Genetic argument and new knowledge creation in strategic management

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Abstract

Purpose – The purpose of this paper is to present a framework of ideation pathways that organically extend the current stock of knowledge to generate new and useful knowledge. Although detailed, granular guidance is available in the strategy literature on all aspects of empirically *testing* theory, the other key aspect of theory development – theory *generation* – remains relatively neglected. The framework developed in this paper addresses this gap by proposing pathways for how new theory can be generated.

Design/methodology/approach – Grounded in two foundational principles in epistemology, the Genetic Argument and the open-endedness of knowledge, I offer a framework of distinct pathways that systematically lead to the creation of new knowledge.

Findings – Existing knowledge can be deepened (through introspection), broadened (through leverage) and rejuvenated (through innovation). These ideation pathways can unlock the vast, hidden potential of current knowledge in strategy.

Research limitations/implications – The novelty and doability of the framework can potentially inspire research on a broad, community-wide basis, engaging PhD students and management faculty, improving knowledge, democratizing scholarship and deepening the societal footprint of strategy research.

Originality/value – Knowledge is open-ended. The more we know, the more we appreciate how much we don't know. But the lack of clear guidance on rigorous pathways along which new knowledge that advances both theory and practice can be created from prior knowledge has stymied strategy research. The paper's framework systematically pulls together for the first time the disparate elements of transforming past learning into new knowledge in a coherent epistemological whole.

Keywords Strategic management, Learning, Knowledge, Genetic argument, Knowledge open-endedness, Origins of knowledge

Paper type Research paper

What has been will be again, what has been done will be done again; there is nothing new under the sun. (Ecclesiastes 1: 9)

Introduction

The above quote ponders a profound question: Where does new knowledge come from? Everything new, including new ideas, has prior roots (Brandt and Eagleman, 2017); nothing comes from nothing. But if "there is nothing new under the sun," how to explain the explosive growth of the strategic management literature over the past fifty years? This growth can only be attributed to researchers creatively mining existing knowledge to generate new knowledge. As the stock of existing knowledge continues to multiply, so do the possibilities for fresh insights. Indeed, what we have learned to date may be merely the tip of the iceberg, with significant embedded potential in existing knowledge to promote future

learning and new knowledge (Kohtamaeki *et al.*, 2022; Kolb and Kolb, 2005). Korin *et al.* (2023) argued persuasively that such future learning can be theoretical as well as practical: "Learning occurs when past experience is captured from practice and *transformed via reflection* into new knowledge to be applied in practice . . .. In this light, experience lies in the past, and past experience enables future learning in strategy practice" (p. 283, emphasis added).

The question of how reflection can transform past learning into new knowledge therefore becomes central, and in response scholars have proposed several innovative approaches. Among them is Kaplan (1964), who suggested that knowledge can grow by "intension" or by "extension", comprising, respectively, a deeper focus on what we already know and extending existing knowledge into adjoining regions. Locke *et al.* (2008) emphasized the often-overlooked power of doubt as a generator of new knowledge in the theorizing process. And in the design literature, Hatchuel and Weil (2003) pioneered the C-K (concept-knowledge) theory, which aims to explain and model the process that produces new ideas and creative solutions.

These individually useful approaches remain fragmented, however. What is needed is a unified framework that systematically pulls together the piecemeal approaches into a coherent epistemological whole, offering researchers a menu of ideation pathways that link existing knowledge to its potential growth avenues. Such a framework would add enormous value and promote the advancement of both theory and practice of strategy.

Drawing upon two fundamental properties of knowledge that have received scant attention in the literature, I offer in this paper a framework of knowledge development pathways for creating new knowledge with greater depth, breadth and novelty. These fundamental properties relate to the origins and the open-endedness of knowledge.

A core principle in the theory of knowledge and knowability (e.g. Rescher, 2009; Williamson, 2000) is that (new) knowledge can only emerge from prior knowledge, since the capacity for knowledge is a shared and socially transmitted accomplishment. Knowledge, in other words, cannot emerge out of thin air: "There is no such thing as immaculate perception", observed Hargadon and Douglas (2001: 478). As Williams (2001) explained, "If acquiring new knowledge presupposes the possession of prior knowledge, how can the process of acquiring knowledge ever get started? Surely, some knowledge must be basic, if the process is to get off the ground. Call this the Genetic Argument" (p. 176).

