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Conceiving *Perfectible* Theories in Management Through Adaptive Framing

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ABSTRACT

Extant studies of theory evaluation rely on hindsight even though editors' entreaties are meant to be studied *ex ante* and applied in real time. The authors elaborate on the definitional requirements of theory and ways to appraise it. The authors present a synoptic chronology of the main trends in management theory evaluation, and discuss the methodological differences between formal theories and actual management schemes. This discussion leads us to adopt a *constructivist* perspective and replace "Popperian falsifiability" when inapplicable to management. The authors then introduce the concept of *adaptive framing* as a tripartite process subsuming the criteria of novelty, practicability and extendibility through consistency, which the authors argue to be the necessary requirements for perfectible theory-building in management.

KEYWORDS

Adaptive Framing, Falsifiability, Ideal Theories, Management Theories, Real-Time Theorizing, Theory Building, Theory Evaluation

INTRODUCTION

The formal study of management continues to expand worldwide. It is also expanding academically as relatively newer subfields (such as Entrepreneurship, Knowledge Management and Cognitive Neuroscience) are being added to its already rich panoply and pertinent academic societies continue to form. The extant plethora of schemes, principles, models and theories has become baffling. Scholars and practitioner jokingly remark: "It's a jungle out there!" At various times, academic societies have prodded research aimed at taking stock of the knowledge already acquired in its several domains and assessing their respective contributions. This conceptual inquiry aims at taking a critical look at the way theory evaluation and development issues are usually approached.

While the domains of management are becoming more sophisticated, they do exhibit different shades of scientific rigor. According to the degree of multi-dimensional complexity of the respective domains, they can still be grouped into the two clusters described by Snow's (1959) fabled contrast of the *two cultures*, the mathematically oriented one and the more holistically driven generalist one. This differentiation of the subfields of management is not contested; yet management theory evaluators very often succumb to the fallacy of treating the field as homogeneous – and as an integral part of the pure scientific domain. Thus, flattering themselves as formal scientists, management authors often turn for guidance to big-S science's logician Popper rather than more pragmatic philosophers (e.g., Ackoff, 1962; Churchman, 1979).

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Another concern is that, beside editorial advice, most of what budding authors find in the extant literature are dated evaluations of the impact of past intellectual contributions (e.g., Miner, 2003; Colquitt & Zapata-Phelan, 2007). This certainly is a worthwhile part of the beginners' panoply of knowledge elements; but more in line with their present needs would be some indication of how to initiate theory building, as well as the prerequisites for being able to venture into it from a beneficial angle. So, our second concern is the common tendency to provide, for what is basically an *ex ante* or real-time decision process, guidelines fitting mostly *ex post* evaluation. The more beneficial question to pose should rather be: Can sensible methodological advice be provided to help the budding management researcher in his or her quest to devise "good and lasting" theories?

In the first two parts of the following development, we will elaborate on the ideal definitional requirements of theory and ways to appraise it, contrasting the hard and softer sciences, and emphasizing the important role of consistency with a clear axiomatic foundation or, at least, an explicit assumptional basis. In its third part, we will discuss the methodological differences between assessing an extant theory and developing a new one. This will lead us to proposing, in its fourth part, three criteria that would embody *adaptive framing* as a conceptual device that can guide the process of theory building in real time, as opposed to being mainly useful in hindsight appraisals of past theoretical work (e.g., as in Bacharach, 1989; or Miner, 1984, 2003).

HARD-SCIENCE THEORIES & MANAGEMENT THEORIES

The Ideal Concept of Scientific Theory

According to an impactful article in the millennial issue of the AMJ (Carson et al., 2000), management schemes abound, and it behooves researchers to clear a path through them. Which benchmark to use?

The successful probing of the universe's constituents by Einstein, Hawking and other hard-science luminaries has had a trickle-down effect on more mundane research domains. Hard science's capacity to connect and thus explain, by means of *consistent* broad-reaching theories, an ever-expanding range of phenomena (Einstein, 1951; Hawking, 1988), has demonstrated that the general laws of the physical universe (and possibly biology and psychology) can sometimes be discovered. As a result of this stunning progress, the methods of the hard sciences have been deemed exemplary by social scientists, who have tended to define desirable requisites for their theories, and the processes for the betterment of such theories, so as to mimic those used by their natural-sciences brethren.

Philosopher of science Campbell (1953), for example, speaks of a theory as the means by which science explains phenomena, extracts laws from them and, most importantly, predict events from them – thus assuring that laws can be verified. Popper (1959), in turn, emphasizes falsifiability (i.e., potential refutation) as a key element of a theory, which is only tentatively confirmed whilst repeated attempts at falsifiability *fail* to invalidate. Similarly, drawing from these and other philosophers of science, classic management theorist Bacharach (1989) then offers generalizable utility and explanatory validity as the two main criteria for theory construction and evaluation in organization science and management. Utility provides for a theory's combined functions of both explanation and deduced prediction. Explanation establishes the substantial meaning of constructs and variables, as well as the justification for the known or assumed linkages among them. Prediction, in turn, provides for a deductive tie-in to newly proposed relationships, which are to be contrasted to empirical evidence as a test of the theory's explanation.

However, there are problems for social scientists with treading too close to the physical sciences. The major theoretical breakthroughs in the physical sciences do not necessarily originate from collecting data or observations that represent anomalies to current theories, as implicit in most of the editorials of management journals (e.g., Bettis et al., 2014), but from *synthesizing* already established partial models or results by proposing a novel more encompassing logic. The abundant science popularization literature (e.g., Tyson, 2017) attests to this difference, and the management literature occasionally acknowledges this non-linearity, even choppiness, of knowledge accumulation (e.g., Harris, Johnson & Souder, 2013). Another difference is, luckily for the physical or chemical researcher, that the immense universe appears made up of combinations of a relatively small number of primal elements. This is what streamlines research in theoretical physics and allows for the consistent "thought experiments" that guide leading scientific thinkers (e.g., Einstein 1951) to fruitful theoretical syntheses. Finally, and most importantly, the core physical sciences deal with the discovery of unchanging laws of nature (or truths), while the social scientist most often must strive to gain understanding of mechanisms driving socially constructed and shifting realities.

