistical model of the complexity in Figure 1 could look like. Notice that an arbitrary choice of the dependent variable has been made. The remaining "independent variables" each have an effect that is measured while holding all the others constant. (Actually, various subsets of the seven independent variables shown could also be used to make this point.) Clearly, Figure 2 is not an accurate or useful model for testing hypotheses or examining causality in the complexity example shown in Figure 1. However, if the goal of the analysis is *making predictions*, not testing statistical hypotheses then a linear model can sometimes be useful. Also, it should be apparent that the linear model in Figure 2 is full of endogeneity regardless of what variable from Figure 1 is chosen as the DV. It obviously has nothing meaningful to say about the real causality behind Figure 1. However, dependent on the strength of the various causal arrows and the size of the sample in Figure 1, there will be some, perhaps many, "significant" coefficients in the model of Figure 2. In other words, there will only be flawed correlation/association information, but little or no useful causal information in this linear model. This would be true for any choice of a DV from Figure 1. However, managers and especially managers concerned with strategy do not live in the linear model of the world illustrated in Figure 2. Instead they live in and must manage with many kinds of complexity as represented conceptually by Figure 1.

We cannot imagine any way the complexity in Figure 1 could be *modeled* appropriately (or causally) by the linear model of Figure 2. Figure 1 simply does not represent the assumed additive sum of linear terms data generating process, even approximately. Furthermore, managers are not interested in the influence of a particular IV on the DV while all the other IV's are held constant. This is not their world since they cannot hold variables constant as illustrated by Figure 1. In fact, experienced managers often assert something like the following: "Solving one problem, usually generates several other problems."

The nature of complexity and the inability to model it directly with regression models strongly suggests the expanded development and use of simulation models (e.g., NK-landscapes, k-armed bandit models, etc.) and perhaps appropriate machine learning algorithms. Qualitative research also has a role to play here in understanding how managers come to understand and deal with complexity. Perhaps experiments using experienced managers in their normal context instead of college students in a department laboratory could also be useful in studying how managers deal with real complexity.

Strategic Reality Two: *Strategic reality is about causality not correlation or association.*

> What is singular about regression is only that a technique so ill suited to causal inference should have found such wide employment to that purpose.

- Spirtes, Glymour, and Scheines, Heckerman, Meek, Cooper, & Richardson[11]

To be relevant, research in strategic reality must go beyond mere influences or correlations and address causality in appropriate and substantial ways. However, for most empirical papers this is not done, although many make causal claims or arguments. Some do *attempt* to address issues of endogeneity mechanically, but even if correct this is only a small start toward improving claims of causality. Natural experiments have become a popular approach, but rather obviously only relatively few natural experiments occur, so most important phenomena and topics in Strategic Management cannot be studied this way. At the same time using the usual regression models to do causal inference is very difficult. For many and likely most published regression studies it is hard to justify any substantial claims of causality.

Capturing causality is best suited to carefully controlled and/or randomized experiments.[12] However, for most of what is of interest in Strategic Management this is simply not possible. By comparison, c*ausality in regression models require a lot of information that is not included in data samples*.[13] Without developing strong theories that emphasize causal mechanisms and then empirically testing them in multiple empirical studies with correct

[11] Spirtes, Glymour, and Scheines. Heckerman, Meek, Cooper, & Richardson 2000: 191. This book has a lot of excellent material on using regression to attempt establishing causality, but simultaneously makes readers acutely aware of the difficulties of using regression models to establish causality – a rather honest and appropriate approach.

[12] Co-Editor Michael Leblein pointed out to us an excellent paper by McGrath (1981) that deals with a variety of experimental choces. Those interested in the the use of experiements will find it very interesting.

[13] It is important to note that the very nature of both convenience samples and observational data can introduce inaccuracies into attempts to model causality with adequate accuracy using regression.

specifications,[14] we lack strong and managerially relevant causal theory. Getting the model specification correct is seldom easy and often extremely problematic.

Most empirical articles over the last 25 years in top quality journals relevant to Strategic Management contain empirical papers that claim to both develop theory summarized in the form of statistically testable "theoretical" hypotheses and then test these hypotheses empirically using some form of a linear statistical model. The "bright line" break point between possible publication and no possibility of publication for these papers is usually coefficient hypothesis tests that yield p-values at or less than 0.0500. Unsurprisingly, most or all of the hypotheses in submitted papers typically report p-values of 0.05 or less. Furthermore, it has typically been assumed by many scholars that this means the "theory" embodied in each hypothesis with 0.050 or less p-value had been "proven to be true" and there is no need for any replication. Among several serious problems this entirely ignores the probabilistic evidentiary nature of sample statistics. A single regression study regardless of the p-values is only *probabilistic evidence* from a particular sample and specification, nothing more and nothing less.[15] This started changing in approximately 2016 when the co-editors of the *Strategic Management Journal* changed the journal's policy by eliminating bright line p-values (Bettis, Ethiraj, Gambardella, Helfat, and Mitchell, 2016). Even today, some of the major relevant journals stick with "bright line" p-values as the only criteria for determining a potentially publishable paper in spite of the strong