This has major implications for strategic management research and practice; the body of accumulated knowledge can, if appropriately channeled, provide a fertile launchpad for developing new, relevant and useful knowledge. Two of the most serious and persistent critiques of the management literature concern the proliferation of theories with little connectivity to prior work (Hambrick, 2007; Pfeffer, 2014) and an overemphasis on empirical rigor to the relative neglect of theoretical and practical value (Haveman *et al.*, 2020; Baum and Haveman, 2020; Cronin *et al.*, 2021). Using extant knowledge as a springboard for organically developing new knowledge can effectively address each of these concerns.

A second important property of knowledge relates to an exciting feature of scientific research, the recognition that knowledge is open-ended. The more we know, the more we appreciate how much we don't know (hence the importance of systematically outlining for one's peers "future research directions" typically found at the conclusion of most research articles). Much as we cannot accept a hypothesis but merely fail to reject it, so we strive not for perfect theories but for less imperfect theories that improve understanding, better explain an infinitely complex empirical reality, and inform management practice. Epistemologist and philosopher of science Karl Popper (1963) expressed it well: "The more we learn about the world, and the deeper our learning, the more conscious, specific, and articulate will be our knowledge of what we do not know: our knowledge of our ignorance. For this indeed, is the main source of our ignorance – the fact that our knowledge can be only finite, while our ignorance must necessarily be infinite."

Taken together, the genetic argument and open-endedness of knowledge provide the means to address stubborn challenges and lead to meaningful progress in strategy research. This paper is organized as follows. First, key aspects in the growth of scientific knowledge are examined. Next, an epistemologically-grounded framework is presented for ideating the development of new knowledge along distinct pathways. The logic and impact of each pathway is illustrated by providing representative examples. The focus turns finally to the multiple, productive ways in which future strategy scholars can execute the research ideas in this paper on timely issues with broad socio-economic implications.

Growth of scientific knowledge

Scientific knowledge advances through an iterative interplay of theory and data. Theoretical conjectures are subject to empirical testing that may result in modification of the theory that, in turn, becomes the subject of the next round of empirical testing, and so on, in an endless pursuit of "improved knowledge." In management research, however, these twin pillars of theory *generation* and theory *testing* are highly unbalanced, with the latter dominating the former. "You might be tempted to conclude that data are everything and theory is nothing", lamented Haveman *et al.* (2020, p. 660), part of a long line of scholars who have emphasized the need to lift theory generation on par with theory testing in the accumulation of knowledge (Dubin, 1969; Reynolds, 1971; Eisenhardt, 1989; Locke *et al.*, 2008; Oswick *et al.*, 2011; Cronin *et al.*, 2021). Despite these pleas, a major gap persists between the rich, granular detail available on all aspects of empirical research versus the relative dearth of actionable guidance on generation of new theory.

Consistent with the genetic argument (Williams, 2001), Kaplan (1964) highlighted the need for connective tissue between the new and the old, and described how new theories are inseparably bound to the old theories that they are replacing: "Advances depend on the way in which each [new theory] takes account of the achievements of its predecessors .... Knowledge grows not only by accretion and the replacement of dubious elements by more sound ones, but also by digestion, the remaking of the old cognitive materials into the substance of a new theory" (p. 304) [1]. Two ways in which new knowledge may grow by building on past knowledge are by *extension* and by *intension*. In growth by extension, a relatively full explanation of a small region is carried over to an explanation of adjoining regions. Growth by extension is implicit in the metaphors of science as an edifice, a mosaic, a jigsaw puzzle – it is built up piece by piece.

In growth by intension a partial explanation of a whole region is made more and more adequate. Darwin, Marx and Freud, for example, didn't so much definitively explain a limited subject-matter which was later enlarged, but rather, laid out lines for subsequent theory and observation to follow, to yield a better understanding of the broad-scale phenomena which were their primary concern. Metaphors for growth by intension include bringing binoculars to a sharper focus or gradually illuminating a darkened room – progress is not piecemeal but gradual on a larger scale.

A third way in which new knowledge may be created is non-linear thinking. Whereas intension and extension apply objective, logical criteria to deepen and expand existing knowledge, non-linear thinking relies on ideation pathways that test the limits of what is known and possible. Fresh ideas, original solutions, innovative products often result from non-linear thinking.