An initial sobering consideration should be that the domain of physical sciences is far removed from the circumstances of social sciences, including the observability problems in management detailed by Godfrey & Hill (1995). In addition, the field of management does not concern itself with ultimate truths regarding the constitution of the universe. Because it is an eminently pragmatic and man-made field, it should be recognized that "managerial facts" differ from incontrovertible scientific facts (Godfrey & Hill, 1995) and can be easily deconstructed (Kilduff & Mehra, 1997). They are incompletely specified and could inadvertently lend themselves to infinite regress (Hallberg & Felin, 2019). Bacharach (1989) himself observes that a problem with organizational theories is that they are often vaguely stated, thus lending themselves to easy dismissal of most discrediting evidence. Similarly, McKelvey (1997) consider them hard to refute because of their subjective epistemology. Although still acknowledged in recent works (Shapira, 2011), Popperian falsifiability cannot be meaningfully laid upon the multi-dimensionality and fluid realities of management, in which mathematical formulations are too restrictive.

Secondly, Popper himself noted (1959) that general statements are not by themselves full-fledged scientific theories. More visibly than in the physical or natural sciences, management research is forked between Dubin's (1978) two paradoxes. It is possible to achieve precision in prediction without understanding the process by which the outcome was produced (the *precision paradox*). It is also possible to achieve an understanding of social behavior without a corresponding precision in predicting the outcomes (the *power paradox*). It should hence be recognized that theorizing in management would differ from theory building in the mathematized hard sciences.

The Need for Theories Specific to Management

While researchers have not come to an agreement on a single definition of what a management theory is, they agree that models and theories play a vital role in the development of our discipline. It becomes important therefore to identify those elements required for or which constitute an ideal theory. Kaplan (1964) maintains that its basic function is to make sense of what would otherwise be choppy and inscrutable findings. Theories serve the function of guiding research while changing the content of knowledge as well as its form.

In discussing the elements of a management or marketing theory, Dubin (1978) considers as main variables the units whose interactions constitute the subject matter of attention. Other potential elements include: the laws of interaction; boundaries; system states; propositions – as logical deductions about the theoretical model; and empirical indicators leading to testable hypotheses. However, Dubin did not require this fifth element to be necessarily present in all theoretical models; only the first four elements need be present to constitute the *formulation* of a theory.

Despite management's multidimensional complexity, there still is a need for theorizing because it helps organizing parsimoniously and communicating clearly (Bacharach, 1989). Sutton and Staw (1995) echo this functional approach in writing that theory concerns itself with connections among phenomena and the why of such occurrences. Whether the intended analysis is statistical or qualitative (Bettis et al., 2014), there is a need for delineating theoretically sanctioned ways of looking at facts, organizing them and representing them for discussion by the research community. It is by its building and assessing of theories that a discipline cumulates knowledge (Turner, 1985). At this broad level, the sought parallelism with hard science holds.

Because theories also play the significant role of encapsulating the stored knowledge of specific domains of inquiry (Turner, 1989), they become the focus of recurrent academic discussions and editorials. Decades apart (e.g., Whetten, 1989; Robinson, 2019), these editorials acknowledge that theory drives the evolution of scholarship in an academic discipline; and delineates a field's boundaries, core questions to be examined and preferred research methods. As Hambrick (2007) puts it even when warning against unbridled expectations, theories provide a framework by which thoughts are organized, coherent explanations generated and predictions, if any, improved. Yet it should be kept in mind that most of these functions, while enhanced by continual or at least periodic evaluations, are not necessarily dependent on iron-tight empirical validation.

THEORY COMPOSITION AND ASSESSMENT

The Crucial Role of Assumptions

Summing up the previous section, despite a lack of precision in the definition and use of "theory", it is possible to identify certain constituent elements relevant to theory-building in management and organization science. Kaplan (1964, pp. 294-295) describes theory as "a way of making sense of disturbing situations so as to allow us most effectively to bring to bear our repertoire of habits, modify or discard them, replace them as situation demands." Thus conceived, the reach of theory building potentially encompasses all aspects of management: the situational (nature of the phenomena), the intellectual (sense-making) and the behavioral (actions that flow from understanding to shaping or responding to situations).

The following constituents of a theory can be identified from Bacharach (1989): constructs and variables; propositions that relate the constructs; hypotheses that relate the variables; and assumptions that establish boundaries for the theory. It is important to note, early on in this discussion, one of the most common fault lines of many theories. While constructs, variables, propositions and hypotheses are usually explicitly stated (albeit not always well defined or adequately stated), the underlying assumptions are often incompletely and ambiguously stated or, worse still, altogether left out. Yet its implicit assumptions strongly influence the way in which a theory's basic concepts are understood, as well as buttress the guiding rationale for propositions and hypotheses. The importance of assumptions *and clear constraints* was stressed by Bacharach, referring to a theory as "a statement of relations among concepts within a set of boundary assumptions and constraints" (Bacharach, 1989, p. 496) so as to reflect the real world.

A few well-defined situations in which lasting formal theorizing *can* be undertaken in management illustrate both: (i) the expendability of empirical falsifiability as an inescapable theory-building criterion, and (ii) the importance of clearly stated assumptions. The following section illustrates how the explicit formulation of its axioms and constraints affect a theory's logical consistency and future extendibility.

Lessons from Formal Theorizing In Management

The interest in probing and controlling quantifiable realities of firms' processes forcefully came to life in the mid-20th century with a two-pronged effort to rationalize management: a simultaneous rise of a formal Decision Theory (DT) embodying earlier theoretical concepts from Economics to optimizing decision making, and the use of mathematical methods for optimizing the use of resources and time in production and distribution systems. Two seminal books encapsulate the two prongs of this new paradigm (in the sense of Kuhn [1962]). Luce and Raiffa's (1957) *Games and Decisions* presents a succinct digest of formal approaches to rational decision making. Coincidentally, in that same year Churchman, Ackoff and Arnoff (1957) introduces the rising field of Operations Research / Management Science (OR/MS) as a means to capture a situation's parameters and constraints by a graphical or mathematical model.