[14] Oliver Williamson, won the Nobel Prize in Economics in 2009 in part due to his 1971 *American Economic Review* paper that was refined in his 1979 *Journal of Law and Economics* paper. These two papers established that "asset specificity" was a crucial omitted variable in explaining and predicting the substitution of the market (and arms-length contracting) with vertical (merger) integration. We thank the referee for making us aware of this very interesting example of how important specification can be in regressions intended to capture causality.

[15] It is vital to realize that two random samples from the same population may yield different results for hypothesis tests. Of course, introducing convenience samples further complicates the replication of results.

and contrary official position of the American Statistical Association (Wasserstein and Lazar, 2016).

There are substantial problems with this approach. First, many significant coefficients in published papers are the result of some form of *ex ante* specification search to find significant coefficients in a sample (e.g., Kerr, 1998). "Theories" are then developed to fit these significant coefficients. This process guarantees the p-values that as reported will be smaller than they actually are (Bettis, 2012) as a result of the multiple repeated tests.[16] This is not theory. Unfortunately, it is simply not science! To be science Popper's falsifiability criterion[17] (Popper, 1935) must be met, but the significant coefficients are at no risk of falsification because the statistical model is based on *ex ante* specification searches for significant coefficients in the particular sample that precludes the possibility of a no significant relationship rejection of the coefficients relevant to the manufactured "theoretical" hypotheses. Such results knowingly or unknowingly misstate p-values that may or may not replicate with a different sample. Unfortunately, many journals still refuse to publish replications of bright line p-value studies, thereby completely ignoring the fundamental nature of both science and statistics.

The need for building and appropriately testing causal theoretical mechanisms should be an overriding goal in strategic reality, regardless of the particular subtopic. Knowledge of correlations or influences is certainly better than complete ignorance, but it is at best, only a minimal start toward the production of useful *causal knowledge.* Furthermore, many of these

[16] This raises important ethical issues that are almost completely ignored because the probability mathematics of repeated statistical tests is widely unstudied and unknown in Strategic Management.
[17] Sadly, the vitally important Popper's falsifiablility criterion, widely considered essential to science today, is widely unknown and/or ignored in Strategic Management. Unfortunatley, for some scholars "absolute truth" has become synomymous with a single significant p-value no matter how obtained.

hypotheses are based on previous empirically "proven theory" from articles built on specification searches. Hence with this kind of *ex ante* "hypothesis test" the potential exists for infecting generations of later papers that cite them without recognizing such results cannot claim to be statistics or science.

In regard to details of the extreme difficulties of model specification and statistical tests of causality, we strongly suggest first reading Freedman (1999). We also recommend a substantial amount of relevant, interesting and readable material in the short textbook by Berk (2004). Pearl, Glymour and Jewell (2016) is strongly recommended for those seeking a practical and readable approach to causality in statistical models. Graphical approaches combined with conditional probabilities as discussed in this book are becoming increasingly popular for certain forms of statistical causality. However, writing about such approaches Spirtes (2010: 1643) states that: "…, in many domains, problems such as the large numbers of variables, small sample sizes, and possible presence of unmeasured variables remain serious impediments to practical application of these approaches." Spirtes, Glymour and Scheines Heckerman, Meek, Cooper, & Richardson (2000) covers material similar to Pearl, Glymour and Jewell (2016), but at a broader and more rigorous level. It is important to note that the graphical approaches require acyclic graphs and this eliminates consideration of any feedback loops, an important limitation for Strategic Management research, or research in any field of study likely to include feedback loops. In thinking about *empirically* modeling causality it is important to think about approaches that are *"demonstrably*[18] *very good,"* but perfection is highly unlikely in most cases with regression models. The quote that appears at the start of this section of the paper should always

[18] "Demonstrably" here should be taken literally.

be kept in mind when developing regression models intended to capture causality.

Empirical hypothesis testing aimed at causality should start with strong causal theory that is developed independent of simultaneous statistical hypothesis testing. Of course, such causal theory may benefit from previous phenomenological research, exploratory data studies, simulations, mathematical models, or qualitative studies that identify potential regularities or anomalies. When we use the term "theory" we refer to *causal mechanisms or causal theories* that by definition act *implicitly or explicitly* across time,[19] and are often path dependent in Strategic Management. Furthermore, there will often be a hierarchy of causal mechanisms to choose from going from high levels of aggregation to much finer grained levels of analysis.