Figure 1 presents a framework of these ideation pathways – intension, extension, non-linear – each of which adds knowledge and also transforms what was previously known, clarifies it, gives it new meaning as well as more confirmation. Such elaboration and dissection of the theory generation process is urgently needed in management research because, as Weick (1989) correctly argued, "Theory cannot be improved until we improve the
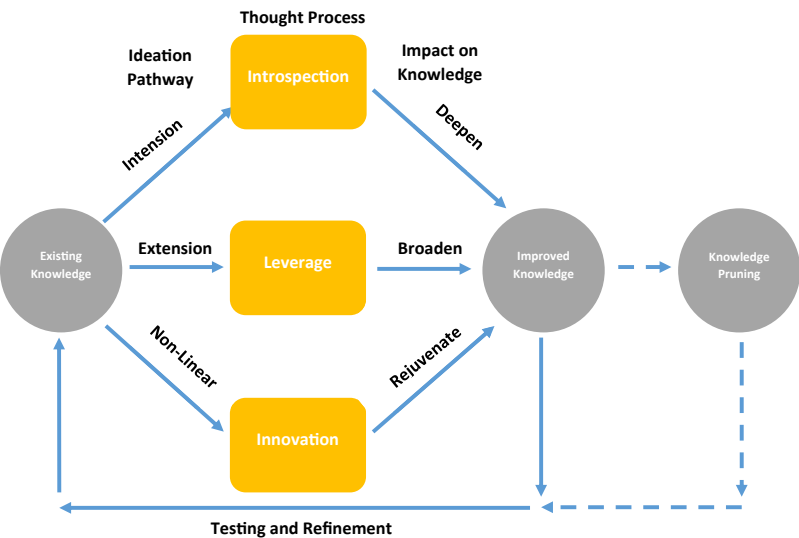
Figure 1.
A framework for
ideating knowledge
development pathways

Source(s): Author's own work

theorizing process, and we cannot improve the theorizing process until we describe it more explicitly, operate it more self-consciously, and decouple it from validation more deliberately" (1989, p. 516). This framework elaborates the possible pathways along which new knowledge can be generated from existing knowledge, as discussed next.

A framework of ideation pathways

Intension: deepening knowledge through introspection
Think inside the box: Groundbreaking new ideas often come from old ideas, such as Tesla's use of an invention first built in 1837, to manufacture modern electric cars (Poole, 2016). Boyd and Goldenberg (2013) described how a contact lens, exercise bicycle and ATM machine all have one thing in common: the elimination of seemingly essential elements (the frame, the rear wheel and the bank employee, respectively). Thus, deeply examining the extant body of knowledge in strategy, and working within the constraints of what we already know, can yield powerful new insights.

In the management literature, for example, the key breakthrough to Staw's (1981) Escalation of Commitment Theory was that commitment to a course of action is tied to the motivation to justify previous decisions, norms for consistency and expected value calculations. Staw investigated the familiar but poorly understood phenomenon of escalation of commitment (sometimes called "throwing good money after bad") by developing a research design that separated economic and psychological reasons for continued investments following negative feedback. In other words, thinking deeper "inside the box" of commitment escalation led him to novel and important findings.

Relax assumptions and boundary conditions: Bacharach (1989) noted that a theory is a statement of relationships between units observed (variables) or approximated (constructs) in the empirical world, in which constructs are related to each other by propositions and variables are related to each other by hypotheses; the whole system is bounded by the theorist's assumptions about values, time and space. The last point about assumptions is

important but often fades into the background. A good example is game theory, founded by John von Neumann in the 1940s. As a "grand theory" (Merton, 1967) game theory was initially abstract and highly generalizable, with restrictive assumptions and boundary conditions that limited its practical usefulness for several decades. But as researchers began to relax the assumptions, the theory's usefulness in solving real-life problems soared, with three game theorists sharing the Nobel Prize in 1994. Today, game theory is widely used in collective bargaining, international security negotiations, auctions and numerous other applications.

Parkhe (1993) demonstrated the power and applicability of game theory in an empirical study of global strategic alliances and highlighted the importance of the theory's context-specific behavioral, environmental and cultural assumptions. The validity of assumptions in a given place may shift over time, and at a given time assumptions may not be equally valid in all cultures. The lack of applicability of ethnocentrically derived game-theoretic and economic formulations provides research opportunities to examine what higher-order rules of the game govern the behaviors of international firms from diverse nationalities partnering in strategic alliances. Thus, explicitly stating, and carefully evaluating, a theory's assumptions and boundary conditions can lead to important new insights.