The initial consulting applications had been focused on resolving specific instances of problem situations, sometimes numerically. But very soon, mathematically bent theoreticians joined in and devised abstract algebraic solutions for archetypical situations that could be described symbolically (using letters to express generic cases). Appropriate theories were developed along mathematical axiom-lemma-theorem deductive structures, from which *solution algorithms* were derived, and then made available through consulting services as well as off-the-shelf software. Based on deduction from an axiomatic basis with little empirical input, these theoretical solutions are not inherently "falsifiable", but only require that they not be inconsistently applied where they did not pertain. Whenever correctly applied, whether addressing production or transportation logistics, they are effectively used worldwide. Their diffusion to most industrial engineering and business colleges sets them up as exemplars to be distilled and potentially emulated in broader theory-making in management.[1]

Moreover, DT and OR/MS authors conceive rules to bridge the gap between modeling specific situations and devising broader, generic theories. Their approach to management-scientific optimization entails focusing the theoretical development and ensuing computations on the contextual decision variables, while undertaking the *holistic* inductive effort of making goals, constraints, assumptions and relationships as explicit as possible. Aiming at optimizing what *can* be controlled within the context of surrounding circumstances, this formalism's thrust is the parsimonious use of clarified and quantified components. As we seek to understand the structural reasons for considerable differentials in longevity among theories in management, we will revisit below this matter of how the explicit formulation of axioms and constraints affect a theory's logical consistency and, therefore, its adequate future extendibility.

The Importance of a Well-Constructed Deductive System

One can abstract from the preceding discussion that the building blocks of presumed knowledge, or theory, are propositions of relationships and explanations of the mechanisms causing them. Complicating the communication between theorists and practitioners is the latter group's much dated view of a theory as simply an intriguing hypothesis in quest of verification, while theorists usually describe the observation-induction-deduction-testing cycle in ways that could stand some clarification for the managerial reader (because it is bound to repeat later, as the newly deduced propositions raise novel questions or paradoxes...). This leads Ackoff (1962) to clarify, for both constituencies, that a theory is best understood as a *consistent deductive system* synthesizing the inductive inputs that engendered its axiomatic foundation. His ideal deductive system comprises: a set of concepts (defined and undefined); a set of axioms, postulates or assumptions; a set of results or theorems ("laws") deduced from these assumptions; and, finally, instances of actual or potential occurrences or applications of those results. Validation itself is not formally included at this conceptual stage, but left as a desirable property for a later stage.

Acar (1988) draws from and expands Ackoff's (1962) presentation of the desired constituent elements of an encompassing theory. He explicitly stresses two integrating elements that contribute to theoretical consistency. First, a theory can capture some of the richness that language can provide by spelling out its *nomenclature* in some detail. Second, one can buttress the internal consistency requirement of sound theory building by carefully designing its *rules of logic*. Formal theories often use mathematics both as a language and as the rules of their internal logic for deducing theorems from their axioms. However, in political and social sciences, a mathematical formulation is seldom possible and hence is not a requisite.

To fix ideas, let us consider two of the most imposing theories developed for resource management. DT uses a minimum of specific vocabulary added to its usually mathematical mode of expression; on the other hand, in addition to its borrowings from financial economics and other disciplines, Accounting Theory has developed its own specific language and rules of logic. Moreover, contrary to the cautioning adage of "paralysis by analysis", enriching a theory by means of Ackoff and Acar's inclusion of language and rules of logic (in their conception of theory as an intellectual edifice) does not preclude eventual testing of its most salient results (Kerlinger, 1964), whether in management or any other discipline. To cite but one example, Sirgy (1988) describes how the above ideas, initially developed for the domain of human enterprise, can be adapted to the study of biology as well.[2]

As mentioned before, in this conception of a theory as a consistent deductive system, empirical validation is not posited as a constituent of the theory itself, but as a *desideratum*. This neither means nor implies that later verification would be unwelcome; when it finally comes, the theory becomes *confirmed*. To the extent that predictions from the theory can be contrasted with empirical evidence in ways that are informative, future learning will be aided and the theory will be made more perfectible.

Nonetheless, rigid dogmatism regarding strict falsifiability may become counter-productive. There are some discussions of informative content of theories in the works of Popper (Thornton, 2007). A theory with high informative content could offer a great number of ways in which it may turn out to be false, as it will result in precise and specific predictions, possibly resulting in a lower probability of being confirmed than less informative ones. Is there a paradox? More often than not, however, it offers a greater number of ways in which it may be improved over time, as informative and insightful evidence accumulates. Overall, only if falsifiability is prematurely emphasized at the expense of conceptual development would learning be impeded. Such delays may not turn detrimental if, in the meantime, the theoretical edifice is continually being probed by *counter-factual scenarios* (Durand & Vaara, 2009) to assess whether the lack of fit with reality is due to an unrealistic axiomatic basis or just faulty derivations from it.[3]

It is important to note that the above does not intend to compare deductive and inductive approaches to the detriment of the latter.[4] A misunderstanding of the 18th debate between rationalists and empiricists has created an artificial semantic distinction between these intertwined theory development processes. Deductive systems do not pop up *ex vacuo* as a palliative to the need for sound and well documented empirical attempts at validation. Our understanding of a theory as a conceptual, deductive system neither denies its original grounding observation and culturally generated assumptions, nor rules out subsequent inductive inference following empirical work.

The point of stressing the compatibility of deduced results with the underlying assumptions and framing constraints, is to clearly flag *a priori* the areas of most likely inapplicability; consequently, a deductive system may serve as the framework for an inferential program. By fixing ideas and providing a means of explicitly couching them, it becomes primarily a framework to guide one's thinking that facilitates learning and communication.

Theory in the Trenches of General Management

Business and military situations comprising mainly spatial and resource variables, and which can be abstracted from human affects or latent feelings, have proven to be successfully prone to formal analysis and sometimes optimization by OR/MS methods as predicted by Simon's seminal work (e.g., Newell & Simon, 1972). In the last few decades more effective techniques and computerized algorithms have routinized standard allocation and search tasks, and this discipline has all but migrated from business schools to industrial engineering departments. Those algorithms' validity itself is no longer an issue, and current counter-factual scenarios now mostly probe *refining* their accuracy and computational sensitivity to changing conditions.