Probably the best current theory development tool for studying complex dynamics is computational modeling (simulation). The language and notions if not the precise mathematics contained in various forms of differential equations can also be useful. Difference equations and recursive relationships also have value.

Consistent with science, causal theory should make predictions that are empirically testable in a manner that could result in falsification of the theory. Also, a statistical model selected *to test theory directly* must be a close fit for the data generation process suggested by the theory. In certain *common cases* such as complex dynamics, we assert that it will be impossible to justify the data generating process assumed by linear statistical models.[20] However, *some*

[19] The fact that theories must operate across time as mechanisms is often ignored due to the limits inherent in certain varieties of otherwise useful regression models. Consider fixed effects for year and firm. This approach takes the dynamics of firm-level strategy completely out of the analysis. Individual firm and temporal performance heterogeneities are classed as "corrections" though, in theory, they are ostensibly at the core of Strategic Management.

[20] Unfortunately, complex dynamics appear to be very common in the pursuit of competitive advantage in real industrial ecosystems.

predictions made by the theory will likely be testable by linear statistical models used with great care. Finally, some theories in strategic management are context sensitive, meaning contextual differences must also be tested to establish contextual boundary conditions.

As an important corollary to the main point of this section we note the discipline of Strategic Management implicitly assumes that the *ex ante quality of strategic managerial decisions* are at least partially and meaningfully causal in achieving competitive advantage. This corollary presents considerable measurement problems. Without considerable and replicated *ex-ante* decision evidence tied causally to *ex post* performance measurement our field lacks adequate empirical grounding. For a critique regarding use of *ex post* firm performance data to measure the performance effect of aggregate decision making by CEOs see Blettner, Chaddad, and Bettis, (2012).

Finally, we want to note that on the upside there are at least several theories in Strategic Management, RBV being the most prominent example, where a strong causal theory was developed outside of simultaneous testing for significant p-values and then was empirically tested independently and extensively. Furthermore, the RBV has evolved, improved and expanded over time thanks to the efforts on numerous scholars.

Strategic Reality Three: *Strategic decision making primarily occurs in the presence of Knightian uncertainty and unseen fat-tailed probability distributions.*

Strategic decisions are often characterized as “risky.” The word “risk” is typically assumed implicitly or even explicitly in Strategic Management to refer to either the standard deviation or variance of a well-behaved Gaussian distribution or a few distinct events with the probability appropriately divided among them. Such assumptions make issues of risk and risk management seem highly tractable. However, we suggest that this does not correspond to strategic reality. In theory and academic folklore, estimation of strategic risks is often based implicitly on two assumptions. First, assume that managers know all the "important" possible future events that may occur. Second, assume that managers can make workably accurate estimates of the probabilities of these events. We suggest that these assumptions are impossible to justify in real strategic decision-making.

By contrast *Knightian uncertainty* (Knight, 1921), from here on simply "uncertainty," means that at least some of the possible outcomes and/or some of the outcome probabilities are unknown. In strategic reality managers making strategic decisions typically have very limited knowledge of the possible future events and even less knowledge of their probabilities. In other words, the risks of strategic decision problems are poorly specified and are very often full of uncertainties. Furthermore, they can usually only become well specified by making assumptions that do fatal damage to actual consideration of the original problem. Obviously, uncertainty is exacerbated as the future time frame is extended. *Uncertainty regarding the future is the strategic reality of what is often called "risk."*[21]

There is a closely related problem – extreme events,[22] that are not database errors but do occur naturally and often for important variables, are impossible in Gaussian distributions. Such extreme events if negative can quickly invalidate strategies or even destroy the current structure

[21] We were pleasantly surprised to find recently that a future special issue of the *Academy of Management Review* will be concern with uncertainty. We look forward to reading it.

[22] It is important to note that an extreme positive event for one firm may represent an extreme outcome for other firms in the industry. At the same time, extreme negative events like the real estate credit crisis roughly a decade ago had broad negative consequences for much of the global economy.

of an industry. Some relevant variables in industry environments and strategic decision variables of considerable relevance to Strategic Management follow what today are often called Fat-Tailed probability distributions. Linear statistical models are primarily based on samples from Gaussian distributions. Gaussian distributions have defined means and variances (*i.e.*, the first two moments). Gaussian distributions have tails that decline exponentially, making 4-sigma sample outliers virtually impossible in Strategic Management research. Gaussian distributions also assume individual data points are *independent* of each other. This is also a basic assumption for regression models as is the existence of the mean and variance. However, for Fat-Tailed distributions, also called Pareto, or Power Law distributions the mean and variance do not exist and individual sample data are *interdependent* instead of independent. Examples of Fat-Tailed distributions include the Cauchy and Log Logistic distributions. There are many others. *For such distributions the variance is infinite. Hence the usual statistical tests are completely irrelevant*! This results from the fact that Fat-Tailed distributions have "fat" tails that decline much slower relative to the exponential tails of the Gaussian distribution. Extreme events much greater than would ever occur based on a Gaussian distribution can and do occur. Measured in Gaussian terms 10-sigma and 15-sigma events are not unusual with Fat-Tailed distributions. This is a huge, likely insurmountable problem for linear statistical models and also vitally important to what is often called "strategic risk management,[23]" an obvious and important application for Fat-Tailed distributions.