Extension: broadening knowledge through leverage
Management knowledge is not an island; it resides, rather, in a knowledge network that includes other sub-fields of business, non-business fields and practitioners. Each of these areas possesses its own vast literature and potential to contribute valuable insights. Unlocking the specialized expertise of areas outside of management scholarship can leverage existing management knowledge, broadening it and opening another promising pathway for new knowledge creation.

Such a shift, away from strictly *intra*-disciplinary research questions and toward new bodies of knowledge, is precisely what Fini *et al.* (2022) found in their investigation of what happens when a scholar/researcher launches a business venture, becoming an entrepreneur/manager. In these cases, the individual's attention turns toward other knowledge areas more relevant for downstream technology development. The clear implication is that actively, purposefully exploring inter-disciplinary avenues for new, relevant knowledge creation can be advantageous for management scholars, as discussed next.

Translational research: New knowledge can be created not only by scholars seeking to advance theory but also by managers seeking to improve organizational performance and competitive positioning (sometimes called incremental or radical innovation). As Pfeffer and Sutton (1999) wrote, practitioners, too, are "creators of knowledge": "One of the most important insights from our research is that knowledge that is actually implemented is much more likely to be acquired from learning by doing than from learning by reading, listening, or even thinking . . .. Taking action will generate experience from which you can learn" (1999, pp. 5–6).

And once created, new knowledge can readily migrate from practice-to-theory or theory-to-practice, benefiting both. This fungibility of knowledge across a porous theory-practice interface, where practice can inform theory and vice versa, is memorably captured by Lewin's (1945, p. 129) oft-cited maxim, "Nothing is as practical as a good theory." Translational research builds a bridge between academics and practitioners, translating business challenges to research scholars and translating research insights to business leaders. Freeman (2005), in an illustration of the practice-to-theory directionality, got exposure to on-the-ground strategic management realities and practical insights through his consulting engagements with executives; these led to his creation of the Stakeholder Theory. Conversely, Porter's Five Forces framework for analyzing the operating environment of a business provided a useful starting point for many strategy consultants and practicing managers.

Import business sub-fields: Great opportunities for new knowledge creation lie in Prahalad and Hamel's (1990) insight that a company's core competence is not about excelling in any single activity in the value chain, but rather in the *bundling* of knowledge and skills that reside in various parts of the company: diverse groups of employees within a company (e.g. R&D, engineering, marketing, finance) coordinating their efforts in a customer-facing way to gain a sustainable competitive advantage. Although we may compartmentalize business into subject areas, or academic departments, or specialized journals, ultimately management and other sub-fields of business are cut from the same cloth, necessitating an integrated assessment. The importance of this interrelatedness of all sub-fields of business was vividly driven home during the Covid-19 pandemic when global supply chains essentially froze; in response, many corporate executives urgently sought permanent alternatives to China, along with entirely rethinking their over-dependent and undiversified sourcing, manufacturing, logistics, sales and distribution practices. Thus, proactively joining forces with other sub-fields of business can broaden management's reach into cognate areas of knowledge in mutually beneficial ways.

Import non-business fields: In developing her Psychological Contract Theory, Rousseau (1995) blended insights from industrial psychology, sociology, clinical psychology and economics to propose that individuals hold beliefs regarding a mutual exchange agreement with an employer, based on promises implicit or explicit, which over time take the form of a relatively stable mental model. This is an example of how theorizing in the context of management can profit from research in non-business fields. Micro-level management scholars, whose focus includes human resource management, organizational behavior and group dynamics, tend to draw heavily upon the field of psychology, with *Personnel Psychology* and *Journal of Applied Psychology* among their top target journals. Macro-level scholars, who study corporate strategy, competitive strategy, global strategy and industry-level issues, draw heavily upon economics and sociology. This is useful. It leaves untapped, however, many other areas of knowledge that can potentially transform management by making it theoretically better grounded and practically more relevant.