However, this is not the case in general management where the activities and relevant phenomena, human, economic and spatial domains overlap in complex and sometimes hidden ways. As a result, in most common managerial occurrences, the requisite sense-making is far less orderly and more variegated. In such a context, Dubin himself (1978, p. 5) acknowledges that a relaxation of formulaic methodological strictures or even formal structures can be more conducive to seeing order amidst "booming bustling confusion that is the realm of experiences."[5] In the trenches of real-life management what is commonly sought is explanation rather than prediction. In accordance with Tsang's (2006) argument, the relevance of the underlying behavioral assumptions then becomes paramount, and formal modeling potentially leading to falsifiable predictions takes the backseat.

Gibson (1960) supports a loose yet rich perspective by honoring as theory any individual or system of logically interconnected general statements. Thus, a mere *classification* can be the initial embryo of a future theoretical development (Bateson, 1972). This broadened meaning would generously cover the myriad of explanatory schemes offered in the literature for the multitude of phenomena being queried. For example, Ocasio, Loewenstein and Nigam (2015) illustrate how just changing category conventions may modify institutional logics. Less striking and more pervasive is the abundant use of typologies in the social sciences; Snow and Ketchen (2014) argue that a typology is often the first step in what promises to become later a fruitful theoretical quest. While some facile typologies have quickly outlived their usefulness, other similar theorizations have been adopted by practitioners and so remained in use for a long time.[6]

Nevertheless, bucking the trend toward increased pragmatism and practitioners' calls for greater initial relevance, caveats have been raised in prior treatises on theory building and evaluation in management. The concern is that not any conceptual framework labeled as "theory" may indeed be a theory. For example, Bacharach (1989) distinguishes mere descriptions of objects and events being investigated, especially categorizations and typologies, from full theories. He proposes that simple categorization of data is not theory, whether qualitative or quantitative; hence much of the work in the subfields of management should not be perceived as theory building. Similarly, Sutton and Staw (1995) point out that the following parts do not (by themselves) constitute theories: references to research findings, data or observed patterns, lists of variables or constructs, diagrammatical representation of relationships, and predictions of what effects might occur with no explanatory hypotheses. Despite similar calls for greater purity by Turner (1989) and others, before reforming it we must consider the reality of what goes on in the trenches of the hard work of gradual conceptual creation in management.

Indeed, seldom do actual theories arise bedecked in their final form the brains of their founders. The axiomatic theory of "purposeful systems" of Ackoff and Emery (1972) may be one such rare instance. More often than not, as documented by Whitney-Gibson and Tesone (2001) through five examples, a multiplicity of pre-theoretic fads and schemes percolate freely from managerial or consulting practices. Most are not devised by means of a single thought experiment or a leap of intuition; rather, they are extracted from beneficial occurrences deemed worth repeating. During lengthy incubation periods of varying length, following Glaser and Strauss (1967), behavioral researchers "ground" themselves in collecting detailed data and clustering them into patterns, often presented as classifications or typologies. Distillation of those schemes whose apparent usefulness survives gradually migrates them from the status of "fad" to the more respected status "representational model" in the sense of Harris et al. (2013).

As mentioned earlier, the vagueness and informality of formulations make it illusory to believe that rigorous tests could be devised to firmly infirm (or confirm) most social science theories. The field should then recognize that its stress on falsifiability as a *definitional* requirement of theory is, in most cases, a misplaced shackle. Our next section will discuss shifting our focus from hindsight-based thinking to assisting the would-be theory builder with advice usable in real time.

THEORY APPRAISAL IN MANAGEMENT

Ex Post Outcome-Based Theory Evaluation

Research publication being a primary channel of knowledge development and dissemination, the number of citations is commonly used as a proxy to evaluate the contributions of individual authors as well as the universities employing them. Because of this double-duty toward academic fame, citation frequency has become the principal driving force behind social science and management research. The occasional ethical *caveat* notwithstanding (e.g., van Wesel, 2016), the proponents of governance by citation count point out that such a system ought not to be considered mechanistic because it encapsulates the choices of countless individuals.

The information revolution of the recent decades has generated an exponential increase in digitized databases and a matching increase in search and classification tools (Rubin, 2007). While admittedly impersonal, they nonetheless provide a tempting first pass at a number of formerly very laborious (and hence rarely attempted) queries. As a case in point, *author co-citation analysis* (e.g., Nerur, Rasheed & Natarajan, 2008) can help detect the structure in which the various fields of a discipline have coagulated. Such hindsight understanding of its structure is bound to be beneficial to most of the discipline's researchers.

Much more common is the straight citation analysis, used alone or in combination (e.g., Ramos-Rodriguez & Ruiz-Navarro, 2004) to document the cascade of influences among published works. The widespread and unquestioning reliance on citation counts seems an easy solution to the complex task of theory evaluation. But it has a downside, having generated a couple of intertwined spiraling syndromes. It is hardly a surprise that the top journals are located in the most prestigious universities, which usually favor submissions from peer institutions. Because these eminent universities can well afford to offer low course loads and generously fund research and related travel, they attract those scholars with the greatest potential (Morgan et al., 2018). Their opportunities for exponential growth in their citation dynamics increase; they gradually become budding stars; and neophyte researchers from less resplendent institutions wisen up to becoming their (citing) disciples. Thus, the self-reinforcing reputations of universities, journals and authors constitute three spiraling syndromes. Mutually reinforcing as well, they become potent drivers of citation dynamics, likely more effective than the true underlying value of the insights being cited, not unlike rising theories' self-fulfillment dynamics described by Ferraro, Pfeffer and Sutton (2005). In addition, it is usually ignored that mature, more populated research topics tend to generate more citations than newer areas of inquiry.