[23] Perhaps it should be called "strategic uncertainty management."

Fat-Tailed distributions result from *interdependence* (complexity) that often takes the form of power laws, generated by positive feedback processes.[24] There are many variables of interest in Strategic Management within relevant industries such as ROA, ROIC, sales, assets, advertising, R&D and profits[25] that are susceptible to power law phenomena. In linear statistical modeling as practiced, samples from Fat-Tailed distributions are assumed to be Gaussian distributions but with database errors that create the outliers. Removal or "Winsorizing" often handles such data points without any further consideration of their origin. A data point at or over 4-sigma is an "*obvious mistake in the data!*" The approximate odds of finding a 10-sigma data point in a Gaussian distribution are 1 in 1.3×10^{23}. As a basis for comparison the number of seconds that have passed since the Big Bang is only a comparatively miniscule 4.34×10^{17}. However, such 10-sigma events are common with large samples from Fat-Tailed distributions.

Furthermore there is a closely related problem. Fat-Tailed distributions often generate what is classed as an influential data point or points that can substantially alter the slope of a regression line. There are a variety of tests in various software packages for identifying influential data points. It appears that the assumption behind this approach is to eliminate "improper" or "incorrect" data points and thus make the regression line have what is then assumed to be the "correct" slope. In many, but not all, cases this is a way to ignore the intractable reality of an underlying Fat-Tailed distribution for some variable(s). Issues like this go to the core of what are the real limits of regression analysis. To resolve such regression issues

[24] Bettis and Hitt (1995) includes a summary of "positive feedback" and relevant references.
[25] Songcui Hu and Bettis have recently demonstrated these results with extensive data and are currently writing a working paper documenting the results.

Strategic Management will likely need the assistance of mathematical statisticians and philosophers of science and statistics.

Andriani and McKelvey have written lucidly and extensively about Fat-Tailed distributions and power laws. Andriani and McKelvey (2009) contains a wealth of material regarding power law distributions. In addition, Baum and McKelvey (2006) is also highly relevant. We strongly recommend both of these articles.

Strategic Reality Four: *Real strategic decisions are often made by managers using heuristics, but research in strategic management has primarily emphasized the errors associated with heuristics.*

> The broad ideas that shape the most critical high-level decisions of a business enterprise may also be viewed as heuristics - they are principles that are believed to shorten the average search to solution of the problems of survival and profitability. Much discussion of heuristics of this sort has been carried under the rubric "corporate strategy."
> -Nelson and Winter (1982)

We start with the preceding quote from *An Evolutionary Theory of Economic Change* that demonstrates along with earlier foundational work by Simon (e.g., Simon, 1955; Simon and Newell, 1958; Newell and Simon, 1972)[26] and Cyert and March (1963), heuristics play a crucial role in strategic decision making. The Nelson and Winter book was published just as the

[26] It is important that Simon and Newell were simultaneously active in establishing artificial intelligence as an independent field of study in Computer Science. Like their work on "bounded rationality," they emphasized search, satisficing, and heuristics as foundational to artificial intelligence. For this work they jointly received the Turing Prize in 1974.

Strategic Management was starting to gain intellectual traction and has been a major influence on the field along with earlier work by Cyert, March, Simon, and Newell.[27]

It is worth noting that shortly before Nelson and Winter (1982), Kahneman and Tversky (1979) had published "Prospect Theory: An Analysis of Decision under Risk" in *Econometrica* (currently over 50,000 citations), that would be partially responsible for a Nobel Prize in Economics and change some fundamental aspects of economic theory – no easy task! Of course, this paper was related to the much broader, "heuristics and biases" literature initiated by Tversky and Kahneman whose 1974 paper in *Science* provides an overview and introduces the three foundational heuristics: representativeness, availability of instances and scenarios, anchoring and adjustment. For a comprehensive collection of much of the work on biases and heuristics, please see Kahneman, Slovic, and Tversky (1982) and Gilovich, Griffin, and Kahneman (2002). A very readable overall discussion of this literature and its implications can be found in Kahneman (2011). Their approach is summarized as: "… people rely on a limited number of heuristic principles which reduce complex tasks of assessing probabilities and predicting values to simpler judgmental operations. In general, these heuristics are quite useful, but on occasion they lead to severe and systematic errors." (1974: 1124). Not surprisingly, because they are so severe and systematic *the voluminous literature here has focused on errors* not on the statement that heuristics are generally "quite useful." It is also notable that this theory is built on the comparison of the heuristics people use versus what they would use if they were appropriately *"assessing probabilities."* An example of this approach applied to managers is Lovallo and

[27] In addition, note that strategy problems are typically classed as "wicked problems" (Rittel and Webber, 1973; Camillus, 2008). Such problems cannot be turned into well-specified problems with solutions, without changing the fundamental nature of the problem and thus producing a "solution" irrelevant to the original problem.