Consider, for example, history, which is often mentioned in passing as "history matters". Jones and Khanna (2006) pushed this thought further by arguing that business scholars must go beyond the rhetoric of history matters, to explaining *how* it matters, and proposed four conceptual channels through which history can be shown to matter, and therefore, history's impact on management theory and practice can be more fruitfully evaluated. Similarly, Jackson and Deeg (2008) agreed that "institutions matter" but wished to explore *how* institutions impact business. In an article that won the 2018 Decade Award for the most influential paper of the past decade in the *Journal of International Business Studies*, these authors compared and contrasted various forms of capitalism, drawing on literature in sociology and political science to examine the topography and diversity of institutional landscapes.

Management is said to be a "social science", which is broadly the scientific study of human society and social relationships (how people interact with one another) and comprises a group of academic disciplines that focus on how individuals behave within society. The main branches of social sciences are anthropology, economics, political science, psychology and sociology. Also included are history, law, criminology, linguistics and communication science. As noted, management research borrows concepts and insights from only a select few of these knowledge areas (primarily economics, psychology and sociology). The impact of other knowledge areas is understudied and largely ignored, providing excellent opportunities to broaden management knowledge.

Non-linear: rejuvenating knowledge through innovation
The two pathways for new knowledge creation discussed above apply objective, logical criteria of intension and extension (Kaplan, 1964) to deepen and broaden existing knowledge.

But many situations, such as those involving puzzling inconsistencies, paradoxes and dilemmas, do not lend themselves to objective, logical criteria. The management literature is full of cases variously known as equivocalities ("multiple or conflicting interpretations of an event", Daft and Lewin, 1990, p. 5) and anomalies ("phenomena for which his paradigm had not readied the investigator", Kuhn, 1970, p. 57). Poole and Van de Ven (1989) observed that little attention has been paid to the types of knowledge-creation opportunities offered by such tensions, oppositions and contradictions of the same phenomenon. Two brief examples serve to illustrate this point.

Locke and Latham (1990) found three conflicting theories in the then-existing literature on expectancy and performance. One theory predicted that a person's motivation is highest when task difficulty is 0.50, suggesting a curvilinear (inverted-U) relationship between goal difficulty and performance. The expectancy theory, in contrast, predicted a positive, linear association between expectancy and performance. But difficult goals are harder to attain than easy goals, so there was a negative linear relationship between expectancy of success and performance. All three theories could not be correct. Locke and Latham resolved these contradictions by proposing their influential Goal Setting Theory, which asserts that *within* a group expectancy theory's positive linear relationship is correct, whereas *between groups*, when goal level is varied, the relationship is negative, and thus apparent contradictions are resolved.

Similarly, Ronald Coase (1937) posed a basic question that economists had missed: If markets are so good at directing resources, why do firms exist? Why are some activities directed by market forces and others by firms? Coase's question led to the birth of transaction cost economics and, eventually, a Nobel Prize in 1991; his Nobel citation read in part, "for his discovery and clarification of the significance of transaction costs and property rights". Such "informed naivete" can be easily extinguished and, therefore, must be deliberately cultivated. Barney (2005) cautioned scholars, especially junior ones, to understand the literature thoroughly, then "learn to ignore that which you've learned, because prior literature is both a guide and a blinder. Knowing the literature too well can actually prevent one from generating new insights" (p. 296).

Though often overlooked, equivocalities and anomalies abound, and they present important research opportunities; this requires a third ideation pathway: non-linear thinking. Finding provocative questions, and answering them, involves the use of Albert Einstein's favorite research tools, *gedankenexperiments* (thought trials), that can lead to illuminating moments of inspiration, also called epiphany, flash of insight, Eureka!, stroke of genius and light bulb moment. Three ways of innovatively addressing "wicked" problems that exist within the current body of management knowledge are abduction, design thinking/design science and fuzzy logic.

Abduction: How do we draw inferences? The two familiar forms of logical inference are Leibnizian deduction (reasoning about what must follow from certain premises or facts) and Baconian induction (reasoning about what is likely to happen in the future given what we know about the past). Philosopher Charles Sanders Peirce introduced a third, non-traditional form, called abduction, which involves reasoning from the data you have, to what could have been the cause of those data. Thus, abductive reasoning is the "development of initial explanation" (Behfar and Okhuysen, 2018, p. 232), or reasoning that offers a potential path for developing plausible explanations, subject to testing, for phenomena that are not explained by existing theories. Abduction is used in two senses, first as explanatory reasoning in *generating* hypotheses (Saetre and Van de Ven, 2021; Stanford Encyclopedia of Philosophy, 2021), and also as explanatory reasoning in *justifying* hypotheses, also known as "inference to the best explanation" (Poole, 2016).