Now and again calls come for taking a less mechanical look at the results of our decades of subsidized research. Occasionally, questions have been raised concerning the quality of research as reflected in journal ranking (e.g., Singh, Haddad & Chow, 2007), but no author has gathered the voluminous evidence needed to dislodge the intuitively satisfying view that citing prior work, the wellspring of *most* knowledge development, may well be the main sources of *all* of it. One such effort, a five-decade study of the *Academy of Management Journal* (Colquitt & Zapata-Phelan, 2007), reveals an upward trend in theory building and testing within articles, indicating that its editorial policy is apparently bearing fruit, and providing partial support to the current reliance on citations. On the whole, the formation syndromes of the dominant theoretical strands can be documented by monitoring the march of citations, and a strong case against the convenient use of citation counts has yet to be made despite its visible weaknesses.

Still, examples of the silos resulting from the clustering of citation networks into separate bubbles can be found. For instance, methodologists Ackoff (1962) and Kerlinger (1964) are known to two rather disjoint constituencies: the former is held a leader within the quantitative OR/MS paradigm described earlier, and the latter a leader within the general behavioral and managerial domain. Consequently, while parsimonious of academic efforts and resources, the current over-reliance of citation dynamics as a form of theory evaluation is not encompassing. It might also not be very effective because it is a contributing factor to the parceling of knowledge into increasingly divergent strands.

Ex Post Attribute-Based Theory Evaluation

The painstaking evaluation of panel-nominated extant theories by Miner offers an enlightening contrast with the convenient appraisal by citation. Although aware of how the reputational and networking factors described in the previous sections contribute to importance (1984, p. 300), Miner goes to the heart of the matter as a sentient scholar not shackled by semi-mechanistic schemes. He directly homes in on cross-tabulating his own estimated ratings of *scientific validity* and *usefulness in application* of existing management theories. As a partial validation from his contemporary authors, while his usefulness criterion differs from Eisenhardt's (1989) "richness of explanation", his validity criterion is similar to her "fit to empirical data". Running over several decades (1984, 2003), his work is an exemplar of attempting to assess directly the qualities of theories. His two articles yielded richer feedback to authors and researchers than the available citation counts, and resulted in a substantial impact on subsequent research in general management.

Yet some ironies and paradoxes fall out of Miner's (1984, 2003) work. The most glaring (and potentially discouraging) is that his approach to theory evaluation cannot be undertaken in real time, but is perforce *ex post*.[7] The second paradox relates to subjective measurement and illustrates the tradeoff that appears unavoidable; it becomes one more impediment to the idea of applying real scientific validity in the social sciences. An exacting yet prolific researcher in psychology applied to management, Miner's own work is highly respected. Still, would his *estimates* of the scientific worthiness of theories from other research streams or disciplinary traditions qualify as reliably unbiased scientific measurements? His ratings being a single rater's assessment rather than an observed outcome taint his ostensible use of chi-square tests.[8]

To better evaluate Miner's (1984, 2003) contribution to management thought, let us ponder on the impact of his 1984 finding of a poor correlation between the validity of a theory and its usefulness…. Minor surmised that management theories were incompletely formulated and had not yet established themselves. Did his personalized study shock the field into action? An implicit recognition was borne in time as the field moves on toward greater specificity, and theories are gradually departing further from their near-truisms beginnings. Thus, despite arguable shortcomings, Miner's (1984) bi-dimensional theory evaluation framework had a positive impact.

Evaluative works such as his are at once an exhilarating and sobering necessity. However, they are structurally hindsight-oriented, and presume that the field progresses in the orderly and logically sequential fashion in which polished theories crystallize and are eventually presented. What is more imperative is that some advice be given *in real time* to the budding theorist. There is, of course, a wealth of advice provided by editors in notes that articulate the goals and policies of their journals regarding worthwhile theoretical contributions, with the intent of preventing unnecessary and costly feedback loops through the review process. Nonetheless, editorial recommendations tend to appear multiple and possibly divergent. As we proceed with our discussion, we will develop a compatible trio of criteria based on a unified process of *adaptive framing*. We will offer it as an encompassing standard for appraisal in real time and, thus, for effective theory building.

Theory Appraisal in Real Time

Are there more direct ways to evaluate theories than appraising the fame they have gathered? The preceding sections suggest that, to the degree that a theory has an axiomatic basis, its logical *consistency* can be appraised and, to the degree it is highly focused and specific, its predictions will be more testable and practical validity more verifiable. The good news is that numerous extant theories appear to have a clear axiomatic basis, if not always a complete one. Motivational theories appear to fall in this category, ranking high on the axiomatic spectrum, and do not generally involve a plethora of confounding social, economic, production- or resource-based variables.

At the opposite end of the axiomatic spectrum, some behavioral theories may not have a clearly expressed assumptional basis, and thus be inadequate as deductive systems that can be investigated for logical consistency – a sort of "deductive reliability check". Even lower on the logical consistency spectrum, we find fashionably novel and quasi-intuitive popular notions, whose ambiguity is initially appealing but soon proves to be shifting sands on which little rigor can be built. Closer to *fads* or "old-wives' recipes" than models or theories, these are generally accorded dubious credence and are applied with caution. Less trusted but more used than established theories, the appeal of their novelty is such that they often guide practice – albeit under a *buyer beware* label. Hence fads are a manifestation of social dynamics. They do not remain still but, subject to the dynamics of scrutiny and revision, some become anemic and vanish. Most others, over time sanitized or enriched, gradually drift upward in the spectrum toward greater acceptance, and become partially formed and accepted theories (Carson et al., 1999).

Because of its emotional appeal, more problematic is a separate category made up of culturally inherited but dubious premises that are confused with *bona fide* theories resting on a clear axiomatic foundation. It comprises conceptualizations based on near-truisms, such as "resources and skills' availability determines one's action potential" or "institutional and environmental constraints determine the domain of feasible action". If examined closely, such "theories" reveal enough overlap with the unstated postulates of today's knowledge to be masking tautologies; as such, they could be considered unprovable "primitives of human thought." This does not mean that they cannot serve as guides for practice – only that they readily lend themselves neither to consistency verification nor testability for failure. The logical consequence is aligned with our present misgivings about the reality of falsifiability, yet startling for the social sciences, in which citing (and building on precedent) is granted the same respect as theorem-proof validation in physics.