Kahneman (2003) where the authors discuss how certain heuristics and biases can cause managers to be overly optimistic.

A second approach to human and managerial heuristics was primarily established as a result of the research efforts of Gigerenzer and various co-authors, but is not widely familiar to North American scholars in Strategic Management. Relevant works include Gigerenzer and Selten (2001) and Gigerenzer, Todd and the ABC Research Group (1999), Gigerenzer and Gaissmaier (2011) and Artinger, Petersen, Gigerenzer, and Weibler (2014). This approach assumes uncertainty, ecological rationality, and the relevance of the bias – variance tradeoff (Brighton and Gigerenzer, 2015). The premise is to represent as closely as possible how managers actually make decisions under such conditions. These assumptions and background theory make the Gigerenzer approach to heuristic theory, underlying assumptions, and predictions very different from those of Kahneman and Tversky. Research with the Gigerenzer approach that focuses on how heuristics can help managers make very good decisions faster has reached a much more positive judgement regarding the managerial use of heuristics. The Gigerenzer approach, since it assumes uncertainty, may be worthy of increased scholarly investigation relative to its use in strategic decisions. The different assumptions made by these two primary heuristic schools of thought lead to different emphases regarding the usefulness of what both call "heuristics" while making very different foundational assumptions regarding "heuristics," that lead to substantial differences in the actual heuristics. As a result, polemics have unfortunately tended to dominate the limited discussions between scholars from the two schools.

Along different lines than Gigerenzer, but similarly affirmative of the managerial use of heuristics is an adaptive approach developed by strategic management scholars based on qualitative research in firms by Eisenhardt and colleagues (e.g., Eisenhardt and Sull, 2001; Bingham and Eisenhardt, 2011). This approach deserves much more research attention in Strategic Management. Loock and Hinnen (2015) contain an excellent review of the various heuristics research in organization and management theory.

The first two approaches to heuristics above are grounded in psychology while the third approach resulted from qualitative research closely related to Strategy Management. However, unknown to most management scholars, computer scientists have developed an extensive theory of computer heuristics that has important implications for the study of managerial and organizational heuristics. This theory of computer heuristics in Computer Science is largely due to the *Theory of Computational Complexity* originating in the 1960's that seeks to identify those problems that any envisioned computers *cannot* solve optimally and generally, due to processing speed and/or memory constraints. Harel (2003) provides a conceptual and readable account of some basic principles of computational complexity. Importantly, this work rapidly led to development of heuristics in the form of computer algorithms that can often solve or come close to solving some seemingly intractable problems.

It would not be an exaggeration to say that heuristics are fundamental to the study of computational complexity. Human including managerial cognition is similarly limited by constraints on memory and processing speed.[28] This leads logically to the idea that computational complexity might provide a better grounding for the concept of bounded

[28] This parallel was fundamental to the way Simon approached both human and artificial intelligence.

rationality and the use of heuristics by managers and organizations (e.g., Bettis and Hu, forthcoming; Markose, 2005). This approach has been applied to a few strategic decision problems firms face (e.g., Rivkin, 2000; Bettis, 2017; Hu and Bettis, forthcoming). The results of these studies suggest that the complete solution spaces of many important strategic management problems are actually enormous[29] for what often seems like relatively straight forward problems. Hence, optimization is impossible and heuristics that severely reduce the search space or exploit regularities in the search space are likely necessary. The application of computational complexity to strategic decision problems faced by organizations suggests that organizations even those equipped with the most powerful computers that can be envisioned still come with processing time and storage[30] constraints that can severely limit their ability to solve real strategy problems in the sense of finding an "optimal" or even near optimal solution in acceptable time before the initial decision problem becomes irrelevant.

Furthermore, this omits the problem of coming up with a precise statement of the entire solution space for any substantial strategy problem, much less a precisely defined algorithm for searching this solution space in anything approaching finite time. Hence, in many cases the actual concept of an "optimal" solution is itself problematic. By comparison, ex ante, the exact performance of a managerial or organizational heuristic or even computer heuristic ex post on

[29] In some specific parameterized cases, the fastest envisioned computers could take trillions of centuries or more to find the optimum. Hu and Bettis (forthcoming) includes example calculations for the combined complex technology/organizational design problem that are relevant to the discussion here. They show that it may be impossible to actually define an opimal solution when designing complex technologies that share task envirnoments constrained by the laws of phyisics, chemistry and computation.