Locke *et al.* (2008) focused on the salience of doubt in driving the research process. Doubt, these authors argued, is the engine that powers the imagination that is central to theorizing,

so "the question is not whether, but *how* to engage doubt" (p. 908). This article unpacks the generative potential of doubt (i.e. the potential of doubt to induce fresh insights not accessible via traditional processes of objective, rational thinking), by proposing three "strategic principles" of ideation: [1] turn toward/embrace not knowing; [2] nurture hunches and [3] disrupt the order. When we highlight only the Eureka! moment, we essentially collapse, hide and minimize the important thought process of growing theory, since we obscure the work necessary in creating the conditions for insights to occur, including the occasions in which "observations are made, hunches occur, ideas are developed, tried out, set aside, transformed, and so on" (p. 916). Shani *et al*. (2020) described how, when confronted with an array of puzzling and paradoxical data, production of new, actionable knowledge requires the use of abductive reasoning.

Further, abduction can transport us beyond what is true – the actual – to the vast class of possibilities that *could* be true – the counterfactual – to ideate innovative solutions (Marletto, 2021). (*Factual:* She forgot to set the alarm, and consequently, was late. *Counterfactual:* If she had set the alarm, she would have been on time.) Folger's (2005) Fairness Theory, for example, relied on counterfactuals (what did *not* happen) to develop a theory explaining accountability for blameworthiness in relation to what would, could, should have happened. However, according to Shtulman (2023), the problem with counterfactuals is that when contemplating alternatives to reality, we tend to fixate on possibilities that are physically plausible, statistically probable, socially conventional and morally permissible. Generative doubt involves transcending conventional thinking and engaging in intellectual risk-taking that may lead to dead ends and other temporary frustrations familiar to scholars engaged in idea entrepreneurship. As Henri Poincare wrote in a recommendation letter for Einstein, the latter "is one of the most original thinkers that I have ever met . . .. Since he seeks in all directions, one must expect the majority of the paths on which he embarks to be blind alleys" (quoted in Locke *et al*., 2008, p. 914).

Design thinking/design science: This distinctive approach to knowledge creation differs from that of explanatory science (Simon, 1969) in that it takes the initial problem only as a starting point, not as a final statement, and drills deeper by iteratively seeking to understand the users (empathize), challenge assumptions (define), redefine problems (ideate), create innovative solutions (prototype) and test the solutions. Root-cause analysis and the Five Whys approach seek to uncover the real issues underlying a problem and, based on this understanding, identify solutions which reflect the genuine constraints of a particular situation (Rindova and Martins, 2021; Romme, 2003).

Dimov *et al*. (2023) argued in their editorial that in entrepreneurship research, design science is an inclusive approach that combines relevance with rigor. It enables researchers to go beyond their traditional roles as observers and analysts of established artifacts to actually help design new artifacts, thus creating new knowledge that improves both the theoretical depth and practical relevance of research.

Just as generative doubt (Locke *et al*., 2008) seeks to mine doubt as a potential fountain of new ideas, concept-knowledge (C-K) theory or C-K design theory (Hatchuel and Weil, 2003; Hatchuel *et al*., 2017) mines the interaction between the concept space and the knowledge space to explain and model innovations and inventions. C-K theory has been applied in several industrial contexts and has also inspired new management principles for collaborative innovation, with the aim of overcoming the limitations of standard design management methods.

C-K theory seeks to formally describe creative thinking, in particular, its generative and expanding logic, by drawing upon the genetic argument (Williams, 2001) that all new ideas have prior roots. Thus, a designer innovates by mapping "new" concepts in the C-space and all the knowledge one uses or needs to imagine or design new concepts in the K-space. Both spaces are expandable with the addition of new concepts and new knowledge, and these

expansions are complementary: new knowledge provokes the identification of new concepts and elaboration of new concepts leads to acquisition of new knowledge.