Still, efforts need be made to understand the theory-to-practice gap by reflecting of the notion of "quality in theory building" (Baldridge, Floyd & Markoczy, 2004). In contemporary marketing thought (e.g., Kotler, 2000), quality is a bidimensional concept, comprising two clusters of attributes: the solidity or durability features, and the excellence or luxury features. Could this accepted duality be translated into a strategy for evaluating theories independently of the self-propelled dynamics of the citation epidemic?

Falling close to this aim, and illustrating where this discussion is headed, is Eisenhardt's (1989) perspective. She pays the requisite homage to the popular search for *novel insights*, which she complements with evaluating a theory by the dual criteria of *richness* of its account (of the relationships between its constructs and variables) and the degree to which it provides a *close fit* to empirical data. Our following sections will focus on the three compatible criteria we propose for realizing adaptive framing.

THREE CRITERIA FOR THEORY DEVELOPMENT

The Novelty /Originality Requisite

Among the *ex ante* theory evaluation criteria discussed in the prior literature, the most prominent for the first step of diffusion, namely publication in a visible outlet, is the "novel insights" or "contribution to the field" sought by most authors (e.g., Di Maggio, 1995) and journal editors. One could go back decades and sample major editorial recommendations; this is a recurring leitmotiv of their advice.

For example, editorializing for the AMR, Whetten (1989) lists seven criteria that cluster into three categories: Is it new and timely? Is it well conceived and written? Does it matter? It is worth noting that novelty is his first-listed requirement. Similarly, in their "AMJ Editor's Forum", Bartunek, Rynes and Ireland (2006) recommend importance /impact and validity /quality as principal criteria; but they do emphasize capturing the reader's attention by challenging one's assumptions in the manner of Davis' classic (1971) plea. Editorializing for the SMJ, Bettis et al. (2014) move in their first page to explaining acceptable variations on creating new theories (namely connecting, modifying or extending existing ones).

A contemporary example can be gathered by Robinson's (2019) presentation of the editorial mission of the still relatively new *AoM Discoveries* journal. She devotes a significant portion of her initial paragraphs to communicate those themes or approaches considered novel enough to be fit for journal submission. The ubiquitous presence of the novelty /creativity /originality /freshness criterion makes it impossible to ignore – It is so regularly implied that it usually goes unstated.

Yet novelty alone does not sustain a theory. To cite but one instance, McKinley, Mone and Moon (1999) capture the merging thrust in the literature by following novelty with the two substantive criteria of *schematic continuity* and *scope* or potential for pragmatic outreach. Closer to us in time, beside the requisite *originality*, Corley and Gioia (2011) similarly propose the related measures of *scientific utility* and p*ractical utility*; their influence is being reflected in the following sections.

Practicability within the Torrent of Theory Creation

In addition to the novelty requisite, models compete in the marketplace of novel ideas based on the managerial need for practicable theories, namely those offering ways by which organizational skills and resources can be used to solve critical issues (e.g., Lengnick-Hall & Wolf, 1999). Every one of the four editorials mentioned earlier stresses the generation of usable real-world knowledge as a *sine qua non* criterion. As a matter of fact, in our first-listed editorial, Whetten's (1989), can be found the priceless pithy judgement "who cares?" In practice, as long as such a scheme or model continues to appear helpful to consultants and/or managers, organizational learning processes will gradually take notice and acknowledge it as an "incipient theory". On order to address the realities and practicalities of business life, a frequent source of sharpening theories is the *feedback* provided during their early unfurling into the academic and consulting spheres.

However, another source of robust design resides in the *creative ability* of the initial theorist to perform thought experiments regarding practical applicability. As outlined by Dubin (1978), potential paradoxes may arise regarding whether to grow the incipient theory in the direction of sharper prediction or better explanation. In the context of Strategy research, Tsang (2006) approaches this syndrome from the angle of congruence between current goals and initial assumptions. In these and other likely cases, some authors' ability of devise counter-factual scenarios (Durand & Vaara, 2009) provides them with a means to investigate *ex ante* problems of longevity and, most importantly, fit with real business situations and their plausible implementation difficulties.[9]

With the increased sophistication of statistical templates and algorithms that can assign, to even moderately-sized data sets, a hard-to-decipher "structure", the need has become pressing for theories to appear to work in practice in ways that can be visualized and hence "explained" to others. For this reason, as well as those mentioned earlier, we should look beyond the inappropriate analogy with the clarity of hard science. Where to look? What seems to matter most is that researchers continue to have a feeling for what they do – and thus continue influencing society by being committed to it. Increasingly, researchers and editors subscribe to *constructivist* philosophy (Mir & Watson, 2000), in which the researcher does not pose as value-neutral but acknowledges circumstantial and ideological influences. In other words, it may be more appropriate to recognize that management researchers *excavate* rather than merely discover.

For example, related to constructivism is the application-seeking pragmatism of the developing field of Knowledge Management (KM), that links information sciences to management theories. Initially, it was based on Polanyi's (1966) point that even non-interventionist researchers should recognize they tacitly taint their work with their own perceptual biases and measuring instruments. Under the label of KM, Nonaka and Takeuchi (1995) devised from this originally scientific perspective a practical approach to human capital management for improving organizational memory. For a while, Nonaka's KM became a cherished pillar of organizational learning, as employers found it beneficial to encourage the sharing by experts of their formerly tacit knowledge.

By now, though, KM's scope has outgrown the mere milking of experts. Its central thrust is to study the gelling of observations into schemes to be documented, by describing the gradual processing of them into coded data, potential information into documented information, its own transformation into knowledge leading to greater organizational understanding (Acar & al-Gharaibeh, 2019). Both this central thrust of KM and Nonaka and Takeuchi's (1995) expert management strand relate to constructivism, and to a practice-driven approach to knowledge development. This is also similar Corley and Gioia's *practice view of knowledge generati*on, whereby theoretical development is seen as a "recursive dialogue between theorists and reflective practitioners" (2011, p. 23).

Yet, just seeking potential usefulness for practitioners does not guarantee reaching it. Reviewing the preponderant use of big-data algorithms for organizational learning, O'Neil (2016) discusses the instability of their results, and shows how nefarious they could become when not closely monitored and judiciously applied. Hence, theory developers' concern should go beyond just reaching for possible practical usefulness; they should further consider the degree to which practical applications of their theory are feasible in view of resource availability, institutional circumstances and legacy constraints. In light of this, we denote our second theory-building criterion as practicability rather than mere "practical usefulness".