[30] Some problems would require that more than every electron in the known universe would be required for storage.

any particular contextual instance cannot guaranteed. The same heuristic may be "successful" in most instances, but fail in others due to structural differences in the solution spaces.

Overall then all of our discussion of heuristics leads to a quandary. We know that managers frequently use heuristics to help them decide a variety of strategic and other organizational problems, but there is precious little research on this, and the primary psychological theory of heuristics (Tversky and Kahneman, 1974) has focused on biases that limit the ability of individuals to deal appropriately with probabilities.

Since considerable strategic decision-making is done with or aided by heuristics, we need much more research to see what, why and how heuristics are used both successfully and unsuccessfully in strategy and related business school disciplines. Graham, Harvey & Puri, (2015) contains extensive evidence that senior executives routinely use heuristics in deciding on capital allocations. This entire section leads to questions such as: How do heuristics originate and gain legitimacy in firms? How can firms keep strategic heuristics from diffusing to other firms, thereby at least partially invalidating their usefulness? How can senior managers know what important heuristics are being used in divisions and functions across large complex firms? What selection pressures act on heuristics within and across firms and industries? How effective are these selections pressures in improving heuristics? What does it mean to "design" a heuristic? How can firms prevent consultants from diffusing their successful strategic heuristics?

At which level of analysis should heuristics be conceptualized? Tversky and Kahneman conceptualized heuristics at the level of the individual decision-maker. However, in strategy, heuristics likely encompass the level of the top management team, large parts of organizations, and perhaps other important stakeholders. Hence, we wonder about the relationship between heuristics and shared mental models (Menon, 2018). At the same time, we assert they have to be somehow related since mental models can be used for predictions of the future impact of strategic moves, and strategic heuristics concern making strategic decisions. While Kahneman and Tversky type heuristics are based on some general, systematic, individual psychological principles, strategic heuristics are often the result of managerial induction that are partially idiosyncratic to a particular organization and its environmental context at the time they were formed. These heuristics are often shared by various executives at the corporate level. This does not preclude the possibility of other sets of strategic heuristics at the divisional level in diversified firms. At both the overall firm and division level heuristics may be transferred to other organization by imitation, consultants or movement of managers among firms in the same or similar industries.

Furthermore, to be meaningful to managers, heuristics need to be embedded in the actual strategic decision context of organizations. Bingham and Eisenhardt (2011) identified specific examples of heuristics that are used in the strategic context of internationalization. These heuristics differ from the generic heuristics used by individuals in the Kahneman and Tversky tradition. Eisenhardt and Bingham identified a portfolio of heuristics and sequence in which organizations tend to introduce heuristics. This raises the question of how context-specific should/can be heuristics be to be meaningful to managers over extended periods of time? Generally speaking, how can researchers make recommendations for using heuristics that are useful for managers?

How can managers sense when environmental changes invalidate particular heuristics? How do managers know when and how to select or develop new heuristics if the old heuristics are no longer valid? How can managers assure that important heuristics do not become "unconscious or automatic," perhaps by incorporation into routines, and can no longer be consciously identified and examined for current validity?

Overall, there is no *generally accepted* precise definition of the term "heuristic" and this leads to considerable confusion.[31] Furthermore, decision making is often divided into only two categories, optimization or heuristics. This raises a question regarding foundations – Does this exhaust the categories for decision making or are there other approaches? Are heuristics simply everything but optimization? This seems highly unsatisfactory! What, if any, are other categories for decision making?

DISCUSSION

We close with two brief discussions. First, we discuss the rapidly emerging methodology of Machine Learning and how it differs from the statistical methods that dominate Strategic Management. Second, we conclude with a brief discussion of the socially constructed nature of empirical research in Strategic Management and the impediments it might represent to the future of the field.

Currently (September, 2018) there is a continuing explosion of machine learning technology in businesses, government, the military, and academia. We are also experiencing the increasing presence of machine learning in business school curricula and research. Many

[31] Bettis (2017) suggested the following definition from the seminal book in computer science on heuristics: "Heuristics are criteria, methods, or principles for deciding which among several alternative courses of action promises to be the most effective in order to achieve some goal. They represent compromises between two requirements: the need to make such criteria simple, and at the same time, the desire to see them discriminate correctly between good and bad choices." Pearl (1984:3)

university statistics and biostatistics departments now include faculty with doctoral specializations in machine learning. A few are also showing up on business school faculties and we expect this to accelerate. Many academic experts in machine learning maintain their field will displace a substantial amount of statistical research methods and complement others. Machine learning is usually associated with big data and may represent a fast-emerging strategic reality with enormous power to disrupt teaching and research across departments in business schools.