Fuzzy logic: Vagueness and uncertainty inheres in much of the data and information consumed by managers to make decisions. And yet our analytical techniques typically treat data and information as in Boolean algebra, where the truth values of variables are *true* and *false*, usually denoted by 1 and 0. Fuzzy logic allows for the representation of vagueness and uncertainty by using a type of mathematical logic involving linguistic variables and "approximate reasoning", where the values of variables may be any real number between 0 and 1, in order to handle the concept of "partial truth", where the truth value may range between completely true and completely false. Fuzzy models strive to recognize, represent, manipulate, interpret and use data and information that are vague and lack certainty.

Bayes's theorem is a way to determine the likelihood of a hypothesis given certain evidence. But we typically think about things the other way around: If something is true, how likely are we to see a certain result? McCann and Schwab (2023) employed the Bayes logic to provide guidance to strategic management researchers on the advantages of Bayesian approaches and to outline the steps involves in conducting and reporting Bayesian analysis. Hutson (2023), however, noted that calculations based on prior beliefs become inherently subjective because evaluations of reality are always best guesses.

Growing segments of business and industry, particularly those with complex and dynamic environments, are using fuzzy logic in various ways, including in consumer electronics (e.g. cameras), appliances (e.g. washing machines), traffic control systems, financial decision making, temperature control systems, product quality management, customer relationship management and more. In supply chain management, for example, fuzzy logic is applied to forecast demand more accurately by handling the uncertainty and imprecision associated with market trends, historical data and external influences; and to optimize inventory levels by considering factors such as demand fluctuations, lead times and supplier reliability, to maintain an appropriate balance between overstock and stockouts.

Future research directions
Organizations are the dominant form of social structure; the mind-boggling variety of organizations includes corporations, government, non-profits, religious, social, political, cultural, charitable, labor unions, educational and so on. Improved theoretical knowledge and better management that results in greater effectiveness and efficiency of all of these types of organizations would have great societal benefits. Indeed, McGahan (2022, p. 25) eloquently stressed the urgency of applying strategy scholarship to today's great challenges: "Progress [in strategic management research] is impeded just at a moment in history when the most important problems of our time – climate change, the pandemic, mass migration, authoritarianism, economic nationalism, among others – raise the stakes on strategic insights for making progress beyond what our field has ever known. We must take on these challenges together as an intellectual community before it is too late for organizations and institutions to work together to accomplish what is needed to secure the future of democratic capitalism as a system for organizing social and economic life". Implementation of this guidance can proceed in two interrelated ways.

Left side of Figure 1: Future opportunities for high-impact research in strategy and management include *community-wide adoption* of the framework, where large numbers of management faculty and PhD students critically examine the current literature and ideate "intensionally", "extensionally" or "non-linearly" to generate new knowledge that is deeper, broader and more novel. Such democratization of research, in turn, can inform improved practice, galvanizing a virtuous spiral of stronger management theory, and higher organizational performance, at an unprecedented scale.

Right side of Figure 1: New knowledge generated at an unprecedented scale in a broad variety of contexts, covering diverse strategy topics, will certainly be a boon. But it may also create the challenge of integrating the newly gained insights in ways that move the field forward. To address the potential problem of "overabundance" of theories, Leavitt *et al.* (2010) proposed theory pruning, defined as hypothesis specification and study design intended to bound and reduce theory. These authors proposed criteria for determining when it is appropriate to test theories or parts of theories against one another, suggested hypotheses for testing competing theories, and provided "reductionist strategies" appropriate for the organizational sciences. Thus, the greater volume of new knowledge can be funneled into a narrower set of generalizable knowledge streams.

More broadly, significant future research opportunities await management scholars to contribute to new knowledge development in timely areas with major socio-economic implications. A good template is provided by Jay Barney, perhaps best known for his work on Resource-Based Theory (1991). His research question, at least on the surface, has always been "Why do some firms outperform others"? But as Barney (2005) explained in personal terms, going back to his childhood, his intellectual journey began with deeper questions of inequality: its existence, its justice, its broader implications. Put another way, Barney linked inequality to firm performance to the resource-based theory. Today's profound developments in digital, green and innovative economies are no less filled with opportunities to study their theoretical and practical implications for management, including in inequality, social justice and access to resources and opportunities.