Complementing Novelty and Practicability: Extendibility

Unlike their OR/MS colleagues, whose profession facilitates explicit listing of all situational constraints because of its frequent use of condensed quantifying conventions, general management consultants do not fall into holism automatically, but must consciously undertake the cumbersome labors involved. Luckily though, however crowded, their competitive field is not so dark as to preclude knowledge of the main guiding conceptual trends.

One of the main philosophical trends of our time for guiding applied research is the *systems approach* (SA). Seemingly proceeding from multiple origins, SA has become a convincing embodiment of the need for holism. Epistemologically, it advocates making use of several ideologies, theoretical perspectives or fields of study (e.g., Churchman, 1979). Operationally, it promotes taking stock of the entire panoply of resources, goals and constraints before embarking on "solving" a task situation by sub-optimizing it (Churchman et al., 1957). In most situation, whether behavioral or resource related, thinking along SA's lines has become *de rigueur*.

A helpful manifestation of it in the realm of social sciences is Goffman's (1974) *frame analysis*. Frames are often found within the narrative account of an issue or event and operate in four crucial ways: they define problems, diagnose causes, make moral judgments and suggest remedies. This latter aspect creates a connection between Goffman's sociological perspective and decision-making processes, and should be approached as a portent of what the thinking seeded by frame analysis can accomplish. Because the framing of an issue or problematic situation determines the way it will be viewed, contemporary analysts work on framing all issues in accordance with SA before turning to modeling them to devise solutions. According to Acar and Druckenmiller (2010), in the short-cut parlance of theory builders and consultants, "problem solving" activities are being subsumed into the larger approach of *situation framing*. Likewise, we propose that authors must view theory building as a derivative or even an affiliate of framing.

Our intellectual landscape is no longer one of tranquil waters in which the odd theory has been leisurely floating for some time, while repeated attempts at testing it through falsification have had ample opportunity to home in. Although this is the scenario one gathers from reading most general management methodologists, it is quite dated. Substantially more accurate is the description, given by Harris et al. (2013), of a tumultuous competition between representational models swimming upstream toward theoretical acceptance. These authors describe three streams cascading toward enrichment and timely use: some models are more contextualized to become more deeply embedded into specific real-world aspects; others are grafted onto newer contexts and thus increase their scope; others yet are being reformed into a different slant. As aptly expressed by McKinley et al. (1999), social science is moving from testing its theories retrospectively to examining them in real time. The point is that as the socio-economic context varies, knowledge keeps on the move and theories must remain in an adaptive mode. Hence, theories will be lasting and thus perfectible, only to the extent that they are embedded in an adaptive framing process.

This syndrome has not been lost on the journal editors we cited earlier. Their requirements do not stop at timeliness appraisal but also encompass gauging whether the theory is logically extendible. Under a "so what?" rubric, Whetten (1989) asks whether a path could be found for remedying past deficiencies; and under a "why now?" heading, he asks whether the theory can advance current discussions and stimulate new ones. One of the six criteria of Bartunek et al. (2006) is whether the theory is *extendible* backward to synthesize previous theories or forward to devise new ones. Bartunek et al. also report results from a Brazilian journal's survey of its editorial board members; in it, "stimulating new empirical or theoretical work" is found to be the primary reason for rating an article as interesting. Bettis et al. (2014) reflect that nowadays much of the growth of business and social activity stems from new markets and sources, and may involve lagged effects; consequently, they recommend *broadening* traditional research venues and including organizational and experience learning. Finally, writing for the *AoM Discoveries* journal, Robinson (2019) naturally encourages studies of new contexts or emergent phenomena – and painstakingly lays out ways for former knowledge to be parlayed by extension into new discovery work.

Extending theories is beneficial because it provides further theoretical development as well as broader outreach for extant research. In addition to the merging and transformational ways described by Harris et al., it can be pursued because there are core and peripheral (subsidiary) assumptions (Tsang, 2006). Differences in secondary assumptions may account for differences among the target groups themselves, or just the framing of them, and relaxing some assumptions may provide a separate framing on which to narrate and build. Also, the possibility of applying some elements of a theory to a different level of analysis contributes to its extendibility (Klein, Dansereau & Hall, 1994); thus, a micro-level theory may be extendible to the macro level. Hence, we propose that the third requisite attribute of a robust and lasting theory is a broad potential scope of application through adaptive framing. We denote this as the theory's *extendibility*. This goal can be removed from the domain of wishful thinking and be legitimately pursued as discussed below.

What of the Need for Scientific Rigor?

Is that invigorating quest for ever larger reach and scope potentially fraught and misleading? For example, it has become noticeable among Strategy scholars that the richly complex treatment of resource-based theory by Amit and Schoemaker (1993) has reached lesser visibility, and presumably impact, that its sketchier and streamlined earlier presentation by Barney (1991). Does this mean that the price of practicability is simplicity and the price of extendibility, sketchiness? Would the model builder be better off avoiding more insightful and sophisticated schemes in order to promote practitioner applications? And would s/he be better off rushing out a half-baked theory deliberately to make room for a multiplicity of subsequent debates and improvements? Appropriately directed methodological and empirical research might be able to establish, or at least suggest, class-specific answers.

A proliferation of avenues grounded in situational specifics may appear gratifying; but what of the need for rigor? How to salvage general validity when mired in varied specifics? Resigned to the fact that, contrary to hard science, Popper-style falsifiability in management research may be unreachable, we surmise the aim of the management theory builder must be rigor emanating from one of the conditions necessary (but not sufficient) for falsifiability, and which should be unaffected by the challenges to measurement and empirical validity in the social sciences: *logical adequacy* (Bacharach, 1989). As illustrated by the examples of OR/MS formal theory building described in our earlier sections, the absolute requisite as well as principal indicator of logical validity is the (sometimes mathematically ensured) consistency among all elements of the theory, starting with clearly stated assumptions and axioms.