What we do know for certain is that machine learning differs substantially from the regression type of statistical modeling widely used in Strategic Management. Most scholars in Strategic Management do not understand the stark differences and their implications. Linear regression models and their associated statistics are based in the assumption of an “additive sum of linear terms” as a close approximation to the actual stochastic data generating process.[32] Researchers ex ante develop the exact specification to be used. Regression models are often evaluated by goodness of fit measures and the distribution of residuals. Goodness of fit measures are problematic due to the bias/variance tradeoff. Furthermore, bright line p-value tests are often run to determine the “significant coefficients.” This practice is strongly discouraged by the American Statistical Association (Wasserstein and Lazar, 2016). Finally, asymptotic adjustments (true as the sample size goes to infinity) are often used to deal with some violations of the regression model assumptions. The end result of all this is that “significant” independent variable coefficients are taken to be “reasonably accurate measures” of the influence each independent variable has on the dependent variable while holding the other independent

[32] As we noted earlier in the paper, complex data generation realities, simply cannot fit this model.

variables constant, for the particular sample used. It is noteworthy that "influence" does not imply causality.

By contrast in machine learning there is no assumption of an ex ante data generating process. The researcher does not determine the model specification. Rather, the data supply the model. The model is generated from the data via one or more algorithms. Hence, specification is an ex post algorithmic outcome, not an *ex ante* researcher decision. This *algorithm generated* model of the data is then evaluated on the basis of *predictive ability*. A very readable and short introductory discussion regarding causality and machine learning by a political scientist is Grimmer (2015). One potential application of machine learning relevant to the discussion of heuristics above is the use with appropriate large databases to develop strategic heuristics. A good entry point for those with limited or no prior knowledge of machine learning is the short book by Alpaydin (2016). A very comprehensive and popular applied text is Witten, Frank, Hall and Pal (2017). It avoids being overly mathematical, but is lengthy and detailed. There are also numerous other books and online courses about machine learning.

Overall, the two approaches (linear statistical models and machine learning) are based on very different logics and require the application of very different mindsets by analysts and the users of the results. An interesting article about the differences that is somewhat critical of the statistical approach is Breiman (2001). The authors of this essay are convinced that machine learning will play some role and perhaps a very important role in the future of scholarship in Strategic Management. We are both in the process of acquiring rudimentary knowledge of machine learning at an applied level, before going deeper. We strongly urge other scholars and Ph.D. students to consider doing similarly.

In conclusion we assert that Strategic Management as a field of scholarship and teaching in business schools may have settled into an inappropriate research paradigm that was *socially constructed* based on successes in business and research environments from past decades that are no longer consistent with at least some current strategic realities. The four Strategic Realities we discuss in this essay pose some important and difficult challenges for the current research paradigm in Strategic Management.

The current *social construction* of appropriate research topics and methods that is the driver of our field is naturally elaborated and propagated forward in the content of our Ph.D. programs. The vast majority of what most Ph.D. students study today as research methodology are the various varieties of linear statistical models, how to run them, how to correct for assumption violations, and how to interpret the results. We suggest there are confusions and shortcomings of regression as often taught and practiced within our discipline. We assert that regression is a very powerful methodology, but its applications are not unlimited and the underlying assumptions must be met or closely approximated for it to be sufficiently accurate.

Most, not all, violations of the assumptions behind regression modeling are seen by a substantial portion of Strategic Management scholars and some textbooks and software manuals as *always* subject to various and well established “accurate” estimates or corrections.[33] All published regressions are assumed by at least some Strategic Management scholars to establish some useful degree of causality. Experimental methods, beyond natural experiments, are seldom used, yet can be very useful in establishing causality if carefully and appropriately designed and

[33] Some regression assumption violations have no acceptable corrections (e.g. interdependent data points). Furthermore, the asymptotic corrections often applied vary in accuracy with sample size.

used. (Interestingly field experiments have become increasingly popular in economics The actual theories of sampling, estimation, p-values,[34] confidence intervals, and hypothesis testing usually receive only cursory treatment, though their deep understanding is essential for effective use and interpretation of regression models. Most of what students read in topical Strategic Management seminars are regression studies often with some technical shortcomings, and sometimes with unsupportable claims of causality. Students are strongly advised to concentrate on building databases and running regressions for their dissertations in order to have the best and most employment opportunities. We believe that this is a *reasonable and pragmatic conclusion, for yesterday and today! However, the future of Strategic Management may be rather different when it arrives.* Taking courses or modules covering other important topics beyond regression such as theorizing, qualitative research, model building, experimental methods, stochastics processes, and simulation is rare though they appear to have many important applications in Strategic Management.[35] We suggest that regression studies *properly conceived and executed* are a valuable component of a research portfolio, but they are not appropriate for all topics and all data. As a field, we are not sufficiently diversifying our methodological bets in a rapidly changing and increasingly complex world.