Ivushkina *et al.* (2021), for instance, focused on the contradictory socio-economic impacts of the digital economy, observing that the widespread use of digital technologies can increase labor productivity, reduce costs and create new jobs on one hand, and on the other, can also increase unemployment in some sectors, create monopolization of markets, and increase income disparities. What insights might present-day management scholars studying digital economies gain from the thought-leading work of Barney (2005)?

On a related note, Venkitachalam and Schiuma (2022) examined how management of customer experiences and operational efficiencies are being impacted by digital technologies, concluding that companies need to not only adopt, develop and integrate new technologies of the digital era, but further, must evolve their business models and redefine their value creation strategies. Similarly, Peter *et al.* (2020) surveyed digital transformation across a sample of 1,854 Swiss organizations to establish a reference framework based on the strategic action field (SAF) theory. Each of these leading lights points the way to timely and relevant future research.

Socio-economic impacts of the green transformation are likely to be profound as well, with huge growth opportunities for companies that are able to transform themselves to serve the rapidly growing global market for sustainable solutions. The shift to a low-carbon economy, likely to gain further momentum with COP28's landmark agreement in December 2023 involving more than 200 countries to transition away from oil, gas and coal use, will challenge companies with fossil fuel-reliant business models and cost jobs. However, moving to a net zero emissions world is also estimated to bring net benefits for employment and the economy, with accelerated climate action creating 37 million jobs by 2030 and a boost in global GDP of 2.5% by 2050. New knowledge is needed in management to address how policymakers, industry and workers can ensure a just transition where individuals and communities are not left behind in the shift from fossil fuels toward renewable and sustainable technologies.

Finally, the innovation economy is visibly reshaping economic growth and prosperity on a global scale. Technology giants (e.g. Apple, Google), unicorns (e.g. Uber, Airbnb) and thousands of startups worldwide are exemplars of the economic potential unleashed by innovation. Through their groundbreaking technologies and services, these companies create jobs, raise productivity and drive economic growth (e.g. Silicon Valley, Bangalore). Clearly, the innovation economy benefits from, and in turn drives, the digital transformation, where countries transition from dependence on finite resources (such as oil) to potentially infinite

resources (such as human creativity and information). New management knowledge is needed to better understand and address the inevitable challenges accompanying the innovation economy, including income inequality, automation-induced job displacement and importantly, privacy concerns.

Conclusions

The real voyage of discovery consists not in seeking new landscapes, but in having new eyes. (Proust)

Building on two foundational epistemological principles – that new knowledge can only emerge from existing knowledge (Williams, 2001) and that knowledge is open-ended (Popper, 1963) – this paper presented a unified framework of ideation pathways through which new knowledge can be created. Each pathway was illustrated with select examples. This is a representative, not comprehensive, list of examples, and many additional ways within each pathway are possible. Indeed, the power and potential of Figure 1 lies in shedding new light on the understudied *process* of new knowledge creation and thereby encouraging broad, community-wide adoption of this framework in order to accelerate future research that is organically linked to, and usefully extends, past work.

The vast body of existing knowledge in management is a proverbial gold mine, containing insightful contributions from thought leaders. As Sir Isaac Newton reportedly said, "If I have seen further, it is by standing on the shoulders of giants." Creating new knowledge in management by building on past work is indeed standing on the shoulders of these giants. The difference, however, is that gold mines eventually run out of gold but management knowledge can grow and improve indefinitely. Figure 1 attempts to provide the "new eyes" through which existing knowledge can be viewed to systematically unpack the process of new knowledge creation and raise theory generation on equal footing with theory testing.

Among the stated aims of the *Journal of Strategy and Management* are the following:

(1) Improving the existing knowledge and understanding of strategy;

(2) Encouraging new thinking and innovative approaches to the study of strategy.

These aims share an important intersection: new thinking and innovative approaches, when applied to *existing knowledge,* can yield improved knowledge, deeper understanding and better practice of strategy. By merging insights from strategy and epistemology, the present paper demonstrated how such improved knowledge of strategy might be achieved.

Note

1. Kaplan (1964) illustrated this point with telling examples: (1) The realization that some of the so-called "nebulae" are not really nebulous but enormously distant galaxies of stars in their own right not only generated new conceptions of the stellar universe, but also changed the conception of our own Milky Way. (2) Freud's discernment that there is really a commonality between the lunatic, the lover and the poet not only disclosed the method in madness but also altered our understanding of the workings of the unfrenzied imagination.

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