To be a fair candidate for insightful practical applications as well as well-reasoned and purposeful logical extensions, a formal theory must first have been logically conceived – in other words, it must be a consistent deductive system. Without consistency, in the muddy trenches of actual management theorizing, practical applications and conceptual extensions of the theory may abound, yet not truly contribute to knowledge creation and accumulation. Witness is the time it takes for mere truisms at one end of the credibility spectrum, and patently absurd fads at the other, to lose popularity.

Would a *constructivist* stance (Mir & Watson, 2000) offer better guarantees or at least provide substantial relief? As the torrent of incomplete theory building keeps gushing forward, academic journals promote learning by encouraging critiques and debates among scholars. The memorable RBV (Resource-Based View) debates between Barney (2001) and articles by Priem (2001), Makadok (2001) as well as Priem and Butler (2001a; 2001b) are a case in point. Scholarly debates usually focus on matters of internal inconsistency as well as the compatibility with external counter-factual scenario tests as described by Durand and Vaara (2009). Hence consistency is a necessary condition of the (effective) extendibility of theories, as it sets the conditions for a theory's adaptivity to changing conditions and new knowledge creation over future time periods. In the ideal case, therefore, the two notions are logically paired; so, we could also call this third and last criterion *extendibility /consistency*.

CONCLUSION

Walking through the concepts of theory development and validation, we have indicated differences among the hard sciences, formal theorizing in management and common theorizing in management. Because of the speed of change in today's business landscape, the parallels and analogies usually implied among them are at best loose and more often misguided. An important case in point is the ritualistic false claim of applying Popperian falsifiability where it neither pertains nor can be strictly applied to the study of constantly fluid management phenomena.

So far, ours is a minority view, but by no means an isolated one. For example, Miller and Tsang (2010) build on a *critical realist* perspective to advocate for a move away from strict "falsificationism" in strategy research and towards a more "modest" perspective on verification and falsification. Also, Shapira's (2011) starts with a definition of social theory that includes the traditional falsifiability requirement but ends by proposing "meaningfulness beyond statistical significance" based on *corroboration*, even though one should be clear that corroboration is not proof. In proposing an adaptive framing process, we go further than Miller and Tsang: in effect, we are adopting the *constructivist* perspective of Mir and Watson (2000) that views researchers as creative actors in real time, not mere after-the-fact observers.

We thus suggest that Popperian falsifiability must be downplayed in management theory in favor of a less ambitious but essential conceptualization of scientific rigor based on axiomatic clarity and deductive consistency. We believe our perspective to subsume Shapira's (2011), as it guarantees that the theory will undergo more purposeful adjustments and extensions as its adoption grows, thus resulting in more effective cumulative learning.

In addition to the ever-popular criterion of novelty /originality (DiMaggio, 1995), we have argued that the velocity of change in the world requires management theorists to be guided in real time rather only through lagged feedback loops. This has led us to point out the merits of two other criteria as requirements for meaningful and relevant theory building in management: *practicability* and *extendibility*. These features are being proposed as a potential for healthier growth of the theory over a longer time period, hopefully generating a healthy cumulative literature. They create the conditions for the theory to be embraced by both practitioners and scholars, as well as for it to remain informative and adaptive to changing business and social realities in future time periods. We also propose that the three above criteria (novelty, practicability and extendibility) are complementary of each other and can thus be conceptualized as the three pillars of a unified, all the while tripartite, *adaptive framing process*.

Nevertheless, some pitfalls remain. In particular, Dubin's paradoxes have shifted into our proposed framing but have not disappeared. Also, as it is the case for any conceptual proposition, our contribution would benefit from further development and empirical testing to operationalize its concepts. In the meantime, it is to be hoped that academic societies will continue to offer venues for honest and, yes, animated scholarly debate.

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ENDNOTES

1 How to proceed? Methodological philosophers warn against losing the holistic perspective by splintering it into sub-issues. For instance, Kaplan (1964) urges keeping sight of the conceptual, contextual and pragmatic aspects in every research inquiry, and Tsang (2006) points out the centrality of behavioral considerations to the foundational assumptions. The methodologist, then, must conceive rules to bridge the gap between modeling specific situations and devising broader, generic theories.

2 This potential outreach from management science to biology shows that the melding of theoretical and pragmatic sides derived from Economics into OR/MS can be imported into other domains – thus reflecting the adage: “there is (should be?) nothing more practical than a good theory” (Lewin, 1945).

3 For example, initially trusted mostly due to their logical consistency, OR/MS methods are now accepted and deemed verified because of their plethora of successful implementations.

4 Nor deemphasize the degree to which their mental activities are often intertwined in their details.

5 The reality is even more prosaic to the point of confusion. A salient instance of theoretical overreach was exemplified by Keys and Miller (1984) in their classic identification of the superior traits of Japanese management in the 20th century. To drive the point harder, they apply the term “theories” to *different explanations* of the apparent superiority of the Japanese system of management….

6 An anecdotally supported paradoxical view has at times gained credence. It suggests that easily understood and easy to apply simplistic schemes outperform more elaborate ones. This blithe statement, however, covers a variety of separate cases. It could be that the simplistic theory happens to be grounded in some very specific situation – in which case it would not fit to others. It could also be that it delivers very rough

estimates valid for initial intervention stages – yet inadequate for deeper probing. Either way, a simplistic theory should be first corrected or improved before any attempt at extending it is undertaken.

7 Miner defines *validity* as the extent to which a theory has been supported by *subsequent* findings. He defines usefulness in practice as the *ex post* extent to which a theory has generated "highly viable practical applications", as opposed to the potential of the theory to generate them.

8 Unsurprisingly, Miner finds psychology to be the best theoretical wellspring for management. Coincidentally contributing to Miner's prior work being deemed scientifically valid is a condition that facilitates formal theory building: as in physics and unusually among the domains of management inquiry, the realm of motivational studies has traditionally involved far fewer variables than the typical management study that may *also* contend with resource availability, competitive forces, regulatory and corporate social responsibility issues.

9 This visualization of future practicality is by no means a foregone conclusion. The torrent of changing fashions, emerging schemes and congealing theories has become so rapid that validation as in the hard sciences (prediction leading to falsifiability) has become untenable, and has to be replaced by a more practicable learning-based process.

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