Does research methodology as currently practiced in Strategic Management sound to like a successful and sustainable approach to rapidly changing academic and business environments? If you answer "yes" and you are over 55 then relax, lean back, pour yourself a glass of wine, and

[34] p-value = prob(sample|H_0 is true). This definition is not well-known in many applied fields.
[35] In view of what we said earlier in the Discussion section we suggest it prudent for all Strategic Management Ph.D. students to take *at least* an introductory course on Machine Learning.

check the performance of your retirement portfolio and/or government retirement program. If your answer is "no" and/or you are under 45 then, **… .**

To all who took the time to patiently read and consider this essay – We sincerely thank you for taking the time and effort. We hope it stimulated your thinking in productive ways whether or not you agree with the issues we have raised and discussed. For our part we remain enthusiastic about and fully committed to a future of Strategic Management scholarship.

Das Spiel Wissenschaft hat grundsätzlich kein Ende: wer eines Tages beschließt, die wissenschaftlichen Sätze nicht weiter zu überprüfen, sondern sie etwa als endgültig verifiziert zu betrachten, der tritt aus dem Spiel aus. (Popper, 1935: p. 23)

The game of science is, in principle, without end. He who decides one day that scientific statements do not call for any further test, and that they can be regarded as finally verified, retires from the game. (Popper, English Translation, 1959)

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Figure 1: Managers Face a Reality that is Complex!

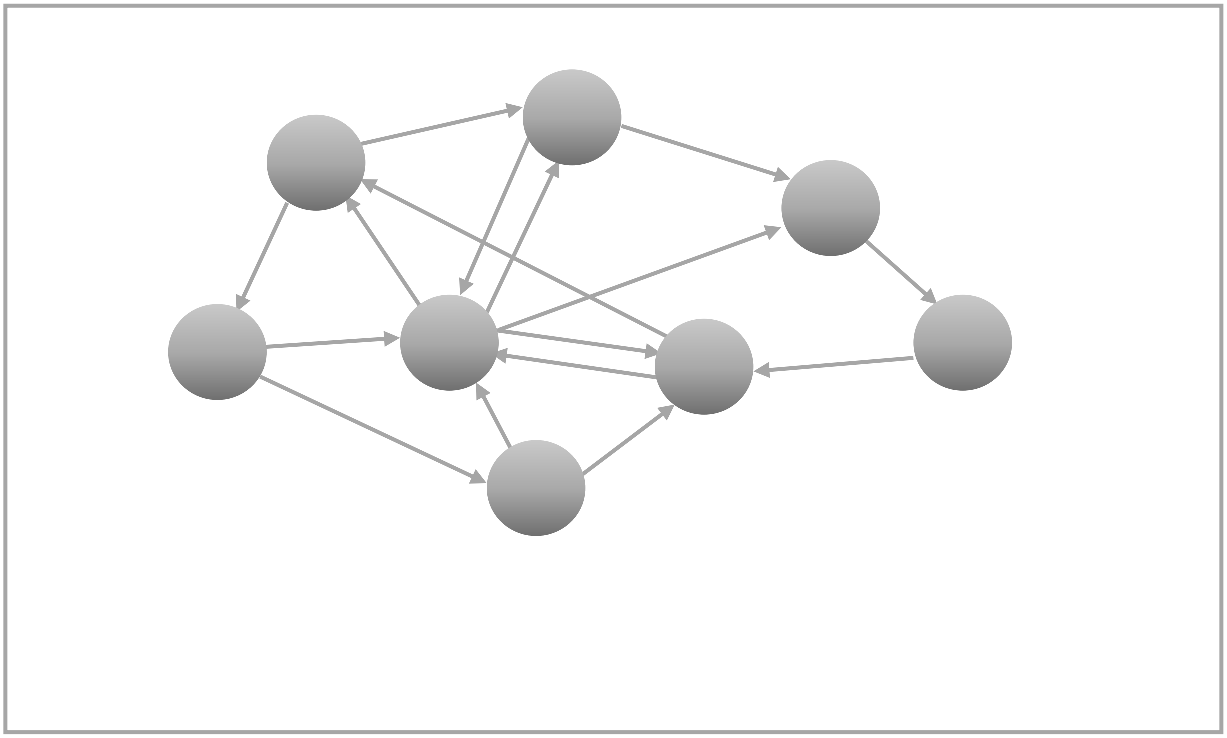

Figure 2: Linear Statistical Model of this Reality Assumes a Very Different Problem